Institute
Vienna Circle
Society for the
Advancement of
Scientific World View

HOPOS 2000

Third International History of
Philosophy of Science Conference
Vienna, July 6-9, 2000

Abstracts

HOPOS 2000

ABSTRACTS

Location

Conference Location:
University Campus
Alserstrasse/Spitalgasse
1080 Wien

Conference Office:
Institut für Zeitgeschichte/
Dept. of Contemporary History (IFZ)
Court 1: IFZ 1st floor

Book display:
Court 2: Room C

Lecture Rooms:
Court 1: Aula
Court 2: Room A
Court 2: Room C
Court 1: Seminar Room 1, IFZ 1st floor

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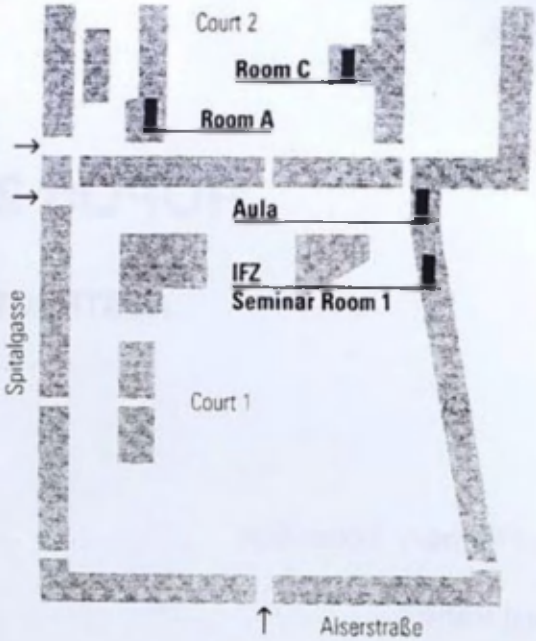

INVITED PLENARY SPEAKERS

Maria Luisa Dalla Chiara (University of Florence, I)

What is a Law?

The term "law" has been used in different contexts with different meanings. We speak of "natural laws", "legal laws", "moral laws", "historical laws", "aesthetic laws", and so on. Is there any deep common root among these different uses and meanings? We will try and answer this question by using some logical tools from the possible world semantics.

The modern notion of physical law can be formally reconstructed in the framework of a semantic approach to empirical sciences (which includes an abstract description of measurements, operational definitions of physical quantities, approximations, ...).

What can be said about the relationships between natural laws and legal norms? At first sight, one could try and explain the main difference in terms of logical modalities. The operator *Physically necessary* (or according to a physical law) seems to represent a typical alethic modality characterized by the principle: Necessary A implies A. In other words, laws of nature cannot be escaped! The operator *Legally obligatory* is, instead, a typical deontic modality: *Obligatory A* does not generally imply A. Norms are possibly violated!

However, the situation is not so simple. In fact, similarly to the case of deontic semantics, physicists often refer to idealized physical situations where some real parameters, which do have an important role in the actual physical world, are neglected. As a consequence, the notion of legal norm and of natural law turn out to be less far from each other than it is usually supposed. Similar arguments can be developed for the notions of historical truth and of historical law.

Michael Friedman (University of Indiana, USA)

On the Idea of a Scientific Philosophy

(= 8th Vienna Circle Lecture and Keynote Lecture)

In the course of an historical discussion of a variety of episodes within the tradition that came to identify itself as "scientific philosophy" (beginning with Helmholtz and the Neokantians and continuing through the logical empiricists) I consider a number of conceptions of the relationship between philosophy and the sciences: that philosophy is itself a branch of natural science (e.g., of cognitive psychology); that it is a branch of mathematical science (of mathematical logic); that philosophy, while not itself a science, should nevertheless strive to be "scientific." Appealing to the theory of scientific revolutions developed by Thomas Kuhn, I argue that none of these conceptions is fully adequate, and that philosophy, although it certainly interacts fruitfully with the sciences, should not strive to be scientific in any of the above senses. The result is a new conception of the relationship between philosophy and the sciences that emerges naturally from the historical tradition with which I began.

Marina Frasca-Spada (University of Cambridge, UK)

Hume and Sense Impressions

This paper is devoted to the nature and roles of sense impressions in Hume's account of perception. I start by considering how Hume introduces sense impressions at the beginning of the *Treatise*. My purpose is to show that even though he explains the distinction between impressions and ideas on the basis of their different strength and liveliness, the crucial difference between them is in fact that ideas are copies of impressions, while impressions are their originals" and do not, in turn, copy anything; they

are what ideas represent, the objects of our thought. This reading of Hume's sense impressions gives rise to two problems: the first is, if impressions do not represent anything, how can Hume talk about "objects" at all - in fact, what are Humean "objects"? Dealing with this problem is the main subject of the paper. The second problem I only consider in a tentative manner at the end, and has to do with the difference between feeling and thinking: why does Hume give such prominence to the different strength and liveliness of impressions and ideas, if their distinction is, in fact, based on their original-to-copy and object-to-representation relation?

Lothar Schäfer (University of Hamburg, D)

Neokantian Origins of Modern Empiricism

According to Popper, critical rationalism owes much to Kant's philosophy, and he explains his own position vis-à-vis the Vienna Circle as a repetition of Kant's reply to Hume's sceptical challenge of the epistemic status of natural science - I shall show: (i) Popper's reading of the *Critique of Pure Reason* is in fact transformed by Neo-kantian positions; and (ii) the program of logical empiricism should be seen, too, as a transformation of Kant's system, rather than a continuation of Hume's approach.

SPECIAL PANELS

Special Panel A: (in German)

The Political Meaning and Cultural Context of Philosophy (of Science) in Austria and Central Europe before and after World War II

Friedrich Stadler (University of Vienna)

On the Political Meaning and Cultural Context of Logical Empiricism

The range of political positions and attitudes of the Vienna Circle's members is widespread: seen from a traditional perspective it reaches from the conservative-liberal label (with Karl Menger, Felix Kaufmann, and Moritz Schlick) via the social-liberal one (as Viktor Kraft's), to the left wing socialist approach (e.g., of Rudolf Carnap, Hans Hahn, Philipp Frank, Otto Neurath, and Edgar Zilsel) between the Wars.

One may characterize the selfunderstanding as an 'autostereotype' with overlapping liberal and socialist Weltanschauungen and Scientific World Conceptions, which both - independently from their apparent differences – have been marginalized by the predominant sociopolitical and academic elites (sympathizing with and/or adhering Austrofascism as well as National Socialism). Accordingly the 'heterostereotype' image of the Vienna Circle/Logical Empiricism during the First Republic was composed by negative philosophical and political elements which where linked to the antagonisms of metaphysical vs. antimetaphysical philosophy and science and autocratic vs. democratic cultures. Thereby an explicit and implicit racist, antisemitic discourse served as a common ideological background.

However, this prejudice emerged both from the German-Nationalist (Nazist) and Christian-Social movements employing antisemitism and anti- science as useful tools for their struggle against rationalism and empiricism, or against the hated liaison of relativism and liberalism. The result was the construction of a "value-free positivism" as opposed to the so called 'German Philosophy and Science' ("Deutsche Philosophie/Wissenschaft"). This trend, which inevitably led to forced migration and destruction of the proponents of LE in Central Europe, can be paradigmatically exemplified by the assassination of Moritz Schlick, especially by the public reactions to it - before and after World War II. Thus there appears a remarkable continuity from the First to the Second Republic - partly becoming topical also in the context of the recent political situation in Austria.

Hans-Joachim Dahms (Currently University of Vienna)

The Absence of the "Scientific World-Conception" from Middle Europe after 1945. Causes and Consequences

None of the expelled and emigrated members of the Vienna, Berlin and Prague group of logical empiricists ever came back to their former homes and teaching positions in middle europe. Only from the beginning sixties began a certain restricted reimport of their teachings. This is all well known and documented. But what were the causes for these processes? Were the emigrants simply not invited to come back? Were perhaps some invited, but did not want to come back, and for what reasons? I try to answer these questions on the basis of unpublished archival material.

"Positivism" was portrayed since the beginning sixties in some influential philosophical and sociological quarters - especially in Germany - as a sort of technocratic ideology. With regard to the old days of logical positivism in the 20ties and 30ties this characterization would be certainly wrong, as the extensive relations of the Viennese and Berlin circles to their surroundings in culture and politics show. But perhaps these relations had not been made explicit enough in the teachings of empiricist writers, so that these aspects could have been forgotten in the meantime? Or "positivism" underwent a change itself from a cultural expression of "Red Vienna" to a technocrat ideology in the aftermath of the Second World War? I answer the first question with a "yes". The second question needs a more elaborated treatment, in which the hypotheses of both such a change in the USA and of an added selective reception of a transformed version of Logical Empiricism by its followers in Austria and Germany in the 50ties and 60ties must be examined. I conclude with some remarks on the cultural and political possibilites and necessities of a renovated "Scientific World-Conception" in the present political situation in middle Europe.

Kurt Rudolf Fischer (University of Vienna, A)

Philosophy in Austria and the United States since 1945

My assertion consists in the claim that modern analytical and scientific philosophy - predominant in the English-speaking countries in the last century - has not died a natural death in Austria, at the University of Vienna, or in Vienna where it originated; its demise was due to undemocratic procedures and political intervention. At first I am presenting an historical sketch of the course of "Austrian philosophy" (Rudolf Haller), and then I continue with the impact of that philosophy on American philosophy, and its merger with the home-grown pragmatism. American philosophy also absorbed British philosophical content, and the style of discussion practiced traditionally in Cambridge and Oxford. There are two main historical lines - international in character: one proceeds from Frege, goes to Russell and then Carnap, and reaches its apex with Quine; and another goes from Frege and Russell and Moore, and goes to Wittgenstein, and to some "ordinary-language"-philosophers such as Ryle and Austin. I am concluding with remarks on how the tradition under consideration: analytical and scientific philosophy, was stopped in the "Staendestaat" of Dollfuß and Schuschnigg, continued to be excluded in the "Third Reich", and - most importantly - never, even after "denacification" in Austria, regained a foothold within the University of Vienna due to the power politics of two chair professors of philosophy.

Gernot Heiss (University of Vienna, A)

Philosophy at the University of Vienna from the First to the Second Austrian Republic

When Arthur Pap stated in the 1950s that the members of the Department of Philosophy in Vienna were now among themselves in their unprecise ways of thinking and talking, almost unencumbered by rules, he was describing(from a very subjective perspective, to be sure) a situation that had developed already before 1938. This development had reached its climax in the discussions about the follower of Moritz Schlick, who had been asssassinated at the Vienna University in 1936, and finally the replacement of the chair (formerly held by Mach and Boltzmann) for 'History and Theory of Inductive Sciences' by one for the 'History of Philosophy'. In 1938/39 we see a break with this development: with the appointment of Arnold Gehlen and Gunther Ipsen a sociological, interdisciplinary school from the "Altreich" prevailed which proved its adaptability to Nazi-ideology by its biological and anthropological approaches and its political usefulness for imperialistic plans of expansion to Eastern Europe. In 1945, after these persons had been dismissed, the old line of the pre-1938 period has been re-established.

Special Panel B: (in English)
The Political Past and Future of the Philosophy of Science

Don Howard (University of Notre Dame, USA)

Vertrieben und Verirrt: Politics and the Philosophy of Science in Exile

In both its Viennese and Berlin forms, pre-World War Two philosophy of science was the creation of thinkers for whom science and the philosophy of science served progressive political ends, whether it was a Marxist socialist like Otto Neurath or a centrist liberal like Moritz Schlick. The same was true of the pre-war North American theory of science of the democratic socialist John Dewey. But after the war, when emigre champions of logical empiricism and scientific philosophy worked together with their North American colleagues to create a new, "professionalized" academic discipline, there emerged a socially disengaged, seemingly politically neutral philosophy of science. After a review of the pre-war situation, this paper seeks to reconstruct the post-war debate over the political situation of the philosophy of science, in both a North American and a British context, the principal aim being to understand the reasons for the failure of Philipp Frank's efforts, in the tradition of Neurath, to promote a socially engaged conception of the philosophy of science, a science-friendly left-liberal empiricism. Central to the analysis will be the effect of the experience of exile in encouraging the insulation of science and the philosophy of science from politics, through devices like Hans Reichenbach's distinction between the context of discovery and the context of justification. But other relevant factors include the "end of ideology" temperament then spreading within North American academic life as a response to 1950s anti-

communist persecutions, the "mainstreaming" of the philosophy of science as a professional academic discipline, as new departments, centers, and institutes were established in the 1950s, the influence of funding sources, ranging from the newly-established National Science Foundation, to the Rand Institute, and various private foundations, such as the Ford and Olin Foundations, and, finally, the right-wing critique advanced by thinkers like F.A. Hayek and Karl Popper. Irony and tragedy characterize the political transformation of the philosophy of science. There is irony in the fact that what first won a place in the academy for the philosophy of science was the perception that it could play a crucial role in defending progressive values of freedom and democracy against totalitarianisms of the right and the left, whereas what emerged was a thoroughly apolitical academic discipline. There is tragedy in the fact that a socially disengaged philosophy of science no longer has anything interesting or helpful to say about the place of science in human affairs.

Alan Richardson (University of British Columbia, CDN)

Tolerance, Internationalism, and Scientific Community in Philosophy: Political Themes in the Philosophy of the Vienna Circle and their Contemporaries

A striking feature of philosophical (as well as historical and sociological) reflection on science in the early part of the twentieth century was its explicitly political character. Just as science as a body of knowledge and a method of inquiry was taken as epistemically worthy of imitation, scientific community was taken as the socially and politically most worthy of communities. Writers from Dewey to Merton, from Popper to Neurath, agreed that the scientific community served as the model of a progressive and democratic social order, and this political lesson was a good portion of the reason for the recommendation of a scientifically-organized philosophy. This view was, of course, not universally accepted at the time, and the many of today's sociologists of science are deeply skeptical of this view of scientific community. Rather than argue directly for or against current sociology of science, I will explore the views of the logical empiricists – and those of Popper, Merton, and Dewey – in their contexts, considering how and why in the 1930s science might appropriately have been thought of as a crucial weapon in the fight against fascism and intolerance. The history of the Vienna Circle in the 1930s will serve as the central case study of my discussion. I will discuss the ways in which its historical and political contexts gave shape and meaning to logical empiricism and its philosophical projects, from the time of the calls for a Wiederaufbau after World War One, through the Anschluss, and into the far-flung places in which the Circle members found themselves during and after World War Two.

Thomas Uebel (University of Manchester, UK) **and Elisabeth Nemeth** (University of Vienna, A)

History of Philosophy of Science and Contemporary (Austrian) Politics

What can the history of philosophy of science contribute to contemporary efforts to combat racism and ethnocentrism and nationalism? Can it do more than provide edifying examples of active concern on the part of past practitioners and show where and how such a concern is legitimately played out on the level of philosophical argument about science? We shall explore two avenues of making history work for the present. First, we shall investigate an example of 'bad science' taken on board in philosophical argument and consider three different degrees of involvement, in each of them, a certain type of '*folk* anthropology' of surprisingly longlasting relevance is turned into progressively more oppressive '*volk* epistemology'. Second, we shall consider whether a complex of arguments once developed to defend modern science against the charge of undermining moral values still holds ready certain strands that could be re-employed with profit in the present situation. The first part of our paper presents a case history with a 'moral' attached, the second questions the renewed applicability of a certain argumentative strategy once deemed effective. Our case history concerns a figure of thought according to which different 'races' of the human species are differentiated also by cognitive endowment owing to biology. 'Savages' of varying descriptions were simply denied the capacity for logical, even abstract thought as such. This supposed fact entered not only the reflections of H. Poincare and the speculations of K. Pearson around the turn of the century, but also the philosophy textbook of E. Mally published only months after the Austrian *Anschluss*. What distinguishes their employments of the 'scientific' argument in question (Poincare's being relatively harmless)? What precisely is it that holds in place not just theirs but also more 'culturalist' postulations of otherness that, under certain conditions, underwrite perceptions of difference and policies of exclusion? The second part focuses on P. Frank's argumentation in *Relativity: A Richer Truth*, written

in response to the fear that scientific reason must remain silent in the debates about the defense against totalitarian ideologies. First we shall investigate whether Frank himself had to hand the philosophical means to reject the claims of Mally's Nazi-tract. Then we shall consider Franks' claim that the history of science itself presents a model where the relativisation of one's standpoint contributes to the objectivisation of the knowledge claims at issue. Does this provide a blue-print for an argumentative strategy that can be employed also in arguments against nationalist rhetorics and racist discrimination?

Special Panel C (in English):

On The History of the Vienna Circle: Personal Accounts

Rudolf Haller (Graz, A), *Chair* Robert S. Cohen (Boston, USA), Brian McGuinness (Siena, I)

PANELS

Panel I:

Theoretical Systems, Unification, and Political Thought Topics in the History of Philosophy of Biology

Philosophy of Biology in its modern connotation is a rather young discipline. However, the issues that are at the center of many present-day debates have already been discussed in previous decades. Questions raised by developments in biology have always engaged philosophers no matter what their intellectual orientation. But biology did not just serve as topic for philosophical reflection. In many cases ideas that were first developed in the context of biology were also incorporated into the body of philosophical systems.

In this panel we want to explore the relations between biological problems and theories and philosophical reflection in the case of a few selected philosophers. We consciously chose thinkers that were part of different intellectual traditions in order to do justice to the immense variety of philosophical reflections that were triggered by developments in biology.

John Beatty will explore the role of Darwinian evolutionary thought in Popper and von Hayek's analyses of totalitarian thought and for their arguments in favor of the virtues of liberalism. He will focus on the intrinsic ambiguities of such a broad notion as 'Darwinian evolutionary thought' and sketch some of the ways in which the tensions within Darwinism are reflected in the different philosophical and political interpretations of evolutionary thought.

John-Michael Krois and Manfred D. Laubichler will focus on the role biological theories played in Ernst Cassirer's philosophy. Krois will focus on the central importance of Goethe's morphology for Cassirer. He will argue that Goethe's particular interpretation of nature provided the framework for all of Cassirer's numerous reflections on biological thougnt. This is true also in the case of Jacob von Uexkull's Theoretical Biology. Laubichler will investigate the importance of Uexkull's theoretical foundation of biology and of related developments in the human and biological sciences (e.g. Gestaltpsychologie) for Cassirer's conception of a Philosophy of Symbolic Forms. He will also analyze the role that the "symbolic relation" played in Cassirer and Uexkull's rather different political views.

Kenneth Waters will conclude this panel with some reflections on reductionism and unification. Biology has always been recognized as a pluralistic science that defies easy attempts at unification. This has been a problem for both theoretical biology as well as the philosophy of science. Waters will explore the historical origin for linking the ideal of reduction with the ideal of unification that continues to dominate present-day debates about the character of biology as a science.

With this selection of papers we hope to contribute to ongoing efforts to contextualize the relationship between biology and philosophy that, at least in the case of political philosophy, is all too often dominated by the one-dimensional notion of "biologism."

John Beatty (University of Minnesota, USA)

Popper and Hayek on Evolution and Politics

Friends and co-conspirators. Popper and Hayek had much in common, including their ambivalent attitudes toward Darwinian evolutionary thought. On the one hand, they associated Darwinism with the sort of "historicist" thinking characteristic of totalitarianism. On the other hand, they employed analogs of Darwinian evolutionary reasoning in articulating the virtues of liberalism. In this paper, I will compare and contrast Popper's and Hayek's attitudes toward Darwinism, and try to make sense of their views in the context of early-mid 20th century evolutionary and political thought more generally.

Popper's and Hayek's concerns about the connections between Darwinism, historicism and totalitarianism were by no means idiosyncratic. Hannah Arendt, Isaiah Berlin and other prominent political theorists held similar views. And at least some biologists gave Popper and Hayek reason to be concerned. For example, Conrad Waddington seemed to be promoting the inevitability of totalitarianism on Darwinian evolutionary grounds, and even urging the importance of accomodating ourselves to that outcome.

Popper and Hayek tried to distinguish and distance their own uses of Darwinism from more

pernicious, less judicious uses, like Waddington's (and sociobiology). In the process of distancing themselves from the all-too-common exaggerations of Darwinians, they also criticized and belittled Darwinism in more fundamental respects.

The anti/pro Darwinian tensions in Popper's (and Hayek's) work is mirrored by the anti/pro Popperian tensions in the work of some influential Darwinians.

John Michael Krois (Humboldt-Universität zu Berlin; D)

Ernst Cassirer's Theoretical Interpretation of Biology

Cassirer wrote numerous historical texts on biological theory, but there was a single unifying element in these various writings: his reception of Goethe's Morphology. In my paper I explain Cassirer's interpretation of Goethe's Morphology and show how this provided the framework for his general conception of evolution and other biological topics. Some comparisons with various philosophical contemporaries of Cassirer will be made to bring out the uniqueness of Cassirer's position.

Manfred Laubichler (Princeton University, USA)

Cassirer and Uexküll Symbolic Forms, Theoretical Biology, and Politics

Aspects of Jacob von Uexküll's Theoretical Biology figure quite prominently in Ernst Cassirer's Philosophy of Symbolic forms. In the Philosophy of Symbolic Forms Cassirer attempts a systematic analysis of the role of "symbols" in a variety of different cultural domains. Yet the idea of a symbolic relation between a subject and it environment also plays a role in the natural and human sciences, and here especially in psychology (Gestaltpsychologie) and in Uexküll's theoretical biology.

In this paper I will investigate the connections between the project of Ernst Cassirer's Philosophy of Symbolic Forms and Jacob von Uexküll's conception of the theoretical foundations of biology. I will address the question in what ways both projects can be seen as related interpretations of the Kantian program in the context of similar developments in the biological and human sciences of their time.

I will also explore to what degree the basic assumptions of Cassirer's Philosophy of Symbolic Forms and of Uexküll's Theoretical Biology inform their conceptions of the state and of politics. In this area the ideas of the liberal German-Jewish philosopher Cassirer and of conservative biologist Uexküll are remarkably different.

Panel II:

Popper's Place in the History of the Philosophy of Science

Chair: I. C. Jarvie (York University, Toronto, CDN)

HOPOS 2000 will take place 6 years after Popper's death, and 2 years before the centenary congress and celebration Karl Popper 2002. It is an auspicious moment to make a preliminary foray into the questions surrounding Popper's place in the history of the philosophy of science. Such questions include: how original was Popper's philosophy of science. How viable have his ideas proved? Were his central problems – demarcation and induction – genuine or spurious? Did he solve them? Just what is his relation to the various Methodenstreit in the social sciences? The proposed panel will be in two parts, first general, second specific. General questions relate to Popper's position in the historical line Whewell to the Wiener Kreis (Wettersten and Hacohen), and his reception by those of the following generations (Hattiangadi and Gardner). Specific science discussed would include biology (Munz), political science (Shearmur), sociology (Bang), economics (Milford).

Part I: General Issues

Commentary on the panel: Gunnar Andersson (Umea, S)

Individual Abstracts

John Wettersten (Mannheim University, D):

Popper's Historical Role

Appraisals of what role Popper has played and will play in the history of the philosophy of science need to offer historical frameworks in which his contribution may be evaluated. The philosophy of science is caught between the perceived need to defend the authority of science, on the one hand, and the need to be open and progressive, on the other. The identification of the philosophy of science with the defence of science, with the defence of the authority of science, with the defence of traditional inductivist defences of science, blocks progress. Although progressive ideas are regularly deemed threats to science, dissident thinkers such as Solomon Maimon and especially William Whewell have played important roles in the philosophy of science. Although thoroughly rejected, Whewell enriched the philosophy of science enormously and changed its agenda radically. Rejected in a similar way, Popper has nevertheless preserved much that is excellent in Whewell's philosophy, while bringing it forward dramatically: previous views either could not explain scientific knowledge or explained it away. Popper lays down a challenge to do better and an excellent foundation for the formulation of new interesting problems, not merely concerning general philosophy of science, but also, e.g. social science and technology. The appropriate task of the minority of progressive sceptics is to continue the tradition of dissidents by formulating new problems and criticisms, so that new ideas enrich the discipline and the majority of true believers do not do too much damage.

Malachi Hacohen (Duke University, USA):

Popper and Neurath

Regarding the Neurath-Popper exchange, Uebel and Cartwright are anachronistic to present Neurath as a poststructuralist. Popper gives better answers than Neurath to poststructuralist questions. This makes Popper's differences with the Wiener Kreis clear and salient.

Jagdish Hattiangadi (York University, CDN):

Popper's Demarcation of Science from Metaphysics: A Centennial Reappraisal

Our estimate of Popper's influence on philosophy of science will depend to some extent on the tenure of his solution to the problem of demarcating science from metaphysics. Popper suggested that scientists take special case to learn from experience, and since experience cannot prove the truth of general theories, that science characteristically takes refutation seriously as tests of such theories. This demarcation criterion is nowadays widely challenged by appeal to Kuhn's demarcation criterion, that science, as demarcated from medieval metaphysical disputation, gives up fundamental debate for professional practice, i.e., the esoteric study of whatever it is that the prevailing paradigm tells the scientists is worthy of study, in which a test tests the practitioner, and is not taken as the refutation of theory. In retrospect it may be allowed that Popper and Kuhn were both correct in their descriptions of science, though it seems also that they were both mistaken in construing their own partially descriptive theories as demarcational. A new demarcation which lacks any criterion will be proposed, and the relation of metaphysical theory to scientific practice (or to certain social institutions) will be explored to see how Popper's influence on the philosophy of science may be assessed in retrospect.

Douglas Gardner (York University, CDN):

Popper in the Text Books

Kuhn sees the text-book as the place where the paradigm gets established in a form that can be taught. The first philosophy of science textbooks appear only in 1940s. By a survey of use and by examination

of contents, the course of Popper's reception, including acceptances, rejection, and emphasis will be described. What does this tell us of the textbook as an historical source on HOPOS?

Part II: Special Sciences

Commentary on the panel: Joseph Agassi (Tel Aviv, Israel and York, CDN)

Peter Munz (Wellington, NZ):

Popper and Darwin

Popper has a firm and epoch-making place in the history of the philosophy of science because he refuted positivism all its forms and provided a viable alternative philosophy of science. In spite of this, there are currently popular attempts to rehabilitate positivism by pseudo-Darwinism (Tooby and Cosmides, Mithen, Pinker). They all talk Locke refurbished by evolutionary arguments and end up by proving that what is in our minds corresponds exactly to the world in which we live, as if our minds were buckets into which the world had been poured. This Darwinisation of Locke is designed to prove Popper hopelessly mistaken. The irony is that the Wason test, Cosmides' preferred starting point, was carried out by Wason who meant to show what Popper's falsificationism amounted to. The new age positivists sound plausible because they talk of innate modules, and because nowadays any claim to be Darwinian gets a hearing. Popper's genuine Darwinism differs radically from this pseudo-Darwinism (described by S.J. Gould as 'Darwinian fundamentalism'). Incidentally, Tooby and Cosmides rejection of what they call the Standard Social Science Model (SSSM) bears a striking resemblance to Wittgenstein's argument in the Tractatus (as interpreted by Gellner) that we acquire knowledge best when there is no culture.

Jeremy Shearmur (Australian National University, AU):

Popper's Political Thought after The Open Society: Issues from the Emory Lectures

Changes and shifts in Popper's views on crucial political questions can take further some of the analysis in Stokes' book Popper and Hacohen's forthcoming Karl Popper – The Formative Years, 1902–1945. Of particular interest are the fragmentary materials in the archives from Popper's Emory Lectures of 1956. These are a new source consisting of one lecture published in another form, some notes, and material on an archaic recording system. The paper will discuss these matters in the light of these materials.

I. C. Jarvie (York University, CDN):

Popper and the Sociology of Science

Karl Popper, Robert K. Merton, and Michael Polanyi are the three insufficiently discussed pioneers of the sociology of science. Each was careful not to reduce science to sociology. Each found a different solution to the problem of explaining the transcendental aspiration and success of science. All were, consciously or not, Durkheimians who located the achievement in forms of social cooperation. Of the three, only Popper was able to characterise the Republic of Science in a way that did not endorse the authority of current science. Even though Popper almost never wrote critically of actually existing science, his sociology fully allows for it, without engaging in any general debunking, which is the strategy forced on current social studies of science by their reading of the legacy of Merton and Kuhn.

Panel III:

The Interplay of Physics, Metaphysics and Mathematics in 17th and 18th Centuries

Hartmut Hecht (Berlin – Brandenburgische Akademie der Wissenschaften, Potsdam, D):

Finality and Existence in Modern Physics

The principle of least action is one of the greatest overarching principles in science which covers a wide range of theoretical concepts governing classical physics as well as quantum mechanics and the theory of relativity. The reason for its outstanding significance is found in the special type of modern physics, wherein each system can be defined by a specific quantity, the so-called action, by which applying a variational procedure simultaneously a condition for the variety of possible motions of the system in question is determined.

As it can be acquired from actual discussions in the philosophy of science the above acknowledged situation is customarily claimed to be fundamental for further developments in physics, but it is, nevertheless, not yet philosophically adequately understood. To promote such a deeper understanding, I will expose a historical perspective which can be traced back to the true origins of dynamics in 17th century. In particular I will display the Leibnizian problem of the choice of the best of all possible worlds as an effective means which paves the way for a philosophical comprehension of the meaning of least action principles in physics.

The problem itself arises from the assumption that there should exist at least one preferable world which may be found and distinguished from the others due to its unique properties by examining the infinite multitude of all possible worlds. The criterion for the discrimination as well as the final choice is the degree of perfection. Furthermore, Leibniz postulates that God would not be willingly to create any other world except this best one of all possible worlds. Therefore, the application of an ordering procedure to the manifold of all possible worlds and the existence and finding of an optimum has to be considered as the conditio sine qua non for God's choice and is, consequently, the reason for the existence of at least only one real world. In that world all things are entirely arranged, i.e., determined and, as a further consequence, let be any even the slightest deviation from it, we were no longer in the same world and would just immediately bump into another one.

I intend to show that against this background the rise of modern physics can be understood as a process by which the Leibnizian perspective has been only transformed into a new shape but neither removed nor replaced. The first of this significant transformations was done by Maupertuis, who insisted as Leibniz did at an optimum as an unavoidable and indispensable criterion for God's action. But, in contrast to Leibniz, Maupertuis did not explicate this reasoning with respect to the world as a whole one but to any single event within it. For this purpose, Maupertuis defined an as new as fundamental physical quantity, the above mentioned action, and it seems to me that just this change from the whole to a part of it is at least the true reason for the successful application of any variational principle in physics. Especially their validity for any event guarantees and rules dynamically the existence and, moreover, the stability of physical systems.

Philip Beeley (Universität Hamburg, D):

Divine Economy and the Mathematization of Nature in the Seventeenth Century

When explaining nature seventeenth-century mechanistic philosophy proceeds from only a few (generally three) principles. In this way it is not only able to clearly distinguish itself from certain trends in later scholasticism - while standing firmly in the Ockhamist tradition - but also to assert its very truth as a philosophical position: it is precisely on the perceived economy with respect to explanatory principles that an important part of the validity claimed for the so-called philosophia emendata by its advocates is made to rest.

In the case of G. W. Leibniz, on whom the contribution focusses, there is the additional factor that economy is conceived as being an inseparable principle of divine action and therefore as being of regulatory nature in respect to the creation of the actually existent world. Thus it can be seen in his philosophy that the reducibility of all natural phenomena to the basic mechanistic principles of magnitude, figure, and motion is at once a result and a proof of the divine principle of economy.

As a hypothetical precondition for the validity of the economy principle but also as a consequence

thereof we find that in Leibniz's explanation of nature the real diverges from the rational, that is to say from what can be derived from pure concepts. However, just as the principle of the best, which implicitly contains the demand for economy, is subsumed under the principle of sufficient reason, it can be shown that this deviation does not in any way contradict the rational comprehensibility of nature. Of central importance thereby is the view that the difference between the real and the purely conceptual is such that it is smaller than could be noticeably perceived.

Similar to the benefits which result from the employment of the concept of negligible error in mathematics, this minimal deviation represents for Leibniz "an incredible divine beneficience". It is namely due to the character of this deviation that on his view all sciences which combine mathematics and physics such as optics, statics, pyrotechnics, and general mechanics "to the envy of pure sciences" do not contradict sense perception. Not least because of the contribution to the common good brought about by the so-called mixed sciences and because this in turn is seen as something which serves to exemplify divine glory, the mathematization of nature in Leibniz's philosophy is inextricably bound up with final causes.

Dieter Suisky (Humboldt Universitat Berlin, D):

Direct and Indirect Methods in 18th Century Mechanics. The Background of the Eulerian Methodological Approach

It will be demonstrated that a new type of methodology for solving mechanical problems allowing, additionally, to remove spurious metaphysical assumptions has been established by Euler in his post-Newtonian criticism of the metaphysical foundation of mechanics by Leibniz and his predecessors. The Eulerian approach comprises (i) a new concept of forces, (ii) an alternative formulation of the Newtonian first Law, (iii) a new method for solving mechanical problems beyond the Newtonian approach. Thereby, the direct method has been completed by the indirect method, allowing for a new type of decisions of the controversially discussed contemporary metaphysical and mechanical questions. As an example we refer to the controversy between the Leibnizian and Cartesian schools concerning the true measure of living forces. This new approach assured also the reformulation of Newton's first Law concerning the mechanical state of a body. Following Descartes, Euler replaced the concept of an impressed force applied to a body by the concept of an external cause that is able to change the mechanical state of the body. The prerequisites and consequences of this Eulerian procedure will be discussed in this contribution.

Euler based his mechanics on the Newtonian program and its two main topics which make hypothesis about the forces avoidable and allows to remove metaphysical assumptions from the concept of forces. The first one has to be considered, if the forces applied to a body are given, and the change in its movement has to be determined, whereas the second one has to be considered, if the changes in the mechanical state of the body are given and the forces have to be determined (L. Euler, Recherches sur l'origine des forces). For solving both problems on equal footing Euler completed the Newtonian equation of motion and developed his famous method of maxima and minima, or, the indirect method (L. Euler, Methodus inveniendi lineas curvas maximi minimive gaudentes). The latter coincides widely with the principle of least action by Maupertuis. Furthermore, Euler based his formulation of the fundamental concept of mechanics on a rigorous semantic analysis, discussing the different meanings and functions of words, names and concepts in scientific theories, which resembles the analogous criticism of the usage of mechanical concepts that was given by Maupertuis at the same time (P. M. L. de Maupertuis, Examen philosophique de la preuve de l'existence de Dieu). In such a way, the forces can be derived from the phenomena, the observed trajectories, and, vice versa, the trajectories can be derived from the assumed forces. Any additional postulates about forces can be proved in a non-metaphysical manner. Therefore, the Eulerian approach is inherently of antimetaphysical character and explicitly a criticism of the some of the main topics of 18th century metaphysics.

Rüdiger Thiele (Universitat Leipzig, D):

What Is Actually Proved or Assured by Any Mathematical Formula?

Almost immediately after Einstein had proposed his general field equations it was recognized by Hilbert that these equations can also be derived assuming a variational principle. This procedure opened a new field for metaphysical reasoning and logical conclusions. Recently, the historical background of this event

in the history of science has been discussed very controversially. However, the fundamental importance of the connection of physics and metaphysics is beyond of any doubt. Moreover, here we can observe the renewal of an old topic of Natural Philosophy, i.e. the assumption of simplicity of the means that applies nature to produce the effects. First and foremost it was Maupertuis who tried to give a metaphysical foundation and obtained his confidence and hope for the general validity of his principle even from this new metaphysical background. The point of controversy is the question: Is there any reason and, why and to which extent can it be, that such high expectations can be solely justified by a purely mathematical formula, or, if it is not so, why and to which extend can the validity and the justification be additionally or alternatively ensured by a metaphysical or any other kind of reasoning? Generally, the Principle of Least Action can be formulated as follows: Whenever a certain change happens in nature the quantity of action employed for it is the smallest of all possible ones. The starting point for the mathematical investigation related to the problem is an integral expression. After applying the variational procedure one obtains a differential equation, the so called Eulerian equation. Than, the mechanical problem is fully described by a set of differential equations. Besides technical questions of such a mathematical description, however, there remains the more basic question of the validity of such mathematical formulas in nature (Euler equations or Hamilton-Jacobi equation). First of all the testing of this validity is a critical investigation outside mathematics and even physics although the physical experience is an important part of reasoning.

The idea of my talk may be roughly described as follows: We have thoroughly to distinguish between the different meanings and stages of reasoning in Philosophy, Physics, and Mathematics. Firstly, it is one purpose of my talk to present a survey of the philosophical, physical, and mathematical problems. I will give a brief summary of the achieved results. Secondly, based on this historical survey I will discuss why and to which extend the philosophical thinking of extremity can be represented by a mathematical formula. It is no problem to derive mathematically any types of "Euler's equations" of a problem by any assumed principle. Nevertheless, the multitude of possible principles is limited, because of the limited number of basic physical concepts and the interconnections between them. However, first of all the acceptance and the usage of any mathematical results depends on the meaning and interpretation that physicists can attach to the derived formulas. In case if the underlying or given meaning and interpretation allow the minimization of any of the involved physical magnitudes we are successfully in assuming an extremal principle.

Panel IV (cancelled):

Grenzprobleme, disziplinäre und ontologische Konflikte in und zwischen Mathematik und Physik

Die Geschichte zeigt, daß die Ausbildung von Identität und Autorität der Wissenschaften mit einem Demarkatinsprozeß einhergeht. Drei Arten solcher Grenzziehungen sollen in dieser "Sektion" exemplarisch an historischen Beispielen vorgestellt werden: die Demarkation wissenschaftlicher von nicht-wissenschaftlicher Tätigkeit (1) wird anhand der "wissenschaftlichen Revolution" zu Beginn des 17. Jhds. analysiert, wo die Geschichtlichkeit der Klassifiation und ihrer Kriterien zwischen Wissenschaft, Literatur und Religion in der Abhängigkeit von theoretischen Resultaten und sozialen Prozessen besonders deutlich ist. Der Demarkationsprozeß wiederholt sich natürlich auch innerhalb der Wissenschaften zwischen einzelnen Disziplinen (2) und innerhalb einzelner Disziplinen zwischen verschiedenen Denkschulen (3). Wie entwickelt sich im 19. Jhd. die Grenzen zwischen metrischem und projektiven Ansatz in der Geometrie, was sind in einer diachronischen Betrachtungsweise die Kriterien zur Einordnung der Himmelsmechanik (Papier 1)? Die Konsequenzen einer Grenzziehung werden an einem sich daraus ergebenden Perspektivenwechsel bei der Betrachtung des wohlbekannten Zwillingsparadoxes erörtert (Papier 2). Der Mathematiker hat keine Mühe, das Paradox als scheinbaren Widerspruch durch eine Betrachtung der 'Viererabstände' der Minkowski-Raum-Zeit aufzulösen: die Weltlinienlänge des reisenden Zwillings ist geringer als diejenige des seßhaften. Ist aber dieser geometrische Standpunkt wirklich "physikalisch" korrekt? Sind die historischen Interpretationen, z. B. von Bergson, einfach nur ein Ausdruck ungenügender Berrschung technischer Fragen?

Eine andere Grenzziehung, die für die Geschichte der Wissenschaftsphilosophie im 20. Jhd. von entscheidender Bedeutung ist, wird im dritten Papier diskutiert. Quines Ablehnung von Carnaps

Unterscheidung interner und externer Existenz in der Mathematik kann als Paradigma einer Grenzbegehung angesehen werden. Neben der Darsellung der jeweils vorgebrachten Argumente soll insbesondere unterstrichen werden, dass Bernays und Gonseth Jahre vor Quine an ganz ähnichen Argumentationsstrangen arbeiteten. An allen drei thematisierten Grenzziehungen wird sich zeigen, daß das bisweilen als objektiv vorgegebene Kriterium der Technizität und Historizität sich auch als Schutzwall gegen Eindringlinge erweisen.

Simone Mazauric (Université de Nancy, F) / **Philippe Nabonnand** (Université de Nancy, F):

Recomposition et construction des frontières au XVIIe et XIXe siècles

Le premier XVIIe siècle voit se construire progressivement une nouvelle topologie des savoirs. Les classifications et les partages hérités de l'aristotélisme scolastique autant que d'une Renaissance pourtant apparemment moins soucieuse d'ordonnancement méthodique subissent une vaste recomposition, tandis qu'on assiste à l'invention de nouveaux principes de différenciation, participant d'une même volonté de construction d'une nouvelle géographie du savoir. Les sciences conquièrent leur autonomie au sein du vaste territoire de la République des Lettres et, parmi elles, les Mathématiques, la Physique, l'Histoire naturelle notamment affirment et revendiquent chacune sa propre identité. Mais l'innovation majeure réside certainement dans la volonté de révolutionner en profondeur cette géographie en traçant une frontière largement inédite destinée à opérer une discrimination rigoureuse entre les "vrais" et les "faux" savoirs, discrimination qui constitue certainement la nouveauté épistémologique majeure de l'âge classique, et dont nous continuons à bien des égards à assumer l'héritage.

Nous nous proposons par conséquent autant d'analyser et de commenter les nouveaux partages produits par ces inventions/recompositions que de retracer la généalogie d'une ségrégation dont la raison d'être est à chercher autant dans les reconstructions opérées dans l'ordre théorique (la "révolution scientifique" du premier XVIIe siècle) que dans le processus social d'"institution" du savoir au terme duquel les académies scientifiques à la fois entérinent le nouveau tracé des frontières entre les disciplines constituées, et valident en l'institutionnalisant une discrimination dont la légitimité n'a fait que très récemment l'objet d'une remise en question.

Cette réflexion sur la recomposition-construction des frontières au 17e siècle se poursuivra par une interrogation sur l'utilité de la notion de frontière en histoire des mathématiques. Nos exemples seront choisis principalement dans l'histoire du 19e siècle car la notion de frontière disciplinaire est un des axes (trop méconnus) de l'activité réflexive des scientifiques sur leurs domaines d'étude et leur métier. En effet, à partir de 1840, principalement motivées par la multiplication des communications et par la professionalisation du métier de scientifique, de nombreuses tentatives de bibliographie ou de classification des sciences émergent.

On peut adopter un point de vue diachronique en étudiant les conditions et les circonstances d'émergence ou de constitution de nouveaux domaines. La séparation explicite des points de vue métriques et projectifs durant la première moitié du 19e participe de réflexions sur la notion de généralisation et d'une volonté de formuler rigoureusement les bases des mathématiques. La géométrie projective sera un des premiers domaines où se poseront les questions de l'axiomatisation.

Le point de vue synchronique permet de rendre compte de l'existence et de l'évolution des domaines-frontière ; l'exemple de la physique mathématiques au 19e est particulièrement étudié. Nous proposerons celui de la mécanique céleste. Comme au 17e, la question des domaines-frontière pose celle de la prise en compte des communautés et des institutions. En utilisant diverses approches épistémologiques et sociologiques, il ne s'agit pas simplement de juxtaposer différents résultats apportés par diverses méthodes mais bien de montrer comment, au delà des a priori méthodologiques, celles-ci peuvent se perfectionner et gagner en précision en se confrontant les unes aux autres.

Gerhard Heinzmann (Université de Nancy, F) / **Joseph Vidal-Rosset** (Université de Nancy, F):

L'histoire d'une frontière: la discussion externe-interne dans la philosophie des mathématiques entre 1930 et 1955

Carnap distingue deux sortes de questions: celle de l'existence interne et celle de l'existence externe. En mathématiques, les questions ontologiques (internes) dépendent, selon Carnap, exclusivement du cadre langagier sans que l'expérience intervienne. Si, par contre, on défend, comme Quine, une théorie

radicalement holiste, il n'y a plus raison de croire que l'on puisse séparer certaines propositions de l'exigence d'une confirmation par l'expérience. Ainsi, la distinction entre vérité factuelle et linguistique s'estompe et, avec elle, celle de la distinction de principe entre la définition d'un cadre langagier, postulé par convention comme analytiquement vrai, et des considérations empiriques au sens large qui influencent l'acceptation de ce cadre.

Bernays semble s'accorder avec Carnap que l'existence, dans la pratique mathématique, est le plus souvent conditionnée par le cadre langagier choisi en vue d'un objectif arrêté : elle est donc "interne" (Carnap) ou "bezogen" (Bernays). Mais veut-on alors dire avec Carnap que le choix du cadre est soumis au principe de tolérance, applicable aux formes linguistiques et déterminé par des considérations pratiques de sorte que les assertions existentielles externes sont dépourvues de tout contenu cognitif ? Ou croit-on, au contraire, le libre choix limité par un engagement ontologique ou par l'intuition mathématique ? On pourrait penser qu'il s'agit ici d'une controverse rappelant la dispute entre le formaliste et le réaliste face au théorème d'incomplétude de Gödel: le formaliste montre la transcendance et par conséquent le non-sens des questions sémantiques, le réaliste considère l'incomplétude comme symptôme de la limitation du formalisme qui serait à élargir, par exemple dans le sens que Gentzen l'a fait. Cependant, une telle alternative est par ainsi dire neutralisée ou transgressée par l'opinion avancée par Gonseth, puis par Quine (et non l'inverse) : les deux philosophes sont d'accord sur le fait qu'il n'existe pas, en principe, une frontière précise entre l'acceptation d'une structure langagière et l'acceptation d'une assertion formulée dans ce langage.

En effet, Quine, dans sa célèbre discussion de la distinction carnapienne, argumente en faveur de l'abandon de la distinction externe-interne en général ou, de toute façon, il la juge inefficace puisque "nous ne pouvons [...] tracer cette distinction par suite du 'manque d'un critère pour distinguer entre ce qui dépend de l'adoption d'un langage et ce qui n'en dépend pas'". Une théorie n'est complétement interprétable que par rapport à une théorie "domestique" qui à son tour peut être mise en cause. Il n'y a pas de sens absolu à dire sur quoi porte une théorie.

On montre que Quine est dans les années 1950 au cœur de la problématique que Bernayset Gonseth attachent au concept de correspondance schématique dès les années trente. Puisque les propositions théoriques à l'intérieur d'un cadre linguistique ne sont pas nettement distinctes des jugements pratiques qui détermine ce cadre, l'espace frontière exige, selon Bernays, à la fois un processus de la construction des objets et leur description à l'intérieur du cadre formel suggéré par la construction effectuée. En d'autres termes, un processus constructif de schématisation et un processus descriptif de structuration constituent deux horizons d'objectivités qui restent dans une interdépendance appelée selon une proposition provenant de Cavailles, dialectique. D'où le nom de "Dialectica" que Bernays, Gonseth et Popper donnent au périodique qu'ils fondent en 1946.

3. **Vincent Borella** (Université de Nancy, F) / **André Coret** (CNRS Strasbourg, F):

The Twin Paradox: Between Geometry and Physics

As a first step, we shall recall the context in which Paul Langevin explains in 1911 his paradox, essentially from a pedagogical point of view: his aim was the introduction of relativity in France. During a long period, he led many debates on this subject with the participation of mathematicians, philosophers and even Einstein himself.

We shall also recall how geometricians clarify the presentation of special relativity, using the four-dimensional space-time of Minkowski, light cones, the worldlines, planes of simultaneity. Using these tools they give full credit to the conclusions of the Langevin paradox : for them the interstellar journey of the B twin is really a youth cure. But one of the consequences of this point of view is that we have to admit that special relativity leads to physiological effects.

We can express the paradox in the following way: suppose A and B are two twins who are on the Earth and are submitted to three successive events which correspond to many points in the dimensional space-time :

– First event: at time t = 0, B is launched into space at a speed v, close to the speed o light. This event corresponds to a point O in space-time.

– Second event : the spatial vessel of B reverses side down and starts up instantaneously towards the Earth. The corresponding point in space-time is M.

– Third event : B meets up with the Earth at point N and the twins compare their mutual age.

When the twin A who stayed on the Earth sees his brother B appearing after this long interstellar journey, he notices, apparently, that he (A) is older than his twin. It is usual to consider this situation as paradoxical because it is violates common sense. However the paradox seems to be fully explained by

geometricians in terms of special relativity theory. That is the reason for which this paradox is usually called a "weak" paradox in order to express that it does not question the pertinence of the theory : when the geometrician says that the twin A gets older more rapidly than his brother, it is certainly surprising, but it is a normal consequence of the application of the relativity principles.

The point we want to develop is to consider that the twin paradox is in fact a "strong" paradox by enriching the thought experiment of Langevin in a certain way. By analysing the position of geometricians, we would ask them two types of questions :

1) Why is it necessary for them to maintain an anthropocentric vision (nature of the "objects" A and B, employment of words like "aging process", or "life line")? What does the use of an "occulted" time mean in the reversal process of B?

2) What is the physical meaning of the instantaneous transition of B from one worldline to a different one?

This communication will be, of course, an attempt to clarify the debate about the twin paradox and to establish that what is "geometrically correct" is not always "physically correct". But it is also an example of the close relationship between epistemological questions and the content of a theory.

Panel V:

Placing the Loewenheim-Skolem Theorems within Philosophy

Analytical Philosophy was, from its beginning at the start of this century, built upon the premiss that traditional issues in epistemology, ontology or ethics can profitably be explored by using the toolset of Symbolic Logic. A well-known formal apparatus was commonly employed to clarify notions like reference, identity or possibility. In the course of its development, however, more exacting technical procedures inducing a somewhat different relationship between mathematical logic and philosophical work have also played an important role. Cantor's transfinite numbers or Godel's incompleteness proof are not as easily applicable to philosophical inquiries as e.g. truth functions. Advanced proof theory or recent techniques in formal semantics are, on their part, largely unconcerned with problems typically discussed in philosophical circles.

To mediate between the more elaborate discoveries of logicians early this century (including the present textbook versions of their theorems) and arguments that can be useful within -- to pick one example -- the theory of knowledge requires an unusual amount of competence and courage. Hilary Putnam's 1980 paper Models and Reality 1 is an attempt to exploit the Lowenheim-Skolem theorems 2 in an onto-epistemological context, transferring certain model-theoretic strategies to foundational refection. Although Putnam's ideas have triggered a vivid discussion their methodological status has rarely been examined. A logician can – tongue in cheek – label some set-theoretically defined universe of discourse THE WORLD. But this suggestive act of baptism is no substitute for an argument purporting to show how this machinery relates to the world philosophy is concerned with. Analytic philosophy's loans from advanced logic need systematic scrutiny.

Several attempts to approach the significance of the Lowenheim-Skolem theorems in the light of philosophical methodology have been published in recent years. Stuart Shapiro has drawn attention to their affinity to well-known problems regarding rule-following 3 and Hartry Field discusses non-standard models as a challenge to our ordinary understanding of finiteness. But it is Paul Benacerraf's challenge 4 that highlights the problem most effectively. Those formal constructions which we deem helpful to clarify intuitive notions like finitude and infinity do not carry any intrinsic meaning. All the meaning they have and, consequently, all the light they can shed onto philosophical issues, result from our own willingness to invest them with a certain amount of significance. Benacerraf convincingly exposes the naivety implicit in any attempts to treat set theoretical representability a la Skolem as a criterion for ordinary meaning.

The proposed panel will consist of mathematicians and philosophers discussing the historical development and methodological setup sketched above. This setting will be approached from the point of view of working logicians puzzled by how their work gets transformed within epistemology. Conversely, philosophers will ask how much intuitive content has to be presupposed in order for mathematical proofs to yield philosophical results. It is assumed that there are no easy, general answers in this area. Focussing on the philosophical impact of the Lowenheim-Skolem theorems serves as a case study on how to approach this widely unexplored terrain.

Herbert Hrachovec (University of Vienna, A):

Ontological Relativity reconsidered: Quine on Lowenheim-Skolem, Davidson on Quine

W.v.O. Quine's John Dewey Lectures on "Ontological Relativity" (1968) explore some important consequences of abandoning R. Carnap's distinction between the realm of analyticity and empirical facts. According to Quine's argument the reference even of scientific terms is inscrutable: there is no extra-theoretical resort which would enable us to judge their correctness outside the pragmatic context embedding those terms. Theories, as Hilbert stressed against Frege's objections, are basically axiomatic constructs, devoid of any "natural" domain. Their "universe of discourse" consists in whatever happens to fit into the pattern prescribed by prior theoretical presuppositions.

In order to strengthen these claims central to his philosophy Quine explores the philosophical relevance of the Löwenheim-Skolem theorems. At first glance they seem to support a somewhat counter-intuitive Pythagoreism. Since theories with non-denumerable models can be shown to also be satisfyable in denumerable domains and since, moreover, those can (via proxy functions) be mapped into the natural numbers, it seems that any statement may be seen as referring to those numbers. Ontological relativism offers an escape from this unattractive inference. Since the reference of a theory's terms can only be determined relative to another – framing – theory, philosophers are not entitled to consider suitable entities as "numbers per se". The Löwenheim-Skolem theorems are a clear indication that ontological commitments of some object-theory can only be discussed within another theory. Usually there are various competing (possibly mutually exclusive) candidate theories to do this.

Closer inspection of Quine's argument reveals that it is vulnerable to a general criticism Donald Davidson has directed against "ontological relativity". Relativity, as Davidson has pointed out, only makes sense in a conceptual setting providing some fixed point of reference for frameworks to be relative to. Quine's "theory forms" do not qualify for consideration as relativizable theories since – lacking interpretation – they are devoid of ontological commitment. But neither can theories that are actually employed be understood as necessarily relative to more or less arbitrary meta-theories. Radical interpretation a la Davidson forces interpreters to regard the terms of a given "language" as directed towards a "world" in order to make sense of them. Once this is done, there may be disagreement with respect to those interpretations between competing meta-theories. But relativity does not follow since this would presuppose independent evidence for some point of view common to those meta-theories.

If Davidson's criticism is taken into account, the Lowenheim-Skolem theorems can no longer be employed to support relativistic claims. This conclusion lends additional support to a recent argument put forward by Paul Benacerraf, questioning the relevance of model-theoretic discussion to our grasp of the meaning of mathematical terms and, therefore, reviving the issue of the proper employment of formal analytical tools within the philosophy of mathematics.

Christian Fermüller (University of Vienna, A):

Lowenheim-Skolem: A Theorem and Three Histories

The piece of history of logic that starts with Lowenheim's 1915 paper and leads to Skolem's (and Godel's and Tarski's and Mal'tsev's) various formulations and proofs of what is now called "Lowenheim-Skolem theorem(s)" is well documented. Also the extensive, ongoing discussion on the philosophical significance of this mathematical statement is easily accessible. Superficial knowledge of these two histories suffices to recognize that they are surprisingly disconnected. Studying the origins and shifts of language and concepts that characterize the winding path from Löwenheim to modern textbook variants of the theorem attributed to him and Skolem is a fascinating exercise in the history of modern logic. But it can hardly assist orientation in the jungle of "Skolemite" and "anti-Skolemite" arguments on the ontology of mathematics and science in general. This part of the history of philosophy of mathematics clearly separated from mathematical logic proper already in Skolem himself.

The purpose of this contribution is to draw the attention of philosophers and historians of logic to a third, again quite independent, stream of research connected to Löwenheim-Skolem: the rich after-life of the theorem. Ironically (given Skolem's skepticism about higher cardinalities) theorems explicitly connected to Skolem's results now mainly appear on the stage of modern set theory. E.g., Kenneth Kunen expects his readers to recognize some statements on uncountable cardinals as variants of the (downward) Lowenheim-Skolem theorem. Other "unintended" mathematical invocations of the theorem include results in finite model theory, definable models of arithmetic, non-classical logics, algebra of abstract logics, category theory, and, of course, Lindstrom's celebrated characterization of first-order logic

with respect to its extensions as well as other results in abstract model theory.

Finally we want to point out a rather obvious fact which hardly seems to catch the attention of philosophers of mathematics. Rather than perceiving the Lowenheim-Skolem theorem as a statement on a fundamental limit of first-order logic, logicians developed the study of relations between first-order syntax and models—i.e. model theory—into an impressively vast and lifely field of research. It is exactly the fact that first-order logic usually does not allow us to single out "intended" models that provides the ground for this rich, deep and, at times, very beautiful mathematics.

Johannes Hafner (University of California at Berkeley, USA):

Model Theory in Philosophy: LST and its Consequences

In 1922 Skolem employed the Lowenheim-Skolem Theorem (LST) to argue for the "relativity" of set-theoretic notions and, consequently, against the adequacy of axiomatic set theory as a foundation for mathematics. These are by no means the only philosophical consequences that have been drawn from LST since (Skolem later defended the axiomatic method in foundational studies vigorously despite the conceptual relativity which he still maintained). However, set theory and, more generally, philosophy of mathematics proved to remain a focus of arguments involving LST within philosphical settings till today. C. McIntosh, for example, tried to explicate and defend a somewhat weaker version of Skolem's original relativity thesis. McCarty/Tennant used LST to argue, indirectly, that intuitionism is the correct philosophy of mathematics. And M. Levin recently re-examined the purported implications of LST for the status of the Axiom of Constructibility (first put forward by H. Putnam).

Outside of the foundations of mathematics, starting with the exchange between G. Berry and J. Myhill, Skolemite arguments appeared in the context of more general ontological issues concerning (empirical) scientific theories. The concepts of ontological commitment, ontological reduction, and ontological relativity have been scrutinized in the light of LST and in certain respects revised or modified. The protagonists in this – still ongoing – debate have been W.V.O. Quine and his critics.

Philosophy of language is a third field in which LST has been appealed to in support of philosphical conclusions. Putnam's so-called model-theoretic argument draws heavily on it in the attempt to show the untenability of realist semantics. His paper triggered a whole series of comments and replies ranging from critical, explanatory, to apologetic. Yet, there are still controversies as to what the precise structure of this argument really is and what role LST is supposed to play in it – in later versions of the argument this theorem doesn't even occupy center stage anymore.

Even if the history of applications of LST within philosophy does not immediately yield a clear and unambigous answer to the question of its philosophical significance it is a necessary starting point for the exploration of the bearing that LST does or does not have on philosophical problems. Against the background of such a historical survey I attempt to first identify main types of arguments. By means of precise reconstructions the peculiar interplay of logical/mathematical tools on the on hand and philosophical methods on the other should be illuminated. Surprisingly enough, this methodological aspect of Skolemite arguments has hardly ever been addressed in the debates (P. Benacerraf being one of the rare exceptions) -- although it is not at all obvious how two quite different methodologies should not only be compatible but also yield interesting results. Or, put in another way, it is prima facie not clear how to draw philosophical conclusions from formal theorems. Clarification of the structure of arguments involving LST in exactly this respect is a prerequisite for any assessment of their success or failure (e.g. because of begging the question or self-defeat). Moreover, such an analysis can exhibit certain patterns of formal/philosophical argumentation which recur in similar fashion in other contexts. Thereby a better, more general understanding is gained of arguments involving metalogical theorems – like Godel's theorem in the philosophy of mind.

Panel VI:

Human Origins Research at the Turn of the Millennium, Paradigm Change and Continuity

Comments: Horst Seidler (University of Vienna)

Since ancient times, questions about human origins have figured centrally in the ways scholars have thought about human history and the epistemological status of human sciences. The changes that occurred during the Renaissance and early modern times in historical reasoning, scientific methods and theory, and philosophy were accompanied by calls for new perspectives on human nature and history. The most influential responses were those of social philosophers who envisaged human history as an extension of the history of nature, and as a sequence of 'origins events' (including the origins of language, tool use, social institutions, farming and sedentary communities, social inequality, writing, and the state) which could be expected to culminate in the Birth of Modernity and Scientific Revolution. During the 19th century these supposed events became the foci of anthropology's various areas of origins research. Throughout the 19th and 20th centuries, major change in methods and theory has always been accompanied by efforts to rethink and remodel the field's diverse areas of origins research. Today, each aforementioned 'origins event' is the focus of one or even several specialized fields of multi-disciplinary inquiry, each with its own combination of techniques, analytic procedures, and interpretive principles. But the importance of origins research to the ways anthropology defines its aims and structures its primary fields of inquiry has not been the only manifestation of paradigm continuity. The most controversial debates continue to turn on opposing perspectives on relationships between (a) human nature, (b) socio-cultural change and diversity, and (c) the epistemological status of human sciences. And the most polemical positions in these debates continue to be structured around a supposed nature-culture antithesis.

Fortunately the last several decades have seen remarkable change in the situation. Beyond methods of analysis and disciplinary definitions, the most fundamental issues in today's debates concern conceptual dichotomies that were taken for granted until the 1960s. Such apparently antithetical categories as nature-culture, evolution-history, the mental and the material, science and society once served as a common axiomatic basis for research. Precisely such dichotomies have come under convergent, if not identical sorts of scrutiny, in fields as diverse in their subject matter, as those closest to physical science, such as the philosophy of science, and the human science which has concerned societies that were at one time characterized as lacking science and even history: anthropology. Little by little researchers became aware that the categories which so evidently structured their fields of inquiry that they went unremarked were products of historically contingent circumstances.

This panel builds on these developments to examine aspects of paradigm change and continuity in the history of human origins research. Emphasis falls on changes that are making it possible to go beyond the difficulties generated by opposing gradualist and punctuationist models, unfortunate disciplinary boundaries and related conceptual dichotomies, and strongly objectivist and relativist philosophies of science. Several general implications of these developments will also be considered, including their relevance to critical and constructive discussion of anthropological approaches to the history of science.

Koerner, Stephanie (University of Pittsburgh, USA):

Vico's New Science and Alternatives to Objectivist and Relativist Philosophies of Human Sciences

In his New Science (1744) the Neapolitan rhetorician, historian and philosopher, Giambattista Vico sought to challenge Cartesian rationalism in light of a novel Newtonian restatement of the key components of his Renaissance humanist predecessor's campaign against scholastic perspectives on human studies and philosophy. The New Science was intended to serve as a critical and constructive alternative to such influential 'modern' systems as (a) Descartes's rationalist and Bacon's empiricist philosophies of science; (b) the social theories based on a State of Nature - Social Contract dichotomy and universalistic conceptions of Reason of Hobbes and Locke; and the new physical science of Galileo, Newton and Leibniz. Most specifically, the work was intended to provide a new science and philosophy of human life, in its various social, ecological and historical dimensions. For Vico (and his humanist

predecessors), the need of such a science was not an exclusively intellectual matter, but an important social ("civic") issue. Without a satisfactory science of humanity, at risk was a philosophy which denied its historicity and the alienation of intellectual culture from human affairs.

Until quite recently only a small number of human scientists and philosophers took serious interest in the potential relevance of Vico's work to important areas of substantive debate in their respective fields. A variety of factors have been involved. Perhaps most importantly, very few human scientists and philosophers were interested in the kind of project suggested and exemplified by the New Science. Such a project was unlikely to become relevant until the emergence of serious dissatisfaction with dualist paradigms for human history, and related objectivist and relativist philosophies of science. Vico's project was also unlikely to be taken seriously until researchers started to re-evaluate not only 'myths' that his work was totally unsynchronized with its times, but also ideal views about the Scientific Revolution and Birth of Modernity which informed perceptions of those times. In addition, many aspects of Vico's project remained difficult to comprehend until Ernesto Grassi (Rhetoric as Philosophy [1980]) drew attention to the philosophical significance of Renaissance humanism, and investigated Vico's attempt to articulate the epistemological principles that underpinned Italian humanism. Examining the ways in which Vico brought new interpretations of these principles to bear upon the problems concerned his New Science brings light to some similarities and striking contrasts between Vico's approach to humanism and philosophy, and the approaches espoused in various German traditions extending from Kant to Heidegger and Gadamer. This paper builds on these developments to focus attention on the relevance of Vico's New Science to current discussion of 'views beyond' dualist paradigms for human sciences, and related objectivist and relativist perspectives on science, philosophy, and the social roles of intellectual culture.

Jeffrey Schwartz (University of Pittsburgh, USA):

Evolution and Adaptation Are Not Synonymous, and Why This Should Make a Difference to Origins Thinking

Current evolutionary debates on issues of "tempo and mode" – the speed and means by which species originate via natural selection and adaptation – revolve around the premise that there is only one set of questions to which, therefore, there can only be one set of correct answers. But for some geneticists, such as William Bateson, the origin of species (by developmental shifts) and the survival of species (by adaptation) were two distinctly separate processes. Recent discoveries in developmental genetics that demonstrate that alterations in regulatory(e.g. homeobox) gene timing and interaction can lead to rapid and profound morphological change make untenable current debates on the origin of species, which are wedded to essentially similar models of selection and adaptation. Mutation is still at the base of genetic change, but its impact on structural versus regulatory genes will be markedly different. As such, the issue becomes a matter not of rejecting Darwinism in favor of punctuation (or vice versa), but of realizing the hierarchy of biological phenomena.

Alexandra Alexandri (Athens, GR):

Aspects of the Historical Contingency of Shifting Philosophical Positions on Human Origins Research

The search for human origins has consistently tried to determine the conditions that gave rise to the unique characteristics which make us human and differentiate us from other species. The emergence of these uniquely human traits have been traditionally posited as distinct events, situated in space and time, providing a series of guiding posts, which ideally allow us to determine the greater or lesser humanity of any subject under scrutiny. The definition of these 'boundaries' of humanity, along with a series of associated sets of oppositions, has been a dominant paradigm in origins research.

The aim of this presentation is to provide a short overview of the major trends that have dominated origins research and to suggest that, although the preoccupation with this question is a persistent feature of human history, the definition of a set of unique and distinct human characteristics on the one hand, and the translation of these characteristics into tangible evidence (anatomical features, physical evidence in the form of tools or writing etc.) on the other, are historically contingent. The recent shift of 'evidential' focus from a series of recognizable activities, like tool-use or symbol-manipulation, to the body itself, is the result of a combination of factors. At the heart of the issue is a question of shifting balances within and between disciplines and a reconsideration of the dichotomy between humanities and

social sciences versus the so-called 'harder' sciences. As strict disciplinary boundaries are apparently breaking up, concepts and methodologies are increasingly being imported from one discipline to another; however, along with the promise of unshakable scientific 'proof', they often carry distinct theoretical baggage which often goes unchecked.

Panel VII:

The Scientific Background of Wittgenstein's *Tractatus*

This is how the story is usually told: "Suppressing his growing preoccupations with philosophical questions," Ludwig Wittgenstein studied to be an engineer first in Berlin, then in Manchester. Some time after arriving in Manchester in the fall of 1908, he discovered the problem of providing mathematics with logical foundations and Russell's Principles of Mathematics which "was to prove a decisive event in Wittgenstein's life" (quoted from Ray Monk's biography). In his first encounters with Russell in the fall of 1911, Wittgenstein "is argumentative and tiresome," thinking "that nothing empirical is knowable" and "that there is nothing in the world except asserted propositions" (from Russell's letters to Ottoline Morrell). In January of 1912, Wittgenstein presents his teacher with a manuscript that convinces Russell of his extraordinary talents.

What ideas did Wittgenstein bring to his encounter with the works of Russell and Frege? How profound a philosophical education did he receive not only by reading Schopenhauer or Weininger in Vienna, but by studying at an early age the works of Hertz and Boltzmann, by receiving a scientific and engineering education at the turn of the century in Vienna and Berlin? These question urge a closer look at Wittgenstein's formative years and a reconsideration of the usual story according to which the Tractatus inherits not only its problems from Russell and Frege, but also the terms in which they are solved.

The following two propositions are at issue for the proposed panel:

– In order to understand Wittgenstein's Tractatus, the philosophy of late 19th century science needs to be considered, including controversies and recent developments in physics, geometry, varieties of vitalism, psychophysics or what we might now consider the beginnings of cognitive psychology.

– This improved understanding of the Tractatus reveals its commitment to a form of naturalism or physicalism according to which the complete analysis of a proposition would involve a (psycho)physical analysis.

Each of the papers explores a particular aspect of the scientific background to Wittgenstein's Tractatus. All four contributions to the panel endorse the first of these propositions; they develop divergent stances on the second. (A clarification of these propositions may also help us better appreciate the relation of Wittgenstein and the Tractatus to Carnap and the Vienna Circle.)

1. **Sabine Döring** and **Thorsten Sander** (Universitat Essen, D):

Wittgenstein and Gestalt Theory

In his early as well as in his late philosophical writings Wittgenstein confronts the problem of aspect-perception. A puzzle picture, namely the Necker cube, already appears in the Tractatus [TLP 5.5423; cf. NB 9.11.14]. From 1935 onwards, Wittgenstein frequently focuses the problem of seeing-as in connection with his discussion of Kohler's theoretical approach [e. g. PI II xi, RPP I & II]. His thorough investigation of the subject suggests that his remarks on Gestalt theory must not be regarded as "Vermischte Bemerkungen". Their systematic significance, however, remains to be shown.

As both the early and the late Wittgenstein aims at solving the problem of aspect-perception, we will, firstly, analyse Wittgenstein's turn from Tractanan to his late philosophy with respect to the question of seeing-as. Secondly, we will deal with Wittgenstein's critique of Gestalt psychology and, in particular, of Kohler's account. To conclude we will, thirdly, show how his remarks are related to his late philosophical approach as a whole, especially to the problem of rule-following and to his rejection of mentalism.

David Hyder (Max Planck Institut für Wissenschaftsgeschichte, Berlin, D):

Logicism and Atomism in Wittgenstein's Tractatus

A traditional view of Wittgenstein's Tractatus takes the book to be concerned primarily with Russell's and Frege's theories of logic. It includes a number of puzzling allusions to the natural sciences, along with a few references to Heinrich Hertz, but these lie outside the main lines of its arguments. More recent work has emphasised these latter aspects by relating Wittgenstein's picture-theory to those of Hertz and Boltzmann, sometimes suggesting that the work is best understood within a German/Austrian tradition of scientific epistemology. But the disjunction should not be exclusive. In trying to understand Wittgenstein's relation to these two apparently distinct traditions, we need to grasp the problems with which he was concerned, and the solutions that he gave to them. Only then will these connections give us philosophical insight into his work, and into that of his first readership, most importantly the Vienna Circle.

In the proposed presentation, I argue that the Tractatus should be understood as a work of logicism - that its fundamental concern is to explain what logic is, and how it is distinguished from other scientific fields. This concern emerges directly from problems encountered by Russell and Frege in their attempts to provide a philosophical basis for the logical systems of Principia Mathematica and the Grundgesetze der Arithmetik. But the solution that Wittgenstein gave to these problems was fundamentally determined by the work of Hertz, Mach and Boltzmann. Of particular importance is Boltzmann's reply to his phenomenologist opponents, where he argues that the rules according to which one applies formal systems in physical theory can only be made determinate in reference to a model, and that such a model necessarily takes the form of an atomised manifold, i.e. of a discrete state-space. The state-space of the Tractatus is easily identified: it is the space of elementary facts defined in the opening sections of the book. But this similarity tells us little until we bring it into connection with what Thomas Ricketts, borrowing a phrase from Sheffer, has called "the logocentric predicament," i.e. the problem of defining logic from a neutral standpoint. Wittgenstein's move to what we may well call a model-theoretic conception of logic represents a decisive break with the views of Frege and Russell. While logic remains, on the view of the Tractatus, a science that is completely distinct from all other ones, it is does not, for Wittgenstein, acquire this status because it consists of 'laws of thought' (Frege) or because of our acquaintance with platonic 'logical objects' (Russell). Logic is the limiting case of a scientific theory, for it describes invariant properties of the state-space in which our experience unfolds. The paper has three sections. In the first, I outline the problems that Wittgenstein inherited from Russell's and Frege's philosophies of logic. The second describes Boltzmann's model-theoretical critique of the phenomenological interpretation of physical theory, and its relation to the epistemologies of Hertz and Mach. The last section offers a condensed reading of the Tractatus's theory of logic, showing how Wittgenstein applied these ideas to his logicist problems. My point of departure is the view of logic common to Russell and Frege, which was first explicated in Jean van Heijenoort's 1967 "Logic and Calculus and Logic as Language."

Timm Lampert (Universitat Bern, CH):

Psychophysical and Tractarian Analysis

It will be argued for the thesis, that Tractatarian Analysis presupposes a mechanistical world view. This will be exemplified in sense-data analysis: On the background of the controversies in sense-data analysis at the turn of the century it will be shown, that Tractarian analysis presupposes a psychophysical sense-data analysis. This is grounded in a mechanistical world description in the Hertzian manner, which Wittgenstein admits in the Tractatus because this description describes all possible atomic facts and only these.

The paper will be divided into the following parts:

1. Sensation measurement and psychophysical vs. introspective analysis.
1.1 Exemplification: Visual sense-data analysis.
2. Tractarian Analysis:
2.1. Analysis results: Points and sensation units.
1.2. Independence of atomic facts: Elementary description of the visual field.
2.3. Mechanistic sense-criterium: Colour exclusion.
2. Later Criticism: An argument against the analysis in sensation units.
4. Tractarian Analysis and natural sciences.

By discussing Tractarian Analysis on the background of sense-data analysis, it will be stressed that contrary to other interpretations a physicalistic interpretation of the Tractatus does fit the text best and has greater explanatory power.

Alfred Nordmann (University of South Carolina, USA):

The Limits of Science, Language and the World: Hertz and Wittgenstein on The Problem of Life

Wittgenstein's picture-theory of language, his discussion of "nets" and mechanics in TLP 6.341 ff., also his conception of analysis have all been traced to Hertz, and even the philosophical method of "solving problems" not by answering the questions but by removing painful contradictions such that "our minds, no longer vexed, will cease to ask illegitimate questions" (quoted from Hertz's Introduction to his Principles of Mechanics).

When Wittgenstein refers to "Hertz's Mechanics, on Dynamic Models" in TLP 4.04, he demonstrates his familiarity not only with the Introduction and architectonic of Hertz's Principles of Mechanics but also with the body of that work. This paper articulates a prominent but as of yet unappreciated aspect of Hertz's influence, one that is stated in the Introduction and finds further development in the discussion of dynamic models. It argues that Hertz's work prepared the ground for Wittgenstein's conclusion that "even if all possible scientific questions be answered, the problem of life has still not been touched at all" (TLP 6.52). Wittgenstein's remarks about the limits of representation and about the problem of life as a problem of will should therefore be situated not only in his reception of Schopenhauer, but also in reference to Hertz's discussion of force and its rejection of vitalism.

The paper will focus on the immediate context of the crucial remarks by Hertz. In his Introduction he notes a limit of his proposed "third" image of mechanics: "Our fundamental law, although it may suffice for representing the motion of inanimate matter, appears [...] too simple and narrow to account for even the lowest processes of life." This limit, however, is said to be a virtue of the proposed image: "While it allows us to survey the whole of mechanics, it shows us what are the limits of this whole." Other conceptions of mechanics treat forces with the tacit stipulation "that, if need be, later on a contrast between the forces of animate and inanimate nature may be established" (quoted from the Introduction). Hertz's proposed mechanics is more sharply delineated (paragraphs 318 to 322 of the Principles of Mechanics).

The paper offers a partial reconstruction of the context which prompted Hertz's remarks. Time permitting, it will also assess the contemporary reaction to Hertz's remarks, especially Richard Manno's 1900 "critical study of mechanicism and freedom of will".

Panel VIII:

A Turning Point in Philosophy? Logical Empiricism's Revolutionary Agenda

The aim of this symposium is to explore the groundwork for the leading ideas of Logical Empiricism, much of which was already set in the decades prior to 1930, and to trace the evolution of what were originally the principal themes of early Logical Empiricism as the movement spread throughout the Europe and North America. Even during the formative period, later leaders of the movement – notably Moritz Schlick, Hans Reichenbach, Rudolf Carnap, and Otto Neurath – were already engaged in disputes that would be resolved through agreement on basic principles, principles that would eventually form the premises broadly shared by later Logical Empiricists. One important vehicle for these discussions was the "first" Vienna Circle, organized well before Schlick and Carnap arrived in Vienna. But there were also disagreements that arose among the eventual founders of Logical Empiricism, and consensus was achieved only through careful consideration of the issues at stake, as well as exchanges of key ideas. The issues at stake involved such matters as geometric conventionalism, the epistemic function of empirical data, criteria of meaningfulnes, concept-formation, even the role of the apriori in the body of scientific knowledge. Of course, Vienna was by no means the only place in which the later ideas characteristic of Logical Empiricism captured the interest of scientifically-minded thinkers. Indeed, philosophers throughout the world were engaging many of the very same questions, often set in quite distinct problem-contexts. Besides the early collaboration with sympathetic groups in Berlin and Prague, many philosophers in Europe and North Amenca were focussing their energies on similar perplexities.

Thus, young philosophers from North America (Quine, Nagel) and England (Ayer) visited the discussion groups in Vienna, Berlin, and Prague to discover how the insights of the thinkers there could be applied to the concerns that interested them and their colleagues at home. At the same time, philosophers from Vienna, Berlin, and Prague were welcomed as speakers and visiting faculty in many other countries. The internationalism of the early movement was institutionalized in journals like Erkenntnis and collaborative works like The International Encyclopedia of Unified Science. Then, as tragic political developments in Europe forced the early Logical Empiricists to find new homes abroad, the movement became more internationalized than ever before, as it reached the mature stage during which it became the leading philosophy of science. The purpose of this symposium is to broaden the scope of discussions of early Logical Empiricism, by extending it beyond its hey-day (1930-1936) as well as its geographic loci in the capitals of Europe.

Alan Richardson (University of British Columbia, CDN):

Revolution Here Now: Logical Empiricism in its Place and Time

There are many recent negative assessments of logical empiricism's revolutionary ambitions. Some argue that logical empiricism never was particularly revolutionary, being from the start a simple variant of a centuries old project of empiricism. Others argue that logical empiricism was not revolutionary since, for example, its rejection of metaphysics failed to stop philosophers from doing metaphysics and its rejection of naturalism has itself been rejected. These are curious ways of arguing. It is as if one could argue that the Sandinista movement in Nicaragua in the 1970s was not revolutionary, since it was, after all, a variety of Marxism and Marxists had been around for a long time, and, anyway, it did not last. My talk takes a different approach, seeking to find it utterly disconnected from all previous philosophy, but by looking at how it presented itself within the context in which it arose.

I will argue that logical empiricism was not revolutionary in virtue of being scientific philosophy, nor by being committed to formal methods, nor by being committed to the rejection of metaphysics; scientific, formalistic, and ametaphysical philosophies had existed in Europe for some time and in many varieties. I will argue, rather, that logical empiricism was a revolutionary variety of scientific philosophy because of three features: its methodological catholicism, its awareness of the problems implicit in trying to turn philosophy into a science, and the progressive social project it found in a scientific, ametaphysical philosophy. These three features of logical empiricism in the 1920s and 1930s – methodological Catholicism, reflexivity, and progressive social engagement – are not features that have been stressed in most accounts of logical empiricism, although my thinking here has been influenced by E. Nemeth, T. Uebel, P. Galison and other recent commentators. A comparative account of logical empiricism and American pragmatism in the 1930s from this perspective predicts that these projects would find one another to be close philosophical kin and, thus, explains the now puzzling fact that American pragmatists were among the most vocal supporters of logical empiricism in the diaspora.

Thomas E. Uebel (University of Manchester, UK):

The First Vienna Circle Revisited

Recent work on Logical Empiricism has done much to correct the myth of its analytic isolation from continental philosophy. This has been most salutary for a number of reasons. Recognizing the debt Logical Empiricism owes to neo-Kantian philosophy relativises the presumption that it offers only radical empiricism and makes it possible to appreciate the advance of its new conceptions of the a priori and of objectivity. And recognizing the opposition of Logical Empiricism to existential hermeneutics and holistic social thought similarly underscores that they understood themselves as part of the rationalist Enlightenment tradition and not just as academic specialists. But has all of the relevant context of the rise of Logical Empiricism been thus recovered?

Beyond these larger influences and oppositions, discernible throughout Central Europe at the time, also local traditions will have to be considered if we wish to do justice to Logical Empiricism not only as a reactive but also as a proactive intellectual movement. Concentrating on its Viennese centre, I will investigate the thesis that there existed something like a first Vienna Circle long before the Circle around Schlick first began to meet. Of particular interest are the questions whether (and if so, in what sense) this first Circle was the conduit for the influence of what has been called the Austrian philosophical tradition, and whether (and if so, in what sense) this first Circle played a founding role for the Vienna Circle as we

have come to know it – thus counterbalancing the neo-Kantian influence and helping to articulate Logical Empiricism as a distinctive philosophical voice.

Thomas Oberdan (Clemson University, USA):

The Trouble with Consensus

Many of the most fundamental insights of early Logical Empiricism were actually garnered from previous work by their early leaders. This is particularly true of the geometric conventionalism that dominated Logical Empiricism throughout the long period of its dominance in the philosophy of science. Based on the philosophical reaction to the new physics of Relativity Theory, Moritz Schlick pioneered a conventionalist epistemology which departed radically from the prevailing orthodoxies of the day (neo-Kantianism and Machian phenomenalism). With Einstein's support, Schlick's views began to find broad support, when it was challenged by Hans Reichenbach, who proposed an alternative based on a modification of Kant's synthetic apriori. Through correspondence Schlick convinced Reichenbach that his views were really just the conventionalist ones Schlick had been urging all along. Undoubtedly, Schlick's success in persuading Reichenbach can be largely attributed to the fact that Reichenbach's neo-Kantian approach already incorporated key ingredients drawn from Schlick's epistemology, in particular his concept of truth and his understanding of concept-formation by means of the doctrine of implicit definition. Yet shortly after Schlick and Reichenbach had reached a consensus, Rudolf Carnap challenged the doctrine of implicit definition as inadequate for the constitution of genuine concepts. Although Carnap's challenge has been the topic of recent discussions (by Goldfarb, Howard and Richardson), no attempt has been made to relate it to the historical context of the Formalist-Logicist debate between Hilbert and Frege. By setting the dispute among the later leaders of Logical Empiricism in the classic context of the controversy between Formalism and Logicism in the philosophy of mathematics, new light is shed on the viability of the doctrine of implicit definition.

Panel IX:

Atoms, Mechanisms, and Mathematics: Modeling Nature in Late 19th Century Physics and Physiology

These papers look at Helmholtz, Mach, and Boltzmann's attempt to model nature at a time when there was no standardly accepted view of what nature was. In the first two papers, McDonald and Pojman examine the role of physiology in the work of Helmholtz and Mach. One of the remarkable features of the late nineteenth century is that physics and physiology interacted. Helmholtz, of course was not only the first to measure the speed of nerve transmission thus opening up new avenues of physiological investigation, but also came to his theory of the conservation of energy through physiological research on animal energy. Similarly, Mach is considered by modern physiologists as the first to realize that our sense have neural nets which pre-process information before it reaches the brain. How then did the physiological orientation of these philosophers' influence their models of physics and science? In the next two papers, Neuber and Wilholt turn to issues arising from the attempt to model atoms. Neuber discusses an episode where Ostwald criticized Boltzmann's attempt to offer a pictorial view of atoms by arguing that since atoms are not visible, visual picture of atoms cannot have any epistemic meaning. Mach suggested that perhaps epistemic content is not the essential component for a model. What, then, are the requirements of a model? In the last paper, Wilholt points out similar themes in Boltzmann's critique of the the mathematical phenomenologists who had argued that physical reality should be modeled by a mathematical continuum rather than by discrete particles. Boltzmann showed that the mathematical phenomenologists had assumed a particular interpretation of a mathematical limit, and that under his own interpretation atomic models were quite viable.

Patrick J. McDonald (University of Notre Dame, USA)

Hermann von Helmholtz and the Methodology of Sensory Science: the Zeichentheorie and Physical Models of Hearing Mechanisms.

A number of recent discussions of the philosophy of Hermann von Helmholtz (1821–1894), have placed considerable emphasis upon his Zeichentheorie (Friedman 1998, Hatfield 1990, Heidelberger 1993, Schiemann 1997). This paper intends to explore the Zeichentheorie in the context of a specific case study from Helmholtz's research in hearing. I do not wish to argue that the Zeichentheorie originated from this aspect of his practice, nor that the practice is simply an instance of the theory in action. Rather I wish to emphasize the manner in which his hearing research illustrates a different and important feature of the Zeichentheorie, or at least that part of the theory emphasizing lawful relations among sensations. The Zeichentheorie refers to Helmholtz's view in his philosophy of perception that sensations are signs to be coordinated with their primary objects. Sensations are not replications of their objects, and they are not understood to resemble objects in any meaningful way. Rather sensations as signs become coordinated by human agents in law-like connections or at least patterns of regularity. Thus given the same objects in the same or similar circumstances, we would expect the same or very similar signs to be perceived. The focus of the relation between sign and signified then no longer revolves around similarity, but rather around law-like coordination. This notion of laws plays a significant role in his general epistemology and not surprisingly, it does so as well in his philosophy of science and scientific practice.

In accounts of the Zeichentheorie, there has been a focus upon law-like patterns among sensations, and law-like patterns among objects in the world. Further there is the understanding that there are coordinating relations between these two distinct realms of entities, i.e. between objects "out-there" and percepts "in-here." But there has not been much in the way of detailed discussion of the law-like patterns within this link between objects and percepts. However, Helmholtz devoted a great bulk of his research, on vision and hearing, to establishing the law-like connections between events or objects in the world and those occurring in perceptual experience. In this work he contributes very significantly to the methodological idea emphasizing the tight links between physical signals, physiological processing, and psychological experience. This aspect of Helmholtz's research has been discussed recently as well (Hatfield 1990, Turner 1993, Vogel 1993, and others).

To illustrate the nature of these tight links, I will focus upon a pair of specific hearing models from Helmholtz's Die Lehre von den Tonempfindungen (1863). The first concerns his attempt to model the vibration of the basilar membrane, a part of the human cochlea in the middle ear. The second concerns his theory of combination tones which he attempts to explain via a mathematical model of the vibration pattern of the tympanic membrane in the outer ear. In these models Helmholtz attempts to coordinate tightly formal physical concepts with concrete physiological mechanisms. These physiological processes then become carefully coordinated with regular patterns of perceptual experiences, namely the range of musical tones and their subcategories. I will discuss briefly the details of these explanations. But I wish to pursue their methodological interest as well.

Their primary interest lies in providing a concrete example of a different way to understand the Zeichentheorie. They reveal one more theater in which Helmholtz developed the ideas brought forth in the later (circa 1878) and more explicit version of the Zeichentheorie. Yet, there are important other issues at stake as well. For one, the Zeichentheorie requires an account of the link between theory and experiment. In this case Helmholtz cleverly synthesized a well-developed theoretical framework, classical mechanics, with an incomplete and developing experimental tradition, precision sound production and experimental physiology. In doing so he went beyond the immediate scope of available evidence. There was room in his methodology for hypothetical-physical models. However, he was quick to point out the tentative nature of his models, and the manner in which they effectively synthesized the available evidence. The paper does not intend to represent a single theme in Helmholtz's philosophy of science. Rather it will illustrate the convergence in practice of a number of prominent themes in Helmholtz's philosophy of science.

Matthias Neuber (Berlin, D):

Physics without Pictures? The Boltzmann-Ostwald-Controversy and Mach's Attempt of a Reconciliation

After a phase of rapid development in the 1860's and 1870's, kinetic theory and with it the whole approach of atomism seemed to run out of steam at the end of the 19th century. Not only theoretical

problems like the "recurrence paradox" or the reversibility paradox", but also the notorious lack of experimental corroboration made it obvious that atomism and the kinetic theory were in a severely critical state. Hence it was not at all astonishing that atomism had to face the rise of rival programmes. One such programme was energetics, which - having its roots in the 1850's - celebrated a short revival in the mid 1890's. Though its influence on the concrete scientific development was rather small, energetics played an important part in the discussion on the methodological foundations of modern physics.

In the talk, I want to give an impression of the controversy between atomists and energeticists as it is reconstructable from the writings of Ludwig Boltzmann (for the atomists) and Wilhelm Ostwald (for the energeticists). A further point of interest will be the position of Ernst Mach, who suggested that a reconciliation of both programmes should be possible.

What was the central issue of this whole debate? As I will try to show, there were at least three levels of dispute - a physical, a mathematical and a methodological one. I will further argue that there was indeed one central issue, which dominated the whole debate. This issue originated from the discussion at the methodological level, and it concerned the use of pictures in the context of physical theory construction. It is this issue that I will concentrate on in my talk.

Now, concerning the use of pictures, the respective positions of Boltzmann and Ostwald may be summed up as follows: While Ostwald, in his famous speech at the "Naturforscherversammlung" in Lübeck in 1895, demanded that physicists should completely dispense with pictures, Boltzmann, at the same event, countered that pictures are for several reasons indispensable. In short, Boltzmann maintained that Ostwald goes astray, if he thinks that there is a categorical difference between concepts for measurable quantities and such pictorial" concepts like that of the atom. Instead, Boltzmann argued, we have to proceed from the premise that all our concepts have the status of pictures.

The Lübeck dispute was only the starting point of a larger scaled debate. Ostwald and Boltzmann continued their battle in the "Annalen der Physik", and they both had allies from their respective camps. Where did Mach stand in this controversy? As indicated above, Mach's pretension was to reconcile the both extremes, i.e. the "no-picture-view" of Ostwald and the "all-picture-view" of Boltzmann. In order to do that, Mach made use of a pragmatic argument. Thus, he appreciated - with Boltzmann - the heuristic value of the atomist picture and at the same time devaluated – with Ostwald - its epistemic import. Accordingly, Mach arrived at the conclusion that pictures are indeed indispensable, but only in so far as they are heuristically useful. This in turn implied that we do not have to argue in terms of epistemology, if we want to justify the use of pictures in the context of physical theory construction. I think it is this line of reasoning which makes Mach's standpoint worth discussing.

Torsten Wilholt (Universitat Bielefeld, D)

Ludwig Boltzmann as a Philosopher of Applied Mathematics

Towards the end of the 19th century, atomism was, at least amongst German-speaking physicists, a position defended only by a minority. It was on the decline in spite of its remarkable past successes in the explanation of phenomena, which were most striking in the kinetic theory. Ludwig Boltzmann was one of the protagonists of the development of kinetic theory and a man of unswerving atomist conviction, which he frequently defended against its opponents. His main argument, however, did not invoke the abovementioned successes of atomist assumptions in physical explanations. In the talk, I want to analyse and comment on Boltzmann's own, highly original argument for atomism: For Boltzmann, the atomist picture is a prerequisite for making the application of the differential calculus a reasonable enterprise. Atomism thus turns into an outcome of a specific philosophy of applied mathematics.

This argument, however, is scattered across Boltzmann's writings in diverse variants, which I intend to elaborate on in the talk. One main line of his thought can be sketched as follows: The mathematical phenomenologists demand the abandonment of atomist principles and their replacement by the idea of a physical continuum. The continuum idea in question is the mathematical continuum concept as it is manifest in the differential equations of physics. However, Boltzmann claims, the phenomenologists thereby obscure the discontinual foundations of the continuum concept underlying the differential calculus, which derives from the mathematical concept of limit. This concept, if applied to physical reality, must from Boltzmann's point of view be understood as follows: The limit of a function is established by increasing a certain number (of particles, of sections into which an interval is divided) until further increment of that number would not have any "noticeable" influence on the result any more. So, for Boltzmann, to forget about these foundations and take the differential equations themselves as the most straightforward representation of reality (as the mathematical phenomenologists do) means only to make an additional assumption: the assumption, that is, that however our means of observation will

improve and however more subtle the differences "noticeable" for us will become, we will never chance upon a difference between the measured facts and the magnitudes given by mathematical (infinitesimal) limits. For Boltzmann, atomism is the position which does not make that additional assumption, and since the assumption is totally unwarranted, the mathematical phenomenologists go beyond the observable facts further than the atomists do. Thus, atomism is the most natural stance for a scientist applying the differential calculus to nature.

How was Boltzmann's peculiar interpretation of the mathematical concept of limit brought about? I will argue that he was a finitist concerning the philosophy of mathematics, and that he was a finitist because he was an empiricist. For him, the concept of number is empirically given and derived from the practice of counting — which is, like any human practice, a finite activity. This conception gave rise to his sceptical attitude towards infinitesimal methods and specifically Cantor's continuum concept. His point of view thus echoes the early modern empiricists' criticism of infinitesimal mathematics and foreshadows the finitism of many a 20th century philosopher of mathematics.

Panel X:

The Rise of Mathematical Physics and its Impact on Philosophy

The influence of philosophically-minded scientists like Mach, Boltzmann, Hertz and Poincaré (to name only a few) on the subsequent development of science has been studied rather closely during recent decades. Perhaps the best inquired example is the influence of Mach on Einstein and the "genesis" of the special theory of relativity. The inverse direction, the impact of the new developments in science on philosophy has not been so closely investigated likewise, some exceptions not withstanding. The panel wants to inquire one particular branch or aspect of this influence, namely the impact of the rise of mathematical physics on philosophy.

It is well known that the first generation of philosophers of science in the 20th century, Schlick, Cassirer, Carnap and Reichenbach, were all eager to study the new theory of relativity and its philosophical consequences. And it is also well known that the new logic (i.e. the logic of functions, relations and quantifiers) was of greatest importance for these philosophers and their doctrine of Logical Empiricism. What is not so well known - or even ignored - is the fact that the rise of mathematical physics during the first three decades of the 20th century has left its imprints on the thinking of these philosophers, their questions and problems as well as the intended solutions. In order to recognize this, one must first become aware that mathematical physics is not just "logic cum experience", but an autonomous enterprise which is neither confined to logicism nor tied up with empiricism, but has its own means and goals. The talks of the panel want to investigate some aspects of these means and goals and their significance for the development of philosophy of science in the first three decades of 20th century.

Ulrich Majer (Universitat Gottingen, D):

Hilbert's program to axiomatize physics (in analogy to geometry) and its impact on Schlick, Carnap and other members of the Vienna Circle

When Hilbert announced in 1900 that physics should be axiomatized according to the pattern of geometry most scientists did not understand what this meant and what was it good for. During the next decade Hilbert demonstrated what he had in mind in his lectures on mechanics and continuum-mechanics. The latter embraced almost all branches of physics such as the theory of elasticity electrodynamics, thermodynamics and so on. These lectures were not published but only circulated in a small number of copies. This may have promoted some misunderstandings and irritations regarding the "axiomatic method" - misunderstandings, which we find replicated decades later in the writings of the logical empiricists. Meanwhile had Hilbert's quite distinct program to formalize logic and mathematics gained great respect such that some of the most advanced philosophers of science tended to identify or perhaps better to merge both programs. This led to an interesting development, particularly in the case of Carnap, which at the same time has some very perplexing and problematic aspects. The paper tries to make, at least, some of these aspects "understandable".

Tilman Sauer (Universitat Bern, CH):

Hilbert's search for an axiomatic foundation of physics and some of the reasons, why it was ignored by most physicists and philosophers

In Hilbert's program of an axiomatic foundation of physics General Relativity played a key role, not only because Hilbert saw in a unification of Mie's theory of electrodynamics with Einstein's principle of relativity the only reasonable approach, but also because in this field his axiomatic approach had accomplished the most impressive result, namely a "deduction" of the general field equations. This is at least the way, in which Hilbert saw his own work. In spite of this undeniable fact it is very astonishing that most physicists and philosophers of science tended to ignore his work in this domain. (Some notable exceptions are Einstein himself as well as Hilbert's disciples H. Weyl and M. Born.) The paper tries to identify some of the reasons for this remarkable fact.

Michael Stöltzner (Wien, A)

The Principle of Least Action as the Empiricist's Shibboleth

Over the centuries, no other physical principle has more been the object of exalted hopes in universality, mathematical counterexamples, and metaphysical controversies than the Principle of Least Action (PLA). In this contribution I will illustrate that for a whole generation of physicists this principle became the touchstone for the weight and independence attributed to universal mathematical concepts in explaining physical facts. Moreover, the principle's mathematical basis, the calculus of variations, provided a highly general scheme that was adaptable not only to mechanics, but also to electrodynamics, thermodynamics and general relativity. Did this indicate that physicists already possessed a "leitmotif in striving for a unified theory of the physical world" (Helmholtz) by expressing all laws in a simple variational principle or were these principles just useful rules that served the economy of thought? As the unification consisted in specifying the same mathematical concept by way of different Lagrangians, the success of the PLA also raised questions as to whether mathematics represented a common foundation of all theorizing sciences.

Hence it makes no wonder that it figures prominently throughout all stages of Hilbert's program of the axiomatization of the sciences. When in 1915 Hilbert published his version of general relativity within an attempted unified field theory, he mainly referred to the general invariance of the action integral and formulated a version of Noether's theorem – a seminal result for all further studies of symmetries. Invariances were also most important in Planck's Kant-inspired epistemology. To Planck, both fundamental constants and the PLA represented a step from the 'relative' to the 'absolute'. Being a formal expression yet to be specified brought them only closer to a Kantian category, admittedly in a 'relativized' sense. In 1866 Boltzmann even attempted to relate the PLA to the Second Law and although this project failed by far still by the end of his life he insisted on their close relation.

Empiricists, on the other hand, typically played down the difference between the PLA and the related differential equations, not only because the PLA smacked of teleology. Mach rephrased it almost tautologously as "precisely so much happens as possibly can happen under the conditions, or as is...uniquely determined by them." Petzoldt considered this principle of unique determination even as an "analytical expressions for the principle of sufficient reason". Unlike Mach, Petzoldt and Ostwald viewed this within a possible world semantics severely criticized by Boltzmann. Moreover, Mach and the energeticists failed to clearly distinguish between the PLA and energy conservation, such that Mach still in 1896 judged Boltzmann's 1866 failure a success. Empiricists' underscoring the PLA continued among Logical Empiricists although, on their account, simplicity represented a major value of physical theory. But their strictly separating 'analytical' theory from the empirical facts coordinated to it prevented a stronger than pragmatic appreciation of a principle of mathematical physics.

Miklos Rédei (Budapest, H):

Mathematical Physics and Philosophy in John von Neumann's Work

Philosophy of science and mathematical physics are most intimately related in the work of John von Neumann, especially in his activity in the field of foundations of quantum mechanics. The talk aims at proving this by analyzing some works, results and methods used by von Neumann. It is shown how the creation by von Neumann in 1926-1932 of the mathematical physical theory known as the "abstract

Hilbert space formalism of quantum mechanics" was motivated and guided by explicitly philosophical and methodological worries and considerations as opposed to the empirical findings or data which form the direct basis of quantum mechanics understood as a physical theory in theoretical physics. It will be argued, further, that von Neumann's critique after 1932 of the abstract Hilbert space formalism of quantum theory and his quest for finding a new mathematical framework for quantum theory in the theory of operator algebras was conditioned to an even larger extent by purely philosophical reasonings. Those philosophical reasonings were close to the spirit of the logical positivism in that von Neumann insisted on eliminating from theory the empirically inaccessible elements, assigned a prominent role to formal logic and related methods and leaned towards operationalism. The mathematical physical investigations by von Neumann into the foundations of quantum mechanics resulted in discovering the structure known as "quantum logic", which had an impact on algebraic semantics in logic, and prompted philosophical discussions about the empirical or non-empirical character of logic. Evidence will be quoted showing that von Neumann distinguished mathematical physics from theoretical physics by pointing out the reflective character of the latter, thereby placing mathematical physics close to philosophy.

Panel XI:

British Empiricism and the Verification Principle: Locke to Hume

In what sense, if at all, was the Verification Principle of Meaning assumed or argued for by the seventeenth and eighteenth century British Empiricists? It is generally accepted by historians of philosophy that the principle plays no role in Locke's account of meaning or his treatment of scientific theory. On the other hand, at least since Popper's famous paper 'A Note on Berkeley as Precursor of Mach and Einstein' (1953), it has often been argued that Berkeley was an instrumentalist and subscribed to some version of the Principle. Hume too is often seen as supporting a verificationist position in his epistemology but exactly how this enters his philosophy remains obscure. The three papers in this proposal examine the ways in which the verification principle does or does not enter into the respective theories of meaning of the three philosophers and reaches conclusions which are somewhat at odds with standardly accepted positions.

John Rogers (Keele, UK):

Locke and the Principle of Verification

Although Locke's account of meaning is given in terms of simple and complex ideas with the simple ideas all having their origin in experience of sensation or reflection little connection between Locke's account and instrumentalist views about scientific theory have almost never been made. The question arises at whether Locke either was, or should have been, an instrumentalist. The paper looks at the development of his theory of meaning from Draft A (1671) of the Essay Concerning Human Understanding through to the latter's last edition. The answer which is given is that he neither was nor should have been and the reasons for this is to be understood through an examination of what Locke understood by complex ideas.

P. J. E. Kail (Cambridge, UK):

Berkeley and the Verification Principle

A significant number of the Vienna Circle were confident that the Verification Principle would remove the possibility of scepticism by limiting substantial and meaningful claims to those related to sensory experience. Superficially, Berkeley's procedures appears the same; for him scepticism is generated by unlinking claims about the real world from sensory experience. This kind of consideration has been pushed in a notable book on Berkeley by A.C. Grayling. Grayling contends that Berkeley is like the positivist and their descendent, Michael Dummett, that no meaning can be given to evidence-transcendent sentences, and that his argument against scepticism is framed in these terms. The paper argues that this is a complete misreading of Berkeley, by showing that Berkeley allows for the possibility of evidence-transcendent facts.

Marina Frasca-Spada (Cambridge, UK):

Hume's Copy Principle and the Meaning of "Vacuum"

The paper will reconstruct how Hume's Principle is used by him to construct a taxonomy of philosophical ideas; and in particular how the principle works in one of the cases in which it singles out a pseudo-idea, namely that of a vacuum. In this way the paper will show in whatsense, and within what limits, this principle can be regarded as an ancestor of the empiricist criterion of meaning.

CONTRIBUTED PAPERS

1. Anderson, Lanier R. (Stanford University, USA)

The System of Science and the Apriority of Causal Laws in Kant

Famously, one of the key problems that led Kant to the formulation of his mature "critical" philosophy was the need to guarantee the objectivity of judgments about causal laws in natural science against Hume's skepticism. Kant insists that causal laws can be genuinely objective only if they are known a priori, or independently of experience. The almost universal opinion of twentieth century Kant commentators is that this conclusion about the apriority of our concept of cause applies only to the general law that every event has a cause, and not to particular causal laws covering individual cases of causal interaction.

These latter laws are supposed to be known through experience, if at all. Michael Friedman has recently challenged this interpretive orthodoxy, arguing that Kant's account of Newton's laws must be understood to claim that particular causal laws – not just the general law of cause – are a priori in some sense. I will argue that a close reading of Kant's specific argument against Hume in the "Second Analogy" chapter of the Critique of Pure Reason supports a strong version of Friedman's conclusion. For Kant, every genuine causal law, even those laws covering the lowest level regularities of nature, must be a priori in a sense.

Given Kant's strong conception of the a priori, as knowledge justifiable in independence from experience, this result places Kant commentators under a burden to explain: 1) how Kant thought this sort of knowledge of causes was even possible, and 2) what the force of the qualification 'in some sense' is, when we conclude that for Kant causal claims are a priori "in some sense." Answering the second question will involve specifying what the relation could be between the a priori and empirical parts of our causal knowledge. I will suggest that the answer to the first question points us toward interesting differences between the conception of the mind assumed by Kant (and others in his historical context), and the one that has been dominant since the middle of the nineteenth century. These differences have important consequences for out understanding of Kant's claim that the transcendental operations of the mind "prescribe laws to nature" (Critique of Pure Reason, B 159), and thereby make a priori knowledge possible. For Kant, these transcendental functions of the understanding are themselves always subject to guidance by our demands, originating in the faculty of reason, for systematicity in our total theory of nature. It is only by elucidating the implications of this systematicity in science, I will argue, that we can understand Kant's conception of the relation between the a priori and empirical parts of our causal knowledge, and thereby make progress on the second question as well.

2. Ashooh, Michael X. (University of Toronto, CDN)

Repaying Lakatos' Debt to Carnap

The idea that Lakatos owes some sort of philosophical debt to Carnap may strike many as not only unfounded, but outright ludicrous. There are, after all, the various remarks found throughout Lakatos' work critical of Carnap's philosophy of science and Lakatos' well-known refutation of Carnap's inductive logic. There is the even more ominous portrayal in the introduction to Proofs and Refutations of Carnap as the arch-formalist, whose adherence to the dogmas of logical positivism has exorcised mathematics of its rational soul: the "situational logic" of proofs and refutations that the history of mathematics reveals. However, this paper argues that despite Lakatos' rejection of various key components of Carnap's philosophy of science, he has nonetheless incorporated some of Carnap's epistemological insights into his own philosophical project. The goal of this paper is to show that inconsistencies result from viewing Lakatos exclusively as a sophisticated falsificationist. Instead, I argue that he was attempting to synthesize what he felt to be the best aspects of logical positivism, and more specifically Carnap's philosophy of science, into his own brand of falsificationism.

This paper begins by showing that Lakatos' notions of "rational reconstruction" and a "rationality theory", as well as his distinctions between internal and external history, share conceptually similar roots to Carnap's notions of rational reconstruction, a linguistic framework and internal and external problems, respectively. In the case of "rational reconstruction" Lakatos, as the proto-Popperian, should have no

need for such a concept. Popper after all is quite skeptical of rational reconstruction and in the introduction to The Logic of Scientific Discovery argues that the initial, "intuitive" stages of theory development are not susceptible of logical analysis. Popper is quite content to take what scientists say at face value, while Lakatos, acknowledging that what counts as rational and objective can only be established relative to a particular theory of rationality and objectivity, is thus much more sensitive to the conceptual relativity of such scientific concepts as "falsifying instance", "successful test", "increasing empirical content", etc. I argue that his motivation for the adoption of a "rationality theory" shares basic epistemological assumptions with Carnap's notion of a linguistic framework and in particular Carnap's attempt to provide an objective basis for the concepts of science through the construction of formal systems. Thus, Lakatos' rationality theory (the methodology of scientific research programs), which he calls a meta-criterion for science, succeeds in providing an objective basis for appraising scientific developments because it allows for the rational reconstruction of such developments internally, that is, in terms of the logic of scientific discovery or proofs and refutations. Finally, just as Carnap sought to avoid epistemological and philosophical confusion by reconstructing philosophical problems internal to a particular framework, Lakatos sought to reconstruct the history of science in terms of competing research programs. However, rather than discard external history as bad metaphysics, Lakatos allows that such social and psychological explanations ("good" metaphysics) may sometimes be necessary supplements to the internal account, owing primarily to human ignorance.

I conclude with the following points. Lakatos was not a neo-Carnapian. Yet, faced with some of the problems of Popper's philosophy, and though he remained opposed to much of Carnap's philosophy, Lakatos attempted to synthesize several of Carnap's insights into his own philosophical project. I argue that this provides a more consistent reading of several aspects Lakatos' work, including his neo-Kantian origins.

3. Atten, Mark van (Utrecht University, NL)

Brouwer's Argument for the Unity of Scientific Theories

The Dutch mathematician and philosopher L.E.J. Brouwer (1881-1966) is well known for his work in topology and his philosophy of mathematics, intuitionism. What is far less well known is that Brouwer did work in the philosophy of the natural sciences as well.

Some of this work found a place in his 1907 dissertation, 'On the foundations of mathematics'. However, his thesis adviser, the mathematician Korteweg, recommended that Brouwer not stray too far from mathematics, and probably this is the reason why most of Brouwer's thoughts on the natural sciences were not included.

While crafting this dissertation, Brouwer corresponded frequently with Korteweg, and it is in those letters that we find one of Brouwer's most interesting excursions into the philosophy of science. It is also briefly mentioned in the 'Rejected parts of Brouwer's thesis', published in 1979 by Van Stigt, but elaborated upon only in an as yet unpublished letter dated November 13, 1906.

This particular excursion consists of an argument designed to answer the following question: assuming that natural science derives from many different phenomena affecting our various sensory organs in widely different ways, isn't it surprising that these phenomena can all be brought together in one, or a few, mathematical systems?

Brouwer agrees that this at first strikes one as miraculous. But, according to him, the simple explanation is that theories in the natural sciences are in fact concerned with the projections of these phenomena on our measuring instruments, and these instruments are very similar to one another in so far as they are all constructed from solid bodies. Brouwer concludes that the unity and similarity of these theories derives from their ultimately being founded on the one theory of movements of solid bodies.

In this talk, which concerns both historical and systematic aspects, I want to 1) present Brouwer's 1906 letter and its context in detail, 2) make a beginning with a critical analysis of the argument, 3) relate the argument to ongoing discussions in the philosophy of science. One application could be the following. Realists sometimes appeal to the occurrence of unifications of different theories as evidence in favour of realism. Brouwer's argument, if sound, might supply the anti-realist with an alternative interpretation of such unifications.

4. Beaney, Michael (Universität Erlangen-Nürnberg, D)

Analysis and Explication

In this paper I explore the relationship between Frege's (late) conception of analysis and Carnap's conception of explication. Although the term 'explication' only first appears in Carnap's work in 1947, there is a striking similarity between Carnap's conception of explication and the conception of analysis that Frege offers in his 1914 lectures on 'Logic in Mathematics', which Carnap attended. This conception of explication, I argue, can itself be seen as an explication of Frege's conception of analysis, and I suggest what actually led to Carnap's own discussion.

In the first part of the paper, I sketch the three responses that can be found in Frege's work to what is essentially the paradox of analysis – firstly, in his early work, based on the distinction between 'content' and 'mode of determination of content'; secondly, based on the key distinction of his mature work, that between 'Sinn' and 'Bedeutung', as offered in response to Husserl's criticism of his Grundlagen definitions; and thirdly, in his late work, articulated in his 1914 lectures. An explication is the transformation of an inexact, prescientific concept into a new concept that is 'similar', 'exact', 'fruitful' and 'simple', and which can 'replace' the old. Carnap himself gives the example of Frege's 'explication' of arithmetical concepts to illustrate the idea, and a further important example is, of course, Carnap's own explication of the concept of 'analyticity', where again there is an important connection with Frege, who can be seen as 'explicating' Kant's conception of analyticity through the idea of 'reducibility to logic' (though interestingly, Frege himself drops talk of 'analyticity' after the Grundlagen). In his earlier work, Carnap used the term 'rational reconstruction' to characterize his method, and what prompted his own talk of 'explication', I suggest, was not any change in methodology, but his response to the debate over Moore and the paradox of analysis which was opened up by the Schilpp volume on Moore in 1942.

At a time when methodological conceptions, and the issues raised by analyticity, are again receiving attention (as illustrated, to take just one prominent example, by Frank Jackson's recent book, From Metaphysics to Ethics), exploration of the Frege-Carnap relation, in the context of the development of analytic philosophy, is instructive.

5. Bonk, Thomas (University of Pittsburgh, USA)

Carnap und der Intuitionismus

Diese Arbeit untersucht Carnaps Auseinandersetzung mit intuitionistischen Ideen in den Jahren 1927 bis 1934. Die Übernahme intuitionistischen Ideengutes geht erstaunlich weit. Carnap akzeptiert die "finitistisch-konstruktivistische Forderung ... die Ablehnung reiner Existenzbeweise ohne Konstruktionsverfahren." (1930e, S. 308) In den "Untersuchungen zur allgemeinen Axiomatik" (1928/29) charakterisert er den Konstruktivismus durch die Festsetzungen: eine Aussage ist "[k-]wahr" falls sie in "endlich vielen Schritten beweisbar" ist; "[k-]falsch" falls sie in endlich vielen Schritten widerlegbar ist. "Der Konstruktivimus lehnt sich an den (von Brouwer und Weyl vertretenen) Intuitionismus an. Er unterscheidet sich von diesem dadurch, dass er den logischen Satz vom ausgeschlossenen Dritten nicht ablehnt. . . . der konstruktivistische [Standpunkt] scheint uns jedoch (aus hier nicht naher zu erorternden logisch-erkenntnistheoretischen Gründen) der richtige zu sein." (Untersuchungen, S.81)

Carnap versteht die "konstruktivistische Forderung", Allsätze nicht zu negieren und keine reinen Existenzaussagen zuzulassen, als eine den drei Positionen im "Grundlagenstreit" der Mathematik gemeinsame Forderung, die daher einen Ausweg weise (1930e, S.310).

Die auch von F. Kaufmann vertretene Behauptung, die drei Positionen stimmten in der "konstruktivistischen Forderung" überein, ist fragwürdig. In Freges Version des Logizimus haben Sätze, die aus der Anwendung von Quantoren auf unendliche Gesamtheiten entstehen, einen objektiven Wahrheitswert - unabhängig von der Möglichkeit idealisierter "Konstruktionen". Die "konstruktivistische Forderung" ist nicht erfüllt. Zudem hat Carnaps Konstruktivismus ca. 1930 mit dem Programm Hilberts wenig gemeinsam. In den konstruktivistischen Beweisen der "Untersuchungen", zum Beispiel, arbeitet Carnap stets mit inhaltlichen und abstrakten Begriffen, wie Implikation und Beweis.

Carnap verwendet den Konstruktivismusbegriff in zwei Weisen, die zu unterscheiden sind. Das Paradigma einer "Konstruktion" ist für Russell und Carnap die Reduktion des Systems der reellen Zahlen durch explizite Definitionen auf das System der natürlichen Zahlen. Dieser Konstruktivismusbegriff

versteht sich als Gegenposition zum "postulatorischen" Ansatz der Axiomatik. Die "konstruktivistische Forderung" spricht dagegen, nach Carnap, ein Sinnkriterium für mathematische Aussagen und Prädikate aus.

Carnaps "formaler" Konstruktivismus tritt besonders in drei Episoden hervor: in dem abgebrochenen Versuch einer "konstruktivistischen" Mengenlehre (1927); in den "Untersuchungen" zur Vollständigkeit axiomatischer Theorien (1928/29); und in der Sprache I von "Logische Syntax der Sprache" (1934f). Nach 1934 spielt die Konstruktivismusproblematik in seinen Arbeiten keine Rolle mehr. Ich zeige an diesen drei Stationen wie sich der konstruktivistische Gedanke entwickelt hat.

Das Problem der "impradikativen Definitionen", der "indefiniten" Begriffe und der Vollständigkeit bzw. Entscheidbarkeit axiomatischer Theorien stellten besonders die zweite von Carnap 1931 formulierte programmatische These des Logizismus in Frage: die Ableitbarkeit mathematischer aus logischen Aussagen. Das erste Problem löst Carnap durch die Festsetzung, dass Allgemeinheit durch offene Aussagen ausgedrückt wird, was die "Verifikation" durch direkte Ableitung ermögliche (1931d). Das Problem der Entscheidbarkeit, und damit verbunden das der Analytizität mathematischer Aussagen, wird erst auf dem Weg der Formalisierung der Metasprache in "Logische Syntax der Sprache" (1934f) und dort mit Hilfe eines nicht-konstruktiven Ableitungsbegriffs gelöst. Die Intuitionismus-Problematik wird, wie andere philosophische Fragestellungen, mit der Wendung zum Sprachkonstruktivismus und zum Toleranzprinzip hinfällig.

6. Brandon, Robert (Duke University, USA)

Hypothetico-deductivism in the Philosophy of Biology: Away from Popper and Back to Bernard

Hypothetico-deductivism (HD) arose in the 19th century and has achieved near hegemonic status in the 20th. There have been various arguments for it, and the understanding of it has varied, especially among philosophers of science on the one hand and practicing scientists on the other. Two primary strands can be seen in 19th century development of HD. First, as dreams of an infallibilist epistemology faded philosophers such as Hershel and Comte argued that a generativist (usually, but not always, inductive) logic, which would simultaneously serve as a logic of discovery and logic of justification, was unnecessary, and that a post hoc method, such as the HD method, was all that was required. Second, and much more important for the purposes of this talk, Whewell and Duhem, and others, argued that inductive methods could not yield the hypotheses that characterized the physics of the time (e.g., the wave theory of light or the atomic theory). This second line of reasoning has been endorsed by a number of figures in the Logical Empincist movement in the 20th century, perhaps most clearly by Carnap. The idea is simple. The premises for such an inductive argument will all be couched in an observational vocabulary, but the conclusion is stated in a different, theoretical, vocabulary, and inductive logic cannot bridge that gap.

Popper's arguments for HD, which have been by far the most influential among practicing scientists, can be seen as an extreme version of this view. Popper agrees that induction cannot bridge the gap between observations and theories, because induction cannot do anything, at least not in a way that can pass philosophical scrutiny. Popper's theory of conjecture and refutation (his version of HD) is meant both as a descriptive claim about how science operates and as a prescription for how to do science. I will argue that the normative aspect of Popper's view has had an enormous impact on the practice of biology over the last 30 years. I will suggest that this impact has not been altogether beneficial.

Interestingly, Claude Bernard (1865), gives a clear statement of HD and an argument for it that is quite different from those discussed above. Bernard was a physiologist who was concerned to transform medicine into an experimental science. To that end, he argued for a nuanced and nonexclusive version of HD. To make medicine experimental he urged practitioners of medicine to explicitly frame hypotheses and then test them experimentally. Bernard was no skeptic concerning induction, nor did he have any worries about any alleged gap between different vocabularies in medicine. Furthermore, he clearly saw HD as a part of good science, not the whole of it. That is, he recognized that the results of past experiments can influence (via something like induction) future hypotheses, which lead to new experiments. Thus he envisioned a two-way interaction between hypotheses and experimental data, not the one-way (from hypothesis to experiment) relation posited by exclusive versions of HD, of which Popper's view is clearly an example.

According to the received view, science must use the HD method because it is so theoretical. In contrast, according to Bernard, biology should use HD in order to become more experimental. It hardly needs to be said that the major premise for what has historically been the primary argument for HD, the idea that there is a sharp distinction between observational and theoretical terms, is rightly rejected today. That premise would have never looked as plausible to biologists as it once did to philosophers thinking primarily of physics. I conclude by arguing that contemporary biology would be better off following Bernard's prescriptions rather than Popper's.

7. Brenner, Anastasios (Université de Toulouse-Le Mirail, F)

The French Connection: Conventionalism and the Vienna Circle

At the turn of the 19th and 20th centuries, a new reflection on science made its appearance in France. The Newtonian paradigm was facing a series of difficulties. Before Einstein carried out his revolution leading physics on a new route, several scientists and philosophers had begun to subject the method and nature of science to an exacting analysis. Among the members of this intellectual movement, often labeled conventionalism, are Poincare, Duhem, Le Roy and Milhaud. What characterizes these thinkers is that they emphasized, each in a particular manner, the importance in scientific knowledge of decisional factors — that is conventions. It is acknowledged that conventionalism had an impact on the Vienna Circle. Hence the project to study the central idea of conventionalism and its reception.

One may call on two noteworthy indications: several passages of Poincaré and Duhem, rarely noted, reveal a genuine controversy between the two authors; Le Roy expressly formulated the project of renovating positivism. One may point out that the alternative represented by Poincare and Duhem, between on the one hand an inductivist analytic conception resting on probability and on the other a deductivist holist conception based on the history of science, has continued to divide the major authors of the 20th century: Carnap, Popper, Quine and Kuhn. The case of Le Roy goes to prove that the attempt to reformulate positivism antedates by ten or twenty years the Vienna Circle. These facts suggest that it would be a mistake to underestimate a French tradition that contributed largely to the elaboration of positivism and the development of the philosophy of science.

The logical positivists read Poincare and Duhem. They learned of the controversies stirred by their conceptions through Abel Rey. Neurath, Carnap and even Popper took up several major theses of conventionalism. But it remains to determine the exact impact of this movement. Another member of the Circle, Philipp Frank, characterized French influence thus: the conception of hypotheses made it possible to mitigate empiricism. It is time to weigh this acknowledgment. By escaping the narrowness of Mach's position, the Vienna Circle was able to bring about the renovation of positivism.

Thereby Philipp Frank pointed to one of the distinctive aspects of the French tradition: the central role ascribed to hypotheses. To the old question of the origin of knowledge, conventionalism provided an entirely new solution. For the first time a claim was made for the existence of an essential feature of knowledge which neither derives from facts nor springs from intuition nor even from the innate, a feature which makes it possible to constitute one system of representation among others. The introduction of the notion of convention opened the way for an assessment of decisional factors in science: the definition of concepts and the construction of theory.

8. Castelao-Lawless, Teresa (Grand Valley State University, USA)

Bachelard and the Development of the Philosophy of Physics

The works of French philosopher and historian of science Gaston Bachelard (1884-1962) represent a systematic attempt at demonstrating that the epistemological and ontological revolutions in physics embodied in relativity theory and quantum mechanics demand a new philosophy of science radically different from the traditional positivistic models or the historiographies of science offered by continuists Emile Meyerson, Henri Bergson, and Pierre Duhem. The conceptual vocabulary that Bachelard created to account for his "philosophy of the new scientific spirit" - terms such as "phenomenotechnique", "epistemological obstacle", "applied rationalism", "technical materialism", etc. – emphasizes the dissolution of the traditional demarcation between subject and object in the context of observation, the

theory ladeness of experimentation and the artificially constructed nature of scientific facts. The Bachelardian concept of "phenomenotechinque" is, I claim, the one which best illustrates the epistemological consequences, for scientific observation, of Bohr's complementarity thesis and Heisenberg's uncertainty principle. Concepts such as "epistemological *rupture*" were later used by Georges Canguilhem (philosophy of Biology), Michel Foucault (historiography) and, more recently, Pierre Bourdieu and Bruno Latour (science studies). Many of the positions of Bachelard also anticipate those of Karl Popper, Thomas Kuhn, Ian Hacking, and Peter Galison.

9. Cat, Jordi (University of Chicago, USA)

Social Rationality and Objectivity in Neurath's Philosophy and Economics: Is there a Tension between the Private Language Argument and the Central Planning Scheme?

It is well-known that Neurath argued against Carnap's rational reconstruction of scientific practice by appeal to a methodological solipsism and a basic phenomenal language. Neurath deployed a private language argument with the famous Robinson Crusoe example: on pain of an unacceptable solipsism-of-the-moment, the possibility of an epistemic agent's conceptualizing his experience systematically required that language be intersubjectively controllable. Neurath's argument anticipated Wittgenstein's own. In a recent discussion of Wittgenstein's private language argument Kripke discusses its application to the problem of rule-following in the form of the problem of the existence of private rules – it is not possible to obey rules privately. Kripke also points to an intriguing analogy between this sort of argument against private rules and languages and the argument put forward by Ludwig von Mises in 1920 (later famously reformulated by Hayek) for the rationality of economic calculations in central planning socialist schemes. One such schemes had been recently favored, and partly implemented, by Neurath himself. (It may be noted that Marx's discussion of similar questions use the example of a Crusoe-type situation.) This raises the question of a possible conceptual tension in Neurath's philosophy. I claim that this question is an important locus for the evaluation of Neurath's implicit conceptions of social rationality and objectivity in scientific practice.

10. Courtenay, Nadine de (Paris, F)

The Role of Models in Boltzmann's Lectures on Natural Philosophy (1903-1906)

The lectures on Natural Philosophy Boltzmann gave at the University of Vienna between 1903 and 1906 are mainly devoted to the study of the basic concepts of mathematics. I wish to show that, while initiating a very lucid philosophy of mathematics, these lectures also help to clarify the epistemology of physics outlined by the Austrian physicist in his *Populäre Schriften*.

The clarification I intend to put forward occurs through the discussion of a topic common to both disciplines : the crucial role Boltzmann claims for models in the construction of science. I wish to argue that Boltzmann's attention to models is linked with the parting of mathematics and physics taking place at the end of the nineteenth century. This parting displays the need to both distinguish and articulate, as two separate but indissociable moments of scientific construction, the symbolic mathematical structure and its interpretation, or model, that is, the more or less concrete realization of this structure in a domain of objects.

I intend to develop the following points :

– The 1903-1906 lectures confirm that Boltzmann's criticism of the reduction of physics to a description of phenomena(advocated by scientific phenomenology) shares its inspiration with the "arithmetization" of nineteenth century mathematics. The meaning of the mathematical representations used in physics (under the circumstances, of differential equations) is not directly fixed by the associated sensible contents but mediated by the systematic reconstruction of concepts provided, without resort to intuition, by rigorous mathematics.

– This amounts to say that science has to do with objective representations (and not with a translation

of sensible, subjective representations into symbols). But, because of its very originality, Boltzmann's position has generally been misunderstood. Indeed, at this point, Boltzmann embraces neither of the usual options : essentialism or formalism. According to him, no proposition or symbolic system can stand universally on its own. Without a model, any syntaxic construction (including logic) is only an abstract arbitrary calculus. Validity, for Boltzmann, is always relative to a specific field of objects. This need for a symbolic system to be completed by an external interpretation applies to both mathematics and physics. In mathematics, Boltzmann accepts Cantor's approach ; but he considers set theory as a mere calculus with regard to its lack of model ; in physics, he insists on considering (concrete as well as accepted abstract) analogies or technical realizations, not as mere side products, but as Ç logical proofs E.

– Boltzmann's originality is confirmed by his attitude towards Hilbert's axiomatic approach. He adopts it with enthusiasm but discards with the utmost lucidity the foundational role it was supposed to play. Boltzmann's opposition to foundationalism goes with his insistence that every theoretical construction (mathematical or physical) starts with the decision to adopt something as given. To conclude, I will show how such an adoption can be paralleled with our adoption of the language of communication through which, according to the 1897' philosophical article of the Populare Schriften, we gain access to the objective point of view – an article which designates Boltzmann as a true member of the Austrian philosophical tradition.

11. Creath, Richard (Arizona State University, USA)

The Apparent A Priori and Four Things to Do about It: A Prolegomenon to History

The self-conception of science nowadays is empirical. Observation is often said to be the only source of news, and what we say, insofar as it is justified, rests on that observation. However meritorious such an empiricism, it renders several disciplines and parts of others problematic. Logic, mathematics, set theory, geometry, philosophy, and even parts of theoretical physics and biology are each difficult to justify on the basis of observation. Our actual practices in these areas do not seem to proceed by express (much less by exclusive) appeal to observation. Moreover, we often claim a level of confidence in our results that we simultaneously deny that we even could get on an empirical basis. These are generalizations to be sure, and our practices are neither perfectly uniform nor fully transparent. Nonetheless, some of what we think we know and on which we routinely rely "seems" to be a prion rather than empirical.

This then is a problem with which any adequate epistemology or philosophy of science must deal. I do not say that it is a phenomenon that must be explained, though that phrase is often used in such a context. What is required for a philosophic account to deal adequately and appropriately with our scientific practices is very different from the explanation of an event or of a datum. So far, however, the philosophic task is characterized only in a negative way.

Historically, many different kinds of philosophic accounts have been given. Here I briefly discuss four: (1) an epistemic platonism associated with Descartes and in this century with Russell and Goedel, (2) Kantianism, (3) Carnap's account, and (4) Quine's. I raise considerations for being dissatisfied with the first two and for exploring the latter two further. The forms and variety of these four accounts and their historical development underscore the importance and recalcitrance of the problem of the apparent a priori. Moreover, the four accounts give shape and substance to the question of what a positive philosophic account of our epistemic practices would look like.

12. Czarnecki, Tadeusz (Cracow University of Technology, PL)

Ajdukiewicz on Language Change and Progress in Science

In 1934 Kazimierz Ajdukiewicz published in Erkenntnis two papers - 'Sprache und Sinn' and 'Das Weltbild und die Begriffsaparatur' - to propose a theory of language and progress in science. The central position in his project is reserved for the thesis of radical conventionalism: No scientific world-picture is

determined solely by experiential data. Every world-picture depends on a conceptual apparatus which is chosen to represent experiential data. In some circumstances the apparatus must be changed.

Ajdukiewicz distinguishes between axiomatic, deductive and empirical meaning-rules, defines a connected language and, finally, a closed language since he thinks that science reveals a clear tendency to closure. A connected language is closed if any attempt at its enrichment results in breaking of its connection or in revision of its expressions' meaning. The set of all meanings attached to the expressions of a closed language is the conceptual apparatus.

The change of language in science is explained by him as a result of two facts: (1) some sentences classified once as inductive generalizations become new axioms and (2) some new axioms collide with the old ones. If within a closed language one discovers a contradiction between axioms, then one has to impose on them new meanings in order to restore their consistency. Facing contradictory axioms science passes to a different language – untranslatable to the former one – and produces another world-picture. The question which of two world-pictures is true cannot be answered because the term 'truth', interpreted pragmatically, has a distinct meaning in every closed language. It is only possible to compare the perfection of conceptual apparatuses and decide which is better. Tendencies towards (1) consistency, (2) changing hypotheses into axioms and (3) increasing the number of empirical meaning-rules are taken to be the basic criteria of perfection. The degree of perfection is the measure of progress in science.

I focus on Ajdukiewicz's explanation of language change to show that it is burdened with difficulties when applied to an empirical language. They stem from the fact that he introduces pragmatic conception of axioms but maintains formal conception of deductive rules. Since Ajdukiewicz does not see the difference between epistemic and semantic deductive reasoning he also fails to define deductive meaning-rules in such a way that they could concern relational expressions. The pragmatic reformulation of deductive rules is necessary to reveal the actual scale of their impact on language change.

14. Davis, Todd N. (Duke University, USA)

Self-authentication and the Constitutive A Priori: Hacking's Styles of Reasoning in Historical Perspective

Recent neo-Kantian readings of logical positivism have emphasized the need to understand the role of the constitutive a priori as the radical heart of early logical positivism's reconception of the role of the analytic and a priori in scientific knowledge. The constitutive a priori is that which makes possible objective (here understood as intersubjective) discourse within some domain.

Hacking's styles of reasoning are historical a priori which are constitutive of a form of objective reasoning. They make new statements candidates for being true-or-false and are thus constitutive of their sense. As Hacking puts it "that a certain complex sentence is a candidate for the truth may depend upon there being a style of reasoning ... [t]here is not a prior truth, deeper, original, independent of reason." Discussing the kind of semantics appropriate for this, Hacking also makes explicit that something of this idea derives from (a reading of) early logical positivism, especially Schlick.

In this paper, I will look at the similarities and differences between the logical positivists' and Hacking's versions of the constitutive a priori as well as what Hacking means by saying that styles of reasoning are self-authenticating and whether or not, or in what sense, this holds for the various forms of the constitutive a priori in early logical positivism. Insofar as the logical positivists' idea was tied to the use of mathematics in science, it will turn out to be a version of one of Hacking's styles. I will also evaluate the reading of early logical positivism Hacking uses to partially motivate his idea. The central contention is that both represent broadly Kantian ideas about the normative role of the form of reason and a particular interpretation of the nature of objectivity. The hope is that reading the history of the concept of the constitutive a priori will help to understand Hacking's idea, both as part of his philosophy of science and as a tool for the history of science. This also gives, I hope, some idea of the larger conceptual space in which the early logical positivists' uses of the constitutive a priori developed.

15. Dickson, Michael (Indiana University, USA)

What Was the Copenhagen Reply to EPR? (And Why Does It Matter?)

Bohr's (1935) reply to Einstein, Podolsky, and Rosen (EPR) has been interpreted in numerous ways. Some of the difficulty arises from Bohr's apparently irrelevant and lengthy discussion of his reply to Einstein's earlier attempts to 'beat' the uncertainty principle by considering a two-slit experiment in which the slits are mounted on springs. Bohr claims that his reply to that challenge applies "equally well to the special problem treated by [EPR]".

Commentators have expressed doubt. Fine (1986), for example, has argued that "Bohr's response to EPR marks a definite break from his previously stated view"—Fine is especially concerned with Bohr's apparent introduction of a 'non-mechanical' influence of the measuring apparatus for one particle on the other particle. While he understands Bohr's reply somewhat differently, Folse (1989) too seems skeptical of Bohr's claim that his reply to EPR offers nothing essentially new, and he too sees in Bohr the assertion of a 'non-mechanical influence'.

In contrast, I find Bohr's reply to be essentially the same as his earlier reply, and I do not see in his reply the assertion of a non-mechanical influence of the sort Fine and Folse find. I argue that Bohr's point was that knowledge of the position (or momentum) of the source is required to infer the position (or momentum) of one particle from that of the other. Then a quick application of the uncertainty principle to the source itself—an application exactly analogous to Bohr's reply to earlier challenges from Einstein—yields Bohr's argument. In particular, Bohr's justification of uncertainty in terms of 'mechanical influences' is precisely as it was prior to EPR. When Bohr asserts that there is no mechanical influence of one particle on the other, he is pointing out that the mechanical influence involved is not between the particles (or their apparatuses) but between the source and whatever apparatus we use to measure its position (or momentum).

Bohm (1951) introduces a new version of the experiment, and therefore the EPR argument. The reply that he offers is the one usually attributed to Bohr, though Bohm does not mention Bohr in this context. Why? The main point is that Bohr's reply (as I outlined above) will not work in this case, because the inference from one particle's spin to another's is not 'mediated' by the source. Bohm's version of the EPR experiment is, therefore, novel, in a crucial respect, and apparently forces the Copenhagen interpretation (of which Bohm was, at that time, an adherent) to adopt a new reply. So it was in 1951, and not in 1935, that the Copenhagen interpretation made a "definite break" (in Fine's words) from its origins. And it was not the EPR experiment that occasioned this break, but the EPR-Bohm experiment. Time permitting, I shall suggest that Bohr's reply to EPR is considerably more plausible than Bohm's reply to EPR-Bohm. I will speculate on whether the EPR-Bohm experiment really does force the Copenhagen interpretation to this radical reply.

16. Dowe, Phil (University of Tasmania, AU)

Leibniz on Causation

The focus of this paper will be Leibniz' views on causation in the period of the Discourse Concerning Metaphysics. In particular, I will be concerned with what I will call the 'Standard Picture' on Leibniz on causation in that period. What I mean by the Standard Picture is a view which represents a broad consensus among contemporary commentators concerning Leibniz' position on causation in that period. I will also be concerned with some of the more speculative analysis of Sleigh. I will not be concerned to assess the standard view from an exegetical point of view.

The aim of the paper is to defend, against Sleigh (1990) and Clatterbaugh (1999), a prima facie outrageous claim, viz. that Leibniz, on the Standard Picture, held no theory of causation.

18. Duhn, Anita von (Université de Genève, CH)

Bolzano on Colours

Bolzano's views on colours contain claims about a certain type of idea (Vorstellung). These claims are

interesting for a number of reasons, although difficult. For Bolzano's ideas must be gleaned from his Wissenschaftslehre, Zur Physik I, the Athanasia, certain mathematical texts and his early article on The Kantian doctrine of the construction of concepts in intuition. Here I reconstruct Bolzano's position about colours from these disparate texts. Two of Bolzano's claims belong in the Austrian tradition: his claim that there is no extension without quality and his claim that colour Vorstellungen act as laws determining our perception of colours. One such law anticipates the descriptive psychologists' theses on colour constancy and here Bolzano's account is close to Hering's views on memory-colours. Like Meinong, Stumpf, Marty and Wittgenstein, Bolzano claims that there is no extension without quality yet, unlike them, he does not explicitly assert the inverse claim, that there is no quality without extension. Like the other members of the Austrian tradition, Bolzano (1810) also objects to Kant's claim that seeing is judging and that our colour perceptions are conceptual.

Bolzano discusses various scientific theories about colours as real properties of objects in his Physik and usually agrees with the view that colours are objective properties. Thus he sides with Osann, of the Würzburg school, who holds that complementary colours are objective. In his discussions of colours as real properties, Bolzano also mentions Herschel's wave-theory and his measurement of the absorption of light rays by coloured surfaces. In Zur Physik he refers to a primitive version of the colour circle, where colours are organized in a certain order, so as to classify them and to determine the continuity between them. He is interested in colour-mixtures and for this reason he examines the relations between colours in the circle. Unlike Newton, Bolzano does not say that colours correspond to light-rays. Perhaps he would agree with the contemporary objectivist view that colours are reflectance properties. The objectivists hold that colours are a dispositional property of surfaces to reflect light rays. There is no explicit mention of such an objectivist view in Bolzano, but there are indications that he considers colours as a relational property between light rays, the surface of objects and us. So, in some ways, he is close to Locke's subjectivist view.

Bolzano distinguishes between (1) colour-adherences or colours as real properties of objects, (2) subjective colours or colour as an intuition or sensation and (3) general colour-concepts acting as laws that regulate our perception of colours. How does Bolzano analyze subjective colour-intuitions? Bolzano explains the fact that we have colour-intuitions by assuming that they are caused by real properties of objects. In the Theory of Science and the Athanasia, he also puts forward a claim about colours as relational properties between us and real things. How does Bolzano analyze colour-concepts? In the main passage on colours in the Theory of Science he draws a distinction between sensory and non-sensory concepts and points out that colour-ideas are not conceptual, because they are accompanied by earlier intuitions aroused from memory, but the laws governing perception are conceptual. He treats these laws as higher-level concepts. Maybe he means that perceptions are regulated by higher-level concepts acting as laws.

19. Esser, Frederick (Berlin, D)

Locke's Resemblance Theory and Boyle's Corpuscularian Hypothesis

John Locke surely had Robert Boyle's corpuscularian hypothesis (1666) in mind when writing essential parts of his Essay Concerning Human Understanding (1690). But what does Locke's reception of Boyle's theory exactly consist in? According to Peter Alexander (1985), Locke is an "under-labourer" for Boyle's account in three ways: as a popularizer of the theory, as an advocat pointing to both its plausibility and its explanatory value with respect to everyday experience, and as a philosopher in the modern sense who explores the implications of the hypothesis for issues on knowledge. Thus, Alexander conceives Locke's epistemology as partly being a grand argument for corpuscularian theory and as partly building on the latter. Atherton (1992) has vigourously argued against this view. Locke's account of resemblance, she specifically insists, shows that he is not interested in establishing Boyle's theory, but in assessing purely epistemological topics in the light of his hypothesis. I agree with Atherton's general intention, but disagree with her argument. She rightly points to programmatic statements where Locke clearly distinguishes his own undertaking from natural philosophy à la Boyle. However, she is too indefinite as to what Locke's own issue consists in, and thus it remains questionable whether she can convincingly make her point. I therefore intend to spell out Locke's discussion of resemblance and make plain that it truly is an epistemological one. It will likewise become manifest that Locke inherits corpuscularian theory as the backdrop of his argument, but transforms the issue.

Perhaps not less important. I will thereby argue for an unorthodox interpretation of Locke's notion and issue of resemblance and, as a consequence, of the so-called reality of primary and secondary qualities. It seems to me that both topics are usually largely misconceived what is differently manifested in a variety of views on what secondary qualities are in this context. To read Locke most coherently, one has to comprehend his comments on resemblance and qualities in the light of Boyle's argument against obscure qualities. Alexander's view on resemblance will prove to be a good starting point regardless of the differences as to the general topic of Locke's explanations. The upshot is, as will finally be illustrated, that Locke's issue is the assessment of our everyday and scientific grasp of bodies from an ideally epistemic perspective which is similar in kind to Bernard Williams' absolute conception (1978) and Thomas Nagel's view from nowhere (1986). This shows that Locke determines corpuscularian claims - which he holds to be true - from a philosophical viewpoint being not specific to Boyle's theory, but applying to every account of bodies. In this sense, Locke's account is theory neutral and not confined to the corpuscularian hypothesis. In sum, it becomes evident that Locke transforms the scientific question of obscure qualities into the epistemological one of resemblance.

20. Futch, Michael J. (Emory University, Atlanta, USA)

Leibniz's Causal Theory of Time

Philosophers of science have long seen Leibniz as espousing a causal theory of time, one in which temporal relations are identified with causal relations. Despite a variety of textual evidence supporting this thesis, some Leibniz scholars have been reluctant to accept it. According to one important objection, raised by Jan Cover, Leibniz does indeed hold that temporal relations depend upon causal relations, but he limits this dependence to a relation of supervenience, as opposed to an identificatory reduction. Since for Leibniz temporal relations depend upon but do not uniquely determine any particular set of causal relations, the former only globally supervene upon the latter. Consequently, one cannot establish coextensive biconditionals between temporal and causal relations, a consequence that preempts an identificatory reduction of temporal to causal relations.

In this paper I argue against the global supervenience construal of Leibniz's philosophy of time and defend the view that he offers an identificatory reduction of temporal to causal relations. Section 1 examines Leibniz's different analyses of temporal relations, utilizing newly published material. Against a view commonly imputed to Leibniz, I argue (1) that temporal relations among monadic states are second-order relational facts explicable only in terms of the temporal ordering of the phenomena expressed by those substances, and (2) that temporal relations among phenomena are grounded in their causal order. Leibniz writes that if one "thing is the cause of another, and they are not able to exist at the same time, the cause is that which is prior, the effect posterior. Also prior is whatever is simultaneous with the cause" (VE 168). On the basis of this and similar statements, I argue that for any phenomenal event C, if C is the cause of and not spatially related to another phenomenal event E, C is temporally prior to E. Additionally, any event that, in conjunction with C, forms a complex causal condition of E is also temporally prior to E. These definitions, I argue, provide a comprehensive and non-circular analysis of the temporal relations of all phenomenal events. I conclude this section by explaining how, following Leibniz's statement to Des Bosses, monadic states are assigned a place "in" time by virtue of their relations to things contained in time.

With these definitions in hand, I turn in Section 2 to the thesis that temporal relations only globally supervene upon causal relations. A class of properties globally supervenes on another only if it is possible that two worlds indiscernible in the distribution of their supervenient properties be discernible in the distribution of their subvenient properties. Drawing from the conclusions of Part 1 and Leibniz's views on the nature of relations, I show why he would deny that any two worlds differing in their causal relations could be indiscernible with respect to their temporal relations. According to Leibniz, two worlds can be indiscernible with respect to the latter only at a level of abstraction at which the mind, not content with an agreement or similarity among different relata, seeks an identical relation that is outside all of the similarly described relata. However, when the particular instances of temporal relatedness are exhaustively and concretely described, so too are the kinds of causal relations obtaining among monadic states. Consequently, no possible worlds meet the criterion imposed by global supervenience. This means that one can posit coextensive biconditionals between temporal and causal relations, a conclusion that, when combined with Leibniz's analyses of qualitative temporal relations, strongly favors an identificatory reading of his causal theory of time.

21. Gattei, Stefano (University of Milan, I)

Lakatos, the Man who would be a Philosopher-King

Arriving from Cambridge upon invitation of Joseph Agassi, Imre Lakatos joined the Popper circle at LSE in the early Sixties. Caught in the magnetic field of Popper's intellect, he immediately began a rapid and brilliant career as a philosopher of mathematics. He took actively part in the Popper seminars and his interest in the philosophy of science grew as the years went by.

In 1961 he attended an International Symposium in the History of Science in Oxford, and there he heard Thomas Kuhn's paper, entitled 'The Function of Dogma in Scientific Research", which challenged the then dominating views of the history and philosophy of science: the standard view of logical-empiricists and, most notably, Popper's position. That paper was a kind of summary Kuhn prepared of his forthcoming book, The Structure of Scientific Revolutions, which appeared the following year and was itself a revolutionary work.

Lakatos was faced with new and powerful ideas, and planned to reply defending Popper's position. He also intended to improve it in the light of the criticism and construct a new methodology. That is why he organized the 1965 Bedford Colloquium for the Philosophy of Science, which gathered in London most of the then leading philosopher of science.

Through documents found in both Popper and Lakatos' personal archives, I reconstruct the long and winding road which took to the famous 1965 Bedford Colloquium. This reconstruction gets particularly interesting if viewed as part of the huge project which Lakatos had in mind in order to establish his leadership and growing influence both within the LSE Department of Philosophy, run by Popper, and in the larger philosophical world-community.

Formally organized to get Karl Popper and Rudolf Carnap, the two old antagonists, calmly debate their ideas, the conference saw two more (significant) events: the confrontation between Kuhn and Popper school, on the one hand, and a big quarrel between Popper and his then closest disciple and friend William W. Bartley, which led to a big rift, lasted twelve years and never fully recovered. Both were pieces of Lakatos' project.

Indeed, the published version of the conference sees one more item: Lakatos. In the fourth volume of the proceedings, which became a true best-seller in the philosophical community for quite some time. Indeed, Lakatos' contribution to the volume is the main one. There he develops his idea of a methodology of scientific research programs – a variant of the (Popperian) idea of the metaphysical research programs – which he elaborated the following year, and with which he meant to take over Popper's leadership.

Lakatos died in 1974, and these plans could not be fulfilled. His influence gradually extinguished, but its traces remain. In my paper I also examine the important consequence this had for the relationship between two main characters of twentieth century philosophy of science, Thomas Kuhn and Paul Feyerabend, who were at first asked to duel at the Bedford Colloquium. In the light of the recently published correspondence both between Lakatos and Feyerabend, and between Feyerabend and Kuhn, I reconstruct their intellectual exchange and show the relevance of Lakatos' shadow over it.

22. Gerner, Karin (Universität Osnabrück, D)

Hans Reichenbach's Period at the University of Istanbul

Hans Reichenbach's exile in Istanbul/Turkey (1933-1938) is a less known period in his biography. The period is very interesting, because these are the fateful years for the philosophical movement of the Logical Empiricism and because of the interesting life in the little new German colony at Istanbul, in which many German and Austrian scientists found a place to hide during the Hitler's regime. The German scientists built up the new University of Istanbul, founded by Ataturk and Albert Malche in 1933.

After Hitler won the last (free) election in Germany in 1933, a government was built, which had a very negative attitude towards art and sciences, so that a real exodus of the intellectualists started. To organize help to the persecuted scientists, a few of them founded 'die Notgemeinschaft der Deutschen Wissenschaft im Auslande' ("emergency organization of German Science outside Germany") in Zurich. The plan was to mediate scientists and medical scholars at any location outside Germany. In April 1933 Reichenbach visited the office of the "Notgemeinschaft" and asked for help. Reichenbach didn't feel safe

any more in Germany, because his last winter lectures at the University of Berlin were often interrupted by Nazi–students. That his fear was justified, was proven in may 1933, when he was betrayed by a "Dr. Georg", who wrote in a letter to the directorate of the University of Berlin, that there was still a "Marxist" and "Halbjude" at the university. About the same time the Turkish minister of education invited Philipp Schwarz (one of the founders of the "Notgemeinschaft"), to the ministry at Ankara. The minister was interested in appointing several German scientists as professors at the new university of Istanbul. It was a historical date – not only for the escaped scientists, but also for Turkey, which hereby showed its tendency toward new sciences and the western culture. Under the first professors, who came to Turkey, was Hans Reichenbach and his colleague from Berlin, the mathematician Richard von Mises. Reichenbach was awarded a full professorship for philosophy at the University of Istanbul. The year 1933 though was the crucial year for the Berlin Group of Logical Empincists, because more and more members were forced to leave Germany. Reichenbach remained a leading member of the "Gesellschaft für empirische Philosophie" and – more important for the community of the philosophical movement of Logical Empiricism – remained one of the editors of the magazine "Erkenntnis". Until 1936, the "Erkenntnis" was the most important publication for the movement, because all members, wherever they were, were able to discuss and publish their new thoughts. Felix Meiner, the publisher, was very courageous, because he resisted the – so-called – "Gleichschaltung" (censorship). Even after the authority for censorship in 1936 became aware of the "Erkenntnis", Meiner remained a strong advocate for the "Erkenntnis". Reichenbach's time at Istanbul ended in 1938, when he was appointed as professor for philosophy at the University of California at Los Angeles.

23. Ghisu, Sebastiano (Sassari University, I)

Antiempiricism and Foundationalism in the West German Epistemology after World War II

I will reconstruct in my paper the outlines of the West German epistemology after World War II (approximately between 1945 and 1953). In my reconstruction these outlines are:
– The criticism towards empiricism, (neo-)positivism and materialism (often used as synonym): Dingler, Max Hartmann, May, Wein, Wenzl, Bavink and so on.
–The spiritualistic or religious interpretations of contemporary physics (quantum mechanics and the uncertainty principle): Bavink, Jordan, Wenzl.
– The projects of a "scientification" of philosophy, alternative to those proposed by logical empiricists. I refer here to the attempts of Max Bense and H. Scholz to axiomatize the philosophy: a sort of new-leibnizianism.
On the base of this reconstruction I will point out:
– The absence of the logical empiricism (with an important exception: Kurt Reidemeister), but its presence in the criticism against it. I will argue that most of the West German epistemology after World War II was formed by marking it out from the theories of logical empincism, often identified with conventionalism, pragmatism, classical positivism or even sensualism.
– The common ground of almost all the considered epistemological theories: (a) the emphasis on the autonomy of philosophy in relation to science and (b) the ontological-foudational attitude. I think that this double element – in contrast with the basic attitude of the Vienna Circle philosophy and in continuity with the traditional German philosophical culture, strengthened (directly or indirectly) by the national-socialist dictatorship – would make difficult the introduction (or reitroduction) of the analytical philosophy in Germany.
– Finally, I formulate the question – without giving an answer – whether and how this dominant foundationalist attitude influenced the later reception in this country of the critical rationalism and the analytical philosophy itself.

24. Glas, Eduard (Delft University of Technology, NL)

The Popperian Roots of Lakatos's Philosophy of Mathematics

In the same year (1963) that Popper's Conjectures and Refutations came from the presses, Lakatos started the publication of a series of articles under the title Proofs and Refutations, in which themes from

his 1961-thesis Essays in the Logic of Mathematical Discovery reappeared and were further developed. The choice of titles alone – besides much else – already makes it unambiguously clear that Lakatos intended his studies in the philosophy of mathematics to be an 'application' or 'test case' of Popper's philosophy of science. Yet Popper had never intended his methodology to apply to mathematics, and although he was delighted with Lakatos's work in the history and philosophy of mathematics, he kept seeing fundamental differences between the methods of science and of mathematics, where Lakatos saw fun-damental similarity.

Various knowledgeable authors (Worrall, Zheng, Larvor) have claimed that Lakatos was mistaken in thinking of himself as elaborating Popper's methodology. The central argument of Proofs and Refutations would, by its 'Hegelian dialectic' structure, even be in outright contra-diction with Popperian orthodoxy. It is also claimed that this 'un-Popperian' element in Lakatos's thought brought him much closer to a Kuhnian-like 'historical' conception of science than would have been possible for a true Popperian.

It is my intention to show that Proofs and Refutations was, in fact, fully in the spirit of Popper's philosophy, not only on the basis of the obvious imitation of Popper's Conjectures and Refutations in its title (and of Popper's Logic of Scientific Discovery in the title of the thesis), but in a far more fundamental sense.

Lakatos not just 'applied' Popper's method in a domain which Popper himself had not envisaged, but brought a much larger part of the Popperian corpus to bear on mathematics, especially a group of ideas clustering around the doctrine of the relative autonomy of scientific (in particular mathematical) knowledge 'in the objective sense'. Apart from this general 'objectivist' framework, there are several characteristic ideas in Proofs and Refutations for which a direct source can be found in earlier works of Popper's, for instance the centrality of problems and their 'dynamics' rather than 'static' definitions in mathematics, the characterization of 'conventionalist stratagems' and the methodological decision not to use them to save a proposal from refutation, the use of models to make hidden lemmas explicit and the associated distinction between global and local counter examples to theorems.

As a test case for Popper's methodology, on the other hand, Lakatos's investigations led to the conclusion that some of the Popperian maxims that he had adop-ted were in need of revision. This necessary further refinement of Popperian ideas (not merely respon-ding to Kuhnian strictures) formed the basis of his Methodology of Scientific Re-search Programs, essential ingredients of which thus originated in his philosophy of mathematics.

25. Godard, Roger (Royal Military College, Ontario, CDN)

L'Approche Diagrammatique en Théorie des Probabilités

Dans son livre *The Theory of Probability, an Inquiry into the Logical and Mathematical Foundations of the Calculus of Probability*, publié en allemand en 1934, H. Reichenbach (p.121-122) donne une brève remarque historique concernant la construction axiomatique du calcul des probabilités. Il distingue deux tendances ou deux approches dans la présentation de la théorie des probabilités. L'approche interprétative, en partant des fréquences, est liée à la logique inductive. Ce groupe commence avec les études de von Mises (1919), Karl D rge (1930), Erhard Tornier (1930), Erich Kamke (1932), et A. Copeland (1928). A partir des fréquences empiriques, on peut aboutir à des règles fondamentales. En suivant les notes de Reichenbach, on trouve une autre conception des probabilités, liée à la théorie axiomatique formelle. A partir de tautologies, on peut prouver une série de théorèmes et développer une théorie cohérente et rigoureuse des probabilités. Ce groupe comprend notamment le travaux de Bohlmann (1901), la théorie axiomatique de Bernstein (1917), de Borel (1925), Reichenbach (1932), Kolmogorov (1933).

Reichenbach distingue un troisième aspect de la théorie des probabilités, lié historiquement à la logique, et qui est présenté maintenant dans les livres de cours avec des rappels sur la théorie des ensembles. Cette approche commence au siècle avec Leibnitz. En 1761, dans les Lettres à une Princesse d'Allemagne, le mathématicien L. Euler a représenté les quatre relations du carré logique par des diagrammes géométriques. A cette école, il faut relier les travaux de Bolzano (137). La logique continua de se développer au milieu du siècle, notamment en Angleterre et aux Etats-Unis avec de Morgan, George Boole qui essaya d'introduire de méthodes algébriques dans l'étude des syllogismes, et John Venn dans Symbolic Logic (1881) qui introduisit une méthode diagrammatique, commode, interprétée à la manière de Boole. C'est la méthode diagrammatique de Venn qui est maintenant la plus utilisée dans les livres élémentaires sur les probabilités. Les travaux de Pierce (1878) et de J. Keynes

(1921) appartiennent à cette troisième ligne de développement. On se rend vite compte qu'entre 1881 et 1930, les méthodes graphiques pour l'étude de la logique étaient beaucoup plus avancées que pour les sciences de probabilités. La valeur de l'idéographie inventée par Frege réside dans le choix des idées fondamentales à figurer. On continue à utiliser son esprit dans l'enseignement des langages d'informatique. Frege a formulé ses projets dans un mémoire de 1882 en ces mots : Mon intention ne consiste pas à représenter une logique abstraite par de formules, mais bien d'exprimer un contenu au moyen de symboles écrits de façon plus précise et plus claire qu'il serait possible de la faire en des mots. Reichenbach (1934) a été un des premiers à utiliser des diagrammes de partition en théorie des probabilités. Il essaya par cette méthode d'illustrer le théorème des probabilités totales. L'utilisation de diagrammes de Venn ne vint que beaucoup plus tard. Et c'est seulement vers 1950 que Shannon illustra les problèmes de bruit dans les communications par des diagrammes.

26. Hamberger, Klaus (WU Wien, A)

"Archeology of Knowledge" and "Morphology of Mind". On certain similarities and some possible connections between the epistemological projects of Michel Foucault and Ernst Cassirer

Almost immediately after the publication of *The Order of Things* (1964), Foucault delivered a review essay of Cassirer's *Philosophy of the Enlightenment*, which seems to have remained his only public statement on Cassirer. Nevertheless, it reveals some remarkable aspects of the significance of Cassirer's work for Foucault's own research program.

In particular, Foucault praises Cassirer for having made possible a new history of thought by virtue of a special method of analysis, which had allowed him to unfold the autonomous space of discourse and thought (discours-pensée) as a genuine field of positive research. This positive research had been crucially prepared (and reflected) by Cassirer's main philosophical idea, namely, the concept of a plurality of distinct "modes of thought" or "symbolic forms", connected by mutual transformations, which account for both the development and the organization of the human mind.

By assimilating this idea to his own "episteme" concept, Foucault not only quotes an important "predecessor"; moreover, and more important, he thus points at the widely neglected question of the philosophical underpinning of his own method of analysis.

Throughout his work, Foucault has remained extremely tacit on the epistemological basis of historical epistemology itself, and this almost systematic disregard for the theoretical core of his own science is particularly marked in respect of the three crucial aspects which characterize the "episteme" concept:

– first, the conception of knowledge as a genuine object of empirical analysis, which can be studied in its own right without being reduced to the laws of thought or the complex of circumstances
– second, the application of the (typically "structuralist") transformation method to the comparative analysis of sciences and systems of thought
– finally, the triadic scheme of historical development, which furnishes the three "epistemes" of the Renaissance, the classical age, and the modern period.

Now each of these aspects has been clearly developed in Cassirer's philosophy. In particular, his model of development may also serve to clarify the rule of progress implicit in Foucault's sequence of epistemes. The model already underlies the structure of "Erkenntnisproblem", is made explicit in "Substanzbegriff und Funktionsbegriff", and finally reappears in the "Philosophy of Symbolic Forms", where it rationalizes the triad of symbolic functions (expression, representation and signification) as well as the relationship between the various symbolic forms, thus imposing a hierarchical and implicitly temporal order on their system.

As can be shown, the projection of this sequence of symbolic forms to Foucault's sequence of epistemes yields a strikingly consistent result. Moreover, Cassirer has stressed the essential discontinuity of the transformation process, thus prefigurating the concept of an "epistemological break". Although the historical evidence for any direct influence of Cassirer's work on Foucault still remains scarce, the close affinity between both oeuvres yet gives rise to some philosophical considerations, which may both allow to elucidate the historical dimensions of a "morphology of mind", and to gain some additional insights into the potential scope of an advanced "archeology of knowledge".

27. Hardcastle, Gary (University of Wisconsin, USA)

The Modern History of Scientific Explanation

According to Wesley Salmon in a recent discussion of explanation (1999), at the beginning of the 20th century, "by and large, [scientifically oriented philosophers and philosophically inclined scientists] held that there was no such thing as scientific explanation – explanation lies beyond the scope of science, in such realms as metaphysics and theology" (338). By the end of the twentieth century, though, scientific explanation was recognized by scientists and philosophers as a legitimate aim of science, and, moreover, one which was in fact frequently achieved. "The temptation to say there is no such thing as scientific explanation," Salmon reports, "vanished" (342). This "remarkable reversal of attitude," in which there emerged an awareness of the possibility of scientific explanation of natural phenomena, is, Salmon notes, "one of the most significant pieces of philosophical progress in the twentieth century" (343; see also Salmon (1989), pp. 4-5 and Salmon (1998), p. 80; and, for similar claims, Carnap (1974), pp. 12-16, and Bunge (1963), pp. 282-286).

In the present paper I argue that a different story about the history of explanation in the twentieth century is more accurate. One thread implicit in the historical story suggested by Salmon is that there were, in the first part of the twentieth century, few if any cases in which natural phenomena received scientific explanations and were widely recognized as such by scientists and philosophers. In response I describe several cases which were seen in precisely these terms. A second thread of Salmon's historical story is that various exhortations (from, for example, Karl Pearson, Ernst Mach, and Gustav Kirchoff) express the view that scientific explanations of any sort were not forthcoming for natural phenomena. I argue that careful attention to these claims shows instead that they typically express opposition to competing scientific programs, and indeed that these exhortations make sense only in the context of the possibility of a particular kind of scientific explanation.

Having cast the "early modern" history of explanation along these lines, I briefly consider a pair of related questions about the most important event in explanation's modern history, the 1948 Hempel-Oppenheim paper, "Studies in the Logic of Explanation."Salmon argues that this paper's significance and success resulted from its articulation of a powerful paradigm of explanation in a context of scientific realism, but on the view I defend neither of these factors is relevant; there were (as I show) earlier paradigmatic accounts of explanation "on the table" and philosophical interest in explanation has been independent of the commitments to scientific realism. Further, my account suggests a motivation for the Hempel-Oppenheim paper distinct from the one proposed by Rescher (1997).

28. Hessbrüggen-Walter, Stefan (Universität Münster, D)

How to Invent a Science: The History of Transcendental Psychology

In 1966, Peter F. Strawson published "The Bounds of Sense", the probably most influential commentary on Kant's "Critique of Pure Reason" in this century. The title of the book was programmatic: It related not only to Kant's philosophical enterprise, namely to find out about the limits of meaningful metaphysical discourse. It also hinted at Strawson's conviction that there are "two faces of the critique": descriptive metaphysics on the one hand, the "imaginary subject" of "transcendental psychology" on the other hand. In 1990, Patricia Kitcher published a book, bearing the provoking title "Transcendental Psychology", reading Kant's philosophy of the mental as a kind of proto-functionalism. But although both, Kitcher and Strawson, may not have much in common, they seem to share the conviction that Kant's intentions can be best reflected upon by projecting philosophical concerns of our day into the past. In my opinion, this is a questionable claim. I want to argue for three related theses: 1) "Transcendental psychology" as a descriptive science of mental goings-on really is an imaginary subject. If it does exist at all, it is a normative science (in this respect I agree with Hatfield 1990, p. 86f). 2) "Transcendental psychology" as a normative science should not be called psychology, but logic: In 18th century, logic as the science of proper thinking is concerned with mental processes as well. In this regard, Hatfield's historical account must be corrected. 3) Kant took the apriority of general logic more or less for granted. But if general logic is an a priori science at all, this can be true only of the rules of thinking. The existence of those entities that are quantified about in these rules is not a priori guaranteed. Although Kant seems to have believed the opposite, the 'tres operationes mentis', namely concepts, judgments and syllogisms, cannot be

deduced a priori. But since the theoretical architecture of the Critique of Pure Reason is built on this trichotomy, the apriority of the critical enterprise thus becomes questionable. So, in the end, Strawson is right: Faculty psychology as the background science of transcendental philosophy must fail. But he is right for the wrong reasons: Faculty psychology fails because Kant put too much trust into the logic of his day - a lesson present day analytical philosophy still may have to learn.

Literature: Gary Hatfield, *The Natural and the Normative*, Cambridge/London 1990;
Immanuel Kant, *Kritik der reinen Vernunft*, Stuttgart 1966;
Peter F. Strawson, *The Bounds of Sense*, London 1966;
Patricia Kitcher, *Transcendental Psychology*, New York/Oxford 1990.

29. Hintikka, Jaakko (Boston University, USA)

Post-Tarskian Truth

A radical change is taking place in our ideas about truth and truth definitions. Once the intrinsic limitations of the Frege-Russell first-order logic are removed, a truth predicate turns out to be definable in any first-order language that can formulate its own syntax. Such truth predicates employ essentially Tarski's T-schema and are therefore closely related to minimalist approaches to truth. The lecture is calculated to be an Auseinandersetzung between the traditional Tarski-type approaches, minimalist approaches and the new game-theoretical one.

Tarski's theorem asserting the undefinability of truth in the same first-order language holds only because the definition requires patterns of quantifiers not expressible in the traditional Frege-Russell logic. Once this flaw is corrected as in the author's IF logic, a truth predicate is available. It is closely related to the T-schema and therefore to minimalist approaches to truth. It is thus a folly to deny the definability of truth for any realistic language in the language itself.

30. Hofer, Veronika (Wien, A)

Philosophy of Biology around the Vienna Circle: Bertalanffy and the Cambridge Theoretical Club

It is widely held that Logical Empincists hardly ever discussed questions on the philosophy of biology in an independent and satisfactory manner. Indeed, in their major works there are only very few passages which mention contemporary questions of biology. The only notable exception is Philipp Frank's book The Law of Causality and its Limits. There Frank not only comes back to his old criticism of Hans Driesch, but he also devotes a section to Ludwig von Bertalanffy's "attempts to give a positivist formulation of vitalism".

Since his Ph.D. under Robert Reininger and Montz Schlick, Bertalanffy entertained various links to members of the Vienna Circle, such that Carnap even suggested him as an author for the Encyclopedia project. But although Bertalanffy adopted various methodological ideas of Logical Empiricists, his main intention was to develop a program sui generis. The purpose of this paper is to study in a historical perspective whether System Theory represented an example of an integrative theory of biology that could challenge the Vienna Circle's 'scientific world conception'.

One important element was the trend in conceptualizations of Theoretical Biology by antireductionistic vitalists around the turn of the century. Two sociological factors are also taken into account: Bertalanffy's philosophical training under Schlick and Carnap, and the influence of the Vienna Vivarium biologists – a group closely connected to the theoretical Club in Cambridge – who inspired the particular biological problems he dealt with.

Bertalanffy combined the antireductionistic claims with a new look at the possibilities that had arisen from modern physics. In construing a non-metaphysical Theoretical Biology, he initially employed metaphors such as 'organizer', 'field' or 'gestalt' that had already been used by Paul Weiss, Needham, Woodger, and others who were close to him. In his early works he introduced the Machian interpretation of causal relations as functional dependencies into biological problems, thus giving the discussions on the systematic place of teleology in biology a new twist. In the late 1930s he developed his concept of steady state in order to attribute a well-defined systematic status to the insight that life phenomena are highly interactive, highly dynamical in the reorganization of organismic functions, and that the epigenetic

process of self-organization is mainly responsible for the production of patterns and processes in living organisms.

In 1939, Woodger – who was closest to Bertalanffy – contributed a booklet on The Technique of Theory Construction to Neurath's Encyclopedia of Unified Science. But the reaction within the Vienna Circle was a mixed one. To Neurath it showed that by merely logical means only minor progress could be made as long as there was not sufficient empirical evidence available to find reasonable definitions of basic concepts. Woodger's axiomatization was generally considered as premature and – at least to Neurath's mind - hardly comprehensible. Instead of heading for symbolization, one attempted a more precise formulation of the problems. Both Neurath and Carnap expected substantial progress from a physicalist language, but they were at pains to emphasize that this was not tantamount to a plea for ontological physicalist reductionism.

31. Houkes, Wybo N. (University of Leiden, NL)

Carnap on Logic and Experience. The Relation between Formal System and Experience from Der Raum to the Aufbau

In this lecture, I will argue that Carnap's changing views on the relation between formal system and experience in the 1920's must be understood from his attempt to reconcile his Neokantian idea of accounting for the objectivity of knowledge with a Russellian conception of logic.

Traditionally, Carnap's work during the 1920's is interpreted as a gradual transition from Neokantianism (Der Raum) to foundationalist empiricism (Aufbau). Recent years, however, have seen a re-appraisal of the Aufbau: it has been plausibly argued that, in this work, Carnap sought to reconstruct objective knowledge by showing its logical structure. In this lecture, I address three closely related problems raised by this re-appraisal.

First, it appears that we ought to revise our view of the transition Carnap underwent during the 1920's. Der Raum and the Aufbau share a commitment to Neokantianism. However, central elements of Carnap's thought changed, e.g., he abandoned intuitive space and Wesenschau as defended in Der Raum. How should we reconcile continuity and change?

This tension is increased by the second problem. The idea that experience has, or is related to, a logical structure raises a question on the relation between a formal system and experience. This issue was one of Carnap's chief preoccupations during the 1920's. In Der Raum, we find two relations between three layers: the physical space of experience is subsumed under intuitive space, which is a substitution for formal space. In the early papers, in which intuitive space has been dispensed with, the relation between formal system and experience is, surprisingly, one of subsumption rather than substitution. Finally, in the Aufbau, this subsumption relation between the physical world of experience and constitution system (§136) is supplemented with various necessary or conventional 'forms' of the system. Yet, ultimately, Carnap urges that knowledge ought to be completely logicized in order to save its objectivity (§§153-155). This does not only appear to annul the subsumption-substitution issue, but it also seems an absurd ideal (Friedman 1988, 1992).

Both the first and the second problem may be solved once we consider a third. Carnap's idea that uncovering the logical structure of experience serves an epistemological goal, clearly expressed in the Aufbau, raises a question regarding his conception of logic. It is commonly argued that, in the Aufbau, Carnap cast Principia Mathematica formal logic in a transcendental role (Richardson 1998). This interpretation claims to make sense of one of the most mysterious features of the Aufbau: Carnap's quest for a complete logicization of experience in §§153-155. I will argue that this quest involves a Russellian, universalist rather than a Neokantian idea of logic. Backtracking, I argue that with this Russelian conception, Carnap could fill the gap left in his philosophy by the elimination of intuitive space. In this way, the Aufbau supplements the earlier papers, which reflect a transitional phase in Carnap's thought.

32. Hudson, Robert G. (Virginia Tech University, USA)

Discoveries, When and By Whom?

Thomas Kuhn (in The Structure of Scientific Revolutions) and Alan Musgrave (in "Why Did Oxygen Supplant Phlogiston") argue that it is impossible to precisely date discovery events and precisely identify

discoverers. They defend this claim mainly on the grounds that so-called discoverers have in many cases misconceived the objects of discovery. In support of this claim, they examine specifically the case of the discovery of oxygen; two of the main contenders for the title of 'discoverer of oxygen', Priestley and Lavoisier, misconceived (they argue) the nature of oxygen in fundamental ways (Priestley claimed that he had discovered 'dephlogisticated air'; Lavoisier thought he had discovered the 'atomic principle of acidity'). As a result, it becomes difficult, they think, to view either Priestley or Lavoisier as the 'discoverer of oxygen' in any definite way. As a result, they leave it quite open and imprecise who to count as the discoverer of oxygen and when to date this discovery.

In this paper, I argue that Kuhn and Musgrave arrive at their conclusion because they lack a substantive account of how well discoverers must be able to conceptualize discovered objects. I remedy this deficiency by providing just such an account. On my view, a discovery of an object (such as a kind) can be said to occur by someone if this individual has 1) a 'base description' for the object, 2) can 'demonstrate materially' this base description, 3) has found something novel, and 4) has discovered something that is real. In the paper, I spend a substantial amount of time explaining the terms 'base description' (roughly, an identification condition) and 'material demonstration' (roughly, an empirical/experimental justification), and illustrate my theory of discovery using a simple, everyday example. I then turn to the discovery of oxygen and, using my criteria, show how the problem Kuhn and Musgrave have raised – that 'discoverers' tend to misconceive discovered objects – is not a problem after all, so long as discoverers conceive of the discovered object well enough to formulate a 'base description' (and, of course, are able to satisfy the other conditions on discovery).

33. Imai, Michio (Sapporo Medical University, JP)

Mach and Freud – A Probable Confrontation

There was no personal contact between Ernst Mach and Sigmund Freud. There is little wonder that Mach scarcely referred to the 18-years-younger Freud. But why did Freud only seldom mention Mach, despite the fact that the latter was well-known in fin-de-siecle Vienna? Thomas S. Szasz thinks that there may be a remarkable connection between them(cf. His Introduction to the Dover edition of "The Analysis of Sensations", 1959). A thinker like Josef Breuer, Szasz says, could have been a mediator between them. But are there any materials which give direct suggestions to their connection?

A most important clue for Szasz is a letter of Freud to Wilhelm Fliess (12. Jun. 1900), in which Freud mentions Mach's "The Analysis of Sensations" (2nd. ed.) and says that Mach, referring to the dream, has failed to elucidate its secret. Interpreting the letter, Szasz insists on two points. (1) Freud tried to show his originality by comparing his "The Interpretation of Dreams" with Mach's "The Analysis" which, however, does not discuss dreams as a theme and has no special relation to his voluminous book. (2) On the other hand, nevertheless, Freud was under the influence of the general methodological view expressed in Mach's psychological writings. Among others, Freud accepted Mach's view concerning the relation between physics (or medicine) and psychology.

Like Szasz, I think it also worthwhile to speculate about their connection from the standpoint of intellectual history, though I must then correct the errors of Szasz' explanations. As to (1), I agree that " The Analysis" does not, it is true, discuss dreams as a main theme, but, nevertheless, I notice that dreams are discussed at least twice. And Freud, deliberating these passages, referred critically to "The Analysis" in comparison with his "The Interpretation". It is true that Freud wanted to show his originality, but the remark was not simply of capricious vanity, but derived from his real understanding of Mach's interpretation of dreams.

As to (2), it is improbable that Freud accepted Mach's methodology. We should rather pay attention to the difference of view between them. Freud could not help regarding Mach's treatment of psychological matters as non-psychological, as he says in his letter to Josef Popper-Lynkeus (4. Aug. 1916). Such a "non-psychological" attitude comes, to be true, from his positivistic, phenomenalistic viewpoint, but it might at the same time have its origin in the mystical tradition of thinking(cf. my paper in "Papers of the 22nd International Wittgenstein Symposium", 1999). The analysis in my paper may be questioned, but, even so, if we examine the phase of "I" in Mach's thought, we can identify its so-called non-psychological character. Freud's conception of psychology (psychoanalysis) could not have come into existence on the basis of the "I" in Mach's sense.

34. Irzik, Gurol (Bosphorus University, TR)

Changing Conceptions of Rationality of Science: Popper, Carnap, and Kuhn

One of the most important changes that occurred with the transition from logical empincism to early post-positivism concerns the rationality of science. The standard interpretation of this change is that Kuhn simply denied the rationality of science which was dear to the earlier logical empiricists and Popper. Curiously, however, such a view is held without serious work done on the logical empiricists' conception of the rationality of science. My aim in this paper is to compare Popper's, Carnap's and Kuhn's views on this issue. I shall argue that (a) Carnap's views on rationality are much closer to Kuhn's than Popper's, and (b) Kuhn should be understood not as denying the rationality of science but redefining it.

35. Janik, Allan (University of Innsbruck, A)

Heinrich Hertz's Influence on Wittgenstein's Concept of Philosophy

Wittgenstein was not only a lifelong reader of Hertz, who gave Hertz's Introduction to the Principles to his students as the paradigm for doing good philosophy, but also actually contemplated taking his motto for the Philosophical Investigations from its pages "when these painful contradictions are removed, the question about the essence [of force] is not answered, but the mind is no longer tormented and ceases to pose illegitimate questions". (PM, 9) It was precisely his mature method for dealing with philosophical problems, i.e., his techniques for "discovering or inventing intermediate cases" (PI, I, 122) for drawing our attention away from the "one-sided diet" of examples (PI, I, 593) in terms of which traditional philosophers posed their problems, that Wittgenstein took to be his major contribution to philosophy. Bringing out the links between Hertz's technique of presenting alternative representations of mechanics to clarify its conceptual problems and Wittgenstein's mature method for dissolving philosophical problems will thus be a way of dismissing the charges of irrationalism and obscurantism that have been leveled against him, and replacing them with an account of the scientific origins of mature view of the nature of philosophy.

Further, looking at Hertz in this context will help us to see the continuity within Wittgenstein's philosophical odyssey. At the same time, it will contribute to the rehabilitation an important neglected philosopher of science. In any case, the crucial point about the concept of philosophy that the physicist, Hertz, developed for handling metaphysical problems in science and bequeathed to Wittgenstein is the insistence on the immanent character of the philosophical enterprise: if philosophical problems arise in physics, then they must be handled in physics itself rather than in some theory about physics. Physics must take care of itself as it were.

36. Jauernig, Anja (Princeton University, USA)

Kantian Philosophy of Space and Time and Modern Science (up to the 1920s)

It seems uncontroversial that the scientific developments in the 19th and the first half of the 20th century, notably the discoveries (inventions) of non-Euclidean geometry and of the Special and General Theory of Relativity, have called into question some of the core doctrines of Kant's theoretical philosophy. It seems less straightforward to locate precisely where the Kantian theory goes wrong (if it does) and to specify in detail which elements of the doctrine conflict with which of the scientific results. Such a specification is the aim of the first part of this paper. In the second part of the paper I explore ways in which the original Kantian doctrine could be modified in the light of the new scientific findings while at the same time preserving essential aspects of transcendental idealistic philosophy.

The approach of the paper is historical. The first part consists in an exposition of Kant's theory of space and time and a discussion of objections raised by Hermann von Helmholtz, Henri Poincare and Moritz Schlick who utilize the mentioned advancements in science in their critique of the Kantian doctrine.

The diagnosis at the end of the first part is that we have to reject the view that our representations of space and time are intuitive in nature rather than conceptual. Additionally, the theorems and axioms of geometry can be classified as analytic rather than synthetic judgments. Moreover, the structure of Kant's formal intuition as well as the structure of Kant's empirical space-time are incompatible with the space-time structures of Special and General Relativity Theory. On the other hand, the Kantian forms of intuition are sufficiently abstract and structure-less so that no conflict with the relativistic space-time structures arises.

The discussion of possible modifications of the Kantian doctrine in the second part of the paper focuses on the proposals made by Ernst Cassirer and the early Hans Reichenbach. One of the aspects of the Kantian theory that is retained in both proposals is the claim that space and time are transcendentally ideal and part of the constitutive scheme we employ in our attempts to construct a coherent, unified picture of the phenomena. It has even been argued that relativity theory supports this Kantian tenet and Einstein's famous dictum that the requirement of general covariance "takes away from space and time the last remnant of physical objectivity" has been taken as an acknowledgment of the ideality thesis. The paper concludes with a brief discussion of the ideality claim and a comment on the future prospects for a philosophy of space and time in a Kantian spirit.

37. Kaluszynska, Elzbieta (Polish Academy of Science, PL)

Can Constructivists call Fleck "Daddy"?

There is a striking convergence between Fleck's view and constructivists' one. It suffices to replace thought collective and thought style with equally imprecise used notions of culture or language in order to get the constructivists' counterpart of Fleck's tenets. Determination of cognition by thought collective (culture) and impossibility of communication between different thought collectives (cultures) are stressed in both cases equally strongly (and equally inconsistently). It is suggested in the paper that affinity between Fleck and constructivists is superficial. They seem to say the same things, but their opinions (views) are radically different. They agree at one point: there is a social conditioning of cognition. Everything else is different: constructivists are relativists, antirealists, subjectivists, post-empiricists, pragmatists and conventionalists; Fleck is not. He is not even a skeptic. He notes: "these efforts would be fanny, if it were not dangerous [...] a generation of future researcher grows with belief that there is already no truth in a good all sense." Thus although an answer to the question "can constructivists call Fleck daddy" should be "yes", to the opposite question – "would Fleck call constructivists children?" – the answer is 'no'. The reasons for this situation will be examined.

38. Kamlah, Andreas (Universität Osnabrück, D)

Hans Reichenbach's "Volitionism" and its Historical Roots

The reader of Reichenbach's writings is puzzled by his strong emphasis laid on free choice in nearly all chapters of his philosophy. In the philosophy of geometry he claims that the metric of space is completely free. We might even use a rubber band as the standard of length. His theory of induction is based on wagers. In his epistemology he talks about "volitional decisions" between different language systems. Since all decisions are volitional - that is analytically true - it sounds rather odd to specify a decision in that way.

These three characteristics of his philosophy are logically independent of each other. They represent, however, a common tendency which could be called "volitionism" (I want to avoid the already repeatedly occupied term "voluntarism"). This becomes especially clear, when we are told that Reichenbach - in distinction from the Vienna circle - throughout his life held that the human will is free, and that he saw the cause for this freedom in the structure of physical laws. In this point he disagreed with Schlick and other members of the Vienna circle, who considered the question if the will is free as metaphysical, either as meaningless or as completely trivial, and thus philosophically uninteresting.

Reichenbach during his live never published a detailed defence of his standpoint concerning the will. He would have been left alone with his conviction against most of his empiricist friends. This became clear at the "preconference of Prague" 1934, when Schlick, Neurath, Frank, and Zilsel argued against Pascual Jordan who claimed that quantum mechanics might make free will possible. Only Reichenbach

admonished his friends to do more justice to Jordan. Nevertheless the issue remained important for Reichenbach. When he died, he left a long manuscript "The Freedom of the Will", which his wife, Maria Reichenbach, published posthumously.

Freedom of choice was also important for Reichenbach in ethics and education. In these fields, too, the printed sources are not at all abounding. The picture he had of men, though, is very important for his philosophy. It is so to speak the ideological basis of his philosophy of science and epistemology, which was laid very early in his life during his student days, when he was influenced by the Jugendbewegung ("youth movement") and by the famous German educationist Gustav Wyneken. If we want to comprehend Reichenbach's philosophy under a common viewpoint, we have to deal with those influences. To shed some light on Reichenbach's thought under this perspective will be the main subject of my talk.

39. Kawalec, Pawel (Catholic University of Lublin, PL)

Carnap's Epistemology - Early and Late

Carnap's *Der logische Aufbau der Welt* has gain wide attention, especially with respect to the epistemological position advanced therein (e.g. Richardson 1998). Despite Carnap's declarations to the contrary, he maintained lots of his early epistemological commitments in his later works. Of them, *Logical Foundations of Probability* figures as the classic in the theory of confirmation, and most comprehensive exposition of inductive probabilities. I studied Carnap's epistemology in *LFP* (as well as in his other works on probability up to his "subjective"-shift in 1962) in greater detail, and identified it as a structuralist version of reliabilism.

Thus, on the one hand, my paper attempts at the comparison of the epistemological role of "structure" in *Aufbau* and *LFP*. The role has been recognized as epistemologically significant in both Carnap's works, and my aim here is to get a good grasp of the (dis)similarities between the epistemological role of the logical structure.

There are two other important respects in which the books must be compared. One of them is identified by Richardson in *Aufbau* as the distinction between the absolute vs. relative a priori. Apparently, Carnap is ambivalent in his early writings on probability between there being only one definite confirmation function (*LFP*), and a continuum thereof (*The Continuum of Inductive Methods*, 1952) – the ambivalence, which seems due to – via the methodological problem of application – the absolute – relative a priori distinction.

Finally, the above mentioned ambivalence in Carnap's work might be perceived as due to his unstable view with respect to the conception of unified science, and the role of physics therein. Some passages in LFP suggest that Carnap conceived of physics as the structurally complete science determining the status of other sciences. On the other hand, however, he attempts to avoid the objection of arbitrariness of a priori probability distributions by letting the probabilities be domain-of-application-relative (and therefore allowing for multiple confirmation functions). I will study then how closely this Carnap's problem in LFP and CIM matches the central epistemological tension he faced in AUFBAU identified by Richardson as the ambivalence between logical-mathematical structure and physical-mathematical one.

After having study the affinities between epistemological traits in *Aufbau* and *LFP*, I will attempt to evaluate in detail which parts of the structural reliabilist position I identified in *LFP* find also their application in the study of *Aufbau*.

40. Kilinc, Berna (Bogazici University, TR)

Philosophy of Science in Two Moods: Bachelard and Fleck on Scientific Change

The authors of the nearly contemporaneous works, Genesis and Development of a Scientific Fact and The New Scientific Spirit, while writing in entirely different quarters, responded to the same kind of issues foregrounded by twentieth century sciences: fragility of facts vis-a-vis the rapidity of theory change. Sharing a central preoccupation with scientific process, Ludwik Fleck and Gaston Bachelard arrived at philosophies of science which are nonetheless in stark opposition to each other. I believe these diverging

positions are worth examining as they document the historical roots of a split within philosophy of science that is still of central importance.

Facts are delineated by concepts and sustained by scientific communities. Resting his case upon history of bacteriology, Fleck maintained that concepts became increasingly more specialised in the course of scientific change—the historical trend was the replacement of the more general concepts by the more specific ones. This historical tendency went against an abstractionist theory of meaning, most conducive to epistemic individualism, which would construe the acquisition of concepts as a movement from the concrete to the general. In contrast to Fleck, Bachelard saw the drift in scientific imagination from the concrete to the abstract, from the particular to the comprehensive. This was true both for concepts, such as space, as well as for theories, such as mechanics. For Bachelard, this trend testified to both single and aggregate creativity of individuals, their ability to transcend historically determined bounds on reason--"epistemic obstacles"--by an implicit commitment to the philosophy of "why not".

These two outlooks on the speciation of concepts have counterparts in the ways in which the two authors conceived of scientific agency. In Fleck's portrayal of history of science, the individual researcher was absorbed, in increasingly more effective ways, within the rigid frameworks of specialised sciences. Sustained by ever thicker institutional formations, these frameworks were deterministic structures, ultimately turning the individual subject of knowledge into an obsolete notion. The vanishing of the subject, along with its free will, was one of the main reasons why Fleck found conventionalism of the sort defended by Vienna Circle unacceptable. In contrast, Bachelard viewed history of science delivering ever more encompassing perspectives which made possible the choice of a theory most suitable for the depiction of specific facts. History of science thereby witnessed the liberation of mental life, as it became ever more competent to deal with the actual and the possible forms of the world. Bachelard was intoxicated by this "new scientific spirit", which could exercise its willpower even for a choice between deterministic and indeterministic scientific theories.

We inherit, at least in rough outline, the same histories of science Fleck and Bachelard examined. The conflicting lessons they drew from those histories underline a central controversy raging within science studies of the last decades, pertaining to the extent to which individuals or communities are able to express facts and represent reality, in fallible yet rationally justifiable ways. Reflections on the two moods of engaging in this controversy, as expressed by Fleck and Bachelard, should help us adjudicate better the divided land we inhabit within philosophy of science.

41. Kjaergaard, Peter C. (University of Aarhus, DK)

Demarcation in Victorian Science

The issue of demarcation has been of paramount importance for 20th century philosophy of science. How this came about remains obscure. Toanswer this question we do not find much help within the logicist tradition. However, demarcation had a high priority on the philosophical agenda of Victorian men of science. This was instigated by a largely ideological campaign in defence of science. Contrary to the received view the Victorians were far from being as self-confident on behalf of science as some of their bold statements might lead one to believe. The Victorian men of science spent a lot of time taking up the challenges from metaphysical philosophy, religion, spiritualism, and other academic disciplines. In this context a great effort was put into defining science. This led to the question of what conformed to scientific inquiry and thereby which questions could be answered within a scientific framework. In return this implied that some questions could not hope to be answered by science. Consequently, the problem of how to demarcate naturalknowledge became a major problem in the project of defending Victorianscientific ideology. I will illustrate this with several examples from theVictorian debates in the 1860s and 1870s. In conclusion I will point to how this discussion was transmitted to 20th century philosophy of science.

42. Klein, Carsten (Universität Bonn, D)

A Pseudo-Problem Reconsidered: Carnap and Reichenbach on Realism

Rudolf Carnap's position on the realism issue is well known. As he explained in his Schein-probleme der

Philosophie (1928), the quarrel between realism and idealism deals with only a pseudo-problem since both doctrines are not empirically verifiable and, therefore, meaningless. In his late paper 'Empiricism, Semantics and Ontology' (1950) he modified his position only to the extent of acknowledging that the question of choosing a realistic linguistic framework, i.e. a language-form which allows the formulation of statements about spatio-temporal objects, is of practical importance. Still the answer to such an external question can neither be true nor false but only more or less adequate for a given purposes and, hence, can never be of cognitive significance. And misunderstood as an internal or theoretical position, realism again becomes meaningless.

Hans Reichenbach's view on this matter is much more complicated. Admittedly, he agrees with Carnap that there can be no scientific, i.e. empirical, justification of choosing a realistic language. But in Experience and Prediction (1938) on the other hand, he tries to argue for the choice of such a language by giving an inductive justification. To resolve this apparent inconsistency, we will take a closer look on Reichenbach's argument. Our analysis will reveal that his defence of realism actually consists of two arguments, which are quite different. Against the tendency of positivism to accept only those statements as meaningful which can be translated into statements about sense-data, Reichenbach claims that the positivistic theory of meaning is inadequate for practical reasons. An acceptable concept of meaning results in a realistic language in which statements about external objects have a surplus meaning as compared to statements about sense-data. Therefore the question concerning the existence of the external world becomes meaningful in such a language. And the realistic language turns out superior to the positivistic language since it renders possible the expression of a wider cognitive content. The decision between the two languages is no convention but a 'volitional bifurcation'.

In a second step Reichenbach defends Realism (as opposed to Idealism) by providing the theoretical argument that there is inductive evidence for assuming the existence of an outer world. (With this interpretation we disagree with Alberto Coffa's reading of Reichenbach's Realism in Experience and Prediction.) Unfortunately, Reichenbach's attempt to establish an inductive justification of Realism failed, as was pointed out by Herbert Feigl in 1950. As a result of this the apparent epistemic advantage of a realistic language as compared to a positivistic language can no longer be maintained since Idealism and Realism are empirically indistinguishable. Rather, the choice between a realistic and a positivistic language must be considered as a mere convention, guided by practical reasons only and being devoid of any cognitive significance. This, basically, is the position that Reichenbach adopts in his later writings. Carnap's view, then, seems to have prevailed in the long run. Still, Reichenbach's attitude to the realism issue turns out to be quite different from Carnap's.

43. Köhler, Eckehart (University of Vienna, A)

Antipsychologism with Bolzano, Frege, Husserl and Gödel

Both Bolzano and Frege are known for their rejection of psychological foundations for logic and mathematics. Bolzano's doctrine of Sätze an sich excludes subjective interpretations of logic, making propositions ideal abstract entities outside anyone's mind. Frege argued that mental interpretations of number would make mush of arithmetic, entirely ruining its clarity and certainty. Husserl followed Frege in the rejection of psychologistic foundations of logic and mathematics. Antipsychologistic statements and arguments of Bolzano, Frege and Husserl are presented. A look at the basic logical terminology of Bolzano and Frege immediately raises doubts, however, as many central terms seem patently mentalistic: Bolzano's 'Satze', 'Vorstellungen' and 'Inbegriffe'; Frege's 'Gedanken' and 'Begriffe'.

On the contrary, Gödel shared no prejudices against "subjective mathematics", which he took seriously: he defined it as a part of mathematics which is provable according to a particular proof notion, i.e. a set of propositions provable by one or more of a particular set of proof rules from a particular set of axioms. Game theoretical approaches to logic explicitly assume agents which apply proof and computation rules. Analogous approaches are used in statistical theories of acceptance (e.g. hypothesis testing), where an agent-usually of unrestricted rationality to make it strong enough to generate classical scientific theories-is assumed to form acceptance sets ("knowledge corpora") by arbitrary deductive and inductive inferences from specified evidence; similarly, "subjectivistic" or "personalistic" probability weighting of evidence is undertaken by Bayesian probability theory. Since the propositions of such subjective theories are mental entities of the ideal agents, we may conclude they are psychologistic, but nothing is wrong, since the agents are assumed to behave perfectly, according to prescription. What Bolzano and Frege really criticized was a foundation of mathematics in empirical, i.e. descriptive psychology based on real human capabilities; whereas the subjective theories of ideal agents are rather

normative (prescriptive) psychology-in Carnap's terminology, "pure pragmatics"-, to which the criticisms do not apply. Husserl realized this, although unclearly. [Based on my explication of Gödel's mathematical intuition, normative theories may be justified by rational value judgments.]

Bolzano's Satze an sich are, to be sure, outside of any human being's mind, but they are in the mind of an ideal agent that conforms to all the norms of logic. Frege's Gedanken are not thoughts of fallible human beings, but they are thoughts of infallible ideal agents. (Indeed, Frege, towards the end of his life, leaned toward a quasi-Kantian position of transcendental idealism similar to intuitionism, like the position he had always taken with respect to geometry-to the consternation of anti-psychologistic logicists who did not know that Bolzano based logical knowledge on intuition-as did the later Carnap.) The distinction between descriptive (empirical) and prescriptive (normative) theories should not be confused with the distinction between subjective and objective theories. Godel contrasted "subjective mathematics" with "objective mathematics". The latter may be explicated by his later notion of "absolute proof", which I conjecture to be a closure of all deductive proof methods which could ever be validated; hence "objective mathematics" is what is "absolutely provable" from any axioms which could ever be validated; it is thus the maximal closure of all "subjective mathematics". (Reminiscent of Hegel, Godel spoke of a "mysterious process by which the subjective becomes the objective".)

A better distinction between subjective and objective theories relativizes them to metatheory and object theory, following Tarski's scheme of semantics (which followed Hilbert's metamathematics, which followed Kant's transcendental theories). According to such a relativization, any theory is simultaneously objective and subjective, depending on whether it is stated as an object theory or as a metatheory, respectively; if it is meta-theoretical, it is subjective, as its conceptual scheme makes up the representational framework of a (type of) mind. [This idea I got from Carnap's interpretation of intensions for robots; it was proposed by myself and independently by Wilhelm Essler, who got it from attempts to explicate Hegel.] Accordingly, a theory is subjective but nevertheless infallible, since the agent it prescribes exactly conforms to logical rules. Moreover, an absolutely objective standard is obtained by assuming an agent is also unlimitedly rational in the sense that it knows absolutely all admissible evidence and all inference rules which could ever be validated. Of course there is still another, much more common distinction between subjective as dubious (fallible) and objective as indubitable (infallible). The presentation concludes by showing how theories of an ideal agents are scientific theologies explicating the maximum of ethical goodness in Bolzano's sense.

44. Koterski, Artur (Maria Curie-Sklodowska University, PL)

Antipositivism in Fleck and Neurath

Neurath's and Fleck's philosophy of science may be connected by well-known phrase: 'tabula rasa does not exist'. Both, though quite independently, pointed at some features of science, unknown (or just ignored) in the framework of the so-called received or standard positivistic view. In some points outcomes of Fleck's and Neurath's researches are strikingly similar. This is surprising, because Fleck and Neurath belonged to extremely different camps in philosophy of science. The paper is to show those similarities: the antipositivistic Fleck's approach has its equivalent just in heart of positivism, i.e. in the Vienna Circle. Both of them emphasized the fact that science is a social activity. Those scientific societies were called, respectively, 'the republic of scientist' and 'thought-collective'. In harmony they agreed that those entities do not act in isolation from the rest of the society; they are influenced by sociological factors. The true theory of science has to take into considerations those factors. This means that the understanding of science requires sociological research.

Just as Fleck, the leader of new positivism was interested in the 'metaphysical' origin of science, e.g. the origin of modern scientific concepts. Both tried to show the historical aspect of the growth of science and stressed the need of historical research in philosophy of science.

The special attention should be focused on the conception of scientific fact, seen as historically and theoretically loaded concept. In thirties Neurath and Fleck with outstanding clarity emphasized theory-ladeness of all scientific enterprises, laying the foundation of the modern historical philosophy of science.

45. Kraai, Jesse (University of Bielefeld, D)

Rheticus' Role in the Copernican Revolution

Recently discovered material offers a new understanding into the reasons as to why the Copernican Revolution came about when it did and why. We learn above all of the astrological natural philosophy of the man who brought De Revolutionibus to the press. Copernicus' student Georg Joachim Rheticus (1514-1574). An examination of this philosophy reveals that heliocentrism was by no means a 'paradigm shift' for Rheticus, but rather a very natural continuation of the mathematical realism which guided Rheticus' endeavors, mathematical truth being both ontologically prior to what we would now term physical reality and also a direct manifestation of God. This resolves the old historical question of whether Rheticus was a 'physical realist' or an 'instrumentalist.'

Yet Rheticus was not the passive student he is often portrayed to be. Study reveals that he cooperated with Copernicus in several projects during his stay with him. These include a) at least the trigonometric parts of De Rev. b) the first attempt at a reconciliation of Scripture and heliocentrism (previously erroneously attributed to Rheticus alone) c) the discovery that comets are a sublunar phenomena (commonly incorrectly attributed to Tycho Brahe), and d) the first map of Prussia.

We also learn that Rheticus briefly returned to Wittenberg in early 1540. From the new lectures of 1540 we see that Rheticus was very apprehensive about teaching heliocentrism at Wittenberg. Despite the complete lack of references to the new system in the lectures, we do know that the students were aware of the new doctrine, as the student who took down the lecture referred to Rheticus as Ioachimus Heliopolitanus, or 'Joachim of the City of the Sun.' Rheticus' reserve in publicly speaking or writing about heliocentrism would continue for the rest of his career for fear of directing the discussion of it in an unfruitful manner. He nevertheless considered the remaining portion of his studies of trigonometry and gnomonics as a direct extension of what he saw as the Copernican endeavor. The discussion of this later work, which historians have often completely severed from Rheticus' work with Copernicus, offers critical insight into what Copernicanism meant to Rheticus.

46. Kutrovatz, Gabor (ELTE University, Budapest, H)

Lakatos' Philosophy of Science in his early Hungarian Period

In this paper, I try to give a comprehensive reconstruction of Imre Lakatos' early philosophy of science that mainly developed in his years of study in Hungary. I attempt to give such a reconstruction by focusing on his early publications that are not available for those who do not read in Hungarian. Naturally, I do not have the opportunity to go into all the details here, instead I would like to give an account that reaches the most essential points in his philosophical views and ideas. I do not wish to avoid mentioning the political and ideological contexts in which his philosophy was embedded at that time, though I will put the emphasis on those views that are obviously connected to his mature philosophy of science built up in England.

In summarizing the early Hungarian publications concerning the philosophy of physics, I attempt to outline the contents of Lakatos' first doctoral thesis submitted to the University of Debrecen in 1947, which thesis was later completely lost. This dissertation could give us a useful picture of what he was concerned with at his most active scientific and philosophical period in his life in Hungary. Following the arguments found in his publications from that time, I would like to point out that such a reconstruction of the lost thesis (entitled On the Sociology of Scientific Concept Building) can be carried out, and it can be used as a map to the contents of his early philosophy of science.

When we have a picture of the set of these early views at hand, we are able to compare this philosophy to his later ideas developed in England, and we can highlight some similarities and differences between the basic principles of the two periods. I will put the stress on those ideas that seem to have remained basically unchanged during his whole life, such as the importance of social (and economic?) determinations of science, the never-ending dynamics of the conceptual framework, or the irrefutable belief in the growth of knowledge and in the rationality of science.

47. Kvasz, Ladislav (Comenius University, Bratislava, SK)

Galilean, Cartesian and Newtonian Physics in the Light of Husserlian Phenomenology

Husserl's interpretation of Galileo in *Die Krisis der Europäischen Wissenschaften und Trans-zendentale Phanomenologie* is original and attracts the interests of philosophers. Nevertheless, Husserl's interpretation did not become the standard interpretation of Galileo's work. Thus still holds what Gurwitsch said in his paper Galilean physics in the light of Husserl's phenomenology: "... you must forgive me also for saying in conclusion, that notwithstanding the voluminous recent literature on the philosophy of science (whose value I do not in the least belittle), we do not yet possess a philosophy of science in a truly radical sense. Husserl's analysis of Galileo's physics indicates the direction in which a radical (i.e. a properly rooted) philosophy of science must develop."

We believe the source of problems with Husserl's interpretation of Galileo is that it is original but incomplete. It seems, that Husserl lacked a sufficient understanding of physics, and thus instead of an analysis of physics he based his analysis on the picture of physics in the philosophical tradition. Therefore what Husserl proposed is an interesting and original analysis, but not an analysis of real physics. Real physics is based not only on Galileo, but in the same way also on Descartes and Newton. Therefore for understanding of physics it is not sufficient to analyze Galileo, as Husserl did, but it is necessary to make a similar analysis also of the works of Descartes and Newton. Descartes brought not only the fulfillment of the possibilities, opened by Galileo, as his work is interpreted in the Krisis. Descartes brought also fundamental corrections of Galileo's mistakes. And Newton again corrected many of Descartes' mistakes. As Hall remarked: "It is hardly too much to say that Newton had to write the Principia because Descartes had corrected Galileo's notion of inertia."

The question, why Husserl confined his analysis of science to Galileo has an interesting answer. Even though Husserl's analysis was a criticism of positivist philosophy of science, nevertheless Husserl unfittingly remained in the framework in which positivism used to discuss science. According to positivism the central issue in science is its relation to experience. When Husserl overthrew the positivist theory of science, he still remained within the positivist framework, which reduced science to experience. A radical rejection of positivism requires to reject not only what positivists say about science, but also the framework, in which their theory of science is formulated. In other words, it is not sufficient to show, that positivism is not right in what it says about Galileo. It is also necessary to reject the positivist reduction of the whole discussion about science to a discussion of the Galilean experimental method. Until we supplement Husserl's analysis of Galileo with a similar analysis of Descartes and Newton, we cannot say to have a phenomenological theory of physics.

48. Lacey, Hugh (Swarthmore College, USA) and Pablo Mariconda (Universidade de Sao Paulo, BR)

Galileo on the Independence of Science

Versions of the idea that science is, or should be, independent (certainly from religion, politics and values; perhaps also from metaphysics) have been part of philosophical reflection on science at least since the time of Galileo and Bacon – e.g., according to Manconda (1997, forthcoming), in Galileo's 1613 letter to Castelli. Lacey (1999, forthcoming) has argued that there are three principal components to independence: impartiality (a view about the criteria for evaluating theories), neutrality (about the consequences of theories and of accepting them) and autonomy (about the conduct of scientific practices and institutions) and he has analyzed all of them in detail.

We propose to explore Galileo's arguments relating to impartiality and autonomy (with a focus on the letter to Castelli and Two Chief World Systems). These arguments have significance well beyond the immediate context of Galileo's project to free scientific inquiry from the authority of the Church. Re impartiality: Scientific judgments have their own criteria, that are independent of the authority of theology, scripture, Aristotelian philosophy [and other outside considerations]. (a) Formulating them requires a specialized(mathematical) language, whose categories are completely distinct from those of scripture and common discourse – so that conflict with scripture is impossible. (b) They are the outcome of following a method, that of "rational demonstration," which deploys such criteria as precision. [We will

attempt to identify the various criteria that Galileo holds to be part of the method.] Re autonomy: In order to pursue inquiry resulting in impartial judgments effectively, research – and the institutions in which it is conducted – need to be freed from religious [and other outside] interference.

There are certain tensions in Galileo's position. His opponents accept impartiality, but only under "instrumentalist" interpretations of scientific results. Galileo rejects these interpretations. But in proposing his own "realist" interpretation, he defends the need for specialist language on the ground that mathematical relations are constitutive of the structure of physical reality— so that in claiming independence from theology he does not gain independence from a metaphysical outlook (and, since his metaphysics is inconsistent with Aristotelian and some theology-related forms of metaphysics, perhaps he does not really gain independence from theology). We will address the question of whether impartiality can ultimately be made compatible with scientific realism.

50. Leiber, Theodor (University of Augsburg, D)

Pragmatistic Indications in 19th-Century Philosophy of Science: The Cases of Hermann von Helmholtz and Ludwig Boltzmann

It is argued that Hermann von Helmholtz and Ludwig Boltzmann, despite some deep differences between them concerning their general philosophical, epistemological and methodological convictions as well as the contents of their scientific enterprises, do share some general commitments to early pragmatistic tendencies in epistemology and philosophy of science.

With the American pragmatists - but (almost) independent of them – Boltzmann and Helmholtz share the acceptance of (generalized) Darwinism, the rejection of aprioristic-apodictic ultimate justifications of knowledge, the experimentalist pragmatisation of (theoretical) knowledge (which provides the basis of the evolutionary advantages of effectively acting and surviving), and the biting critique on trans-empirical speculations of the type which is, once and for all, immunized from empirical tests. Actually, we can ascribe to Helmholtz and Boltzmann "mereley" restricted pragmatistic tendencies, insofar as they do not really (i. e. comprehensively) share the pragmatists's instrumentalist interpretation of theories, and insofar as they do not mention the following aspects: an extension of the holistic conception of knowledge to all regions of human culture; an explication of a coherentist (or a consensualist) concept of truth; a systematic analysis of the purpose-and-means rationality etc.

At the same time, these perspectively restricted pragmatistic philosophies of science of Helmholtz and Boltzmann are to be posited most closely in the line of reasoning of Peirce's original "objectivistic pragmatism", and thus instantiate a specific perspective of methodological, i. e. method-oriented, and criterial, and basically knowledge-internal pragmatism. Helmholtz and Boltzmann both construe pragmatic "success" as successfulness in the essentially epistemic sector of effective prediction, explanation, (experimental and theoretical) control, and technological application; in addition, Helmholtz gives some hints to the importance of ethical (i. e. knowledge-external) purpose, while Boltzmann indicates the importance of empiro-pragmatic holism of meaning as constituted in theory nets.

The analysis will show that Helmholtz anticipated pragmatistic positions already during the 1860s while Boltzmann explicitly expressed them only since the late 1880s. According to the historical text sources both, Helmholtz and Boltzmann, never seem to have had any direct (informational) contact or communication with the American pragmatists; and both developed their epistemological and philosophical convictions independently from each other. Similar to (early) objectivistic-methodological pragmatism, Helmholtz and Boltzmann give emphasis to the relation of (scientific) meaning to (experimental and methodological) action, to the denial of intuitive self-evidence (and strict epistemological aprionism), and to the important role of quasi-evolutionary development of knowledge. It is true, however, that Helmholtz and Boltzmann did not systematically work out and comprehensively emphasize the important role of (knowledge- and technology-external) purpose and language in knowledge, and the connection of knowledge and (knowledge-external) value.

51. Leroux, Jean (Ottawa University, CDN)

Bachelard and Lakatos on History of Science

Gaston Bachelard is a notable absentee in most expositions of the familiar debates concerning method, rationality, theory-choice, conceptual change, etc. in science. Yet, all these topics constitute Bachelardian themes well-know to such authors as Canguilhem, Foucault and Althusser. Moreover, the same authors have not failed to acknowledge the strong impact and important influence that the Sorbonne professor's teachings has exercised on their own views. By strong contrast, authors usually figured in the above mentioned standard expositions, namely Popper, Kuhn, Lakatos, and Feyerabend, nowhere mention nor take notice of Bachelard's some dozen works on epistemology ranging over a period of 25 years, from the publication of Essay on approached knowledge (1928) to Rational materialism (1953). Our intent is not to dwell on this factual omission, which actually pertains the history of philosophy of science, but rather to touch upon a subject to which both Bachelard and Lakatos have given some elaborate thought, namely, the relation between science and history, or better, historiography of science. The question, as we want to treat it, is two-fold: How does science relate to it own history, and is there some epistemological working to this relation? Bachelard and after him Lakatos, who, incidentally, were both influenced by Hegelianism, offer a positive answer to the second question, while conceiving differently of the relation between science and its own history.

A brief presentation of Bachelard's "Applied Rationalism" and Lakatos' "Methodology of Scientific Research Programs" will suffice to show how in both cases, historiography of science is found to be tributary of the epistemological framework within which it is pursued. We will then focus on the diverging epistemological function respectively bestowed upon historiography of science, with a view to showing a) that Lakatos' idea of taking historiography of science as a quasi-empirical basis for judging competing methodologies of science meets with well-founded critique from the part of historians of science like Kuhn and Feyerabend, and b) that Bachelard's notion of "recurrent history" eschews construing history of science as some falsifying instance of scientific methodology; rather, historiography of science is assigned a positive role in unraveling pledges of rationality at the conceptual level. In an ironical way, Bachelard, whose sensibility to the manifold character of scientific method and whose "internal" standpoint in matters epistemological kept him forever distant from methodology or "demarcationism", is nearer to the Logical Empiricists' mentality of subordinating methodology to the foundational role of conceptual analysis in science.

53. Look, Brandon (University of Kentucky, USA)

Causal Relations in Descartes' Rules for the Direction of the Mind

Nearly 25 years after its original publication, Jean-Luc Marion's Sur l'ontologie grise de Descartes remains the deepest and most interesting work concerning Descartes' Rules for the Direction of the Mind. Marion's central thesis is that in the course of the Rules Descartes replaces the primacy of Aristotle's ousia with the ego and thus makes epistemolgical order, rather than a kind of ontological order, central to projects in (natural) philosophy. While this thesis has been countered by some later important works, there is still something very true to the idea that the Rules constitute Descartes' first real attempt to overthrow Aristotelian orthodoxy and replace it with an ego-based philosophy.

Curiously, what to the best of my knowledge has not been discussed in this relation is Descartes' treatment of another aspect of Aristotelian natural philosophy: the doctrine that true or certain knowledge – episteme or scientia – must involve knowledge of causes or of the genetic history of an effect. Sometimes, Descartes places the relation of cause and effect alongside other (less interesting) relations. At other times, it seems that his reliance on simple natures is somehow a way of introducing intuitive knowledge of primitive causes into the picture, and, indeed, one might say that his entire reductive-deductive method is based on the idea of reducing knowledge to knowledge of causes. In either case, one might expect Descartes to confront Aristotle directly on the issue of the centrality of knowledge of causes, and one is disappointed in this expectation.

In this paper, therefore, I wish to examine the role that causal relationships play in Descartes' "new" approach to the method of natural philosophy, and, in particular, I will concern myself with Descartes' relation to Aristotle on this issue. I hope to show that a thorough analysis of Descartes' appeal to knowledge of the relation of cause and effect undermines in part Marion's claim of the primacy of an

epistemological order over an ontological order.

54. Lyre, Holger (Ruhr-Universität Bochum, D)

On the Modern History of Philosophy of Gravitation

Spacetime theories and, hence, gravitational theories have been of particular interest for philosophers of science in our century, starting with well known names such as Poincare, Carnap, Reichenbach, Grünbaum and their various followers. The discussion is about conventionalism, substantialism vs. relationalism, and further problems.

Curiously enough, the physicist's interest in spacetime theories was almost reciprocal to this: of course, it was high in the heyday of general relativity, but considerably slowed down in the decades between 1930-1960 - mostly because of the narrow experimental basis for theories of gravitation. The history of gauge theories in physics also explains and partially mirrors this development. The gauge idea was born, with less success however, in Weyl's early treatment of general relativity in 1918, and then, again due to Weyl in 1928, changed successfully to quantum theory, with it's first application in terms of quantum electrodynamics and, in 1954, in terms of the well known Yang-Mills theories. Eventually a proper gauge approach for gravity was discovered by Utiyama in 1956 - and this has become the modern advanced viewpoint of gravitation.

After the mid sixties the physicist's interest in theories of gravitation blossomed again, first, in the form of modern big bang cosmology (after Penzias' and Wilson's discovery of the microwave background), and, next, in the form of a new foundational interest in gravitation with regard to quantum gravity, grand unification, strings etc. Thus, within the last two decades, we may see a convergence of activities and interests between both sides: philosophy of physics and physics.

In my paper I like to argue that, most of all in the context of conventionalism in spacetime theories, the gauge approach of gravity could have been discovered much earlier on the basis of a better interplay between philosophers of physics and physicists themselves. On the one hand side, it seems, philosophers of science missed a real chance to make a convincing contribution to science. On the other hand side - since nowadays the interest in gravitation from both sides is high - we might get a better chance for a fruitful discussion in the near future - and maybe the gauge approach turns out to be a key.

55. Macbeth, Danielle (Haverford College, USA)

When Derivations Explain: Logical Generality and Fruitful Proof

In their classic essay "Studies in the Logic of Explanation" (1948), Hempel and Oppenheim suggest that deductive-nomological explanations can be given both of particular facts and of general regularities. Their account of DN explanation, however, is confined to the case of particular facts. They explain why in a well-known footnote (fn 33): conjoining Kepler's laws (K) and Boyle's law (B) to form the law K&B enables a derivation of K that is not properly an explanation—by contrast with a derivation of Kepler's laws from Newton's laws of motion and gravitation; it is, however, an "open question" how adequately to characterize the difference between these two cases; until the problem is solved no account of DN explanations of general regularities is possible. Frege's distinction between fruitful and unfruitful definitions and proofs—first drawn in an unpublished essay (c1890), then again in Grundlagen (1884)—suggests just what is wanted. Put in Frege's terms, we can say that while deriving Kepler's laws from the conjunction of Kepler's and Boyle's laws is merely "taking out of the box again what we have just put into it" (GL §88), deriving that same law from Newton's laws is properly fruitful, an extension of our knowledge. It is furthermore clear that Frege thinks that it is his conception of logical generality that grounds the critical difference between the two cases. The problem is that on the standard reading of Frege's notation as a variant of our notation for a quantificational logic, it is (I argue) impossible to draw the desired distinction between fruitful and unfruitful proofs. I develop an alternative conception of (Fregean) logical generality, one that is adequate to ground Frege's distinction. That distinction in turn provides a compelling answer to the question Hempel and Oppenheim pose regarding the difference between a derivation of a general regularity that serves as an explanation of it, on the one hand, and a mere derivation of it, one that is not an explanation, on the other.

56. Mancosu, Paolo (University of California, Berkeley, USA)

Phenomenology and Mathematics: Weyl at a Crossroads

It is well known that Weyl's approach in Das Kontinuum (1918), Raum, Zeit, Materie (1918) and other works from the early twenties is strongly influenced by Husserl's phenomenology. Husserl himself praised Weyl's works as the paradigm for how a phenomenological analysis of science should be carried out. Starting in 1924 Weyl grew increasingly skeptical, and by 1928 was downright negative, on the ability of phenomenology to account for pure mathematics and mathematical physics. The stages of this development remain obscure in Weyl's published writings. However, a much more detailed and interesting picture of Weyl's changing attitude to phenomenology can be gathered by exploiting the long correspondence between Weyl and Oskar Becker, which is still preserved in the Wissenschafts-historische Sammlungen of the ETH in Zurich. Of all the phenomenologists interested in mathematics – including Hans Lipps, Felix Kaufmann, Dietrich Mahnke, and Moritz Geiger – Becker was by far the most competent in technical matters and worked extensively as Husserl's assistant. He was thus in a privileged position to act as a discussion partner with Weyl.

The talk will be divided into two parts. The first part will describe Weyl's polemic against positivism (in particular against Moritz Schlick) and his espousal of Husserlian phenomenology as the background for his conversion to Brouwer's intuitionism in 1920. The second part will analyze, exploiting the correspondence with Becker described above, Weyl's abandonment of intuitionism and phenomenology after 1924.

57. McClellan, Chris (Georgia Tech University, USA)

Ernst Mach on the Grounds for Unified Science

Ernst Mach viewed unification as a continuing future achievement for science that could be reasonably expected. I question the grounds for that claim and look for its basis in his theory of science. I first consider Philipp Frank's (1970) proposal that Mach understood the unification of science to be the use of a universal, phenomenalistic language for science that would facilitate a unification of scientific concepts. According to this proposal, Mach develops his phenomenalism as a unified perceptual vocabulary that would serve as the best means for unifying concepts of nature, or as the best means for economizing representations of experience. The main virtue of this interpretation is that it does not imply a metaphysical presupposition of a highly ordered universe. The unified language, in other words, would not necessarily yield unified concepts or understanding because different collections of sensations may not be intelligibly related in any way beyond the fact that they are the use of incompatible languages. This is similar to Carnap's notion of a reduction basis for science, and Frank's interpretation thus has the further virtue of establishing a clear link between Mach and the Vienna Circle (which appears to be his primary intention).

There are, however, several problems with Frank's proposal. Firstly, Mach's claim in "The Analysis of Sensations" is that the monistic structure for science he is offering will unify science conceptually, and not merely linguistically, by bridging a conceptual gap between physics and psychology. And although in that case his optimism about conceptual unification is quite guarded, in other situations he makes much stronger claims. Secondly, he articulates a notion of unified language for science that is not based on a perceptual vocabulary but is rather described as an international ideographic language incorporating many common, scientific symbols. Finally, it is clear in his writings that he holds conceptual unification as an aim for science independent of his phenomenalist thesis.

In the second half of this paper I present as an alternative a view Mach refers to as a "biological-economic representation of epistemology." According to this view, science is essentially a social process of reducing the intellectual effort of dealing with changing environmental circumstances. As a result, selection processes have fixed in human psychology innate functions expressed as habits of thought, including a general motivation to economize thought and unify knowledge. Economizing on thought means weighing the mental efficiencies of over-generalized principles against the costs of conceptual inaccuracy and imprecision, and thus the aim of conceptual unification is not merely to preserve the truth of experience. Beyond this, however, conceptual unification functions as more than a means of dealing efficiently with environmental circumstances on an individual basis. It also provides for social solidarity by harmonizing divergent conceptions of reality, and Mach's view is that social solidarity and cooperative

effort are fundamental aspects of human life and practical activity. He subordinates science to this end as a main part of the process of 'universal evolution,' and it is in such contexts that he makes his strongest statements regarding the future unification of science.

58. Missberger, Ulrich (Universität Mainz, D)

Theory Dynamics, Scientific Change and the Role of Mathematics in Gaston Bachelard's (1884–1962) Philosophy of Science

Topics and results of Bachelard's epistemology:
Bachelard's work treats scientific progress, theory dynamics, continuity and rationality of science, the status of observations, experimental success, the question of verification, falsification and approximation. Bachelard scrutinizes the role of mathematics in twentieth-century microphysics and chemistry. Reasoning has no absolutely sure starting point. Scientific thought is under permanent revision. Mathematics turns out to be the really progressive discipline which enables scientific newness and theory dynamics. The different disciplines constitute regions of rationality. Thanks to the achievements of mathematics the sciences nevertheless get an integrative direction. Mathematics directs empirical sciences towards the unity of rationality.
Historistic foundation as method:
Bachelard founds his epistemology on historical case studies. Since science is the most refined form of reason, rationality can be best studied by examining scientific theories. The structures of reason are not given by timeless, a priori categories. The results of the Einsteinian relativity theory and quantum mechanics show that concepts like the permanence of substance, the localization of a particle and the continuity of energy belong to particular historical theories. In such settings they proved their success. Therefore Bachelard supposes recurrent history as adequate conception for the historiography of science. History of science must evaluate its objects from the contemporary state of science.
Scientific change and theory dynamics:
Scientific reasoning starts with an epistemological rupture. It breaks with intuitions, everyday-experience and direct observations. The shift from pre-scientific practices to science is not accumulative.
Epistemological breaks also characterize the change from former to later scientific theories. They are often enabled by new experimental methods. The meaning change of the concept of combustion in Lavoissier's chemistry established a progress towards greater generality. Now combustion includes the physiological process of oxidation.

Scientists often use traditional terms, whereas the underlying concept has radically changed. Such meaning changes are intensional ones. When twentieth-century physicists speak of "weighing a star", their instrumental operation has completely been redefined.
The role of mathematics for empirical sciences:
Mathematics builds the theoretical framework which enables new experiences and, at the same time, restricts the number of possible experiences. Mathematics imposes its norms on the empirical sciences. Since mathematics restricts creativity and the inventory forces to the boundaries of logical consistency, scientific truths are systematically co-ordinated in a system of accepted propositions. The development of mathematics pushes the historic process ahead and determines the direction of the scientific change. Concepts which are narrowly connected with mathematical achievements have reached a degree of generalisation or abstraction that guarantees their future validity. At the same time mathematics generates discontinuity in the sciences in form of scientific ruptures.

The development in mathematics leads to more general and more comprehensive structures. The shift from Newtonian to Einsteinian physics is a step to a more complex and increasingly abstract empirical theory with fewer fundamental concepts. On the ontological level the concepts of 'space' and 'time' are no longer independent. Since the development of mathematics rationalises scientific changes, the process of science is discontinuous but progressive.

59. Müürsepp, Peeter (Tallinn Pedagogical University, Estland)

The Problem of Determinism in Ludwig Boltzmann's Philosophy of Science

The philosophical ideas of Ludwig Boltzmann have certainly not been given as much attention as they deserve. Today, in the light of deep methodological changes concerning the emergence of self-organisation theory, Boltzmann's ideas about determinism have become especially important. This is so mostly because discussion about determinism and indeterminism necessarily employs the notions of necessity and chaos. The following quote is characteristic in this respect: "But it is my repeated experience that it is infinitely improbable that all observed regularity would be accidental, and infinitely probable, that something that really exists takes place." There exists the fallibilist viewpoint on determinism that most physical and mental behaviour , at least on the macroscopic level, while largely but by no means fully predictable would emphasize that cause and effect may best be understood in various degrees of apparent probability. Boltzmann's thoughts about determinism have often been expressed in connection with the concept of free will. Nevertheless, these thoughts are sometimes in full accord with the problem of determinism in philosophy of science today, although it seems that determinism makes real sense only on the microscopic level. On the one hand, idealized categories as necessity, chance, possibility are tools of human understanding. They do not exist in nature. On the other hand, other categories such as probability, propensity and disposition need not be absent from nature. One of the latest 'discoveries' of contemporary methodology of science says that probability and chance cannot be taken as result of human ignorance any more. Studying Boltzmann's (and Brentano's) ideas about the problem of determinism can help to make sense of this claim. This can be done by making clear distinction between the basic conception of determinism: necessity-chance reasoning for many idealized 'deterministic' laws, power-impotence reasoning for political and many other real situations, and varying-degrees-of-probability thinking for the application of both of the other theories.

61. Nazaran, Stephen (University of Notre Dame, USA)

Spatio-temporal Separation and Individuation in Scholastic Thought

In this paper I will examine the relation of concepts of the principle of individuation of physical objects to concepts of space in scholastic thought. This paper is motivated by Einstein's separation principle, which expresses his intuition that the only way to individuate physical systems is by spatio-temporal separation; otherwise, physics is impossible. This conviction led him to reject quantum mechanics. This principle is a metaphysical postulate that Einstein felt was fundamental to our understanding of the world. Recent work has shown that this principle is a conjunction of two logically independent principles, one of which (the separability principle) is an ontological statement regarding the being of objects at separate points in space-time.

I begin by asking whether spatio-temporal separation was understood to be the ground of individuation in scholastic thought on nature. In short, the answer seems to be that it was not. Spatio-temporal separation was a consequence of the individuation of corporeal bodies, and not a principle (at least for thinkers such as Averroes, Aquinas, and Suarez). Spatio-temporal separation is a result of the accident of quantity, which, although perhaps the most fundamental accident to any corporeal substance, is nevertheless posterior to substance. The fundamental principle of individual unity of a substance must likewise be prior to its accidents according to the standard medieval argument.

What was the status of spatio-temporal separation with regard to individuation for scholastic thinkers? For some the spatio-temporal character of a material substance is intimately connected with its principle of individuation. Aquinas, for example, held that dimensive quantity individuates, but that this is a result of the formal or substantial character of the thing. Several commentators agree that for Aquinas the primary cause of individuation is the act of being, although substantial form and matter are secondary co-principals. Dimensive quantity is the means by which we first know things as individuals and is the basis of our notion of individuality. While spatio-temporal separation may be a principle of discernability of individuals, it is not an ontological cause of individuation. I examine what metaphysical repercussions this doctrine has for modern physical science within the context of Einstein's problem: while according to scholastic metaphysics every substance is spatio-temporally separated from others,

does spatio-temporal separation necessarily imply different being? For the scholastics, at least, I think that the answer is no.

The scholastics approached individuation within a different heuristic than did Einstein et al.: the scholastic question was how a common nature can be multiplied or communicated to many individuals, and is thus of a metaphysical or logical character. Most recent work on the problem of individuation in scholasticism has remained within this heuristic. I examine the traditional answers to this problem, as well as scholastic theories of space and time in order to present a scholastic analysis of Einstein's separability principle and to determine whether scholastic metaphysics offers a coherent framework within which the problem of particle individuation and indiscernability may be comprehended.

62. Nola, Robert (University of Auckland, New Zealand)

Kuhn's Changing Views on Theory Choice

This paper distinguishes three phases in Kuhn's discussion of the nature of theory choice in science. The first phase is to be found in the 1962 book The Structure of Scientific Revolutions in which Kuhn, using the notion of a paradigm, appears to claim that, even though there might be criteria for theory choice within a paradigm, there are no transcendent grounds for choosing between paradigms. A number of reasons for this claim are examined, while also noting that on occasions Kuhn does admit that criteria based in puzzle posing and solving can yield ways of choosing between successive paradigms.

In his second phase, found in the 'Postscript' to the 1970 edition of his book and in other writings of the 1970s, Kuhn abandons the notion of a paradigm and replaces it with the notion of a disciplinary matrix in which the values which our theories ought to exemplify play a central role. Kuhn develops a model of theories choice in which there is an objective aspect, the shared values, and a subjective aspect, individual or idiosyncratic weightings of these values. It is argued that Kuhn's list of values is somewhat restricted and the values are neither always as imprecise, nor do they always form an inconsistent set pulling us in different directions, as Kuhn alleges. However the model does give us an account of 'paradigm' transcendent choice in science.

In his third phase in writings from the 1980s, Kuhn attempts to give a meta-methodological justification of his values and the model in which they are embedded. He argues that the very term 'science' gets its meaning in a specific lexical context which is such that to claim that some science X is less accurate than some non-science Y is to break the 'local holism' of the word science. Thus Kuhn attempts to give an analytic justification of the values that he endorses.

The paper ends with a brief discussion of the way in which Kuhn has been coopted by sociologists of science to their cause (largely on the basis of his earlier writings). However it is pointed out that the later Kuhn rejects such overtures and is highly critical of both postmodernist and sociological accounts of science and theory choice. In so distancing himself Kuhn finds common cause with many of the traditional occupations of methodologists of science that are not always reconcilable with particular readings of Kuhn phase I.

63. Oeser, Erhard (Universität Wien, A)

Philosophy of Science and Epistemology Naturalized

Until now, logical analysis and reconstruction of proposition systems was regarded as the main task of philosophy of science. However, to reconstruct the process at generates science according to certain rules, i.e. the process that generates science methodically, is a task prior systematically and genetically. (Whewell, Mach). This results in a further differentiation between the two tasks of philosophy of science. On one hand, logical reconstruction is concerned with checking the logic truth or correctness of scientific propositional systems, while on the other hand epistemological reconstruction deals with the process of scientific theories being shaped and abandoned, i.e. in the widest sense, with methodology.

What "naturalized epistemology" (W.V.O. Quine) does for philosophy of science can be demonstrated by paraphrasing a famous sentence of Brouwer. To the formalist, science exists only on paper, to the epistemologist it resides in the human mind. Using naturalized epistemology as the basis for empirical methodology, it is feasible to circumvent a difficulty which had always troubled formalistic logic of science. Rational analysis and reconstruction of propositional systems results in just a

momentary glimpse of the respective condition of a discipline, but not in a reconstruction of dynamic transitions. Using naturalized epistemology to provide a functional model of a methodically regulated cognitive process, it is possible to imbed the dynamics of theories in the dynamics of methods.

Between naturalized epistemology and the methodology of science exists a fundamental parallel: just as pure philosophical epistemology gains its empirical foundation by being "naturalized" and such becomes a theory of the real human subject of knowledge, so methodology of science attains a reconstructive quality to explain a real process by its relation to the real facts in the history of science.

64. Ogawa, Yoshi (University of British Columbia, CDN)

Bernays on Friesian Critical Philosophy

As is well known, in characterizing the epistemological status of his brand of finitism, David Hilbert makes an explicity appeal to a Kantian notion of intuition. But the reference to the celebrated fellow Konigsberger is not limited only to the particular aspect of the so-called Hilbert's program. In fact, it is sometimes said that Hilbert goes so far as to identify the general framework of Kant's critical philosophy as a central feature of his philosophy of mathematics. As is the case with the interpretation of other nineteenth and early twentieth century thinkers, however, an important and difficult question arises with that of Hilbert as to the precise character of his understanding of Kant. In this paper, I will attempt to provide an answer to this question by taking a rather novel approach; instead of consulting directly with Hilbert's writings, I will focus on those of Paul Bernays, who was Hilbert's collaborator in the development of his foundational program and also a close associate of Leonard Nelson's "Neue Fries'sche Schule." More specifically, my primary objective in this paper is to examine Bernays's appraisal of the philosophical views of the nineteenth century Kantian thinker, Jakob Friedrich Fries, upon whose interpretation of Kant, I will argue, Bernays (and Hilbert) base theirs. Through this investigation, I hope not only to be able to obtain a clearer picture of Bernays and (Hilbert's) interpretation of Kant but also to reach a deeper understanding of their philosophy of mathematics itself. In this latter respect, there are at least two points that are of a particular interest: Bernays's positive assessment of the anti-sensationalist, anti-reductionist element found in the Friesian transcendental idealism and the logical, inferential, rather than phenomenological, verificational, character of Fries's notion of pure intuition. It is my contention, then, that these points lead us to see the need for a re-consideration of the currently prevalent instrumentalist reading of Hilbert's program.

65. Ongley, John (Boston University, USA)

On the Idea That There is No Logic of Discovery: A Partial History

This paper begins with a brief introductory sketch of the general history of the idea that there is no logic of discovery, and then gives a detailed textual account of one part of that history, namely, of the immediate sources of that idea in late 19th c. and early 20th c. German philosophy for philosophers of science of the latter two-thirds of the 20th c., such as Carnap, Einstein, Popper, Reichenbach, and Hempel.

In the brief, introductory sketch, it is maintained that the idea that there is no logic of discovery is a Kantian one, belonging, that is, to a Kantian tradition of thought that does not occur before the early 1800s. Whewell's relation to this tradition is also sketched, as well as Lotze's central role in it.
In the longer, detailed, textual account of the immediate sources of this idea for Carnap, Einstein, Reichenbach, Popper, and Hempel, it is shown that the idea is derived by these people from the anti-psychologism of the neo-Kantians, and in fact, just is the neo-Kantian anti-psychologistic distinction between psychology and logic. Statements of the above 20th c. philosophers of science are compared here with those of the neo-Kantians Cassirer, Windelband, Lotze, and Kulpe. It is shown that the all philosophers of both these groups asserted the thesis that there is no logic of discovery as a part of the anti-psychologistic distinction between psychology and logic, thus establishing a conceptual identity between the views of the two groups of philosophers.

At the same time, the anti-psychologistic theses of Gottlob Frege and Edmund Husserl are examined, and both are seen not to imply the thesis that there is no logic of discovery, so that neither Frege nor Husserl can be taken as having been a significant figure in the anti-psychologistic tradition that

held that there is no logic of discovery. This last point is just one of the useful results that a careful textual consideration of this history can produce. Another such result that is at least as significant is that an examination of the texts shows that Ernst Mach also did not belong to this particular tradition of the idea that there is no logic of discovery. This is just because he was not anti-psychologistic. Thus, Mach cannot be considered a source for the later twentieth century philosophers of science of the idea that there is no logic of discovery.

The historical connections between the neo-Kantians on the one hand and the later twentieth century philosophers of science on the other are then discussed, in order to establish that the idea that there is no logic of discovery was actually transmitted from the neo-Kantians to the later 20th c. philosophers of science. If time permits, the roots of the idea that there is no logic of discovery in the works of Kant and Schelling will also be shown with textual arguments, thus establishing that the idea belongs to a Kantian tradition.

66. Peckhaus, Volker (Univ. Erlangen-Nürnberg, D) und Reinhard Kahle (Univ. Tübingen, D)

Hilbert's Paradox

In 1903 Gottlob Frege sent a complimentary copy of the second volume of the Grundgesetze der Arithmetik (Frege 1903) to the Gottingen mathematician David Hilbert, containing in the postscript the description of Russell's Paradox and Frege's admission that this paradox can be formulated in the system of the Grundgesetze. In his response Hilbert declared that the paradox described had been known in Gottingen for a long time. He himself had found other, even more convincing examples four to five years ago, and after having informed Zermelo the latter found the one mentioned by Frege three to four years ago (Frege 1976, 79–80).

It is well known that Zermelo indeed discovered a set-theoretical paradox in Cantor's theory, independently of Russell (cf. Rang/Thomas 1981). But what were these contradictions Hilbert claimed to have found around 1898/1899? There are some further traces of Hilbert's Paradox in correspondences of the time. The most explicit hint can be found in Blumenthal's biography of Hilbert where we read that Hilbert formulated the contradictory notion of the set of all sets which arise from union and mapping on themselves (Blumenthal 1935, 421–422). Hilbert never published this contradiction.

One of the author's was able to discover the paradox mentioned by Blumenthal which is most likely the one Hilbert referred to in his letter to Frege. It is presented in an unpublished lecture course of Hilbert's delivered in the summer term of 1905 on Logische Principien des mathematischen Denkens (Hilbert 1905). There Hilbert discusses the paradoxes of set theory mentioning Zermelo's paradox and a contradiction of "purely mathematical nature" which was never published, as Hilbert stressed, but known to set-theorists, especially to Georg Cantor.

In the historical part of the lecture the story of Hilbert's Paradox will be told and it will be shown that Hilbert's axiomatic program was indeed affected by the paradoxes, contrary to the standard view. In the systematical part of the lecture Hilbert's Paradox will be described and reconstructed using modern tools. It will be shown that it is closer to Cantor's Paradox than to Russell's.

67. Poland, Jeffrey (University of Nebraska, Lincoln, USA)

Hempelian Standards for the Classification of Mental Disorder

In a 1959 address to the American Psychopathological Association, Carl Hempel identified several considerations pertinent to the development of a scientifically sound classification system for mental disorder. His address was significant in, at least, three ways. First, it presented a sophisticated application of Hempel's general philosophical framework to an important area of scientific inquiry. Second, it very likely exerted a strong influence on the development of the much acclaimed DSM-III (published in 1980) and its successors (DSM-III-R, DSM-IV) which are expressions of the currently dominant approach to the classification of mental disorder. And third, the standards identified in the address remain an important starting point from which to develop a more up to date set of scientific standards for the classification of mental disorder. This last point is of considerable contemporary importance because the DSM approach to classification is under much pressure regarding its scientific status. In this paper we consider three questions: (1) what were Hempel's proposed standards for

evaluating the scientific status of a classification system for mental disorder?, (2) in the light of subsequent developments in the philosophy of science, how should those standards be revised or supplemented in order to yield a more up to date set of standards?, and (3) what is the significance of the revised standards for the issue of the scientific status of the DSM approach?

In his address, subsequently published under the title 'Fundamentals of Taxonomy', Hempel identifies two features (i.e., empirical import and systematic import) that should be regarded as the marks of a scientific system of classification. In addition, he notes four developmental trends (i.e., trends concerning the form, the content, and two key features of a classificatory system) that he regarded as desirable for the future development of a classification system for mental disorder. Both the features and the trends are expressions of Hempel's deeper views about scientific objectivity, the meaning of scientific concepts, the nature of scientific explanation, and the structure of scientific theories. There have been at least four developments in the philosophy of science since 1959 which have a direct bearing upon the adequacy of Hempel's proposed standards and the character of more appropriate standards. These developments concern: the 'observation/theory distinction', natural kinds, reduction and the unity of science, and scientific objectivity. In this paper, after clarifying Hempel's proposed standards and trends, we show how these subsequent developments lead to the formulation of a revised set of standards apt for assessing current approaches to the classification of mental disorder. Finally, we outline the significance of these standards for the future evolution of approaches to classification in this area.

68. Portides, Demetris (University of London, UK), Psillos Stathis (University of Athens, GR)

The Origins and Early Stages of the 'Received View of Theories'

As is well known Carnap's views on the structure of scientific theories were influenced by the hypothetico-deductive approach of Duhem and Poincare and by Hilbert's formalistic program. Yet, in developing his views in the late 1930s and subsequently, Carnap seemed to place extra emphasis on the Hilbert side of his early influences, living behind the Duhem-Poincare side. What is particularly interesting is that both Poincare and Duhem took theories to be, essentially, mathematical structures. They dealt in great detail with the issue of whether and how these structures should be interpreted, and in particular with the epistemic status of the basic principles (or hypotheses) of physical theories. But their analytical tools were those of mathematics and not of meta-mathematics. Unlike them, and despite the influence they exerted on him, Carnap insisted that an adequate characterization of scientific theories can only be achieved by means of their formalization and axiomatisation in a rigorous language: the language of first order logic with identity, in the early stages of his career, and richer type-theoretic languages, later on in his philosophical development. But surely, the matter of whether the tools of analysis of the structure of scientific theories are mathematics or meta-mathematics has more to do with how close one wants to stay in actual scientific practice and how realistic one wants to be in his analysis.

In this paper, we shall try to explore in some detail the early developments of the so-called 'received view' of theories, especially in connection with the views of Poincare and Duhem. We shall examine Schlick's view that a Hilbert style presentation of scientific theories gives a clear sense to the view that scientific knowledge is structural knowledge. We shall also look into Carnap's development of the Hilbert program and argue that what he found attractive in it was that it seemed to offer a neutral platform for the characterization of scientific theories which made the realism/instrumentalism debate intelligible and which could form the basis of an empiricist approach to theories. Finally, we shall examine Hempel's recent attempt to reconcile the received view with the thoughts of Duhem and Poincare. This attempt, which can be traced back to the young Hempel of the late 30's, is a recent revival of the Neurath/Carnap debate. Hempel seems to be combating Carnap's program of Logical Reconstruction or Explication of scientific theories, in favour of a naturalism closely linked to Neurath's views about the language of science.

69. Rahnfeld, Michael (Kiel, D)

Carnap's "Logical Construction" as an Example of Cassirer's Theory of Symbolical Forms

The aim of this lecture is to show that the empirical approach in Carnap's "The Logical Construction of the World" and the transcendental approach in Cassirer's "Philosophy of Symbolical Forms" are not to be regarded as incompatible positions as is often asserted but that they converge in essential aspects of the object constitution theory.

In his "Symbolical Forms" Cassirer provides a programmatic framework for a symbolic resp. object constitution theory. For object constitution, three functions are crucial, and these vary in their transcendental performances: the expressive function, the depictive function and the semantic function. These are the three basic functions which are, in principle, given within the transcendental circumstances of any symbolism, although the extent of their relative clarity will change in myth, language and science. The expressive function is the basic layer of object constitution, it is directed towards the given, conceptually unanalyzed existence of the phenomena of consciousness which can only be named as whole. In the depictive function, the varying facts are considered as representatives of a constant object (substance). This is the stage where the actual object constitution takes place, which synthesizes objects according to categories of similarity or topological invariance, extracts them from the flow of experience and thus turns them into the object of linguistic attribution. The semantic function no longer refers to the objects of the sensorial world, but to their pure structure. The paradigms of the symbols of the semantic function are the (non-interpreted) formal structures of mathematics and logic. These three functions are characterized by an increasing "reflexive distance" to the immediate facts.

Carnap's "Construction" can be understood as a technically formal application of the vague, extensive symbol conception defined by Cassirer. The systematic procedure used by Carnap when constructing a constituent system of all empirical terms easily fits into the triassic of expressive function, depictive function and semantic function:

On the lowest level, the elementary experiences correspond with the expressive function. They are unanalyzed overall impressions, as they light up within the consciousness of their entirety. Here, Carnap falls back on the results of Gestalt psychology, according to which the impression of the whole is primary, and only resolved through successive abstractions into distinguished sensations. The elementary experiences are synthesized by logical means according to the depicitve function. The only descriptive term required by Carnap for the construction of the objects is the relation of "similarity memory", which has the character of a transcendental category (cf. "Construction", 75, 83). This relation exists between two elementary experiences x and y if the memory, when comparing x and y, recognizes them to be somewhat similar. The phrase "somewhat similar" here means that two elementary experiences both correspond entirely with one another in at least one component of each experience. On this minimal basis Carnap lays down precise, logical and structural rules for the generation of quality classes, their orders and all other constituted objects. This results in the problem that the similarity memory is a descriptive constant which is included in the structural characterizations of the different object levels and thus violates the objectivity criterion according to which all scientific statements are structural statements. The way Carnap gets out of this, is to replace the simularity memory by a certain variable, through which all definitions turn into implicit definitions, which means that they are restrictedly interpretable structures. However, for the time being, they are sufficient for the unambiguous characterization of the objects. These implicit definitions are formal symbols in a line with Cassirer's semantic function.

70. Rediehs, Laura (St. Lawrence University, USA)

Fashioning a New Intelligibility: Retelling the History of the Debates Concerning Scientific Realism

In reflecting on the implications of early quantum theories, Niels Bohr noted that the incorporation of the "irrational element" (the quantum of action) into physics created an epistemological crisis: a loss of the ability to picture what is happening at the subatomic level. This loss of visualizability is equivalent to the loss of an ability to map out clear causal patterns in space and time, regarding subatomic phenomena. In the background essay to an anthology on more recent developments in the philosophy of quantum

mechanics, James Cushing claims that the collected papers "are attempts to fashion an explanatory discourse with a view to producing an understandable view of our world" (Cushing and McMullin, 1989). But this fashioning of a new understanding of intelligibility has not been an activity limited to the philosophy of quantum mechanics. The epistemological crisis in the history of physics has had a profound, although diffuse and perhaps not widely recognized, effect on philosophy more generally. For example, an examination of the history of the debates regarding scientific realism in the twentieth century reveals that the terms of the debate have changed over time, and there is a larger pattern in these changes that is only now just starting to become apparent.

The shift occurring in these debates can be described in terms of a gradual reformulation of our most basic epistemic goals. The old characterization of the epistemic goals is perhaps best represented by the logical atomism of Bertrand Russell and (the early) Ludwig Wittgenstein. If we could represent our knowledge purely in terms of language, and map the basic atomic elements of language onto the basic constituents of reality, then we could say that our language perfectly mirrors the ultimate structure of reality itself. This characterization of the basic goal of philosophy was taken up and pursued by the logical positivists, although the logical positivists, in general, were wary of arguing about scientific realism (seeing questions about realism as problematic metaphysical questions). Questions regarding realism began to become prominent again when philosophers began to realize that the logical positivist project was facing deep and perhaps intractable difficulties.

In the very recent history of the debates concerning realism, there seems to be developing a convergence towards more relational accounts of scientific realism. This convergence can be seen to reflect a subtle reformulation of our basic epistemic goals, which in turn reflects a deeper changing account of our most basic understandings of reality. The atomistic picture of both knowledge and reality is being replaced by an account that is relational (and in certain aspects, irreducibly relational). Thus, we can also say that the physical discovery early in this century that certain physical quantities thought to be separable turn out to be bound together in irreducible ways did indeed have profound epistemological implications. These implications have emerged into academic philosophy in the historical shift from the older objectivist or atomistic versions of realism to the more recent relational versions of realism.

72. Rhee, Jyoo-Hi (Universität Bielefeld, D)

The Mind-Body Problems: From Logical Empiricism to the Philosophy of Mind

In this paper I present the project of resetting the mind-body problem in the "wissenschafts-philosophische" framework. In this framework, the mind-body problem [MBP] is conceived as issuing from conflicts between two distinct epistemic systems, the perceptual world view and the scientific one. The MBP complex can profitably be broken up into two distinct components: the problem of the mental-physical dichotomy [MBP-I], and the problem of the mind-brain interrelation [MBP-II]. MBP-I emerges from the diversity of the logical structure of each epistemic system, the perceptual representation system of phenomenal properties on the one hand and the scientific representation system on the other hand. The problem is therefore logical in nature, and to be placed in the comparative study of the logical structure of both representational systems. MBP-II, which the current philosophy of mind inappropriately defines as the mind-body problem proper, is then to be situated in my framework as a subordinate problem. Its solution provides a neuroscientific explanation of the disharmony between both knowledge sets, which creates MBP as such. The MBP can be said to be solved, if the two epistemic systems which often appear to collide with one another have been brought into accord.

The task of this paper is to make clear the distinctiveness as well as the relatedness between MBP-I and MBP-II. The distinctiveness leads to the consequence that we have to differentiate methods in order to solve each problem appropriately: the comparative analysis of logical structures for MBP-I; empirical research methods of natural sciences for MBP-II. Clarifying the relatedness between MBP-I and MBP-II could offer a guideline how the research results of each domain can be adequately applied to understanding and solving the MBP as a whole.

I choose a historical approach in order to fulfill the task. I observe a paradigm change from MBP-I to MBP-II between the first and the second half of our century. It took place in parallel to the transition from "wissenschaftliche Philosophie" of logical empiricism, especially of the Vienna Circle, to the philosophy of science of Post-World-War-II analytical philosophy. My investigation in this paper is focused on the framework shift from Schlick through Feigl to the contemporary philosophy of mind. Feigl

is chosen, because he reflects the phase of transition symptomatically, and Schlick as the representative of the time before the turn. The critical moment of transition comes in the late 1950s, when the philosophy of mind has begun her career, launched through the mind-body identity theory. I focus on Feigl's work in 1958 and 1961, and characterise his "identity thesis" with respect to the above sketched framework. My further study is continued from this critical moment retrospectively as well as prospectively. Retrospectively, I compare the identity thesis of Feigl with that of Schlick. Schlick's identity theory is generally taken to be the precursor of the identity theory, but is in fact fundamentally distinct. The distinctiveness between them offers a cue for clarifying the distinctiveness between MBP-I and MBP-II. Schlick's proposal is worth pursuing, not simply for the sake of history, but also with respect to the contemporary perplexity of the qualia debate. Prospectively, we encounter the familiar debate of the contemporary philosophy of mind which has developed partly in accord with Feigl's prospect, partly through a misunderstanding of it.

Before the turn, the subject matter was MBP-I in the sense of the perceptual-scientific dichotomy, whereas after the turn MBP-II took center stage. On the basis of the comparison of Schlick and philosophy of mind I will make clear why and how both problems must be kept apart. The analysis based on the "wissenschaftsphilosophische" framework will tell why the philosophy of mind fails to offer a solution or resolution. The paper ends by suggesting how, in spite of their distinctiveness, both problems are to be jointed together in the whole complex of the mind-body problem as such.

73. Ribeiro, Henrique Jales (Coimbra, P)

The End of Philosophy of Science in the History of Analytical Philosophy

The author deals with the historical and philosophical presuppositions of the idea of an end of philosophy of science, advocated by Rorty and Feyerabend in the last twenty years. In particular, it is shown that that idea was in various ways explicitly suggested along the evolution of the analytic movement, whether among the Viennese logical positivist or in the English philosophy of ordinary language, and that that kind of suggestions has clearly influenced not only Rorty and Feyerabend but also other contemporary analytical philosophers of science (as Kuhn). The justification of this perspective passes, in this paper, through a reformulation of the current concept of the history of analytic philosophy, which is suggested being in the origin, deceitfully, of the equal current notion, but false in general, that that end of philosophy of science is something characteristic only of the contemporary analytic philosophy. In contrast, it is suggested that Rorty and Feyerabend are not, in fact, the authors of that death of the philosophy of science, but, rather, its agents more visible in the history of analytical philosophy. Finally, the author examines some foundationalist alternatives to the end of the philosophy of science, and concludes with the suggestion that, in order to save it, we must completely reformulate some historical and philosophical essential presuppositions of contemporary analytical philosophy.

74. Rollet, Laurent (Université Nancy 2, F)

Poincaré's Conventionalism and French Philosophy at the Turn of the Century

Henri Poincaré's philosophy has often been described as a 'geometrical conventionalism'. This shortcut expresses the essence of a thinking which has mostly dealt with the constructive foundations of scientific principles. The principal themes of this philosophy find their roots in a scientific practice. Poincaré's philosophy is essentially a scientist's philosophy: it has its origin in a regular contact with scientific methodology, and its content in the epistemological reflections which arouse from this methodology. Consequently, it is not surprising to discover, in Poincaré's writings, a very large amount of technical references (Analysis Situs, group theory, set theory, fuchsian functions, etc.) The scientific and technical roots of Poincaré's philosophical thinking have been widely studied by historians and philosophers of science and their theoretical importance have been demonstrated by logical positivism and analytical philosophy. Nevertheless, by focusing on theoretical and scientific contents, these studies introduced a kind of imbalance and, in fact, the philosophical origin of Poincaré's thinking has been generally neglected. Did Poincaré have contacts with the philosophers of his time? How did he get involved in

philosophical reflection? What did he know about the philosophical discussions circulating within the French philosophical community? All these questions have been rarely asked. The purpose of this paper is to deal with these interrogations and to put forward several leads about Poincaré's relationships with the French intellectual community at the turn of the century. A vast amount of indications speaks for a strong anchorage of his writings in traditional philosophy. The exploration of Poincaré's correspondence and the analysis of his contacts with philosophers like Emile Boutroux, François Évellin or Xavier Leon could give birth to an alternative interpretation of Poincaré's philosophy.

75. Ropolyi, Lászlo (Eötvös University, H)

Lukácsian Elements in Lakatos' Philosophy of Science

Lakatos constructed his major contribution to the philosophy of science, the methodology of scientific research programs (MSRP) in the late sixties and early seventies in England. We try to show that his decision with the MSRP was motivated by his philosophical and political ideas of the forties and fifties in Hungary, when he was imbued with the communist ideology and was influenced by the philosophy of Georg Lukács. From this point of view the MSRP can be considered as a special representation of Lakatos' earlier political values and practice in the field of history and philosophy of science.

Lakatos perfectly knew the Lukácsian ideas. Besides his early studies on the History and Class Consciousness, it is evident from his early Hungarian papers. On the other hand, in the mid-forties in Hungary Lakatos followed a communist political practice. In this political system the political praxis and the ideological, theoretical values has formed a special kind of unity.

In England Lakatos was influenced by the Popperian views, but in the sixties Lakatos returned to a Lukacsian rationality conception, as a valid representation of the relation of theory and the political praxis. This returning did not mean his returning to the communist ideology. His actual political standpoints sometimes were directly anti-Communist, but in contrast to these opinions he accepted the essential aspects of the political philosophy of Lukacs. Moreover, he applied it in his philosophy of science. For Lakatos this political philosophy was the model of the scientific methodology, where the unity of scientific praxis and the abstract, theoretical rationality can be considered. The appearances of the Lukacsian elements in the MSRP can be identified in the followings:

The socio-historical context. Lakatos considered research programs instead of theories. A research program is a special kind of collection of interconnected entities. This view comes from Lukács' History and Class Consciousness.

The concept of progress. In the Lakatosian MSRP the concepts of progression and degeneration has a fundamental role. Here Lakatos applied a universal features of the progress that is close to the Lukácsian one, but replaced it with the concrete aspects of the scientific progress.

The concept of rationality. This is the most important point of the Lukacsian influence. The Lukacsian concept of rationality appears in clear form many times in the MSRP. The Lakatosian progressive shift unifies the theoretical and practical and heuristic aspects of the progression, i. e. the (Lukácsian) decision with the progress can unify the theoretical and practical rationality.

The alliance policy. The "protective belt" around the "hard core" of the program, and their functioning are the perfect representations of the organization of the party and the Popular Front political praxis. This ideas can be contacted with his own political practice and at the same time with the Lukácsian Popular Front ideas.

The historical progress. Applying these ideas he was able to identify a more meaningful historical process in the case of science. In this process the progression of the unity of praxis and theory can be considered and constructed.

76. Ruttkamp, Emma (University of South Africa)

The Role of Models in Semantic Accounts of Science

In this paper I shall offer a critical overview of work and publications in philosophy of science dedicated to analyzing, in particular, the nature of scientific theories in terms of conceptual (mathematical) models of scientific theories and the various semantic relations between such models, scientific theories, and aspects of reality. I shall base this review of the state of the art of typical so-called "semantic" accounts

of science on the interpretation and use of the term "model" in philosophy of science over the past fifty years. In conclusion, I shall review briefly some of the usual realist inspired questions about the possibility and character of relations between scientific theories and reality as implied by the various accounts of science I shall discuss in the course of the paper.

The use of the notion of model is nothing new in either philosophy of science or the (empirical) sciences themselves. Writers such an Achinstein (1968), Hesse (1963), and more recently, Redhead (1980) have paid much attention to the heuristic uses of models in the development of scientific theories. In philosophy of science literature there are two main approaches to the structure of scientific theories, the statement or syntactic approach - advocated by, among others, Carnap, Hempel and Nagel - and the nonstatement or semantic approach - advocated, among others, by Suppes, the members of the structuralist school (such as Sneed, Stegmüller, Balzer, and Moulines), Beth, Van Fraassen, Giere, and Wojcicki. In both the statement and nonstatement accounts of science the notion of model, almost by definition, plays a central role. The most interesting questions, in this sense, centre around the ways in which writers distinguish between theories and the mathematical structures that interpret them and in which they are true of, e. between scientific theories as linguistic systems and their non-linguistic models. The statement description of scientific theories is made in terms of an analysis of scientific knowledge as embodied by theories formulated in some (appropriate first order) symbolic language with more or less direct links of correspondence to objects and relations in reality. Defenders of the nonstatement approach in their turn, place more emphasis on the (mathematical) structures satisfying the sentences of scientific theories in the Tarskian sense, than they do on the language in which particular theories are formulated. Philosophers of science who seem to have incorporated principles from both the statement and nonstatement accounts of scientific theories in their own work — such as Nancy Cartwright, and defenders of so-called model-theoretic realism — also centrally employ the notion of models to illustrate their views on the nature of scientific theories.

77. Rynasiewicz, Robert (Johns Hopkins University, USA)

Zuordnung und Eindeutigkeit: Conventionalism in Reichenbach's Earliest Philosophy of Space and Time

The origins of Hans Reichenbach's conventionalism have become the scrutiny of a number of recent studies. (See Friedman 1994 , Coffa 1991, Ryckman 1994 and 1996, and Gimbel 1999). In his first book concerning the philosophy of space and time, Relativitatstheorie und Erkenntnis A Priori (1920), Reichenbach explicitly casts himself as an opponent of certain "mathematicians [who] asserted that a geometrical system is established according to conventions and represents an empty schema that does not contain any statements about the physical world," and footnotes Poincare as the principal representative of this school. Yet, by 1924, in the introduction to his Axiomatik der relativistischen Raum-Zeit-Lehre, he includes himself with Helmholtz and Poincare among those holding "the view that every spatial and temporal metric presupposes coordinative definitions," a view which, he says, "is known as conventionalism." Was this apparent sea change the natural outcome of pursuing his wissenschaftsanalytische Methode in attempting a "constructive" axiomatization of special and general relativity, as maintained by Gimbel, or was it due to the influence of Schlick , as Friedman suggests, or perhaps the influence of others, as Ryckman argues?

It is difficult to adjudicate between these views without first achieving a sufficiently complete understanding of the basis of Reichenbach's wissenschaftsanalysische Methode as set out in Relativitatstheorie und Erkenntnis A Priori. Central to this are his definition of truth in terms of eindeutige Zuordnung (univocal coordination) and the role of Zuordnungsprinzipien (principles of coordination) for physical theories in determining the correlates in reality of the variables appearing in the laws of physics. (Zuordnungsprinzipien are the obvious forerunners of coordinative definitions in the Axiomatik.) Part of my paper is devoted to an explication of these, in particular, the relation of the Zuordnungsprinzipien to the criterion of the Eindeutigkeit (uniqueness) of a coordination, which has been confused by a certain infelicities in Maria Reichenbach's translation. Another part of the paper seeks to explicate the range of the conventional in Relativitatstheorie und Erkenntnis A Priori in both principle and fact. As a matter of fact, Reichenbach believes that, although distant simultaneity is conventional, spatial geometry is empirical (as suggested above). As a matter of principle, things are more complicated. Reichenbach recognizes that different sets of Zuordnungsprinzipien may be equivalent, and thus the choice between them conventional. But the question how or in what sense they achieve the same coordination remains

a bit mysterious given his stance on the criterion of Eindeutigkeit.

78. Schickore, Jutta (Dibner Institute for History of Science and Technology, Cambridge, USA)

Delusions, Confusions – Attempts to Differentiate Errors, Mistakes, and Illusions in the Early 19th Century

Explanations of the growth of scientific knowledge with regard to inquiries into scientific error management have recently gained importance. Peter Galison, Deborah Mayo, and others have studied in great detail how scientists seek to uncover, avoid, and get rid of errors in empirical investigations. However, except for the logical study of fallacies, the history of the concept of error itself has not yet attracted similar attention.

My paper reconstructs analyses of errors, delusions, and failures in late eighteenth and early nineteenth century German philosophy of science, thereby focusing on the works of Johann Heinrich Lambert, Jakob Friedrich Fries, Bernard Bolzano and Matthias Jacob Schleiden. I argue that in this period, the conceptual field of 'error' [Fehler/Irrtum] underwent significant changes. Whereas Lambert applied both the concepts of Fehler and Irrtum exclusively to the errors of human judgement, the conceptual framework of scientific errors and failures and their sources became much more elaborate in the first decades of the nineteenth century. Erroneous judgements, intrinsically imperfect instruments, and lack of attention to the performance and limits of instruments and to the proper and pathological functions of sensory organs gradually became separated as different sources of incorrect results. In the course of this development, the concept of Fehler came to be applied to instruments. It is argued that as a result, the meaning of the concept of error was radically transformed. According to the older view, only the processes of reasoning could be erroneous, since they were not subjected to natural laws. This view became untenable. For Fehler now had to be determined in terms of laws of nature.

79. Schildknecht, Christiane (Universität Konstanz, D)

Realizing Experiments in Thought and Science

Like real experiments thought experiments play a pivotal role in the history of philosophy of science. From Ernst Mach's coining of the term and rather wide conception of what constitutes a Gedankenexperiment (Mach 1976) to Sorensen's recent narrowing of the concept in the sense of a demonstration of paradoxes (Sorensen 1992), almost all grades situated in between have been explored. In view of the standing disagreement as to the methodological status of thought experiments, this paper offers an analysis of their methodological import by way of focussing on the criterion of realizability. It will be argued that the degree of realizability not only determines the success of thought experiments and accounts for their failure from a systematic point of view (underdeterminacy of relevant components; overspecification as begging the question etc.) but also accounts for the history of their being favoured or dismissed in philosophy of science. The historical analysis presented takes a close look at the arguments for or against thought experiments, especially at those offered by the doctrines of Mach, Duhem and Kuhn, and relates them to the import they concede to questions of realization. Thus, the value that Mach attributes to thought experiments reflects his empiricist stance in that experiments carried out in thought are taken to mimic the processes of nature. While the variation of components of reality in thought, here, becomes a matter of assessing correspondence and coherence, with Duhem questions of method prevail. Focussing on the theory-dependent character of experiments in terms of observation and interpretation, thought experiments are taken as variations of components of a theory and are subjected to the question of whether or not they can be reconstructed as components of argumentation; if not, they are dismissed as mere 'games with words'. Kuhn, on the other hand, takes a favourable stance towards thought experiments and views them as inducing a crisis in that they sometimes fail to fit the structure of the world, and as a result of this force us to reconceptualize. Still, since Kuhn primarily conceives of thought experiments as teaching us something about our conceptual scheme and derivatively about the world, his conception differs from a full-blown realist approach (Brown 1991) where thought experiments are conceived of as primarily teaching us something about the world

and secondarily about our conceptual scheme. In sum, then, realizability turns out to be the crucial feature where the systematical approach to the methodological status of thought experiments coincides with the historical approach to the stance taken by philosophy of science.

80. Schirrmacher, Arne (Georg-August-Universität Göttingen, D)

What is the Place for the Scientists' Philosophy of Science?

That scientific research raises philosophical questions is undisputed and consequently it is the scientists themselves who are first challenged. But what is the value of their response? Much philosophy of science rightly starts with a thorough investigation of the original papers of scientists but peculiarly rather often falls short of consulting the scientists' own reflections on the philosophical motivation, content, and impact of their ideas. The claim that philosophy from scientists comes only after their productive periods and hence is a special sort of palliated retrospective arm-chair philosophy of no use for any serious discussion, however, cannot be maintained for a number of reasons:

– Philosophical writings of scientists ex post are just much more visible than philosophical thought and discussion concurrent with scientific work. There are, however, many sources made available by the historian of science that can account for the concurrent philosophical thinking: notebooks, letters, lecture notes etc.

– If philosophical thinking is seen as a kind of a silent part of scientific activity that does not enter in the written product of the scientific endeavor, then it may have a similar status as tacit knowledge about apparatus and manual skills that usually does not materialize neither.

– Even retrospective accounts of the philosophical significance of scientific theories sometimes entail genuine thought, e.g. when one can find Max Planck anticipating some decades before Hanson the theory-ladenness of observation or a kind of three-world distinction analog to Popper.

– Probably the discussion of the new experimentalism — arguing that experimenting has a life of its own not interfering with theorizing — may have contributed to repress a more all-penetrating understanding of theoretical and philosophical concepts and notions as an integral part of cognitive processes in science.

In order to address the main question of the proper role of the scientists' philosophy of science, some examples from modern mathematics and physics will be discussed in some detail: Both Max Born and Hermann Weyl did comment on philosophical questions of mathematics and physics in articles and books when they were amidst pursuing influential research projects. Their teacher David Hilbert, however, left only unpublished material on his philosophical ideas. We will survey some of their writings and try to relate them to contemporary and current philosophy in general and philosophy of science in particular. E. g.: role and status of theories, interpretation of symbols and formalism, impact of Gestalt psychology, contacts to philosophers (Frege, Husserl, Nelson, Cassirer, Wertheimer etc.), relation to current philosophy of science and interpretational philosophy.

81. Senk, Janez (Faculty of Humanities, Ljubljana, SLO)

Wilhelm Ostwald and his Philosophy of Science

In this paper, I would like to present a section from the history and philosophy of science on the turn from the 19th to 20th century. After the end of the era of German idealism in continental philosophy (Kant, Fichte, Schelling, Hegel), the second half of 19th century was marked by the efforts to make a new beginning in philosophy based on recent scientific discoveries. For this direction of philosophy - for neokantianism and positivism - Kant and his transcendental approach remained one of the most important references, although his idea of "das Ding an sich" was mostly considered as an unnecessary remainder of metaphysics. On the other hand, science itself lacked theories, which could successfully explain the new discoveries. The field of thermodynamics with its concept of energy was first one to link together different branches of physics.

In this sense, the position of German philosopher, chemist and physicist Wilhelm Ostwald was quite significant. Although sometimes controversial – especially because of his antiatomistic views – Ostwald was undoubtedly a brilliant scientist awarded in 1909 with the Nobel Prize for chemistry. His philosophy was largely influenced by Ernst Mach and Auguste Comte and there is also a constant

reference to Kant in many of his books (e.g. Der energetische Imperativ – The energetistic imperative). In Ostwald's system of energetics or energetism (die Energetik), which was constructed analogue to Mach's sensualism, the energy itself was the substance and not merely the property of matter. Furthermore, he considered every occurrence in nature as the result of measurable changes of energy in space and time. In the science, this concept should replace the old mechanistic point of view - "the scientific materialism" - by which every physical phenomenon is a result of mechanical movement of atoms. His strongest scientific opponent regarding this question was Ludwig Boltzmann (with whom - despite their scientific differences - he was always on good personal terms). Ostwald also believed that his concept of energy and the knowledge of the principle of natural order in the world - based on the scientific knowledge - could eventually abolish the mind-body dualism and therefore all religions. This aim he pursued in his writings Monistische Sonntagspredigten (Monistic Sunday sermons) and Arbeiten zum Monismus (Works on monism).

Finally, I would briefly try to present another interesting moment of this time-period - the beginning of Freud's psychoanalysis. For a short time, Freud and Ostwald were in correspondence. In 1910, Ostwald invited Freud to publish an article in his newspaper Annalen der Naturphilosophie (Annals of Philosophy of Nature - where few years later the first edition of Wittgenstein's Tractatus Logico-Philosophicus was published). Anecdotically, Freud first accepted this offer, but then never wrote the article. Never the less, it seems quite obvious that a lot of concepts which Freud used in his earlier works and also later (free and bound psychical energy), were largely influenced by the Ostwald's system of energetism.

KEYWORDS: Ostwald, Mach, Comte, Kant, Freud, neokantianism, positivism, energetism

82. Simonsen, Kenneth (REHSEIS Université Denis Diderot Paris, F)

Concerning some philosophical reasons for the recourse to mathematics in the study of physical phenomena in the thought of Newton and Leibniz.

The extensive use of mathematics in the study of the nature is one of the main characters of the traditionally called "scientific revolution" of the 17th century. Throughout this century, the idea that the Book of nature was written in mathematics got more and more accepted. But, even so, the question of how the mathematics could be used and apprehended in this domain wasn't always clear. Concerning the dynamics, or rational mechanics, Isaac Newton and Wilhelm G. Leibniz both proposed theories where mathematics plays an essential role. Taking Newton's Principia and the many articles on the subject by Leibniz from the 1680s and 1690s as representatives of their thoughts in physics, one can easily show - and it has been shown by many scholars - that their mathematical theories of physics are quite different. We shall, however, not concentrate on these differences but rather have a closer look at the reasons and arguments they advance to ground the possibility of using mathematics in the study of the nature. To this question, which is of great philosophical interest, Newton and Leibniz provide different answers which plausibly have an impact on their respective theories. We propose to explore this aspect as it is related to the "mathematisation" of physics.

Even though their answers are different, they have, at least, two things in common. They are both referring to Descartes in a way or another, and they are both affirming the necessity of something absolute in the construction of their theories of physics. For both, these absolutes are essential for the possibility of using mathematics in a such theories. On the other hand, their references to Descartes are different, and this "something absolute" required is not of the same kind.

In his preface to the first edition of the Principia, Newton claims that geometry can be used in rational mechanics because geometry itself "is founded in mechanical practice". However, in a manuscript of 1668, the De gravitatione, he criticizes the Cartesian physical system. In this text, we find the source of the Newtonian absolutes, the absolute space and the absolute time. These absolutes makes it possible for Newton to speak about motion and, in some respect that we propose to discuss in detail, they are a condition for the mathematical approach in the Newtonian system.

Leibniz seems – according to the manuscript De arcanis motus of 1676 – to be inspired by the Archimedean conception of equilibrium and by the mathematical notion of equality. He transfers this conception to the domain of motion, and concludes that something must be conserved through physical processes. Embracing at first the Cartesian law of the conservation of force, he adopt in 1678 (cf. De concorum concursu) the conservation of living force (vis viva). This concept is by Leibniz regarded as

something absolute and is soon given an ontological connotation as connected to his theory of substantial forms.

Newton and Leibniz thus apprehend the problem of the possibility of using mathematics in physics differently, representing somehow two distinct approaches of how their epoch deal with this problem whose actuality goes beyond the sole 17th century. The comparison we propose might then be of interest for more than 17th century specialists.

83. Snyder, Laura (St. John's University, USA)

Whewell and the Scientists: Science and Philosophy of Science in 19th century Britain

What is the relation between science and philosophy of science? Does it matter whether a philosopher of science knows much about science or is actually engaged in scientific research? One of the most interesting people to consider in relation to these questions is William Whewell. Whewell was both a scientist and a philosopher of science. Moreover, he claimed to be inferring his philosophy of science from his extensive study of the history of actual scientific work (the full title of his major work on methodology is Philosophy of the Inductive Sciences, Founded upon their History). My recent study of his unpublished letters and notebooks indicates that the intimate relation between science and philosophy of science was always foremost in Whewell's mind. In this paper I will use an examination of the relation between science and philosophy of science in Whewell's work as a springboard to consider the more general questions.

Whewell was intimately involved in science in a number of ways. He frequently asserts his wish to contribute actively to science; thus he writes to Herschel that "when I was admitted into the Royal Society I intended, if possible, to avoid belonging to the class of absolutely inactive members, and I have since been on the look out to find among the speculations that come my way some one which ought possibly be worth presenting to it" (15 October 1823) To this end Whewell became involved in a number of scientific enterprises, including his important work in crystallography and mineralogy, his experiments with G.B. Airy to measure the density of the earth, his organization of a large-scale, government-supported scientific project, viz., the project of world-wide tidal observations (he did eventually win a Royal Society medal for this effort, thus proving to himself that he was no mere "inactive" member).

Whewell's involvement with science went beyond his own researches and experiments. As many of us know, he invented the English word "scientist." More interestingly, however, he also created terminology for many of the new sciences and new discoveries of his time. In no less than seven letters to Michael Faraday, Whewell suggests (at Faraday's request) terms, including most famously "anode" and "cathode." He also provides geological terminology to Charles Lyell and Adam Sedgewick. Another interesting aspect of his correspondence with scientists—one which has not yet been adequately noted—is that he quite frequently attempts to guide their experimental work. Faraday, Forbes, Lubbock, and others are gently pushed by Whewell to perform certain experiments, to make specific observations, and to try to connect their findings in ways interesting to Whewell.

Further insight into the question about the relation between science and philosophy of science can perhaps be gained by contrasting Whewell's strong involvement in science with the lack of such involvement in the case of his antagonist, J.S. Mill. I have argued elsewhere that Whewell's philosophy of science is more adequate than Mill's in accounting for what scientists do. I will end this paper by considering whether we may be justified in concluding that there is a causal relation between Whewell's greater scientific expertise and his more adequate approach to philosophy of science.

85. Staley, Kent W. (Arkansas State University, USA)

Mill on Scientific Method in the "System of Logic" and "On Liberty"

Commentators on John Stuart Mill's scientific method have typically focused on his System of Logic, while neglecting the essay On Liberty. When philosophers of science do discuss On Liberty, they, as did Paul Feyerabend, tend to interpret its epistemology as fundamentally at odds with that of the System of Logic. One such interpretation regards the methodology of the System of Logic as "verificationist" while describing that of On Liberty as "falsificationist". The latter claim is based on Mill's arguments for the

in the field, are revised. In the third part of my talk, I shall analyze that point systematically, thus further sharpening our insight into the epistemic significance of those generative roles of experimentation.

87. Steinvorth, Ulrich (Universität Hamburg, D)

Physics and Philosophy in the 20th Century

Physics has changed our knowledge of nature in this century perhaps no less than it did in the 17th century. The response of philosophy to the changes of the 17th century has been a series of revolutions, beginning with Descartes and Hobbes and ending with Kant and German idealism. By contrast, the response of academic philosophy to the physical revolutions of this century has been rather negligible up to now, which is one reason modern physicists sometimes complain of what they think is philosophers' disinterest or non-understanding of modern physics. On the other hand, there is much popular and theological speculation on the meaning of quantum mechanics and modern cosmology, which might be interpreted as showing that this century's physical revolutions imply philosophical revolutions. I consider how modern physicists (Feynman, Weinberg, Hawking, Roland Omnes) describe in a non-technical manner the results of quantum mechanics and modern cosmology and argue that the changes modern physics has written into our picture of nature, revolutionary though they are, are not a revolution of the kind 17th century's physics has performed. Newtonian physics has replaced a transcendent world-view by one admitting only immanent explanations. Modern physics has confirmed classical physics' program. In replacing determinism by probabilism and an infinite universe by one bound by spatial and temporal boundaries it eliminates traits in Newtonian physics that are relics of a transcendent world-view, scil. a view of a world predetermined by an omniscient and omnipotent spirit. Moreover, it spells out the consequences of temporalization or historization of nature started by Darwinian evolutionary theories. Hence, there is no reason to expect a philosophical revolution following the physical revolutions of this century.

88. Stephan, Achim (Universität Karlsruhe, D)

Logical Empiricism's Impact on the Standing of Emergentism

A look upon the perspectives emergentism has had during the 20th century reveals that its standing is more or less contrary to that of logical empiricism. While emergentism has its roots in the work of British empiricist John Stuart Mill (see Stephan 1996), its decline was triggered by the critical work of logical empiricists such as Bergmann, Hempel, Henle, v. Mises, Nagel, and Oppenheim. Still, these philosophers of science were ready to take emergentism worth a critical examination. Closer inspection convinced them that emergentists' theses had to be relativized in several respects. The result was a notion not really worth to be considered thereafter: A systemic property of some complex system may be called emergent if relatively to some partition of the system and relatively to our best known theo-ries about the members of that partition we do not understand why the system has the properties it has (see Hempel and Oppenheim 1965, 263; Nagel 1961, 369). However, a notion boiled down to 'up to now we don't know' has lost its theoretical impact. It deserves no further interest by philosophers of science. Thus, it is no miracle that emergentism had lost its attraction then. However, as Kim remarks, with the decline of positivism and the demise of 'unified science' emergentism has been showing strong signs of revival (Kim, forthcoming). Particularly in the philosophy of mind and the philosophy of science more ambitious notions of emergence (e.g., an absolute notion that is associated with the notion of irreducibility) are back in the game.

In this paper, I want to discuss the relationship between the mutual rise and fall of emergentism and logical positivism respectively. Particularly, I am interested in the question whether or not logical empincists were forced to draw their relativistic conclusions by their overall doctrine, and what of their critique is still worth to be considered even if we give up some of their claims (e.g., verifica-tionism). At the outset, however, I'll start with a short outline of what was considered to be essential for emergentism by its exponents Alexander, Lloyd Morgan, and Broad.

89. Sterrett, Susan G. (University of Pittsburgh, USA)

Physical Pictures: Models in Engineering Practica circa 1914 and in the Tractatus

In 1914, Wittgenstein recorded an incident in his Notebooks that he later mentioned to several friends as occasioning a major insight for his views in the Tractatus that propositions represent by being pictures. The entry reads: "In the proposition a world is as it were put together experimentally. (As when in the law-court in Paris a motor-car accident is represented by means of dolls, etc.)" This incident, he said, was pivotal in coming to the view in the Tractatus that propositions represent by being pictures. In his later writings as well, investigations of what it is to understand a proposition remain tied to investigations of what it is to understand a picture.

Numerous scholars have looked to Hertz' Prinicples of Mechanics as the element of Wittgenstein's milieu from which he drew the notions of model and picture used in the Tractatus; that they have done so may be due to a brief parenthetical remark in a much later section of the Tractatus. However, I think that a far more relevant source of a notion of model in Wittgenstein's milieu was the engineering scale model. The methodology of scale modelling is strikingly different from analytical methods, in just those ways that are important to the notion of picturing found in the Tractatus: the primary notion is that of translatability between two physical situations, rather than between a physical situation and a mathematical or linguistic representation, or, even, between two physically similar situations whose similarity is established by showing that they are both instantiations of the same more general equation or general description. The notion fits well with the remark: "The essential nature of the propositional sign becomes very clear when we think of it as made up of spatial objects (such as tables, chairs, books) instead of written signs (3.1.4.3.1)." It's also significant that the methodology of scale modelling can be used when one has no theory by which the behavior of the model can be predicted, or, even, a theory of the phenomenon being investigated.

Since wind tunnels were already in use in Germany when Wittgenstein did his engineering studies there, the concept of scale model would actually have been in his milieu much earlier than the pivotal 1914 notebook entry. However, the methodology of scale modelling was then more a matter of engineering practice than it was a formal methodology. At the time Wittgenstein recorded the insight about a world being "put together experimentally", the field was at a threshold as far as the formalization of its methodology; logical and mathematical foundations for the practice were just then beginning to be developed. It was in 1914 that Buckingham's proof about the minimum number of dimensionless groups needed to identify physically similar situations was presented in London. I'll also explain why I think the use of a scale model in the context of a courtroom, rather than a laboratory, lent significance to the incident.

90. Stump, David (University of San Francisco, USA)

From Scientific Philosophy to the Philosophy of Science

Members of the Vienna Circle played a pivotal role in defining the work that came to be known as the philosophy of science, yet the Vienna Circle itself is now known to have had much broader concerns and to have been more rooted in philosophical tradition (especially neo-Kantianism) than was once thought. Like current and past philosophers of science, members of the Vienna Circle took science as the object of philosophical reflection (whether to provide a foundation for the sciences or simply to clarify scientific terminology and assumptions.) but they also endeavored to render philosophy in general compatible with contemporary science and to define and promote a scientific world view. This latter task seems to continue the work of various philosophers not generally considered philosophers of science who nevertheless developed scientific philosophy and strove to keep philosophy compatible with science in the late nineteenth and early twentieth century, such as the neo-Kantians, Husserl, Carus, Royce, and Russell during the period when he was applying modern logic to philosophical problems. Russell's program influenced Carnap very directly, though the idea of applying modern logic to philosophical problems became a defining feature of analytic philosophy and was applied to many areas of philosophy, not only to the philosophy of science.

While the more narrow concern for science as an object of philosophical reflection defines the philosophy of science as it is practiced today, many earlier philosophers engaged in philosophical

reflection on science as well, almost from the time that science broke away from philosophy, the major difference being that contemporary philosophers of science specialize in one area of the sciences, such as physics or biology, or in scientific method, while for earlier philosophers, Mill and Kant, for example, reflections on science would have been a part of a much more sweeping set of philosophical views. Since specialization is part of a general trend in academia, it alone cannot be used to define a difference between contemporary and earlier philosophers who are concerned with science.

By exploring the various meanings ascribed to scientific philosophy in the late nineteenth and early twentieth centuries, I will investigate whether the promotion of scientific philosophy is merely part of a transitory social context within which Logical Positivism developed or if it is an enduring part of the philosophy of science.

91. Westerhoff, Jan C. (Trinity College, Cambridge, UK)

Frege, Gödel and Leibniz's Dream

Leibniz's project of a universal characteristic appears to the modern mind to be the paragon of an unrealisable philosopher's dream. It was supposed to provide a universal philosophical language, in which the conceptual constitution of the subject-matter talked about would be made entirely perspicuous by the system of notation alone. By the help of this characteristic, philosophers would be able to settle disputes as mathematicians did all the time, namely by following the Leibnizian 'calculemus', that is, by rigorous proofs in a formal system. It is largely unknown that this project, often criticised and even more often ridiculed as it is, constitutes an important ancestor and guiding ideal for a substantial part of modern analytic philosophy, in particular that represented in the philosophical writings of Frege and Godel. It is often thought that the aim of Frege's logical works was something very close to present-day modern logic, rather far removed from the intensional ideas contained in Leibniz's demands for a formal conceptual analysis. But in fact, Frege considered his works as a 'partial approximation' ('eine schrittweise Annaherung') of the universal characteristic. The system of formal logic he created was considered by him as that part of Leibniz's project which Leibniz himself called the 'calculus ratiocinator'. Nevertheless, as Frege stresses several times, he wanted to create a system which was able to express a conceptual content, something quite different from Boole's system, and very much on the lines of what Leibniz had in mind. As he tells us in his essay 'Über den Zweck der Begriffschrift': 'Ich habe nicht eine abstracte Logik in Formeln darstellen wollen, sondern einen Inhalt (...) in genauerer Weise zum Ausdruck bringen, als es durch Worte moglich ist. Ich wollte (...) nicht einen blossen 'calculus ratiocinator' sondern eine 'lingua characterica' im leibnizschen Sinne schaffen (...). Today it is often argued that the Godelian incompleteness results showed that Leibniz's project was necessarily unattainable. Apart from the fact that none of these arguments really work, Godel himself wasn't of this opinion at all. His own philosophical, theological, religious and scientific outlook was thoroughly Leibnizian. Hao Wang, a fellow logician and friend of Gödel's even argued that 'all of his major results and project can be viewed as developments of Leibniz's conception along several directions'. In any case, he did definitely not regard the idea of a universal characteristic as a utopian project. He himself worked on the possibility of finding a partial decision procedure for mathematics which would be sufficient for getting Leibniz's system off the ground. The above observations bring to light a quite fascinating intellectual thread in the history of modern logic and analytic philosophy which is often overlooked. We can regard Leibniz as the first ancestor of an ideal of exact philosophy which was disregarded for a long time after his death, resurrected by Frege and Gödel, and which is still present in the modern developments of 'formal ethics', 'formal ontology' and 'exact metaphysics'.

92. Woleński, Jan (University of Cracow, PL)

Tarskian Truth and Philosophy of Truth

Tarski claimed that his truth definition belonged to Aristotelian tradition. In fact, one can argue that several traditional philosophical problems about truth are well displayed by the semantic definition of truth. It is absolutistic modulo the relativization to models. It distinguishes truth and its criteria. It seems to be essentially rooted in classical logic (there is a problem whether non-classical logics admit model-theoretic truth definition). That Tarskian truth for rich languages is not definable inside such languages

reminds the medieval thesis that so called transcendentals go beyond all kinds. However, recently Tarski's undefinability theorem is challenged by Jaakko Hintikka with connection of IF logic. This claim requires a serious discussion. My first hypothesis is that the success of embedding of truth-predicate in IF logic is due to two facts: (a) it has no classical negation, (b) it is only apparently first-order because the interplay of quantifiers is equivalent to quantification over domains, that is, second-order quantification. Thus, every details about the expressive power of IF must be carefully checked.

93. Yuann, J.-J. (Thunghai University, Taiwan)

Between Foundation and Convention: Neurath's Influences on Carnap

Traditionally, the foundationalist image of logical positivism has been considered a matter of course. It is basically accepted that R. Carnap has a great deal to do with the emergence of this traditional image. Recent researches reveal that this image is not entirely true; Carnap can be deemed as a conventionalist also when some key conceptions are understood from a different point of view. Indeed, by examining some Carnap's works, we are urged to realize that his view of science is somehow situated somewhere between foundationalism and conventionalism. We hold the view that this recently rediscovered fact would not be made plain unless the influences of O. Neurath on Carnap are taken into account. It is well known that Neurath influenced Carnap on both the philosophical and political grounds. However, we believe that although Neurath's conventionalism is crucial to a tremendous extent, Carnap does not cease to hold a position between foundationalism and conventionalism. In order to clarify this position, what we intend to stress particularly in this paper are the following two theses: 1) the conventionalist stand of Carnap is a direct proof of Neurath's influences on him, whereas the foundationalist stand is an outcome of Carnap's personal commitment; 2) the reason that the apparent incompatibility between foundationalism and conventionalism does not really prevent Carnap from pursuing his philosophical objectives is, in the first place, methodological rather than anything else.

94. Zach, Richard (University of California, Berkeley, USA)

The Syntax-Semantics Distinction and Hilbert's "no ignorabimus"

One of the major advances in the foundations of mathematics of this century was the formulation of a formal framework that provides for the possibility of drawing a distinction between syntax and semantics, and between object and metalanguage. The syntax-semantics distinction was a necessary precondition for many important results, among them the completeness and incompleteness theorems, and Tarski's work on truth, as well as much of logical positivist philosophy of science.

The history of the syntax-semantics distinction is complicated and obscure. In a recent paper (Bulletin of Symbolic Logic 5 (1999) pp. 331-366) I tried to draw attention to one aspect of this history, namely the axiomatic investigations of propositional logic in Hilbert's school in the 1920s. I argued there that one of the first explicit distinction between calculus and semantics, and the first metatheoretical result using this distinction (the completeness theorem for propositional logic) was obtained by Hilbert and Paul Bernays in 1918.

Several questions have been left open in that paper. Foremost among them is the question of the connection between Hilbert's geometric completeness axiom of 1902 and the completeness result of 1918; as well as the connection between completeness and decidability in the work of the Goettingen logicians. I will pursue these questions, arguing that the conceptual development leading to the metalogical investigations of the late 1920s was an outcome of a project which took Hilbert's proclamation of "no ignorabimus" as its starting point and the decidability of mathematics as its goal. In doing so, I will not only look at the work of Bernays and Hilbert on logic, but also the work of Heinrich Behmann on the decision problem. A major influence on Hilbert can be found in two lectures by Husserl in Goettingen in 1901.

95. Zemplén, Gábor (Technical University of Budapest, H)

Forerunner or Latecomer – Goethe's Ideas on the Nature, Aim, and Limit of Scientific Investigation

The paper attempts to summarize and evaluate Goethe's sporadic utterances on methodology and philosophy of science. It will concentrate on Goethe's reasons for rejecting Newton's approach to treat colour phenomena, his criticism of the experimentum crucis, Baconian induction, and the use of mathematical (or other) language in science.

Though in recent years much light has been shed on Goethe's controversial polemics against Newton, most commentaries are still strongly biased. While fifty years ago Sherrington likened Goethe to a medieval, Aristotelian scientist (and many shared his opinion), numerous writers now see him as a forerunner of alternative science (Henri Bortoft, Arthur Zajonc, Theda Rehbock, Klaus Michael Meyer-Abich, etc.). K. J. Fink likened his ideas to some of T. Kuhn's (for which Stephenson - to my belief rightly - criticized him), Nisbet claimed that Goethe preceded Popper in rejecting "the Baconian myth that we must begin with observations in order to derive our theories from them", etc.

The main aim is to investigate these claims and try to show to what extent they are valid. The overall aim is to understand Goethe's 'philosophy of science', while escaping both a pro-Goethean (and anti-Newtonian) and an anti-Goethean (i.e. pro-Newtonian) attitude.

epistemic value of free discussion, which partly derives from the potential for criticism of false opinions, which might otherwise flourish unexamined. Even Struan Jacobs, who argues for the overall consistency of the two works, salvages consistency only by excluding scientific subjects from the purview of the arguments in On Liberty.

I propose a more unified reading of these two works. To see how the two works complement one another one must attend to distinctions often ignored in discussions of Mill's methodology. In the System of Logic, Mill distinguishes two aspects of logic. He describes logic as the "science of the operations of the understanding". Yet he also seeks to establish, on the basis of that science, "rules of art", i.e., practical rules for achieving certain aims. He discusses rules of art most thoroughly in the last section of the System of Logic, which concerns social policies. But I argue that the general points made in that discussion can and should be applied to Mill's scientific methods. In so doing, I explain how the underlying principles of Mill's science of logic should be distinguished from those methods constituting the practical rules of his art of logic.

Mill advises strongly against blindly implementing rules of art. In applying such rules to particular situations, there are a number of ways in which such rules can fail to apply. The prudent practitioner of any art, therefore, will always be mindful of the principles of science that underwrite any particular rule of art. I argue, first, that such precautions apply no less to the rules of art embodied in Mill's methods of inductive reasoning than to rules of social policy. Second, I argue that the particular ways in which such rules can fail to be applicable require that we take scientific opinions to lie within the scope of the epistemic arguments of On Liberty. The critical assessment encouraged by the free expression of opinions can serve to reveal that an opinion based on an inductive argument is unwarranted due to the failure of one or more of the preconditions necessary for the valid application of the methodological rule underwriting that argument. It is worth noting that the resulting positive conception of scientific method can easily be maintained while accepting the "against method" arguments that Feyerabend found in On Liberty.

86. Steinle, Friedrich (Max-Planck-Institut für Wissenschaftsgeschichte Berlin, D)

How Exploratory Experimentation Disappears: Scientific Research Practice, Self-representation and the Generation of Language

For many decades, philosophy of science has regarded experimentation as just a special case of observation. Moreover, the predominant ‚standard view' told that the only epistemic function of observation and experiment was to form a testing instance of hypotheses and theories. An essential background for that view was given by the distinction between the contexts of discovery and justification. In denying any epistemic significance to the first of those contexts, philosophers dismissed any significant role of experiments in phases in which theories were not yet formulated.

In stark contrast to those views, recent historical and philosophical research into research practice has shown that experimentation may have decisive functions in processes of generating and stabilizing scientific concepts and theories. Far from being epistemologically unimportant, those functions point to a very high significance of experimentation on a fundamental epistemic level. In the paper, I will briefly outline those developments.

I shall discuss, furthermore, some factors which have made philosophers to overlook those generative aspects of experimentation for so long a time. After all, those who introduced the distinction between the contexts of discovery and justification did not discuss in isolation, but took a close look to science itself. One main reason why the importance and significance of exploratory experimentation escaped their attention is that it typically did (and does) not show up in the public statements of scientists. Published scientific papers do often not mention exploratory phases, even if those phases may have taken a major part of the work. In order to illustrate that point, I shall sketch two examples from 19th century electromagnetism which show very typical differences between the way in which the actors actually did their research the way in which they present it to the public.

The background of those differences can only partly be found in the particular historical circumstances. To a large extent it has to do with features of exploratory experimentation itself, taking typically place in situations in which fundamental concepts or, in other words, the very language used

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DM 29,80 / ATS 218,- / CHF 27,50

Verschüttet scheinen die Zusammenhänge zwischen den Naturwissenschaften sowie den exakten Wissenschaften auf der einen und der Poesie auf der anderen Seite. Die in diesem Band vereinigten Beiträge suchen nach Gemeinsamkeiten auf beiden Seiten und nähern sich der Fragestellung von verschiedenen Positionen.
Enthalten sind folgende Beiträge: Wendelin Schmidt-Dengler: Literatur und Philosophie in Wien in der ersten Jahrhunderthälfte / Karl Sigmund: Musil, Perutz, Broch - Mathematik und die Wiener Literaten / Herta Blaukopf: Stifters literarischer Protokollsatz - Ein Mittel zur Darstellung der „wirklichen Wahrheit" / Carl Djerassi: Science in Fiction ist nicht Science Fiction - Ist sie Autobiographie? / Peter Malina: „Ein anderer Zirkus" - Science in Fiction.
Die Welt der Wissenschaft in der Literatur / Lutz Danneberg und Hans-Harald Müller: Brecht und der Logische Empirismus.

Band 4

Hrsg. Volker Thurm-Nemeth
Konstruktion zwischen Werkbund und Bauhaus
Wissenschaft - Architektur - Wiener Kreis

Wien 1998, 244 Seiten, 80 Abb., kartoniert
ISBN 3-209-02587-8
DM 53,- / ATS 387,- / CHF 48,-

Die zentrale Anstrengung der Autoren liegt in der Darstellung der „inneren Verwandtschaft der Haltung" von bildenden Künstlern und Architekten der Moderne zu Vertretern des Logischen Empirismus. Untergegangene und unbeachtete Beziehungsstrukturen werden aufgespürt und neu bewertet.
Inhalt: Volker Thurm-Nemeth: Die Konstruktion des modernen Lebens - Ein Fragment (Wiener Kreis und Architektur) / Margarethe Engelhardt-Krajanek: Der Werkbundgedanke und seine Verbindung zum Wiener Kreis am Beispiel von Josef Frank / Sabine Plakolm-Forsthuber: Josef Frank an Trude Waehner (1938-1965) - Das Nachleben des Werkbundes in der Kritik am Bauhaus / Ulrich Winko: Von der Kunst zur Wissenschaft - Avantgardistische Kunst- und Architekturtheorie im Kontext der wissenschaftlichen Weltauffassung / Dokumentation: Josef Frank - Schriftenverzeichnis / Parallelchronologie Kunst und Architektur - Wissenschaftliche Weltauffassung.

Band 5, In Vorbereitung! Erscheint im Herbst 2000

Hrsg. Friedrich Stadler und Martin Seiler
(Mitherausgeber: Universität für Musik und darstellende Kunst in Wien, Abteilung Musikpädagogik)
Kunst, Kunsttheorie und Kunstforschung im wissenschaftlichen Diskurs
In memoriam Kurt Blaukopf

Wien 2000, ca. 336 Seiten, Hardcover
ISBN 3-209-03147-9
ca. DM 78,- / ATS 570,- / CHF 73,-

Der Band „Kunst, Kunsttheorie und Kunstforschung im wissenschaftlichen Diskurs" ist dem einzigartigen Leben und Werk des österreichischen Intellektuellen Kurt Blaukopf gewidmet und versammelt Originalbeiträge von seiner langjährigen Mitarbeiterin und Frau, sowie von SchülerInnen und Freunden, die thematisch auf den von Blaukopf viele Jahre im Institut Wiener Kreis geleiteten Forschungsschwerpunkt „Wissenschaftliche Weltauffassung und Kunst" Bezug nehmen.
Ein Auszug aus der originellen Korrespondenz Blaukopfs rundet das forschungsorientierte Buch ab, das zugleich eine kritische Bestandsaufnahme des fachenübergreifenden Projektes über die Beziehungen und Wechselwirkungen zwischen Musik, Literatur, Kunst, Architektur und wissenschaftsorientierte Philosophie darstellt.

Veröffentlichungen **des Instituts** Wiener Kreis

Herausgegeben von Friedrich Stadler

Band 1

Hrsg. Paul Kruntorad
unter Mitwirkung von Rudolf Haller und Willy Hochkeppel
Jour Fixe der Vernunft
Der Wiener Kreis und die Folgen

Wien 1991, 294 Seiten, Broschur
ISBN 3-209-01221-0
DM 60,- / öS 437,- / sFr 54,50

Jeden Donnerstag-Abend traf sich ein privater Diskussionszirkel im mathematischen Institut der Universität Wien und schuf dabei neue Standards, an denen sich jede künftige Philosophie, die als rationale auftreten will, orientieren muß. Der Band gibt einen Einblick in die Arbeitsweise und Wirkung einer der bedeutendsten philosophischen Gruppierungen dieses Jahrhunderts.

Band 2

Hrsg. Rudolf Haller und Friedrich Stadler
Wien - Berlin - Prag
Der Aufstieg der wissenschaftlichen Philosophie
Zentenarien Rudolf Carnap - Hans Reichenbach - Edgar Zilsel

Wien 1993, 710 Seiten, Leinen mit Schutzumschlag
ISBN 3-209-01598-8
DM 178,- / öS 1298,- / sFr 158,-

Erstmals beleuchten Beiträge einer internationalen Autorenschaft den Hintergrund einer epochemachenden Theoriendynamik und den Wissenschaftstransfer Europa - Amerika, bedingt durch die Vertreibung des Logischen Empirismus. Der Band bietet die Sammlung der Vorträge eines Symposiums, das 1991 in Wien stattgefunden hat.

Band 3

Hrsg. Rainer Hegselmann / Heinz-Otto Peitgen
Modelle sozialer Dynamiken / Models of social dynamics
Ordnung, Chaos und Komplexität / Order, Chaos, and Complexity

Wien 1996, 245 Seiten, Leinen mit Schutzumschlag
ISBN 3-209-01889-8
DM 120,- / öS 875,- / sFr 106,50

Inhalt / Contents:
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Die Reihe „Veröffentlichungen des Institut Wiener Kreis" wird im Springer Verlag (Wien) fortgesetzt.

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