

**Nickolay Milkov and Volker Peckhous, eds. *The Berlin Group and the Philosophy of Logical Empiricism*. Springer, 2013.**

*Clark Glymour*

Alumni Professor

Department of Philosophy

Carnegie Mellon University

e-mail: cg09@andrew.cmu.edu

This volume is a collection of essays, most of them focusing on philosophers within and around Reichenbach's Berlin group. Inevitably in such collections the quality of the contributions is uneven, but some pieces provide insights that are otherwise not to be found in the secondary literature. The editing can be faulted only for the inconsistency of quotations: in some essays, quotations are in English, in others in German, and in others in English with German footnotes. The breadth of readers would I think prefer the last. Readers may be disappointed that most of the authors write as reporters, without assessment of the views they describe. That may be because what is reported is too thin to support an assessment.

The essays focus chiefly on personal relationships and differences in metaphilosophical viewpoints and general epistemological doctrines, especially differences with Kant or readings of Kant. Some of the essays recount aspects of these viewpoints and differences in some detail, but, with the passing exception of Flavia Padovani's discussion of Kurt Lewin on genidentity, Christian Thiel's devastating essay on Walter Dubislav, and the part of Arkadiusz Chrudzinski's essay on formalization of Gestalt theory by Paul Oppenheim and Kurt Grelling, none of them detail attempts of members of the Berlin group to substantively contribute to mathematics or the sciences, the philosophical ambition for which the Berlin group is best remembered. You will not find any details of Lewin's field theory in psychology, or of Reichenbach's axiomatic for relativity or quantum logic or theory of probability. Critical evaluation of the Berlin group's doctrines and attempted scientific contributions is largely absent except for Thiel's essay. Except incidentally in Andreas Kamlah's remarks on Reichenbach's youthful associations, there is scarce discussion of the political currents of

the time or of their role in the relations between Carnap, Reichenbach, and others. So take the volume for what it is and learn what you can from it.

After a valuable introductory survey by Milkov of the relations between the Vienna and Berlin philosophical communities, I learned from Helmut Pulke of the neo-Friesian school of followers of Jacob Friedrich von Fries, an early 19<sup>th</sup> century Kantian and active anti-Semite.<sup>1</sup> On Kantian grounds, that group--chiefly in Gottingen--which included Leonard Nelson, remembered by Wikipedia at least for the "Grelling-Nelson" paradox, and Paul Bernays, opposed the theories of relativity, and of course physical non-Euclidean geometries. Milkov does not detail their arguments, other than to suggest that they are reminiscent of Kant's Third Analogy of Experience, about which I shall not reminisce. Naturally, Reichenbach saw the neo-Friesians as squarely in the opposition, but Pulke concludes that Fries' philosophy "could have been a stimulating source and point of systematic orientation for Reichenbach" (p. 61). Pulke's remarks on Fries' empiricism notwithstanding, it's hard to see how.

Jeremy Heis' essay is on the influence of Ernst Cassirer on Lewin and the contrast between Cassirer's views and Reichenbach's on almost everything except that philosophy should concern itself with the sciences. Both studied with Cassirer, and Lewin accepted or adapted some of Cassirer's views, while Reichenbach edged away from Cassirer's version of neo-Kantianism and particular epistemological theses. One neglect in this useful essay is notable. For Cassirer the "givenness of 'bare' sensation...proves ...to be a fiction" (p. 90), a

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<sup>1</sup> See his *Über die Gefährdung des Wohlstandes und des Charakters der Deutschen durch die Juden*, Heidelberg. Mohr and Winter, 1816.

claim that Heis contrasts with Reichenbach's opinion at the time, but fails to note that by the publication of *Experience and Prediction* Reichenbach had adopted something very close to Cassirer's view: "bare sensations" are not given; while perhaps not fictions, they are entirely theoretical objects.

Padovani's essay on Lewin and Reichenbach (mostly Lewin) on genidentity is interesting enough to make one want to read Lewin. Or read Lewin again. Full disclosure: I wrote my master's thesis on genidentity. When I presented the bound volume to him, Wesley Salmon, my advisor, asked me only one question: how did I know that was the same thesis he read? Of course, in Lewin's sense of genidentity, it wasn't, but the epistemological aspect of identity through time—how we can know when two appearances are of the same object—is neglected in both Reichenbach and Padovani's Lewin. She gives a clear account of Lewin's thought that interconnected causal sequences could provide a universal time order, and of the influence of that idea on Reichenbach's first proposal in the 1920s for a direction of time. But her presentation of Lewin's attempts to reduce time to genidentical "series" is a bit maddening in its reluctance to comment. Lewin, not fully loose from Kant, assumes a causal order and begs question after question. Missing from her essay is the denouement: Reichenbach's unhappy but prescient attempt in the *Direction of Time* to reduce causal order to statistical order.

I found little in Michael Stolzner's essay on Reichenbach's claimed anticipation of quantum mechanical indeterminism that is not described better in other sources, the *Stanford Encyclopedia of Philosophy*, and Frederick Eberhart's essay on Reichenbach's doctoral thesis.<sup>2</sup>

Kamlah's essay discusses Reichenbach's notion of volition and its connection with his views on free will and ethics. The line is that Reichenbach joined several anti-authoritarian organizations as a student, which bolstered a sense of individual choice that echoed through his conventionalism—for what is a convention but a shared choice?—and into his ethics. The essay is a useful introduction to a part of Reichenbach's thought that is justly neglected. I will do a little injustice. Reichenbach avoided the usual fallacy that quantum indeterminacy implies

freedom of the will, recognized that determinism makes "free will" something between a pun and an oxymoron, but insisted on "volitions." Without attempting an analysis of what Reichenbach could have meant—and in fairness there is not much in Reichenbach's writing to guide an interpretation—Kamlah's suggests that Reichenbach was a compatibilist along the lines of Harry Frankfurt. Perhaps he was just inconsistent.

Kamlah is brief, and I think not entirely on point about Reichenbach's ethics. Everyone is free to hold his own moral views and claim they apply to everyone, Reichenbach says, but what Kamlah insufficiently describes is how Reichenbach thought that would produce a stable, just moral code. Reichenbach was a Marxist anti-Hobbesian (although I have no idea if he ever read a word of Hobbes. I bet he read some of Marx.) His Marxism is evident in his remarks in *The Philosophy of Space and Time* where the scientific community is described as producers, like farmers and factory workers. A producing community makes mistakes, leaves things undone, goes awry—the farmers plant Brussel sprouts instead of cabbage, fail to put their equipment away, neglect the soil (my example, not Reichenbach's). Labor groups require supervision and help, best from outside the group. So, too, scientific workers can get tilted and leave things undone, and the task of philosophers of science—also laboring, Reichenbach imagined, as a coherent factor of production—is to take note and help straighten things out where needed. Reichenbach's vision seems to have been of philosophers in agreement functioning as quality controllers, some combination of the clean up crew and the commissars.

Hobbes famously argued that the state of nature is war, each man against each man. Reichenbach seems to have thought that the state of nature is cooperation, tolerable disagreement, and civil conversation. Let's plant a garden and have a beer. While each person may have his own moral code chosen by volition, "social friction"—Reichenbach's phrase—will work out the compromises. Reichenbach's Pollyannaism knowing the horrors of the Third Reich is breathtaking. Perhaps he regarded World War II as social friction.

Thiel's remarkable essay on Dubislav's logic details Dubislav's failed attempts to provide a decision procedure for classical monadic logic, which had already been provided by Lowenheim. Vienna had Godel, Berlin had Dubislav. No contest.

<sup>2</sup> F. Eberhardt, 2011. Reliability via synthetic a priori: Reichenbach's doctoral thesis on probability. *Synthese*, 181: 125-136.

Temio van Zanwijk adds a dense but readable essay on Dubislav's views on transcendental arguments, and Anita Kasabova provides a transitive historical essay on Dubislav on Bolzano.

The essay on Kurt Grelling by Volker Peckhaus evokes a sadness for the man, who accomplished less than might have been hoped of him. One thing he did accomplish in collaboration with Paul Oppenheim was a logical reconstruction of the claims of the Gestaltists, admirably presented by Arkadiusz Chrudzinski.

From the essay by Paul Ziche and Thomas Muller, I learned something of Paul Oppenheim's intellectual background before meeting Carl Hempel. Oppenheim was focused on ordering and unifying the sciences from mathematics to metaphysics, along multiple dimensions, a vision he continued in later years in collaboration with Hilary Putnam, notwithstanding Hempel's criticism that a science of metaphysics was a step too far. I get a sense from Oppenheim's role in several Berlin circle projects that he was much more than a wealthy hanger-on and sponsor, more a significant and talented figure than I had thought.

In "Carl Hempel: Whose Philosopher?" Nikoly Milkov presents a rather annoying account of Hempel's intellectual development. Much of the essay is devoted to discussing other figures, and the essay has its disputable throwaway lines: "Wittgenstein's method of conceptual analysis is what grounds Carnap's (and thus Hempel's) method of explication" (p.304).

On the interesting question of why, in his last years at Princeton, Hempel endorsed Kuhn's historicism, anti-formalism and epistemological relativism Michael Friedman has argued it was the belated influence of Otto Neurath, but Milkov claims it was Reichenbach. Surely, neither. Although until he left for Turkey, Reichenbach was Hempel's thesis director, Hempel fell early under Carnap's sway and never after essayed philosophy of science devoted to analyses and reconstructions of particular scientific theories—Reichenbach's kind of philosophy of science. Until Carnap's English improved, Hempel was Carnap's voice to Anglophone philosophers. Carnap, throughout much of his career, held anti-foundationalist and radically relativist views, with no concern for reliabilist epistemology, and Hempel endorsed those views, sometimes by omission. The aim of science, he famously announced, is explanation, not *true* explanation. Truth, Hempel insisted, is a notion of

which science has no need. Judging from his assessment of Thomas Kuhn's contribution to the *Encyclopedia of Unified Science* Carnap seems to have seen *The Structure of Scientific Theories* as an historical vindication of his philosophical views. Hempel's turning point came when Kuhn, with his radical imagination, offered to be Hempel's teaching assistant and did. The teacher became the taught, it all fit together in Hempel's mind, and, much to the shock of his audience just before his retirement there, Hempel gave a public lecture at Princeton endorsing Kuhn's views.<sup>3</sup>

The concluding essay, by Erich Reck, is a disappointing discussion of Hempel's covering law model of explanation, much of it focused on whether Hempel's theory was a Carnapian explication or something else (answer: a Carnapian explication, with addenda), and in passing celebrating Hempel's influence in philosophy of science. That Hempel had such an influence is undeniable, that it has been benign is certainly deniable. Few sentences in 20<sup>th</sup> century philosophy of science have sent the subject on a snark hunt so well as "The aim of science is explanation."

Since the volume contains no assessment of the work of the Berlin Group—except for Dubislav—I offer mine. Many of these essays reveal the heavy burden the logical empiricists carried from the emanations of Kant's diseased brain.<sup>4</sup> While Reichenbach surely thought he had freed himself from Kant, in the most important respect he never did. Kant changed the game of philosophy of science from contributing to science and methodology to commentary, to clean-up, to "foundations," and throughout his career Reichenbach played Kant's game, the game that dominates philosophy of science to this day. Others in the Berlin group, Lewin, and the not quite competent Dubislav, even Grelling and Oppenheim in their attempts to clarify Gestalt ideas that really needed clarification, had larger scientific ambitions.

<sup>3</sup> My authority is first hand. Hempel was my colleague and neighbor in those years (I mowed his lawn), and while we were not intimate, the families were close enough that my wife, Alison Kost, helped at his bedside while he was hospitalized when we were colleagues again in Pittsburgh.

<sup>4</sup> That Kant had a brain tumor that affected his habits, writing, and manner of thought is no certainty but more than a slur. See J. Marchand, (1997). Was Kant's dementia symptomatic of a frontal tumor? *Revue Neurologique*, 153: 35-39.

Reichenbach offered an axiomatization of relativity on grounds he thought epistemologically legitimate, and did so less well than English writers had already done; he developed an incoherent quantum logic after a coherent one had been offered by Birkhoff, and a probability theory that disappeared in the light shown by Kolmogorov's. In each case, Reichenbach was doing what he took to be semantic and epistemological clean-up, not successfully. We have a saying for that in my home state Montana: Day late, dollar short.

If Reichenbach was not useful to scientific production, at least he was not a hazard. Hempel was, or tried to be. In Hegel's footsteps, Hempel announced the impossibility of a kind of science, or at least technology. For Hegel it was the number of planets, for Hempel it was machine learning. No machine, he announced, could do science because science requires the introduction of novel concepts, and no machine could do *that*. Ironically, DENDRAL, the first commercial machine learning program, deliberately used some of Hempel's ideas on explanation, contributed by Hempel's doctoral student, Gerald Massey.

This volume focuses almost exclusively on the relations of Berlin logical empiricists with other philosophers. Scientists are mentioned—Erwin Schrodinger's correspondence with Reichenbach is discussed briefly, and there is an opaque reference to Max Born, and unsubstantial references to Einstein. But one wonders what the German scientists really thought of the group and its members. (There is a challenging Ph.D thesis in the waiting.) Reichenbach obtained a university teaching position in physics on

the footstool of his Axiomatic, but Herman Weyl was all but incensed that the local construction of the work failed to capture global properties of Minkowski space-time. Weyl took Husserl's phenomenology more seriously and acknowledged it in the first edition of *Space, Time, Matter*. John von Neumann pointed out that Reichenbach did not get inertial frames right in special relativity. At best, Einstein seems to have thought of Reichenbach as a valuable cheerleader for relativity, and he was.

If we gave awards to philosophers of science only by their contributions to science, we would need few medals, and for the Berlin group and the Vienna Circle there is a broader and more important judgement. In a time of intellectual, political and moral darkness, they sought enlightenment.

the links between the hermeneutics of science and rhetoric of science.

Comparing this book to some another works of the author (*A Passage to the Idea for a Hermeneutic Philosophy of Science, Essays in the Hermeneutics of Science, Critique of Epistemological Reason: Perspectives to Philosophy of Science, Art Criticism and Multiculturalism*) at least two minor comments can be made. There can be seen in these books an admirable dedication to developing a valid, original, and coherent version of a hermeneutic philosophy of science. Nowadays this intellectual adventure has significant outcomes, such as the elaboration of the context of constitution, which has already appeared in this book and will play a central role in the next book of the author, entitled *The Context of Constitution: Beyond the Edge of Justification*.