

WORKSHOPS

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WORKSHOP "IMPLICIT DEFINITIONS AND A PRIORI KNOWLEDGE"

Organized by Thomas Grundmann, Nikola Kompa, Christian Nitz
 Room GAP-I

DONNERSTAG THURSDAY 14.9.	15:00-15:15	Introduction	
	15:15-16:45	Timothy Williamson Frank Hoffmann	Understanding and Analyticity prepared commentary
	16:45-17:15	coffee break	
	17:15-18:45	David Chalmers Christian Nitz	Conditional Conceptual Analysis meets 'Two dogmas of Empiricism' prepared commentary
FREITAG FRIDAY 15.9.	10:00-11:30	Jason Stanley Nikola Kompa	Knowledge and Certainty prepared commentary
	11:30-11:45	coffee break	
	11:45-13:15	Crispin Wright Niko Strobach	Abstraction Principles as Implicit Definitions prepared commentary
	13:15-15:00	lunch	
	15:00-16:30	Paul Horwich Andreas Kemmerling	On the Alleged Normative Import of Implicit Definition prepared commentary
	16:30-17:00	coffee break	
	17:00-18:30	Stathis Psillos Andreas Berg-Hildebrand	The A Priori: Between Conventions and Implicit Definitions prepared commentary

I Chalmers

Conditional Conceptual Analysis meets 'Two dogmas of Empiricism'

David Chalmers

Australian National University (Australia)

chalmers@anu.edu.au

In "Two Dogmas of Empiricism", Quine argues against an analytic-synthetic distinction (and in effect against an a priori/ a posteriori distinction) in part by arguing that any statement is revisable, and that one can hold true any statement "come what may" given sufficient adjustments elsewhere in one's web of belief. In response, defenders of these distinctions have held that that changes of this sort will often involve change in meaning. The Quinean challenge is then to draw a principled distinction between those cases that involve change in meaning and changes that do not. I discuss a principled way of drawing this distinction, using tools drawn from conditional conceptual analysis and from Bayesian confirmation theory

I Horwich

On the Alleged Normative Import of Implicit Definition

Paul Horwich

New York University (USA)

phorwich@gc.cuny.edu

Our beliefs are the products of certain general epistemic propensities, including (roughly speaking) our approximate conformity with classical logic, our commitment to certain basic principles of arithmetic, and our tendency, in the case of certain properties (e.g. colors), to think that objects have them when they seem to have them. My paper is a critique of the thesis that such general propensities – or at least some of them – are justified in virtue of being concept constituting. Equivalently, my target is that the practices of sentence-acceptance that are correlated with those propensities are justified in virtue of being implicit definitions.

To that end I will criticize the two components of this thesis:– first, that if a given acceptance practice with a word constitutes its meaning then the practice must be rational; and, second, that certain vital epistemic practices, like the three just mentioned, are meaning-constituting (i.e. are implicit definitions).

Against the first component, I will offer objections to each of the three ways in which it might be thought to hold. One popular thought about it has been (i) that we (implicitly) stipulate, regarding certain terms, that they are to have the meanings (if any) that will make their governing acceptance-practices truth-theoretically correct, and (ii) that our awareness of these stipulations provides us with the premises for sound, justifying ar-

guments in support of the conclusion that the practices in question are correct. Alternatively, there's the idea that facts of meaning-constitution help to make up the underlying explanatory ground of the phenomena of epistemic normativity. And a final proposal I'll be objecting to – I guess it's the most attractive of them – is that it is that the truly basic epistemic norm (or one of them) is, "Meaning-constituting practices are rational", and that this ur-norm enables the derivation and unification of (some of) our familiar epistemic norms.

Having thus attempted to undermine one leg of the semantogenetic theory of epistemic normativity, I will then turn to the other leg. I will argue that none of the general epistemic procedures whose justification we are concerned to explain are implicit definitions (– for someone could reject one of those procedures without failing to understand the words that it governs). So even if my earlier objections were mistaken – even if those practices that really are meaning-constituting must in fact be rational – that wouldn't illuminate the norms of most interest to us.

I will conclude by suggesting that we should not infer from these negative results that some other account of our epistemic normativity must surely be correct – e.g. an account in terms of reliability, or rational intuition, or the liberty we have to decide on the rules of our language games. For it's not implausible that the fundamental fact here is that a certain long and diverse list of acceptance-propensities are rational. In other words, it would be unsurprising if the correctness of our familiar epistemic norms simply cannot be explained.

Psillos I

The A Priori: Between Conventions and Implicit Definitions

Stathos Psillos

University of Athens (Greece)

psillos@phs.uoa.gr

This paper aims to articulate in a relatively precise way the core views of a philosophical tradition that has tried to defend some notion of relativised synthetic apriority against both the Kantian absolute conception of it and the empiricist denial of it. It focuses its attention on the claim that the a priori marks a mode of being entitled to rationally and justifiedly hold a belief independently of experience. It separates the Kantian criteria of necessity and universality and argues that a theory of conventions along the lines of Poincaré holds the key to understanding the sense in which some principles are independent of experience (and constitutive of it), though they might well have some *fundamentum in re*, as Arthur Pap put it. What emerges is a notion of constitutive (a priori) principles which are strictly (unconditionally) universal and relatively necessary. The natural way to develop this theory of constitutive-conventional a priori is by means of an account of implicit definitions and their role in imposing a priori restrictions on what the world is like and in creating the meaning of certain concepts (Sellars;

Pap). The paper moves on to a discussion of Carnap's way to formulate implicit definitions (and to mark the space for the a priori within a theory) by means of the so-called Carnap-sentences (which say that if the Ramsey-sentence of a theory is true, then the theory is true). Though ingenious, this way faces an unexpected difficulty which comes from the fact that without some restrictions being imposed on the domain of discourse of a Ramsey-sentence, the claim that a theory is true has no substantive excess content over the claim that its Ramsey-sentence is empirically adequate. The paper concludes with some discussion of the a priori presuppositions that need to be in place for non-trivial knowledge of the world (as expressed by Ramsey-sentences) to be possible.

I Stanley

Knowledge and Action

Jason Stanley
Rutgers University (USA)
jasoncs@rucss.rutgers.edu

A number of philosophers have argued that knowing that p entails being certain that p . I argue, by a reconsideration of the grounds for this conclusion, that knowing that p is perfectly consistent with being uncertain that p . I conclude by considering some of the consequences of the result for a successful account of practical rationality.

I Williamson

Understanding and Analyticity

Timothy Williamson
Oxford University (UK)
timothy.williamson@philosophy.oxford.ac.uk

The talk will argue that no non-metalinguistic sentences are analytic in the epistemological sense that one must assent (or be disposed to assent) to them as a necessary condition of understanding them. Likewise at the level of thought: there are no conceptual truths in the corresponding sense. The best candidates for such sentences are synonymous with simple logical truths; for example, 'Vixens are female foxes' is synonymous with 'Vixens are vixens'. However, proponents of deviant logics can understand such logical truths without assenting to them. Contrary to a popular view, this does not require assigning non-standard meanings to some of their constituent expressions. Nor is there good evidence for a logic module that might embody logical orthodoxy at a sub-personal level in those who are logically heterodox at the personal, conscious level. For similar reasons, the original candidates for epistemological analyticity fail. The argument has affinities with the epistemological ho-

lism of Quine's 'Two Dogmas of Empiricism', but makes no concession to behaviourism, verificationism or scepticism about meaning. Contrary to the claim by Frank Jackson and others that shared meanings require a basis of agreement, participation in a common practice is consistent with unorthodoxy at any given point, if adjustments are made elsewhere to permit fluent social interaction. Even if any two participants in the practice agree on much, it does not follow that there is much or indeed anything on which all participants agree. As for synonymy or conceptual overlap between non-interacting practices, it consists in identity of truth-conditional semantic features (such as Kaplan character), which can themselves be determined at a social level.

Wright I

Abstraction Principles as Implicit Definitions

Crispin Wright
University of St. Andrews (UK)
cjqg@st-andrews.ac.uk

WORKSHOP "MODELS OF PREFERENCE CHANGE"

Organized by Till Grüne-Yanoff, Sven Ove Hansson
 Room GAP-M

DONNERSTAG THURSDAY 14.9.	14:15-14:30	Welcome	
	14:30-15:30	Richard Bradley	Representing Preference Change
	15:30-16:30	George Loewenstein	The Role of Affect and Deliberation in Preference Change
	16:30-17:00	coffee break	
	17:00-18:00	Till Grüne-Yanoff & Sven Ove Hansson	An Input-assimilating Model of Preference Change
	18:00-19:00	Wlodek Rabinowicz	Preference Revision and Utilitarianism

FREITAG FRIDAY 15.9.	9:15-10:15	Edward McClennen	Strategies for Dealing with Preference Changes
	10:15-11:15	Wolfgang Spohn	Dynamic Choice Problems Require Second-order Evaluations
	11.15-11.45	coffee break	
	11:45-12:45	Hans Rott	Belief Change as Preference Change: Bounded Revision
	12:45-14:00	lunch break	
	14:00-15:00	Werner Güth	Population-dependent Costs of Detecting Trustworthiness – An Indirect Evolutionary Analysis
	15:00-16:00	Rainer Hegesemann	From Opinion Dynamics to Preference Dynamics
	16.00-16.30	coffee break	
	16:30-17:30	Klaus Mainzer	Preferences, Computer, and Cognition. Decisions in a Complex World
17:30-18:30	Robert Sugden	Opportunity and Responsibility: Why Preference Change Need not Be a Problem for Normative Economics	

M Bradley

Representing Preference Change

Richard Bradley
London School of Economics (UK)
R.Bradley@lse.ac.uk

Gary Becker famously characterised the economic method as "... the combined assumptions of maximising behaviour, market equilibrium and stable preferences, used relentlessly and unflinchingly". But Becker's commitment is not to the (false) empirical claim that *all* preferences are stable, but rather to the methodological doctrine that all changes in agents' revealed preferences can be represented as consequences of fixed tastes interacting with variable information. In this paper, I examine versions of this claim, formulated within an idealised model of rational agency. I give sufficient conditions for its truth in terms of the standard characteristics of Bayesian conditioning, but argue that these conditions are not universal. Finally I consider the prospects for a theory of preference revision freed from the Becker doctrine.

M Grüne-Yanoff / Hansson

An Input-assimilating Model of Preference Change

Till Grüne-Yanoff / Sven Ove Hansson
Royal Institute of Technology; Stockholm (Sweden)
gryne@infra.kth.se
soh@infra.kth.se

We propose to model preference change as the change of an agent's preference state in response to the agent accepting a preference affect. The preference state of an agent is ruled by various inferential commitments. Accepting a preference affect will likely bring the preference state into inconsistency. The model shows how the preference state needs to be adjusted to restore consistency. In particular, it shows which path restoration will take, conditional on the previous preference state and the available dynamic information, and it determines how the ensuing preference state will look like.

M Güth

Population-dependent Costs of Detecting Trustworthiness – An Indirect Evolutionary Analysis

Werner Güth
Max Planck Institute of Economics, Jena
gueth@econ.mpg.de

If the (un)trustworthy are rare, people will talk about them, making their detection more reliable and/or less costly. When, however, both types appear in large numbers, detecting (un)trustworthiness will be considerably more difficult and possibly too costly. Based on Güth and Kliemt (2000), we analyze how the composition of a population of trustworthy, resp. untrustworthy individuals evolves if the cost and reliability of type detection depend on the population composition.

Hegselmann M

From Opinion Dynamics to Preference Dynamics

Rainer Hegselmann
Universität Bayreuth
Rainer.Hegselmann@uni-bayreuth.de

In the last decade theoretical research on the dynamics of opinions has dramatically intensified. It was modelled and studied how individuals influence each other by interaction and opinion exchange. Important questions are: When does opinion formation within an interacting group lead to consensus, polarization or fragmentation? As to the research methodology and heuristics used in that research the keywords are: Agent based modelling, dynamical systems, KISS-principle, simulations. Probably methods and results of the research on opinion dynamics can be used to model and understand a bit better endogenous preference change under certain types of social interaction, especially group discussion. The talk will present approaches and results from the opinion dynamics research and then discuss problems of transferability.

Loewenstein M

The Role of Affect and Deliberation in Preference Change

George Loewenstein
Carnegie Mellon University
gl20@andrew.cmu.edu

There is an emerging consensus among psychologists and economists that human behavior is the product of two qualitatively different neural systems: a deliberative system that is very flexible, but slow and severely capacity constrained, and an affective system that is more rapid but reflexive and rigidly programmed. This introduces the possibility that preference change can occur at different levels, that preferences can change in one system and not the other (creating conflict between them) and that the deliberative system, which is reflective, can possess more or less insight into the nature of preference changes associated with the affective system. I will discuss empirical research into these issues

and implications for understanding and formally modeling preference change.

Rabinowicz **M**

M **Mainzer**

Preferences, Computer, and Cognition. Decisions in a Complex World

Klaus Mainzer
Universität Augsburg

Preferences and their representation in formal systems have a long tradition in logic and philosophy. Automated reasoning about preferences with ontologies and categories had been discussed in philosophy, before they were formalized in artificial intelligence and applied in databases. But, in a complex world with nonlinear dynamics, there are no universal representations of preferences. Preferences are personalized, situated, context-dependent, and dynamic. We must act and decide with incomplete and fuzzy knowledge under the conditions of bounded rationality. Even in commercial affairs, motivations, emotions, and embodied interactions play an important role in our decision making. Thus, cognitive science and brain research come in. The computational theory of preferences and decisions must be supported and supplemented by cognitive studies, in order to develop human-oriented information systems. Interdisciplinary collaboration is a challenge in a complex information world.

M **McClennen**

Strategies for Dealing with Preference Changes

Edward F. McClennen
Syracuse University
efmcclen@syr.edu

In recent decades there have been numerous proposals regarding how to effectively deal with situations in which the agent anticipates that the plan they now prefer to act upon may be one that they will subsequently no longer prefer to execute. I want to examine the relative merits of these proposals, starting with Strotz's theory of "sophisticated" choice, and moving through my own proposal for a strategy of "resolute" choice to strategies that have been explored by Wlodek Rabinowicz and George Ainslie. Throughout I shall be concerned with the normative validity of these proposals, and especially with the question of the perspective from which their effectiveness or value is to be assessed.

Preference Revision and Utilitarianism

Wlodek Rabinowicz
Lund University (Sweden)
wlodek.rabinowicz@fil.lu.se

This talk is based on a paper written jointly with Bertil Strömberg. Richard Hare's classical argument for preference utilitarianism, in terms of hypothetical role reversals, contains a serious gap: Contrary to Hare's suggestion, a rational deliberator has no need to balance her preferences for the hypothetical cases in which she occupies different roles. The reason is that these preferences are all directed to different situations and thus are mutually compatible. We suggest that the gap can be filled, if one takes the universalizability requirement to imply that the deliberator should revise her diverging preferences for the different situations, so as to end up in a uniform preference state. If that move is to be minimal and the Euclidean metric is used, then uniform preference is obtained by averaging the input preferences, in a distinctly utilitarian fashion.

Rott **M**

Belief Change as Preference Change: Bounded Revision

Hans Rott
Universität Regensburg
Hans.Rott@psk.uni-regensburg.de

In this talk, I first review the basic idea of modelling (qualitative iterated) belief change as a kind of preference change. As an application, I then present the idea of 'bounded revision', a new binary revision operator taking an input sentence and a reference sentence, similar to the model of 'revision by comparison' as introduced by Fermé and Rott (Artificial Intelligence 157, 2004). In contrast to revision by comparison, bounded revision satisfies the Darwiche-Pearl axioms. It covers the ground between 'radical revision' and 'conservative revision', including 'moderate revision', which are all unary special cases obtained by setting one argument of the bounded revision operator to certain values.

M Spohn

Dynamic Choice Problems Require Second-order Evaluations

Wolfgang Spohn

Universitaet Konstanz

wolfgang.spohn@uni-konstanz.de

There are dynamic choice problems indistinguishable in their standard description, but intuitively to be solved by different decision rules, sometimes by sophisticated choice, sometimes by resolute choice. I shall argue that the problem can only be solved by a second-order evaluation of decision situations as superior or inferior that provides a general rule of how to adequately intertwine sophisticated and resolute choice in complex dynamic decision situations.

M Sugden

Opportunity and Responsibility: Why Preference Change Need not Be a Problem for Normative Economics

Robert Sugden

University Of East Anglia (UK)

r.sugden@uea.ac.uk

Traditional normative economics treats each individual's preferences as an indicator of his/her welfare. Given this approach, preference change poses a severe problem: if an individual's preferences over outcomes change, which preference should be used in assessing welfare? One familiar answer to this question is to appeal to some supposed higher self which makes judgements (or expresses 'metapreferences') between these preferences. (For example, in discussions of self-control, there is often a judgement in favour of the ex ante preference for self-control over the ex post preference to evade control). An alternative answer is to appeal to some non-preference criterion of the person's well-being and to use this to judge which of a person's preferences best reflects his/her real interests. (Variants of this approach can be found in the current literature of 'libertarian paternalism'). In this paper, I present a critique of these answers from a perspective in which value is attributed to individuals' opportunities to choose their own paths through life and to take responsibility for the outcomes. Building on the analysis presented in my paper 'The opportunity criterion: consumer sovereignty without the assumption of coherent preferences' (*American Economic Review*, 2004), I argue that normative analysis does not require a standpoint from which a person's different preferences can be assessed relative to one another. Instead, we can value the individual's opportunity to act on whatever preference he or she has at each moment of choice, and to be consistent or inconsistent over time as he or she chooses.

WORKSHOP "PHYSICALISM AND BEYOND"

Organized by Vera Hoffmann, Achim Stephan, Sven Walter
 Room GAP-P

DONNERSTAG THURSDAY 14.9.	14.00-14.15	Introduction	
	14.15-15.45	Terence Horgan	Materialism: Matters of Definition, Defense, and Deconstruction
		Martine Nida-Rümelin	prepared commentary
	15:45-16:15	coffee break	
	16.15-17.30	Vera Hoffmann	Complex Microphysical Properties Within a Physicalist Framework
	17:30-18:00	coffee break	
	18.00-19.30	Brian McLaughlin	Does Mental Causation Require Psychophysical Identities?
		Michael Schmitz	prepared commentary

FREITAG FRIDAY 15.9.	9.00-10.30	Robert Richardson / Achim Stephan	What Physicalists Should Provide Us with ...
		Benedikt Göcke	prepared commentary
	10:30-11:00	coffee break	
	11.00-12.30	Marc Slors	The Vacuity of Physicalism
		Julie Yoo	prepared commentary
	12:30-14:15	lunch break	
	14.14-15.45	Lynne Rudder Baker	Against Reductive Physicalism: Causation Without Reduction
		Peter Schulte	prepared commentary
	15:45-16:15	coffee break	
	16.15-17.30	Sven Walter	Does Mind Matter? And Does It Matter?
	17:30-18:00	coffee break	
	18.00-19.30	Dan Hutto	From Physicalism to Idealism
		Martin Fricke	prepared commentary

SAMSTAG SATURDAY 16.9.	9.00-10.30	Uwe Meixner	The Reductio of Reductive and Non-Reductive Materialism
		Annette Dufner	prepared commentary
	10:30-11:00	coffee break	
	11.00-12.30	Torin Alter	Imagining Zombies
		Eva Schmidt	prepared commentary

Imagining Zombies

Torin Alter
 University of Alabama at Tuscaloosa (USA)
 talter@ua.edu

Zombies make trouble for physicalism. Intuitively, they seem conceivable, and many take this to support their metaphysical possibility—a result that, most agree, would falsify physicalism. I will defend the zombie argument from two objections. The first, due to Eric Marcus and others, is that, since imagining the absence of consciousness makes sense only against a background of the presence of consciousness, imagining the total absence of consciousness is impossible. I argue that this objection rests on confusions about the nature and limits of imagination. The second objection I will consider has been developed independently by John Hawthorne, David Braddon-Mitchell, and Robert Stalnaker. These philosophers argue that phenomenal concepts have a conditional structure—referring to non-physical states if such states exist and otherwise to physical states—and that this explains the intuitions driving the zombie argument. But, I will argue, the considerations Hawthorne, et al., adduce in support of their analysis in fact do no such thing. Moreover, I will argue that these considerations are self-defeating: exactly similar reasoning would undermine the view the considerations are meant to establish. Finally, I will argue that both objections I will consider underestimate what explaining away the relevant intuitions would require.

Against Reductive Physicalism: Causation without Reduction

Lynne Baker
 University of Massachusetts at Amherst (USA)
 lrbaker@philos.umass.edu

Jaegwon Kim supports reductive physicalism with an influential argument against nonreductive mental causation. My paper critically examines Kim's argument, and considers the extent to which the argument generalizes to a denial of all macro-causation. After showing that Kim's argument does generalize to an important class of nonmental intentional properties, I formulate a new nonreductive version of intentional causation (with mental causation as a special case). Then, I show how my view of causation may be used to save (a very weak) nonreductive materialism—whether my view is considered to be physicalist or not. The upshot is that reductive physicalism is false, and that nonreductive materialism is a promising approach to understanding the world as we encounter it.

Complex Microphysical Properties within the Framework of Physicalism

Vera Hoffmann
 Universität Tübingen
 vera.hoffmann@uni-tuebingen

According to the standard account of physicalism, higher-level properties instantiated by macroscopic objects, e.g. mental properties instantiated by human beings, systematically depend on microphysical properties instantiated by proper parts of these objects. Thus, each higher-level property is dependent on a complex configuration of microphysical properties. However, not each complex configuration of microphysical properties realizes a higher-level property. I argue that in order to avoid an eliminativism concerning higher-level properties, the doctrine of physicalism requires a criterion that determines which configurations of base-level properties are (possible) realizers of higher-level properties; yet, a linguistic or epistemological criterion is not sufficient to fulfill this function. The aim of my paper is hence to develop an ontological criterion which a configuration of base-level properties has to fulfill in order to constitute a higher-level property.

Materialism: Matters of Definition, Defense, and Deconstruction

Terence Horgan
 University of Arizona (USA)
 thorgan@email.arizona.edu

How should the metaphysical hypothesis of materialism be formulated? What strategies look promising for defending this hypothesis? How good are the prospects for its successful defense, especially in light of the infamous "hard problem" of phenomenal consciousness? I say something about each of these questions. In addressing the first question, I stress the importance of formulating materialism in a way that precludes what I call "Moorean emergentism"—the view that there are ontologically brute relations of metaphysical necessitation linking physics-level properties to other properties. In addressing the second question, I raise worries about the strategy of defending materialism by appeal to Ramsey-Lewis-Jackson style functional analyses of non-physics-level concepts, and I stress the potential promise of an alternative mode of defense that appeals to ontological austerity and to a construal of truth as indirect correspondence. In addressing the third question, I argue that none of the principal strategies currently on offer for defending materialism can be successfully applied to phenomenal consciousness.

P Hutto

From Physicalism to Idealism?

Dan Hutto
University of Hertfordshire (UK)
phlqddh@herts.ac.uk

Those versions of physicalism which are committed to strong explanatory agendas are implausible given the serious, and arguably intractable, problems that attend the development of a theory of consciousness. There are reasons to think that it is not possible, even in principle, to make the relation between the mental and the physical intelligible. Yet, even so, there are compelling arguments for believing in the truth of a psychophysical identity theory. The primary one being that acceptance of such a theory circumvents problems about mental causation. I take this to be the best reason for wanting to endorse some form of monism, making the most stable and defensible version of physicalism that of a purely metaphysical sort.

But monism does not entail physicalism. In light of the above argument, our best reasons for rejecting dualism are not, in their pure form, arguments for physicalism per se. Against this backdrop, I argue that we also have grounds to doubt that a privileged specification of an underlying reality is so much as possible – not even, say, in terms of an ideal physics. This, I hold, constitutes good reason to take some form of idealism seriously. I promote that of a Bradleyian variety. In arguing for it, I make heavy use of Davidson's claims that (i) causal relations are extensional and that (ii) causal explanations, even when true, are always only intelligible in relative to a particular vocabulary.

P McLaughlin

Does Mental Causation Require Psychophysical Identities?

Brian McLaughlin
Rutgers University (USA)
brianmc@rci.rutgers.edu

Jaegwon Kim has offered an argument, "the supervenience argument," to establish a bold conclusion: the nature of our world is such that mental events are causes only if they are physical events. He thus argues that every mental event is such that either it is a physical event or an epiphenomenon. Many philosophers recoil from Kim's position with revulsion. I will not attempt to determine whether his supervenience argument is successful. But I will assume two of its premises, namely that (a) the physical is causally closed and that (b) an event can have a causal effect only if it has some physical effect or other. And I will examine the leading line of response to his position. That line of response is that mental events are not physical events, but are in-

stead realized by physical events; and the fact that they are realized by physical events is perfectly compatible with their being causes, even given (a) and (b). This line of response obviously makes heavy use of the notion of realization. I consider the leading notions of realization that have been articulated in the literature (by my count there are three) and argue that none of them are available to proponents of this position. I conclude that no one has yet articulated a viable "realization" alternative to Kim's position.

Meixner **P**

The Reductio of Reductive and Non-reductive Materialism

Uwe Meixner
Universität des Saarlandes
u.meixner@mx.uni-saarland.de

In my paper I will attempt to show that the positions in the philosophy of mind that are advertised under the name of "materialism" (or "physicalism") are either glaringly implausible or turn out to collapse into dualism. A symptom of this situation is the uneasy history of materialism within the last 40 years: a history of complications and modifications, to the point that the name – "materialism" – seems much more important than the matter. At the end of the paper, I will offer some speculations about why materialism, in spite of its philosophical precariousness, is so attractive to so many.

Richardson & Stephan **P**

What Physicalists Should Provide Us with ...

Robert Richardson
University of Cincinnati (USA)
richards@uc.edu

Achim Stephan
Universität Osnabrück
Achim.Stephan@t-online.de

Physicalists should be able to reductively explain (i) phenomenal consciousness, (ii) the content of propositional attitudes, and (iii) the capacity to be responsible for what one does.

Concerning phenomenal consciousness, reductive explanations should lead to definite answers about whether or not some given system is a pc-system (that is, a system that has phenomenal consciousness), concerning propositional attitudes, reductive explanations should lead to definite answers about whether or not some given system is a pa-system (that is, a system

that has propositional attitudes), and concerning responsibility, reductive explanations should lead to definite answers about whether or not some given system is a r-system (that is, a system that is responsible for what it does). If physicalism fails to provide us with answers to these issues it leaves the most important questions concerning mentality open.

P Slors

The Vacuity of Physicalism

Marc Slors

University of Nijmegen (Netherlands)

marc.slors@phil.ru.nl

The doctrine of physicalism in the philosophy of mind requires that for every mental state there is a physical state to which the mental state stands in a relation of reduction, identity, realization or constitution. In this paper it is first argued that all versions of physicalism must (and do) presuppose that the mental and physical relata of these relations can be specified in principle. Next it is argued that if mental holism of a Quinean sort is correct, such specification is principally impossible. Mental holism of this kind is controversial. Finally, therefore, it is argued that there is a common sense version of mental 'holism' that is not controversial but yields the same conclusion. The upshot, then, is that physicalism defined in terms of reduction, identity, realization or constitution is vacuous.

of the world'. I offer a diagnosis of some of the most important objections that have been raised against epiphenomenalism and show that this claim is mistaken insofar as there is no obvious fact that a typical physicalist of the kind that has dominated the past five decades can accept but epiphenomenalism has to reject. With regard to those features that are often said to elude an epiphenomenalist perspective on the mind, the epiphenomenalist and the typical physicalist actually are in the very same boat. This shows, first, that most of the standard objections against epiphenomenalism cannot even be raised by those who are currently dominating the philosophy of mind. Second, it shows that there is not much of a difference between the epiphenomenalist and the typical physicalist when it comes to mental causation and the 'obvious facts of our manifest image'. Maybe, thus, mind doesn't matter – but it doesn't matter.

P Walter

Does Mind Matter? And Does It Matter?

Sven Walter

Univesität Bielefeld

s.walter@philosophy-online.de

Philosophers and common sense say it is obvious that *epiphenomenalism* – the claim that the mental is a causally otiose byproduct of neurophysiological processes – is absurd and thus a dead end for an understanding of our mind and its relation to the physical world. Epiphenomenalism is said to be a counterintuitive theory of last resort to which one is driven, if at all, only in an act of desperation because all alternatives are even less satisfying. I think commonsense is wrong, as are the majority of professional philosophers. What is obvious is that after four centuries of hard thinking by brilliant minds we still lack a satisfying account of *mental causation*. In other words: We still have no clue how the mind could *not* be an epiphenomenon, and this is ample reason to ask why we are so certain that epiphenomenalism is false.

Typically, epiphenomenalism is said to be incompatible with some obvious facts of our 'manifest image

WORKSHOP "RUDOLF CARNAP"

Organized by Steve Awodey, Bernd Buldt
 Room GAP-R

DONNERSTAG THURSDAY 14.9.	14:45-15:00	Welcome and Introduction	
	15:00-15:30	Sandy Berkovski	Carnap's Debt to Frege
	15:30-16:00	David Stern	Wittgenstein vs. Carnap on physicalism: a reassessment
	16:00-16:30	Wolfgang Kienzler	Carnap on Metalogic
	16:30-16:45	plenary discussion	Carnap's relation to Frege and Wittgenstein
	16:45-17:00	coffee break	
	17:00-17:30	David McCarty	Intuitionism and Logical Syntax
	17:30-18:00	Eckehart Köhler	Gödel vs. Carnap: Platonistic Intuition vs. Convention
	18:00-18:15	plenary discussion	Tolerance, relativism, and conventionalism
	18:15-18:30	coffee break	
	18:30-19:30	Michael Friedmann	Carnap and Quine: 20th-Century Echoes of Kant and Hume
	19:30-20:30	Reception	Open Court presents Carnap's "Collected Works"

FREITAG FRIDAY 15.9.	9:00-9:30	Abe Stone	On the Completion and Generalization of Intuitive Space in "Der Raum"
	9:30-10:00	Pierre Wagner	"Der Raum," Elements for an Evaluation
	10:00-10:30	Pawel Przywara	Carnap's and Husserl's Theory of Space
	10:30-11:00	plenary discussion	Carnap's "Der Raum"
	11:00-11:30	coffee break	
	11:30-12:00	Gary Hardcastle	Quine's 1934 'Lectures on Carnap'
	12:00-12:30	Lieven Decock	Carnap and Quine on Some Analytic-Synthetic Distinctions
	12:30-13:00	Jean-Louis Hurdy	Carnap's Pragmatics and the Vindication of Analyticity
	13:00-13:30	plenary discussion	Carnap and the problem of analyticity
	13:30-15:00	lunch break	
	15:00-16:00	Richard Creath	The Logical and the Analytic
	16:00-16:15	coffee break	
	16:15-16:45	Thomas Uebel	Explication and Ramseyfication
	16:45-17:45	Holger Andreas	On The Notion of Partial Interpretation
	17:15-17:45	Angelo Cei	The Analyticity of Theoretical Terms in Carnap's Later Production

	17:45-18:15	plenary discussion	Carnap and theoretical terms
	18:15-18:30	coffee break	
	18:30-19:30	André Carus	Carnap's Approach to Ethics
	20:00	conference dinner	

SAMSTAG SATURDAY 16.9.	9:00-12:00	Symposium	On the Banishment and Return of the Philosophy of Science after World War II: Rudolf Carnap and Wolfgang Stegmüller
		Organizer	Friedrich Stadler
	9:00-9:30	Friedrich Stadler	The Forgotten "Third Vienna Circle" – A Hidden Story of the Survival and Return of Philosophy of Science in the Cold War Period
	9:30-10:00	Christian Damböck	Wolfgang Stegmüller's Conception of Synthetic A Priori Judgments in the Light of Carnap and Kripke
	10:00-10:15	coffee break	
	10:15-10:45	Hans Joachim Dahms	Philosophy of Science after Hitler. Its Development in both German States till the Construction of the Wall (1961)
	10:45-11:15	Christoph Limbeck-Lilienau	Carnap's Encounter with Pragmatism
	12:00	lunch and excursion (Einstein-Haus, Potsdam)	

The Rudolf Carnap Workshop gratefully acknowledges the support of the Center for Philosophy of Science, The University of Pittsburgh. Through its Visiting Fellows program, the Center for Philosophy of Science invites philosophers of science to work at the Center for a term or a year. Applications are open to philosophers of science with projects in all areas. The University of Pittsburgh's Archives of Scientific Philosophy Houses the papers of Rudolf Carnap. See www.pitt.edu/~pittcntr/VFIInfo.

On The Notion of Partial Interpretation

Holger Andreas
Leipzig
holger.andreas@web.de

In Carnap's most elaborated method for the logical analysis of scientific theories, his two-level conception, the semantic properties of theoretical terms are characterized by the doctrine of partial interpretation. This doctrine, though wide spread among adherents of the Received View in philosophy of science, is said to be notoriously unclear. Undoubtedly Carnap has failed to give a clear explanation of the notion of partial interpretation in his first paper on theoretical terms, i. e. The Methodological Character of Theoretical Concepts. Nevertheless such an explanation can be derived from remarks in his Foundations of Logic and Mathematics as well as from Beobachtungssprache und theoretische Sprache which is his second paper on theoretical terms. The particular kind of interpretation which confers meaning to the theoretical terms is explained in these writings with the help of an analogy to the introduction of a term by a definition. According to his explanation both kinds of terms, theoretical terms as well as defined terms, are interpreted indirectly. This means that the interpretation is not provided by a designation rule of the meta language, but rather by one or several sentences of the object language. Every indirect interpretation rests on the meaning of primitive terms which are directly interpreted. In the two-level conception of scientific language the observational terms are taken as primitive.

A theoretical term is interpreted by sentences called postulates, whereas a defined term is interpreted by a definition. The former are, unlike the latter, not required to have the status of a conservative extension of the language system of primitive terms. It turns out that the partial character of the interpretation of theoretical terms must not be understood as if the postulates were capturing only the observational part of a genuine theoretical meaning which the theoretical terms are supposed to have independently of their connections with the observational terms. The reason for calling the interpretation of a theoretical term partial is rather the fact that the limitation of admissible extensional interpretations of such a term can, in general, be strengthened by setting up further sentences as postulates. By contrast, if the meaning of a term is introduced by a definition, there is one and only one admissible extensional interpretation satisfying the definition with respect to a given interpretation of the primitive terms. There is thus no further limitation of the range of admissible interpretations possible. To sum up, a better understanding of the semantics of theoretical terms can be achieved when the analogy between definitions and postulates is taken into consideration. As a conclusion, the presentation will arrive at a formal account of the truth-value assignment for theoretical sentences.

Carnap's Debt to Frege

Sandy Berkovski
Bilkent University (Turkey)
sandy.berkovski@gmail.com

On several occasions Carnap acknowledged Frege's influence on his work. However, one area where he believed that Frege got it all wrong was ontology. According to Carnap, claims of ontology can be either analytic, or else truth-valueless. Since philosophers are interested in those claims which are truth-valueless, the project of philosophical ontology purporting to clarify what exists in the world is ill-defined. One apparent motivation for Carnap's view was the absence of a uniform criterion by which one could arbitrate between ontological disputes. Frege's context principle, interpreted as a principle about reference, supplies such a criterion. In fact Carnap's internal/external and formal/material distinctions inherited some of the key features of the context principle. I then locate the source of Carnap's and Frege's disagreement in conventionalism. I also note that the adoption of the context principle severely reduces the motivation of the nominalist approach in mathematics. I conclude with general remarks on the place of conventionalism in Carnap's philosophy.

Carnap's Approach to Ethics

André Carus
Chicago (USA)
awc23@aol.com

In his most explicit formulation of a general approach to normative statements, published in 1963, Carnap articulates a logic of values or 'optatives' strikingly similar to that previously set forth in greater detail by Richard Hare (1952). Unlike Carnap's exposition, Hare's has been the subject of much discussion and debate, over a long period. It is worth investigating, therefore, whether Carnap's formulation is subject to the same criticisms that have seemed persuasive against Hare, or whether it is in some respects more robust.

The Analyticity of Theoretical Terms in Carnap's Later Production

Angelo Ceï
University of Leeds (UK)
phlace@leeds.ac.uk

Carnap's early production, culminating in the Logical Syntax of Language, has been interpreted as a conventionalist reaction to the crisis of kantianism due to the rise of Relativistic theories and to the problems stemming from the Fregean foundational program (Coffa, Friedman, Rickett et al). This reading tends to neglect the role played by his later production. On the other side, a great deal of recent interest has been attracted by Carnap's reformulation of Ramsey sentence. Linked to the contemporary debate on structuralism such interest led to read his later position as an early form of structural realism (Maxwell, Psillos, Zahar) in which the structural content of theories is captured via Ramseyfication. Accordingly such a structural content not only would allow for a realist interpretation of Carnap's understanding of scientific theories but also would be exactly the kind of theoretical content enabling the (structural) realist to address the concerns related to theory change and Pessimistic Meta-Induction.

I find compelling the reading of Carnap's early agenda (Friedman et al) as focused on the role of a priori knowledge and especially on the attempt of reconciling the conception of relativized physical a priori and the Fregean notion of absolute analytical truth. Analyticity is the crucial notion of that agenda. Analyticity, on the other hand, is still the main issue of Carnap's later concerns about science. I think that considering the peculiarities of the Carnapian style of Ramseyfication, and their relation with his main conceptual concerns any realist view of Carnap can be rejected. Nonetheless, the structuralist solutions remain coherent with that agenda. Carnap's use of Ramseyfication in order to pursue the project of providing explicit definitions for theoretical terms presents, I think, striking similarities with some of the conceptual choices of the Aufbau. In particular, implementing the Ramsey sentences and the Hilbert ω -operator, Carnap develops a purely functional characterization of the theoretical terms conceptually very similar to the one resulting by the adoption of the structural definite descriptions in the Aufbau.

R Creath

The Logical and the Analytic

Richard Creath
Arizona State University (USA)
Creath@asu.edu

It has been pointed out by a number of authors that Carnap's definition in General Syntax of The Logical Syntax of Language of 'logical expression' is defective and that his definition there of 'analytic' presupposes that of 'logical expression'. This paper reviews some of these difficulties and tries to determine the extent, if any, to which they infect the subsequent development of Carnap's notion of analyticity.

Philosophy of Science after Hitler. Its Development in both German States till the Construction of the Wall (1961)

Hans-Joachim Dahms
Institut Wiener Kreis, Wien (Austria)
hj.dahms@freenet.de

Surprisingly the initial conditions for the philosophy of science were better in the German Democratic Republic (Soviet Zone/GDR) than in the Western Zone/FRG. In Jena there was Paul Linke, a philosopher knowledgeable about logic and philosophy of science who could survive undamaged the Nazi period despite his social democratic affinities. There was also Max Bense, an engineer, strongly interested in semiotics and cybernetics. The pressures of the administration made him rapidly leave for Western Germany. Soon gathered around Linke a circle of young scientists who intended to achieve their theses about subjects like Frege's logic or Schlick's ethics. But defamed as "bourgeois" and "positivists", they mostly emigrated to the West. In Berlin, there was even appointed a direct disciple of Schlick on a specially created Chair of logic and theory of knowledge, Walter Hollitscher. He experienced great success with his teaching. But already the interdiction of his encyclopaedic book "Lectures" was a big backstroke. For reasons still not completely elucidated, he had to leave immediately the GDR in 1953 and his position was taken over by Georg Klaus.

In the Western Zone/FRG, the situation initially seemed desperate for philosophy of science and only slowly in the fifties, this area of research was introduced by a few scholars.

But surprisingly, an institutionalisation of philosophy of science took place almost simultaneously in 1957/58. In Erlangen, Hamburg and Munich chairs with an emphasis on philosophy of science were created, and Paul Lorenzen, Carl Friedrich von Weizsäcker and Wolfgang Stegmüller were appointed. At the beginning of the 1960s Hans Albert was appointed. Around these Centers soon emerged philosophic schools: the constructivists of the Erlangen school and the Neo-Empiricists of the Munich school. In the long run the Munich school around Stegmüller was the most successful of these groups, also because of his contacts to banished philosophers of science, for example to Rudolf Carnap, Carl Gustav Hempel and Herbert Feigl. Regarding the question whether the philosophy of science in both German states went beyond a reception of the Anglo-American literature on philosophy of science, the answer is negative, at least for the period till 1961.

R Damböck

Wolfgang Stegmüller's Conception of Synthetic A Priori Judgments in the Light of Carnap and Kripke

Christian Damböck
Institut Wiener Kreis, Wien (Austria)
christian.damboeck@univie.ac.at

In his less well known paper 'Der Begriff des synthetischen Urteils a priori und die moderne Logik' (1954), Wolfgang Stegmüller presents an interesting account for *synthetic a priori judgments*. Stegmüller identifies a judgment as synthetic a priori if it is necessarily true but not analytic: "... so sind die synthetisch-apriorischen Sätze von den rein logischen dadurch unterschieden, daß sie nicht in 'allen möglichen Welten' gelten, sondern nur in bestimmten" (p.555).

This conception is of historical interest for two reasons. First, it is an astonishing fact that no one of the Vienna Circle philosophers tried to present a formal *definition* of the notion of synthetic a priori, although this notion turns out to be the core of all metaphysics in a postkantian era and therefore appears to be the most straightforward enemy for a twentieth century anti-metaphysician.

The second point of interest in Stegmüller's arguments has to do with Kripke's semantic conception of modality. It is obvious that Stegmüller's proposal is formally related to the conception of a 'possible-worlds-semantics' for S5 (compare Kripke's 'A Completeness Theorem in Modal Logic' (1959)). Given this constellation we gain an interesting perspective on Kripke's more philosophical conclusions in 'Naming and Necessity'. Ultimately, Stegmüller's semantic reading of metaphysics indicates some shortcomings in the Kripkean (rather strictly metaphysical) worldview.

R Decock

Carnap and Quine on some Analytic-Synthetic Distinctions

Lieven Decock
Free University Amsterdam (Netherlands)
LB.Decock@ph.vu.nl

I want to analyse the Quine-Carnap discussion on analyticity with regard to logical, mathematical and set-theoretical statements. In recent years, the renewed interest in Carnap's work has shed a new light on the analytic-synthetic debate. If one fully appreciates Carnap's conventionalism, one sees that there was not a metaphysical debate on whether there is an analytic-synthetic distinction, but rather a controversy on the expedience of drawing such a distinction.

However, on this view, there can be no longer a single analytic-synthetic distinction, because several kinds of statements could be regarded as analytic (L-determinate). L-equivalence between extra-logical lin-

guistic predicates has already been heavily debated. The recent consensus states that Quine's rejection of this analytic-synthetic is pragmatically grounded in his linguistic behaviorism. However, Carnap's logical frameworks also contain other kinds of statements, and it is worthwhile to compare both Quine and Carnap's grounds for considering these statements as analytic or not analytic.

First, I will discuss logical statements. I will argue that Quine draws a very sharp distinction between first order logic and set theory, which should be regarded as a (pragmatic) analytic-synthetic distinction (as Quine admits in an interview, see *Theoria*, 40, 1994, p. 199). In fact, Quine's major worry is whether identity statements are analytic. Second, I will discuss mathematical statements. In Carnap's *Foundations of Logic and Mathematics*, it is clear that mathematical statements are analytic. For Quine, all mathematical statements are reducible to set-theoretical statements. Third, I discuss the analyticity of set-theoretical statements. For Quine, the membership predicate should be regarded as an interpreted extra-logical predicate. Quine's work in set theory and his later philosophy of set theory naturally lead to the view that set-theoretical statements cannot be analytic. A major complication for the Quine-Carnap comparison is that Carnap has no elaborate reflections on set theory, while the influence of set theory on Quine's views can hardly be underestimated. I conclude with some lessons for the contemporary debate on analyticity.

Friedman **R**

Carnap and Quine: Twentieth-Century Echoes of Kant and Hume

Michael Friedman
Stanford University (USA)
mlfriedman@stanford.edu

The famous debate between Carnap and Quine over the analytic/synthetic distinction is one of the defining moments of the twentieth-century analytic tradition in philosophy. This paper examines this debate from an historical point of view, by tracing Carnap's evolution from his early exposure to Kantian ideas at the University of Jena through the development of his mature conception of "the logic of science [Wissenschaftslogik]," and, at the same time, tracing the philosophical interaction between Carnap and Quine from Quine's Carnapian "discipleship" in the early thirties through his decisive break with Carnap in "Two Dogmas of Empiricism." Special attention is given to Quine's intervening Humean-inspired nominalist phase, the importance of which has recently been brought to light by examination of Carnap's notes on the discussions between himself, Quine, and Tarski in 1940-41 and the publication of Quine's lectures on Hume from 1946. The upshot is that Quine's philosophical orientation, from the very beginning, was completely different from Carnap's, and, as a result, Quine never really understood Carnap's position. Since our current understanding of Carnap's position is fundamentally influenced, in turn, by Quine's forceful critique, this paper attempts to foster a better apprecia-

tion of Carnap's deeply revolutionary approach by way of contrast with Quine's.

R Hardcastle

Quine's 1934 "Lectures on Carnap"

Gary Hardcastle
Bloomsburg University (USA)
ghardcas@bloomu.edu

In November of 1934, over successive Thursdays, the 26-year-old Willard van Orman Quine gave three "Lectures on Carnap" at Harvard University, the ostensive aim of which was a presentation of the "central doctrine" of Carnap's *Logische Syntax der Sprache*, "that philosophy is syntax." These were among Quine's very first public lectures, and they constituted the American premier of Carnap's *logische Syntax* program. As such, these lectures are of considerable significance to the history of analytic philosophy. They show, for example, one way Carnap's syntactical program was presented and understood in the 1930s, and indeed they show how Quine, emerging even in 1934 as one of America's brightest logicians, understood that particular project. Moreover, they promise to tell something about how Quine himself was thinking about central philosophical issues—the a priori, analyticity, and philosophy itself—early in his career, before he wrote the papers and books on those topics that established his reputation. This present paper takes up this last topic. My aim is to reconstruct and understand how Quine was thinking about the a priori, analyticity, and philosophy itself in 1934, what he aimed to accomplish in the "Lectures on Carnap," and the considerable extent to which he accomplished that aim. What Quine accomplished, in short, was the outline of a fascinating and original anti-metaphysics, with conventionalism (specifically, implicit definition) at its heart. This was an anti-metaphysics that invited (but, significantly, could not demand) adoption of a particular conception of philosophy.

R Hurdy

Carnap's Pragmatics and the Vindication of Analyticity

Jean-Louis Hurdy
University of Edinburgh
jl.hurdy@ed.ac.uk

In his *Logische Syntax der Sprache* (1934), Carnap claims that philosophy is the logic of science, i.e. "the syntax of the language of science". His position evolves in his *Introduction to Semantics* (1942), when he asserts that the logic of science is about not only syntax but also semantics, and that philosophy should be understood in a wider sense by including the relation of natural language to experience. Thus, the task of philosophy is

"semiotical analysis", and the structure of the language of science includes pragmatics, i.e. the way a thing or person interacts with natural language. In the reprint of *Testability and Meaning* (1950), Carnap then divides the theory of language (or semiotics) into three parts, namely pragmatics, semantics and syntax, so that pragmatics is distinct from logical analysis. The present paper aims to show that Carnap's pragmatics constitutes a necessary condition for our understanding of analyticity. First, pragmatics must be clearly distinguished from pure semantics; indeed, the formal rules of any constructed language system are devoid of empirical reference, and prevent pragmatics from being relevant. This does not mean that pragmatics has no semantic content; in other words, pragmatics cannot be identified with non-semantic metaphysical doctrines. The in-between view consists in defining pragmatics in terms of descriptive semantics; but Carnap broadens the usual notion of pragmatics by identifying it with not only the theory of extension (meaning, truth) but also the theory of intension (analyticity, synonymy). Intensional pragmatics amounts to generating empirical methods that test hypothetical intensions, i.e. the possible conditions that objects must fulfil in order for a given speaker to accept to apply predicates to them. Analyticity is based on pragmatic considerations, since analytic statements require meaning postulates that have to be accepted by a speaker. Acceptance is a matter of practical decision, which has no cognitive content and is devoid of logical status. As such, Carnap's intentional pragmatics vindicates the concept of analyticity, and completes the pure semantics of natural language.

Kienzler **R**

Carnap on Metalogic and Syntax. Between Wittgenstein and Gödel

Wolfgang Kienzler
Universität Jena

Wolfgang.Kienzler@uni-jena.de

While Carnap attributes the most basic ideas of *Logical Syntax* in turn to Frege, Wittgenstein, Gödel and Tarski, Steve Awodey has suggested (in his "Carnap's Dream") that actually he used Gödel to get away from Wittgenstein. It is correct that Carnap used some technique from Gödel to define syntax from within the same language (something of which Wittgenstein said that it could not be done). On a more fundamental level, however, Carnap moved towards a more Wittgensteinian view and away from Gödelian Platonism. Even while employing Gödelian "arithmetization" Carnap points out that it is incorrect to say that a proposition of syntax "speaks about" anything. (In the translation of *Syntax* Carnap weakens his expression to "is concerned with".) This mode of speech is "only figurative" because no analytical sentence has any content. While Carnap did not seem to notice that this observation could have led him to reassess his way of representing the results of Gödel's famous incompleteness proofs (questioning any claims as to any p forwarding claims about itself), Gödel

on the other hand did realize the fundamental disagreement. This became quite obvious in Gödel's attempts from the 1950s attacking Carnap's idea that mathematics was basically nothing but syntax.

R Köhler

Gödel vs. Carnap: Platonistic Intuition vs. Convention

Eckehart Köhler
University of Vienna (Austria)
Eckehart.Koehler@UniVie.Ac.At

Carnap's Conventionalism was highly innovative, since Poincaré hadn't wanted to apply it to logic at all. Gödel insightfully recognized an advantage of Conventionalism in its ability to establish a modal difference in theories based on it from those based on sensory observation. Gödel then tied himself into knots to refute Carnap's syntax program. He succeeded, but was paradoxically unable to exhibit the nature of mathematical knowledge and withheld his refutation. If Gödel had only realized that Conventionalism is consistent with Platonistic Intuitionism, he would have saved himself (and Schilpp) much grief. For in fact, conventions constitute evidence for intuitions, rather than evidence against them; borrowing Samuelson's idea of "revealed preference", it is easy to show this. The key issue is the reliability of conventions, which proves objectivity of the intuitions they "reveal". (Boltzmann already proposed stability of belief as a criterion of objectivity.)

R Limbeck-Lilienau

Carnap's Encounter with Pragmatism

Christoph Limbeck-Lilienau
Institut Wiener Kreis, Wien (Austria)
christoph.limbeck@univie.ac.at

The emigration of Rudolf Carnap to the United States at the end of 1935 and therefore the change of his cultural and philosophical environment brought him in closer contact with pragmatist philosophers who played only a marginal role in his earlier European writings. Although a dialogue between Carnap and philosophers close to pragmatism was already initiated in Prague (so with Quine, Charles Morris and Ernest Nagel), it is only in America that Carnap was explicitly influenced by pragmatic positions and responded to pragmatic criticisms of logical empiricism. It is our purpose to show, how Carnap reacted to this interaction with pragmatism, specially concerning two problems: Carnap's response to pragmatic criticisms of verificationism, specially in *Testability and Meaning* (1936) and Carnap's acceptance of the distinction between semantics and pragmatics, specially in *Introduction to Semantics* (1942),

both writings dating from a period when pragmatism was still dominant in American philosophy.

In his semantics Carnap adopts from the semiotics of Charles Morris a distinction between syntactical, semantical and pragmatical questions. Morris used the distinction between the meaning and the use of signs, or between semantics and pragmatics, to show two aspects of signs but underlined at the same time, that a sign always involves both dimensions. Carnap acknowledges this only for natural languages where it is impossible to determine the meaning of expressions without an empirical observation of the use speakers make of these expressions. Here evidently, semantics is grounded on pragmatics. But Carnap distinguishes furthermore descriptive semantics on the one side, which relies for the determination of meaning on the pragmatic description of a community of speakers, and pure semantics i.e. systems of semantical rules that are constructed and postulated. In pure semantics Carnap separates radically questions of semantics from pragmatics. This separation is possible, because in Carnap's pure semantics the semantical rules are arbitrarily postulated. We will show that this strict separation of semantics from pragmatics becomes problematic specially in the controversy with Quine on analyticity, where Quine attacked the arbitrariness of semantical rules fixed as meaning postulates.

McCarty R

Intuitionism and Logic Syntax

David McCarty
Indiana University Bloomington (USA)
dmccarty@cs.indiana.edu

A prevailing doctrine concerning intuitionism is that the language of its practitioners is similar syntactically to but radically distinct semantically from the language of classical mathematicians. With his *Logical Syntax of Language and Foundations of Logic and Mathematics*, philosopher Rudolf Carnap became one of the chief proponents, if not the originator, of the idea that the statements of intuitionistic mathematicians carry their own nonstandard intuitionistic or constructive meanings, generally distinct from the meanings of statements made by classical mathematicians. According to Carnap, an understanding of intuitionism is most effectively conveyed to the nonintuitionist by providing it with a translation manual. For example, Carnapian doctrine would have it that the statement, made by an intuitionist, that there are no total discontinuous functions of a real variable (a corollary of Brouwer's Continuity Theorem) stands in no direct and immediate contradiction with the statement, made by a classical mathematician, that there are uncountably many such discontinuous functions. We trace this idea back to Carnap's Principle of Tolerance and claims he made on behalf of his notion of pure syntax. From premises independent of intuitionistic mathematics, we argue that the Principle and attendant claims are mistaken, especially Carnap's repeated insistence that, in defining languages and

treating them formally, logicians are free, that is, not committed to any mathematical statement that intuitionists would find debatable.

R Przywara

Husserl's and Carnap's Theories of Space

Paweł Przywara
Rzeszów (Poland)
pprzywara@wsiz.rzeszow.pl

Usually we do not connect theoretical approaches that of Edmund Husserl and of Rudolf Carnap. The fact of writing by Carnap some of his early works under the strong influence of the founder of phenomenological school is still not well-known.

In the paper, at the beginning I arrange some terminological questions concerning space and then having described shortly Husserlian theory of space (including especially theory of perceiving of space by a subject), I show Carnapian different and changeable attempts to explain problems of structure of space(s) and of structure of our spatial experience(s) (from his dissertation called *Der Raum* to *Der Logische Aufbau der Welt*). Then I try to gather all the terminological and methodological similarities between Carnapian and Husserlian approaches. My general thesis is following: Carnapian early works are logistic criticism of Husserlian phenomenology of space.

R Stadler

The Forgotten "Third Vienna Circle" – A Hidden Story of the Survival and Return of Philosophy of Science in the Cold War Period

Friedrich Stadler
Universität Wien (Austria)
friedrich.stadler@univie.ac.at

The Transfer and Transformation of Philosophy of Science from Europe to America in the Nazi-Period have been investigated within the last 2 decades. Less known is the story of the survival and attempts to reintroduce banished Logical Empiricism in Vienna after 1945.

This process was initiated by Viktor Kraft (1880-1975), who – after being dismissed by the Nazis in 1938 and working in inner emigration during the war – founded and led the so called "Kraft-Kreis" (Kraft-Circle) 1949-1953 and contacted some former members of the Vienna Circle (Herbert Feigl, Philipp Frank, Rudolf Carnap, and Karl Popper).

This discussion group was a remarkable short renaissance of the Viennese heritage in Philosophy of Science with to date ignored, but sustainable impacts on the second wave of emigré philosophers of science after World War II – like Paul Feyerabend, Ernst Topitsch, and Wolfgang Stegmüller. Ten years later another retransfer

of philosophy of science took place at the "Institute for Advanced Study" in Vienna with Carnap, Feigl and Popper as visiting lecturers.

A decisive event was Kraft's invitation of Arthur Pap as a Visiting Professor to Vienna in 1953/54, who published the book *Analytische Erkenntnistheorie* (1955) with the assistance of Feyerabend, and which was dedicated to the Vienna Circle. Together with Feigl's article "Existential Hypotheses" (1950) this book formed the philosophical background to be debated controversially in the "Kraft Circle". The main issue on the agenda was realism, especially the existence of an external world.

The paper will critically reconstruct this short episode and will focus on the central debate on realism vs. phenomenalism in philosophy of science, which was to be continued at Feigl's "Minnesota Center for Philosophy of Science", and which obviously influenced Feyerabend. From a broader perspective of the *Methodenstreit* we can thereby identify the dualism of the hypothetico-deductive (critical or constructive) realism and inductive phenomenalism. The transfer of this controversy to England and America obscured its origins in the "Third Vienna Circle". But the return of Analytic Philosophy and Philosophy of Science back to the German speaking countries was realized by Stegmüller as a late consequence of the Kraft Circle.

Stern R

Wittgenstein versus Carnap on Physicalism: A Reassessment

David Stern
University of Iowa
david.stern@uiowa.edu

The "standard account" of Wittgenstein's relations with the Vienna Circle is that the early Wittgenstein was a principal source and inspiration for the Circle's positivistic and scientific philosophy, while the later Wittgenstein was deeply opposed to the logical empiricist project of articulating a "scientific conception of the world." However, this telegraphic summary is at best only half-true and at worst deeply misleading. For it prevents us appreciating the fluidity and protean character of their philosophical dialogue. In retrospectively attributing clear-cut positions to Wittgenstein and his interlocutors, it is very easy to read back our current understanding of familiar distinctions into a time when those terms were used in a much more open-ended way.

The paper aims to provide a broader perspective on this debate, starting from the protagonists' understanding of their respective positions. Too often, the programmatic statements about the nature of their work that are repeated in manifestoes, introductions, and elementary textbooks have occupied center stage in the subsequent secondary literature. Consequently, I focus on a detailed examination of a turning point in their relationship. That turning point is Wittgenstein's charge, in the summer of 1932, that a recently published paper of Carnap's, "Physicalistic Language as the Universal Language of Science", made such extensive and unac-

knowledgeable use of Wittgenstein's own ideas that Wittgenstein would, as he put it in a letter to Schlick, "soon be in a situation where my own work shall be considered merely as a reheated version or plagiarism of Carnap's." While the leading parties in this dispute shared a basic commitment to the primacy of physicalistic language, and the view that all significant languages are translatable, there was a remarkable lack of mutual understanding between them, and deep disagreement about the nature of the doctrines they disputed. Three quarters of a century later, we are so much more conscious of the differences that separated them than the points on which they agreed that it takes an effort of historical reconstruction to appreciate why Wittgenstein once feared that his own work would be regarded as a pale shadow of Carnap's.

Uebel **R**

Explication and Ramseyfication

Thomas Uebel
University of Manchester
thomas.uebel@manchester.ac.uk

This presentation will consider whether Carnap's philosophical programme of explication is threatened by what many theorists consider a misadventure late in his career, namely, his forays into the ramseyfication of scientific theories. In doing so it seeks (i) to highlight one (under-discussed) aspect of the long-running debate about the propriety of employing the distinction between analytic and synthetic statements, (ii) to distinguish Carnap's use of Ramsey sentences for expressing the content of the non-observational parts of scientific theories from that of structural realists, and (iii) to present a qualified defense of Carnap's explicationist programme.

The problem at issue is the following. The impossibility to formulate the analytic/synthetic distinction for theoretical statements prompted Carnap to look beyond his arguably defensible criterion of empirical significance published in 1956. Using Ramsey's method of replacing descriptive theoretical terms by variables bound by higher-order quantifiers, Carnap, in publications dating from 1958 to 1966, claimed to be able to give a characterisation of the cognitive content of theoretical terms so as to distinguish synthetic and analytic statements concerning them. Now according to Newman's well-known objection, a ramseyfied theory is trivially satisfied once the empirical constraints set down by its observational part are met. This speaks not only against structural realists but also against Carnap's avowed intention to use ramseyfication to exhibit the cognitive content of theoretical terms and to reestablish the analytic/synthetic distinction for theoretical statements. The question arises how much damage ensues for Carnap's explicationist programme.

Wagner **R**

"Der Raum," Elements for an Evaluation

Pierre Wagner
Paris (France)
pwag@wanadoo.fr

It has been noted that Carnap's thesis contains the germs of several ideas that were to be developed later in his philosophical career. It may be illuminating, for some of them, to find these germs and state the contrasts with the ideas as they were eventually developed.

R **Stone**

On the Completion and Generalization of Intuitive Space in *Der Raum*: Husserlian and Drieschian Elements

Abe Stone
University of California Santa Cruz (USA)
abestone@ucsc.edu

The discussion of intuitive space in §II of *Der Raum* has three main stages: (1) axioms to determine the infinitesimal structure of space; (2) postulates or demands (*Forderungen*) which, in conjunction with the axioms, supposedly prescribe a certain type of global structure; and (3) a process by which that first type of global structure is subordinated to another, more "general" type.

Here I focus on some puzzles about Carnap's intended epistemological point in the second and third stages (leaving aside the technical problems which also arise). Since any global structure at all requires that eidetic intuition be supplemented with freely-chosen postulates and/or intuitively unmotivated generalizations, it is unclear, as several authors have pointed out, how and in what sense "intuitive space" as a whole represents a distinctive, a priori contribution to our knowledge. I suggest a way of approaching this issue based on Carnap's sources in particular, Husserl and Driesch, both of whom he repeatedly claims to be following. The idea of a severely finite realm of possible intuition, which both requires and allows supplementation with an infinite conceptual structure, is central to Husserl's thought, and, I argue, it would be natural for Carnap to rely on it in attempting to reconcile Husserlian eidetic intuition with the general theory of relativity. That this larger conceptual structure owes its details to free postulation is, on the other hand, decidedly un-Husserlian. But here, I claim, Carnap takes his cue from Driesch's view, in the first edition of the *Ordnungslehre*, that natural actuality is the result of a certain demand for order: we demand such order in natural things and reject as non-actual (hallucinatory, dreamed, etc.) whatever nature-like elements of experience fail to fit into it.

Here are four examples of questions that might be studied in order to get a better understanding of Carnap's thought in 1921. What I propose is to examine more precisely one or two of them in my paper.

1- In later publications, Carnap explains that in many disputations between philosophers, the latter do not realize that their oppositions reduce to the use of different languages. In the introduction of *Der Raum*, Carnap also proposes to resolve contradictions, in this case between theses about space. Here, the idea is not that people use different languages but that they confuse different objects: formal space, intuitive space and physical space. How do these different ways of explaining philosophical disputations relate to each other?

2- In later publications as well as in *Der Raum*, Carnap maintains that any theory of science should recognize the essential use of conventions or free stipulations. But here and there, these conventions are not the same, they do not have the same meaning, and they are not accounted for in the same way.

3- In later publications, Carnap displays different methods for a theory of science, methods like the constitution of concepts or, later, logical syntax. *Der Raum* is also a contribution to the theory of science, with a specific method, which is not made explicit. How should it be characterized?

4- In *Der Raum*, Carnap makes an essential use of logic, though the method of logical analysis is not recognized as such. It is probably not sufficient to refer to Russell's *Principles of Mathematics* or to the *Principia Mathematica* in order to account for logic as it appears and is used here. What is Carnap's notion of logic in 1921, and how would it evolve in the following years?

WORKSHOP "TOWARDS A NEW EPISTEMOLOGY OF MATHEMATICS"

Organized by Bernd Buldt, Benedikt Löwe, Thomas Müller
Room GAP-T

DONNERSTAG THURSDAY 14.9.	14:15-14:45	Opening	
	14:45-15:45	Keith Devlin	The Useful and Reliable Illusion of Reality in Mathematics
	15:45-16:00	coffee break	
	16:00-16:30	Dirk Schlimm	On the Importance of Asking the Right Research Questions: Could Jordan Have Proved the Jordan-Hölder Theorem?
	16:30-16:45	coffee break	
	16:45-17:15	Valeria Giardino	Mathematical Perception and Diagrams
	17:15-17:45	Alan Baker	Experimental Mathematics
	17:45-18:00	coffee break	
	18:00-18:30	Curtis Franks	David Hilbert as an Epistemological Naturalist
	18:30-19:00	Brendan Larvor	Whose Job Is it to Study Mathematical Practice

FREITAG FRIDAY 15.9.	10:15-11:15	Bart van Kerkhove	Informal Aspects of Mathematics
	11:15-11:30	coffee break	
	11:30-12:00	Poster Session I: Presentations	
		Vyacheslav Moiseev	Towards a Platonic Epistemology of Mathematics
		Andrei Rodin	From Set-Theoretic Foundations to Categorical Integration
		Thomas Zoglauer	Can Mathematics be Naturalized?
	12:00-14:15	lunch break	
	14:15-15:15	Katja Lengnink	Didactical Perspectives on Mathematics and its Philosophical Implications
	15:15-15:30	coffee break	
	15:30-16:00	Michael Stöltzner	On the Ontology of Theoretical Mathematics
	16:00-16:30	Oliver Petersen	Some Wittgensteinian Reflections on the Jaffe/Quinn-Thurston Debate on Theoretical Mathematics and Mathematical Proof
	16:30-16:45	coffee break	
	16:45-17:15	Tatiana Arrigoni	Towards a New Philosophy of Set Theory
17:15-17:45	Niko Strobach	Teaching Logic with Good Conscience – Experiences in Moderate Formalism	
17:45-18:00	coffee break		

Workshops

	18:00-18:30	Kenny Easwaran	The Role of Axioms in Mathematics
	18:30-19:00	Felix Mühlhölzer	Mathematics and the Lifeworld

SAMSTAG SATURDAY 16.9.	9:30-10:15	Poster Session II: Discussion	
	10:15-10:45	David Corfield	Why and how to Write a History of Higher-Dimensional Algebra
	10:45-11:00	coffee break	
	11:00-11:30	Johanna Pejlare	Felix Klein and the Role of Intuition in Mathematics
	11:30-12:00	Kajsa Bråting	Visualization in Mathematics
	12:00-12:15	coffee break	
	12:15-13:15	Leo Corry	From <i>Algebra</i> (1895) to <i>Moderne Algebra</i> (1930): Changing Conceptions of a Discipline. A Guided Tour Using the <i>Jahrbuch über die Fortschritte der Mathematik</i>
	13:15-15:00	lunch break	
	15:00-18:00	Excursion: Einsteinhaus	

Towards a New Philosophy of Set Theory

Tatiana Arrigoni
 University of Genua (Italy)
 arrigonitiana@hotmail.com

My contribution will concentrate on some situations implied by set theoretical developments, which, as I'll show, are usually passed under silence or even misinterpreted by the philosophies of set theory subscribed by most practitioners and philosophers today. Two situations will be, in particular, focused on: the multiple-universe-phenomenon and the epistemic status of the notion of forcing. As to the former, I'll hint at the ways it is usually interpreted and argue that they leave alternatively unexplained views like Jensen's that $V=L$ may be a very attractive axiom (Jensen 1995) and like Martin's that referring to the universe of all sets may make sense after all, the multiple-universe-phenomenon notwithstanding (Martin 2001). As to the latter, I'll notice that the notion of forcing has switched from being conceived as a mere technical device to being regarded as a fundamental set theoretic concept (see H. Friedman's FOM-report of the panel on CH held in Atlanta in January 2005) and observe that a philosophical explanation of this fact is yet to be given.

I will interpret these situations as witnessing the need of a novel philosophy for set theory.

Visualization in Mathematics

Kajsa Bråting
 Uppsala University (Sweden)
 kajsa@math.uu.se

I will discuss visualization in mathematics from a historical and didactical point of view. A common notion, historically as well as today, is to make a distinction between 'visualizable' and 'non-visualizable' mathematics. I criticize this distinction since it does not take into consideration where and to whom we visualize. The main example I will discuss, both historically and didactically, is continuous nowhere differentiable functions. In connection to this I will consider Felix Kleins distinction between naive and refined mathematics.

Why and how to Write a History of Higher-Dimensional Algebra

David Corfield
 MPI für biologische Kybernetik
 david.corfield@tuebingen.mpg.de

In a recent paper 'How Mathematicians May Fail to be Fully Rational'[1], I advocated the adoption in the philosophy of mathematics of Alasdair MacIntyre's general notion of tradition-constituted enquiry. A central component of this notion requires of a rational tradition that it know the history of its successes and failures. This raises the question as to whether, were such a history to be written, it would fall foul of the criticism contemporary historians of mathematics have levelled at mathematicians' histories that they are largely 'Royal-road-to-me' accounts. I shall address this question in the context of a research programme known as 'higher-dimensional algebra', and consider the charge mathematicians may make in return that historians are unable to treat research programmes which run for decades, supported by tens or hundreds of mathematicians from many countries and institutions.

[1] <http://www.dcorfield.pwp.blueyonder.co.uk/HowMathematicians.pdf>

Experimental Mathematics

Alan Baker
 Swarthmore College (USA)
 abaker1@swarthmore.edu

The rise of the use of computers in mathematical research is raising interesting new philosophical questions about the nature of proof and of justification within mathematics. On the one hand, mathematicians have produced 'computer-aided' proofs - for example Appel and Haken's 1976 proof of the celebrated Four-Color Theorem - which are much too long and complex to ever be fully checked by human mathematicians. On the other hand, computers have been used to verify large numbers of specific instances of general mathematical claims - for example for Goldbach's Conjecture - thus building up a large body of what might be seen as inductive evidence. The term "Experimental Mathematics" has been applied to both of these developments. I am interested in whether there is any conceptual unity to the use of this term by mathematicians, and what philosophical issues are at stake in accepting the legitimacy of experimental mathematics.

T Corry

From *Algebra* (1895) to *Moderne Algebra* (1930): Changing Conceptions of a Discipline. A Guided Tour Using the *Jahrbuch über die Fortschritte der Mathematik*

Leo Corfield
Tel Aviv University (Israel)
corry@post.tau.ac.il

The rise of modern, structural algebra may be characterized in terms of the consolidation of a certain image of the discipline that developed gradually since the turn of the century, received special impetus with the work of Emmy Noether beginning in 1920, and eventually became epitomized in van der Waerden's famous textbook of 1930, *Moderne Algebra*. The paradigmatic presentation of the discipline as conceived at the turn of the century is the one presented in Heinrich Weber's classical *Lehrbuch der Algebra*, whose first volume appeared in 1895.

The way from the "classical" to the "modern" conception of the discipline can be investigated from several perspectives. The most immediate, and perhaps necessary, one is to look at the milestone articles that progressively produced the main concepts, theorems and techniques that came to stand at the center of algebraic research as it was practiced along the 1920s.

Parallel to this, however, one may look for additional hints that clarify how the practitioners of the discipline interpreted this progressive evolution and how their image of algebra changed accordingly. One illuminating way to do so is to look at the leading, German review journal of the period, the *Jahrbuch über die Fortschritte der Mathematik*. It turns out that the changing classificatory schemes adopted by the journal to account for the current situation at various, important crossroads of this story add significant insights to our understanding of it.

T Devlin

The Useful and Reliable Illusion of Reality in Mathematics

Keith Devlin
Stanford University (USA)
devlin@csl.stanford.edu

Recent years have seen a growing acknowledgement within the mathematical community that mathematics is cognitively/socially constructed. Yet to anyone doing mathematics, it seems totally objective. The sensation in pursuing mathematical research is of discovering prior (eternal) truths about an external (abstract) world. Although the community can and does decide which topics to pursue and which axioms to adopt, neither an individual mathematician nor the entire community can choose whether a particular mathematical

statement is true or false, based on the given axioms. Moreover, all the evidence suggests that all practitioners work with the same ontology. (My number 7 is exactly the same as yours.) How can we reconcile the notion that people construct mathematics, with this apparent choice-free, predetermined objectivity? I believe the answer is to be found by examining what mathematical thinking is (as a mental activity) and the way the human brain acquired the capacity for mathematical thinking.

Easwaran T

The Role of Axioms in Mathematics

Kenny Easwaran
University of California at Berkeley (USA)
easwaran@berkeley.edu

In order to further the debate on whether mathematics needs new axioms, it seems useful to say what role axioms play in mathematics, so that their need can be analyzed. If we see what role axioms have been called upon to play historically, then we can see whether ZFC is sufficient, or whether more axioms might be necessary.

While it may seem plausible that axioms are inherently obvious statements that can be used to establish theorems unassailably, I point out that this may be neither possible nor necessary. In addition, it doesn't seem to fit the historical facts. Instead, I argue that the role of axioms is and has been to systematize relatively uncontroversial facts that mathematicians can accept from a wide variety of philosophical positions. Once the axioms are generally accepted, mathematicians can expend their energies on proving theorems instead of arguing philosophy. The fictionalist and the platonism can adopt the same axioms, and thus use each other's theorems, despite each thinking that the other has a confused picture of their shared goal.

Given this account of the role of axioms, I suggest that in order for a new axiom to be adopted, it must meet four requirements: it must be widely acceptable, it must have interesting consequences, it must help avoid philosophical disagreements, and it must be proven independent of existing axioms. Violating the third requirement gives what Feferman calls "structural axioms" rather than "foundational axioms", while violating the fourth gives a "conjecture", "hypothesis", or "theorem" rather than an axiom.

Penelope Maddy has recently proposed a similar view in *Naturalism in Mathematics*, but she suggests that the philosophical questions bracketed by adopting the axioms can in fact be ignored forever. I contend that these bracketed arguments are in fact important, and should ideally be resolved at some point. I concede that their resolution is unlikely to affect the ordinary practice of mathematics, though they may have effects in the margins of mathematics, including with regards to the large cardinal axioms Maddy would like to support.

T Franks

David Hilbert as an Epistemological Naturalist

Curtis Franks
University of Notre Dame
cfranks@uci.edu

The German mathematician David Hilbert's most distinctive contribution to the debate about the foundations of mathematics was his insistence that ordinary mathematics could stand on its own without recourse to philosophical foundations. In order to develop this line of Hilbert's thought, one must reconsider the traditional conception of Hilbert as a "finitist" and "formalist" philosopher of mathematics. According to that conception, Hilbert endorsed formalism as a thesis about the nature of mathematics and found in finitism an especially secure foundation on which to situate dubious infinitary techniques. I argue that Hilbert's actual position was more subtle:

No ordinary mathematical techniques are of questionable legitimacy, and any attempt to secure mathematics on an epistemological base is misguided because the security inherent in mathematical methods already is greater than the security in the philosophical systems that recommend particular epistemological foundations. Hilbert's finitism and formalism were instead methodological constraints consistent with his thoroughly naturalistic conception of mathematics. He designed his foundational program, not to show that mathematics is epistemologically secure, but to show why no philosophical defense of mathematics is needed to demonstrate its security.

T Giardino

Mathematical Perception and Diagrams

Valeria Giardino
University of Rome (Italy)
valeriagiardino@libero.it

The subject of my paper is the investigation of mathematical processes which can be acknowledged as 'perceptual', in particular visual/spatial. Such investigation is part of the broader question of which epistemology of mathematics is more suited to account for its real processes.

In the first section, I present the background of my investigation, arguing that there is a need for a conception of mathematics alternative to the standard or 'logocentric' one typical of last century studies. I discuss the idea that mathematics is a human activity and a cognitive process, and thus it is similar to other sciences, proceeds testing hypotheses and is fallible. Mathematics deals with problem solving, and to this aim it makes use of different formats as tools to display the given information. There is a pluralism and gradual-

ism in the forms of representation used in mathematics: each of these formats is a legitimate tool and is cognitively and computationally relevant.

In the second section, I take into account cognitive science results, to show that an interdisciplinary investigation into mathematical diagrams is possible. Psychology provides evidence for (i) the origin of mathematics in perception and their usefulness to solve problems; (ii) the existence of regularities and grouping laws in our mathematical perception that allow us to see configurations in diagrams. Nevertheless, mathematical diagrams are used specifically to solve mathematical problems: the investigation into mathematical perception has to relate also to more theoretical assumptions based on background knowledge. Mathematical diagrams are both icons, when they are considered only in their depictive and 'literal' sense, looking at the spatial and topological relations they display, and symbols, when they are interpreted as meaningful configurations, at a semantic level. The perceptual element is thus the necessary but not sufficient condition for diagrams to display mathematical information.

The motivation for a study of this kind is twofold: (i) my intention to put forward arguments in favour of a renewed epistemology of mathematics, showing that it is characterised by the use of different representations to display information; (ii) my wish to evaluate whether philosophy can assume a leading role in managing with epistemological interdisciplinary investigations.

Larvor **T**

Whose Job Is it to Study Mathematical Practice

Brendan Larvor
University of Hertfordshire (UK)
b.p.larvor@herts.ac.uk

Paying attention to mathematical practice is easier said than done. Mathematical practice must become the matter of some determinate academic discipline before it can yield any philosophical fruit. Otherwise we will not move beyond anecdotes and the uncritical reproduction of mathematicians' folklore. But which discipline? Formal Logic, History, Sociology and (why not?) Evolutionary Psychology are all candidates (there are others). In this talk I will argue that the obvious answer—all of the above—is unworkable because at least some of these candidates (History and Logic) have incompatible presuppositions. I shall then sketch some practical consequences for the project of studying the epistemology of mathematics through mathematical practice.

T Lengnink

Didactical Perspectives on Mathematics and its Philosophical Implications

Katja Lengnink
Universität Darmstadt
lengnink@mathematik.tu-darmstadt.de

A key focus of mathematical didactics is the area of mathematical learning. This requires a special view on mathematics and its internal structure. The presentation will carve out that mathematics can not be seen as an objective body of knowledge, legitimized by proofs, when approached from the perspective of mathematical learning. Instead, the subject of mathematics has to be interpreted as a discursive discipline, developed by human beings within their social and cultural environment.

T Moiseev

Towards a Platonic Epistemology of Mathematics

Vyacheslav Moiseev
University of Moscow (Russia)
vimo@list.ru

A logical system, so called Projectively Modal Ontology, is described. The formal language of St. Lesniewski's Ontology and ideas of Platonic Philosophy are united in the system. Many mathematical objects (e.g. variables, functions, vectors etc.) have a basic semantics where principles can be restricted by conditions to form aspects of the principles (relation of principle and its aspects is like the relation of a three dimensional body and its two dimensional projections). Projectively Modal Ontology presents this semantics in a formal way. Language, axioms, definitions and theorems of the system are considered in detail such that a more platonic version of Foundations of Mathematics (with no sets but "unities" as primitives) can be presented.

T Mühlhölzer

Mathematics and the Lifeworld

Felix Mühlhölzer
Universität Göttingen
fmuehlh@gwdg.de

Despite its abstract nature, mathematics is firmly rooted in our lifeworld. This is not only true in the obvious sense that the historical development of mathematics and that mathematical education start with the lifeworld, but – as I want to show in this paper – also in the much deeper sense that even the most abstract

concepts of mathematics correspond with concrete and pictorial lifeworld notions. This correspondence is vital for our creative understanding in mathematics, and it is documented by the fact that mathematical understanding aims at making theorems obvious or even trivial.

Petersen **T**

Some Wittgensteinian Reflections on the Jaffe/Quinn-Thurston Debate on Theoretical Mathematics and Mathematical Proof

Oliver Petersen
Universität Duisburg-Essen
oliver.petersen@uni-duisburg-essen.de

On the one hand Ludwig Wittgenstein holds that a mathematical proof constitutes the meaning of the sentence proven. This has such counterintuitive consequences as 1) that an undecided mathematical sentence has no meaning and is not understood, 2) that by creating a proof you change the meaning of the proven sentence, i.e., when you have finished the proof you have proven another sentence than the one you started with, 3) that no unambiguous sentence can be proven by different proofs. On the other hand Wittgenstein explicitly denies some of these counterintuitive consequences, especially 1) and 3). How can this apparent inconsistency be explained? I think an interpretation of Wittgenstein's ideas which explains away this apparent inconsistency also has the merit of shedding some light on the debate between Quinn/Jaffe and Thurston about the role and importance of mathematical proof.

Pejlare **T**

Felix Klein and the Role of Intuition in Mathematics

Johanna Pejlare
Uppsala University (Sweden)
johanna.pejlare@math.uu.se

David Hilbert's (1862-1943) formalization of geometry around the turn of the last century led to a battle among mathematicians whether you can do without intuition in mathematics, or not. Among others Felix Klein (1849-1925) believed that mathematics can not be treated exhaustively through logical deduction and considered it to be necessary to use intuition in mathematics. He emphasized the importance of interaction between the intuition and the results attained through the axioms. I will investigate Klein's thoughts and ideas about intuition and the distinction he makes between what he calls naïve and refined intuition. Further, I will discuss Klein's belief that the axioms are not truths a priori, but the result of the idealization of our inexact intuition.

T Rodin

From Set-Theoretic Foundations to Categorical Integration

Andrei Rodin
Ecole Normale Supérieure, Paris (France)
rodin@ens.fr

The Category Theory (CT) is progressively replacing (or complementing) the Set Theory (ST) as a mathematical lingua franca in which mathematicians working in various areas present their results and communicate them to each other. This suggests the view on CT as a “new foundations” of mathematics alternative to standard ST-foundations like ZFC. A list of first-order axioms for CT can be indeed written down (like in the case of ST), and a foundation of mathematics as a (or even “the”) category of categories can be reasonably argued for (see Lawvere F.W., 1966, “The Category of Categories as a Foundation of Mathematics” in: La Jolla Conference on Categorical Algebra, Springer). However, in my view, this approach doesn’t capture entirely what is potentially involved in the conceptual change “from sets to categories”. It seems me more pertinent to rethink the standard notion of foundation (relevant to mathematics), and in particular the idea of Hilbert-style axiomatics, from the new categorical perspective.

In my talk I shall argue that CT should be seen as a powerful means of mutual interpretation of mathematical concepts and theories allowing for integration of mathematics into a connected whole rather than a foundational core to which mathematical theories are ultimately reduced, and that CT provides an epistemic integration rather than foundations of mathematics in the usual sense. I shall show that the Hilbert style axiomatic is a special (“centralized”) case of the categorical integration. I shall also argue that the claim according to which CT supports the structuralist view on mathematics cannot be accepted without a profound revision of this view. Finally, I shall show how the old problem of relationships between mathematics and logic looks from the categorical perspective.

In 1870 Jordan proved that the composition factors of two composition series of a group are the same. Almost 20 years later Hölder (1889) was able to extend this result by showing that the factor groups, which are quotient groups corresponding to the composition factors, are isomorphic. This result, nowadays called the Jordan-Hölder Theorem, is one of the fundamental theorems in the theory of groups.

The fact that Jordan, who was working in the framework of substitution groups, was able to prove only a part of the Jordan-Hölder Theorem is often used to emphasize the importance and even the necessity of the abstract conception of groups, which was employed by Hölder (see, for example, Wussing 1984, van der Waerden 1985, Nicholson 1993, and Corry 1996).

However, as a little-known paper from 1873 reveals, Jordan had all the necessary ingredients to prove the Jordan-Hölder Theorem at his disposal (namely, composition series, quotient groups, and isomorphisms), despite the fact that he was considering only substitution groups and that he did not have an abstract conception of groups. Thus, I argue that the answer to the question posed in the title is “Yes”.

I suggest two possible reasons for why this observation been overlooked by most commentators on the development of group theory: First, Jordan’s paper received only scant attention and reviewers did not even mention his use of quotient groups (e.g., Netto 1875). Second, at first sight Hölder’s own proof indeed appears to rely essentially on the use of abstract groups. Nevertheless, there are later proofs of the Jordan-Hölder Theorem (e.g., in Weber 1896) which make only use of methods and conceptions that were available to Jordan in 1873.

Thus, I conclude that it was not the lack of the abstract notion of groups which prevented Jordan from proving the Jordan-Hölder Theorem, but the fact that he did not ask the right research questions that would have led him to this result. In other words, the historical episode discussed above shows that mathematical progress depends in part on considerations that go beyond purely mathematical ones.

Stöltzner **T**

On the Ontology of Theoretical Mathematics

Michael Stöltzner
Universität Wuppertal
stoeltzn@uni-wuppertal.de

The spectacular success of string theory in geometry has divided the mathematical community. Many heuristic proofs advanced by string theorists could be made rigorous by geometers. But there were failures as well. Apart from sociological concerns, among them who should be credited for a result, mathematicians wondered about the ontological status of such non-rigorous results. Were they more than simple conjectures? The eminent mathematicians Arthur Jaffe and Frank Quinn have proposed to consider them as “theoretical mathematics”, because theoretical results still

T Schlimm

On the Importance of Asking the Right Research Questions: Could Jordan Have Proved the Jordan-Hölder Theorem?

Dirk Schlimm
McGill University (Canada)
dschlimm@gmail.com

This paper consists of a historical and a philosophical part. In the first one I will clarify a common misconception about the development of group theory, in the second part I will draw some philosophical conclusions based on these findings.

require independent corroboration. In my contribution I discuss ways how to obtain a suitable ontology for theoretical mathematics.

A natural first candidate is Lakatos's quasi-empiricist ontology of mathematics. On this account, heuristics and conjectures are the driving force of mathematical progress. Proofs themselves are constantly refined thought experiments. But considering theoretical mathematics as informal ancestry fails to account for the axiomatic character of the concepts relevant for today's mathematical physics, including string theory. Axiomatics, to Lakatos's mind is merely justificationist, if not dogmatic.

In this respect, John von Neumann's opportunistic axiomatics performs better. Insisting that the best inspirations of mathematics stem from the physical sciences, he proposed a set of pragmatic criteria of success, some of which are distinctive for mathematics. In virtue of its great reliability and its ability to rigorously define the limits of a certain concept, mathematics occupies a special position among the sciences. Freedom in concept formation is counterbalanced by the interaction with the empirical sciences which is necessary to prevent aestheticism. Within von Neumann's pragmatist ontology, axiomatization is thus both exploratory and justificationist. One may always hope that opportunistic strategies can be given a sound mathematical meaning. At this point, von Neumann reverberated Hilbert's optimism about the applicability of mathematics in the sciences.

As recent work of Mark Wilson has shown it is quite difficult to disprove mathematical optimism even in the domain of applied mathematics. Combining this perspective with the earlier approaches teaches us that the problem of the ontology of theoretical mathematics is not one-sided. Setting up mathematical structures is, on pain of aestheticism, not fully independent of how these structures are applied to real-world problems. Mathematicians might thus constantly oscillate between an opportunistic stand, driven by the quest for application, and an optimist stand, in which one tries to give mathematical meaning to successful application strategies.

T Strobach

Teaching Logic with Good Conscience - Experiences in Moderate Formalism

Niko Strobach
Universität Rostock
niko.strobach@uni-rostock.de

Is it possible to explain to beginners what formal logic is really about while teaching it? Can it be taught in such a way that nothing that has been sold before as a law of logic has to be renounced when it comes to non-classical logics? The traditional approach starts from inferences in natural languages and arrives, by step for step abstraction, at "formalization". I tried to do it the other way around, starting with uninterpreted formal semantics (though not uninterpreted calculi) as games

alongside with and independently of the informal reasoning and linking the two later on. This can be seen as a way of making Hilbert's basic idea fruitful for teaching logic by transferring it to formal semantics. Does it work? Well enough to have resulted in a logic book that follows the new method (Niko Strobach, Einführung in die Logik, Darmstadt: WBG 2005) and to provide me with interesting classroom experiences to discuss.

van Kerkhove **T**

Informal Aspects of Mathematics

Bart van Kerkhove
Vrije Universiteit Brussels (Belgium)
bvkerkho@vub.ac.be

Most mathematicians appear to uphold a hybrid conception of mathematical truth. On the one hand, they entertain a consensual notion of it, whereby what is true is what is accepted by the community. On the other hand, as this seems to open the door to relativism, they also defend its objectivity, in terms of correspondence (realism) or coherence (formalism). This hybridity might be framed in Peircean terms, with discursive or social practices not diminishing but enhancing objectivity, which is only ever asymptotically reached. In any case, the community-model of mathematical practice is of high practical value, since mathematicians are at least as interested in whether provisional results can be relied on, as in their ultimate verity. Consequently, trustworthiness is a very important issue to mathematicians. However, recently, there seems to be a dramatic increase in the essential 'informality' of mathematical results, provisional or definitive, with a rising number of extremely long, complicated, digital, specialized, experimental or otherwise elusive proofs putting to the test the limits of human's mechanical or intuitive mathematical powers. Might we be at the dawn of a new crisis and/or revolution in the philosophy of mathematics? In this paper, some of the empirical material that is possibly relevant to effectively coping with this question is presented.

Zoglauer **T**

Can Mathematics be Naturalized?

Thomas Zoglauer
Universität Cottbus
zoglauer@tu-cottbus.de

Mathematical naturalists like Putnam and Quine reject the traditional separation between mathematics and the natural sciences. As a consequence, mathematical theories are subjected to the same empirical confirmation procedures as scientific theories. If the terms of our best scientific theories typically refer to real objects, then also the mathematical terms of our best sci-

entific theories must refer to real entities. I will reveal two severe problems that threaten the naturalization program.

1. The problem of underdetermination: A consequence of Quine's holism is the underdetermination of theories. It means that two theories might be empirically equivalent (i.e. they cannot be distinguished on empirical grounds), but logically incompatible. If mathematics is an integral part of science, underdetermination also affects mathematical theories. The controversial axioms of set theory (axiom of choice, continuum hypothesis, axiom of constructibility) might be a source of underdetermination. But underdetermination stands in conflict with scientific realism, at least that kind of realism that claims a convergence of theories (convergent realism). The dilemma for the naturalist is thus: either he accepts underdetermination or convergent realism, he cannot have both.

2. The problem of ontology: The naturalists are not quite clear what ontological status mathematical objects have and in what relation they stand to physical objects. Are they both equally real or do mathematical objects depend on physical objects or vice versa? Mathematical naturalists always waver between platonism and constructivism. I will argue that the only way out of this dilemma would be to decide between the alternatives.