

REVIEWS

JEAN LEROUX, *Une histoire comparée de la philosophie des sciences*, Volume I: *Aux sources du Cercle de Vienne*, Volume II: *L'empirisme logique en débat*, Les Presses de l'université Laval, Québec (Canada) 2010

As Paul Feyerabend once remarked, philosophy of science is a subject with a great past. Let me for the moment leave aside his disillusioned impression that it had only a sad present and no future and concentrate on its past. It is surprising indeed that much has been published on the history of science in the last few decades, while only very few efforts have been made to give an overall description of the history of philosophy of science. That of course presupposes a definition or at least a rough idea of the subject. And along with that goes an answer to the question when it started and what has been part of it during its development. Some (as for instance John Losee, who in 1972 published the first “Historical Introduction to the Philosophy of Science”, an early sort of history of the discipline) seem to think that philosophy of science already began with Aristotle’s *Analytica Posteriora*, while others would be inclined to have it start more than 2000 years later, let’s say with the Vienna Circle.

Jean Leroux, the author of the two volumes under discussion here, makes a wise decision when he dates its beginning somewhere in between (but not just in the middle). He takes as a starting point the natural scientists (like Helmholtz and Hertz) of the late 19th century discussions on the foundations of their disciplines and then continues his history with the professionalization of the subject in the Vienna Circle and the Berlin Group. Quebec-based—and thus of course French-speaking—Leroux is known mainly for his logic textbooks and some fine articles in the philosophy of science. Now, however, he brings his knowledge from many years of teaching philosophy of science to bear on the two volumes. This, of course, includes knowledge of the French connections. But he also studied in Germany in the 1970s and thus acquired first-hand knowledge of the German debates, which are otherwise largely neglected as a consequence of language barriers in a field mostly dominated by Anglo-Saxon players.

Leroux divides his history into two parts. The first one (*Aux sources du Cercle de Vienne*) focuses on the forerunners of the Vienna Circle and on the circle itself. By forerunners he has especially in mind active scientists who kept an interest in the philosophical foundations of their disciplines. These are Hermann Helmholtz and Heinrich Hertz on the German side and Henri Poincaré and Pierre Duhem on the French. This strategy seems particularly appropriate given the fact that both Wittgenstein and Schlick were much influenced by Hertz and Helmholtz, and the

so-called first Vienna Circle was influenced not only by the local heroes Mach and Boltzmann, but also by Poincaré, Duhem and other French conventionalists.

The chapters dealing with the Vienna Circle itself sum up the vast recent literature on this singularly influential group.

The second volume (*L'empirisme logique en débat*) is devoted to an analysis of two classical examples of philosophy of science in the Logical Empiricist style by Rudolf Carnap and Carl Gustav Hempel. It then continues with a criticism of two contemporaries and critics of the Vienna Circle (Karl Popper and Gaston Bachelard), before it then turns to the historization of philosophy of science in the work of Thomas S. Kuhn and the reactions to that movement by two former Popperians: Imre Lakatos and Paul Feyerabend. In some chapters the description opens up perspectives onto more recent discussions. That happens for example when Leroux describes and discusses the Sneed/Stegmüller-approach to formalizing the Kuhnian picture of scientific revolutions (pp. 111-114). Here Leroux can rely on a series of papers he published in the late eighties. Erhard Scheibe, who died in 2010, was also one of the leading German philosophers of science who contributed to the so-called structuralist view of scientific theories and who is mentioned and discussed by Leroux.

The two volumes, each comprising approximately 200 pages, are self-contained (including their respective extensive bibliographies) and can serve well as textbooks for courses and seminars in the philosophy of science. But they also are useful for scholars and experts in the field who are interested in gaining an overall picture of the development of their discipline. Of course the new histories of the philosophy of science written by Leroux and Carlos U. Moulines (see the review by Thomas Mormann in this volume) are only a beginning. They both rely exclusively on published material. And as in historiography in general, many surprising insights in the history of philosophy of science are only to be gained by delving into unpublished papers and correspondences.

Hans-Joachim Dahms (Wien)

ILKKA NIINILUOTO AND HEIKKI J. KOSKINEN (eds.), 2002, *Wienin piiri*, Helsinki: Gaudeamus (261 pp.).

In Paul Edwards's prestigious *Encyclopedia of Philosophy* (1967) John Passmore wrote that logical positivism is about as dead as a philosophical movement ever becomes. Yet according to a new anthology on the Vienna Circle, written in Finnish by Finnish philosophers, the Circle and its philosophy—logical positivism or empiricism—is currently the subject of growing research, in addition to an ex-

tensive reassessment of the history and legacy of the movement. Was Passmore's obituary thus untimely?

The influence of the Vienna Circle on Finnish philosophy originated with Eino Kaila who was in close contact with it, and who introduced the new logic and philosophy of science to students and colleagues at the University of Helsinki. The enormous achievements of G. H. von Wright and Jaakko Hintikka on philosophical logic would hardly have been possible without him. This tradition is continued by the current holder of Kaila's chair, Ilkka Niiniluoto who is strongly influenced by Karl Popper in his studies on the philosophy of science. It is thus quite appropriate that all these authorities have also contributed to this anthology on the Vienna Circle.

Ilkka Niiniluoto has written two articles. The first serves as an introduction to the historical background of the movement and to the new research on its historical significance. The second describes Eino Kaila's philosophical development and career and his relationship to the Vienna Circle. It is interesting to note that Kaila was critical of phenomenalism and the verification thesis. Moreover, he provided the first detailed critique of Carnap's constitution theory, which was presented in *Der Logische Aufbau der Welt*. According to Niiniluoto, Kaila was actually an antireductionist realist who tried to combine his view with empiricist epistemology.

Juha Manninen gives an in-depth treatment of the birth of the circle and its manifest *Wissenschaftliche Weltauffassung: Der Wiener Kreis*. He emphasizes that in spite of the manifesto there never were any common doctrines accepted by all its members. There was in fact a hot dispute between two camps: one composed of Schlick and Waismann, who were close to Wittgenstein, and the other composed of Neurath and Carnap. Neurath in particular was very critical of Wittgenstein, accusing him of being a metaphysician and a mystic. He objected to phenomenalism and the verification thesis advocated by Schlick and Waismann, arguing that it is untenable metaphysics to think that one can compare sentences directly to experience. He was a physicalist and a coherentist. Carnap's views developed more closely to Neurath's. Manninen says that it is wrong to assume that the doctrines of the circle are dead, since there never were any common doctrines.

Leila Haaparanta relates how the philosophical content of Frege's logic changed in the hands of the Viennese philosophers. For Frege, philosophy is Kantian transcendentalism purified of all psychological elements. Logic reveals the pure forms that are the necessary conditions of the possibility of knowledge and experience. For the members of the Vienna Circle, on the other hand, there are no transcendental truths or any other philosophical truths. Logic is a tool for the analysis of language and the form of language. For this reason, epistemology became less significant, and linguistic symbols became the primary object of study.

Gabriel Sandu explicates Carnap's attempt to include the truth predicate in the syntax of language in *Logische Syntax der Sprache* (1934). Unlike Wittgenstein Carnap thought that it was possible to talk about the syntax of language in that

very same language. Yet this view makes it problematic to include the truth predicate in the syntax: the important distinction between the formal mode of speaking and the material mode of speaking is erased and paradoxes result. The paradoxes could be avoided if the truth predicate were included in the meta-language, but this was incompatible with Carnap's syntacticism, which attempted to reduce meta-language to object language. Sandu reminds us that the discussion on the definability of truth in the language itself continues in modern logic, in the work of Jaakko Hintikka, Tapani Hyttinen, and Sandu himself.

Arto Siitonen writes on Hans Reichenbach who had his own philosophic-scientific discussion group in Berlin. According to Reichenbach, the task of science is to make predictions on the basis of observed regularities. Unlike Schlick, he thought that knowledge is based on probabilities and cannot attain certainty.

Georg Henrik von Wright reminisces how his original plan to do post-graduate studies in Vienna foundered when Schlick was murdered, and the Circle disbanded. Von Wright subsequently went to Cambridge, in which Wittgenstein, to his surprise, had also settled down. Having been invited by Wittgenstein's sister to visit Vienna in 1952, he participated in a seminar led by Victor Kraft who was retiring at the time and who proclaimed that session to be the last one of the Vienna Circle. Afterwards, von Wright received a letter from Kraft who asked him to become his successor. Von Wright likes to think that the spirit of the Vienna Circle and Wittgenstein might have survived longer in Vienna had he accepted the offer.

Pertti Lindfors writes about Georg Klaus who, according to Lindfors, continued the work of the Vienna Circle, but who, under the pressure of official Marxism in the DDR, was forced to transform his logical positivism into dialectical materialism. Klaus distinguished between dialectical contradiction and logical contradiction, and attempted to develop a general theory of the former. Lindfors does not believe that non-logical contradictions contain any interesting common features, but, due to Klaus, modern logic and cybernetics developed a stronger hold on Marxism.

Sami Pihlström searches for similarities between logical positivism and American pragmatism. He discovers pragmatic features particularly in Carnap's later philosophy and in Neurath. Both had a strong influence on W. V. Quine, who brought the pragmatization of empiricism even further. Pihlström is not just searching for historical connections. He believes that the pragmatic side of positivism deserves to be rehabilitated.

Heikki J. Koskinen relates how the ideas of the Vienna Circle were transformed in the course of their reception by W. V. Quine, perhaps the most influential American philosopher in the 20th century. Quine rejected two central dogmas of logical empiricism: (1) the distinction between analytic and synthetic truths and (2) reductionism. The result was holistic empiricism, in which a demarcation between meaningful science and senseless metaphysics cannot be drawn. Neither are there analytical or conceptual truths that could be studied by *a priori* philosophy. Metaphysics and philosophy become a part of empirical science. Metaphysics is

once again, due to Quine, a central subject of Anglo-American philosophy. It differs from traditional *a priori* metaphysics in that it takes the results of empirical science into account.

Metaphysics that was taken to be poetry or cognitively meaningless nonsense is thus returned to philosophy. Metaphysics and the classical problems of philosophy seem to be stronger trends in current philosophy than the pragmatism that Pihlström emphasizes.

Logical positivism had in fact a great deal of the spirit of the Enlightenment. The members of the Circle were inspired by the belief that all genuine problems concerning reality could be identified and solved with the help of new logical tools. Jaakko Hintikka shares this belief. He assures us that the philosophy of the Vienna Circle is not dead, and he attempts to defend it against unjustified criticism.

According to Hintikka, the philosophy of science advocated by Thomas Kuhn and others cannot replace logical positivism, because it does not deal with the same problems. Kuhn's question is how science is in fact done. The positivistic philosophy of science is concerned with the completely different question of the right method of science and its structure. The attempts of neo-positivists were problematic because of underdeveloped tools of logic. Hintikka believes that now we have the logical tools to answer the question of the right scientific method. He also criticizes the exaggerated conclusions that the Kuhnians draw from the theory-ladenness of observation, and defends the positivistic thesis of the analyticity of logic.

Not all would share Hintikka's optimism about the efficacy of logic in solving the problems of the philosophy of science. For them, a third alternative exists between Kuhn's historic-sociological approach and Hintikka's logistic approach: Philip Kitcher's and Alvin Goldman's social epistemology studies science as a social institution and attempts to evaluate its practices from a veritistic point of view. The central question is to what extent these social practices enhance the attainment of truth. Just like the positivists, both also defend the veritistic superiority of science compared to other practices.

The book *Wienin piiri* offers interesting new insights into the activities of the Vienna Circle. The essays are mostly historical. The reader may want more of the kind of assessment of the philosophical significance of logical positivism that Hintikka provides. In any case, the point is well taken. The movement was comprised of many different, even conflicting, doctrines, many of which are far from dead.

Markus Lammenranta (Helsinki)

THOMAS UEBEL, *Empiricism at the Crossroads. The Vienna Circle's Protocol-Sentence Debate*. Open Court, Chicago, Ill. 2007.

All of us now agree that the Vienna Circle was a tale of sound and fury. There are only a few diehards who would say that it meant nothing. Within and around the Circle there was a series of explosive intellectual developments and fresh insights into the presuppositions of contemporary science. The most prominent personalities of philosophy of science cannot avoid explaining how their heroes related to the Circle. Even the person whom many consider to be the greatest philosopher of the century, Wittgenstein, was in many ways (only partially studied) involved with the Circle. The ongoing rebirth of studies dedicated to the Circle is fuelled not only by the timely re-orientations of analytic philosophy but also—I would say, mainly—by archival studies concerning the Circle, by discoveries of forgotten sources.

Thomas Uebel has been a prolific writer on the Circle since the publication of his *Overcoming Logical Positivism from within: The Emergence of Neurath's Naturalism in the Vienna Circle's Protocol Sentence Debate* (1992), one of the best informed works on the subject. His new book is much more ambitious than the earlier one. Outwardly, it still contains the clumsy classification of the stages and sub-stages of the protocol-sentence debate, but it is actually both an up-to-date review of recent research on the Vienna Circle and an attempt to reconstruct some of its main arguments and to consider their relevance for contemporary research. Although all the three empiricists that deserve most attention in the book, namely Carnap, Neurath and Schlick, ended up accusing each other of different kinds of betrayals of empiricism, Uebel describes what he calls Carnap's and Neurath's "bipartite metatheory" as the winner of the debates. It consists of Carnap's logic of science with its different frameworks together with Neurath's suggestions for empirical, social studies of science. However, the tensions between the two were never resolved.

In addition to the original group, active already at the beginning of the twentieth century, the Vienna Circle was made possible by two of Moritz Schlick's friends, Carnap and Wittgenstein. Without the two of them, compatible only in the specific Viennese circumstances, the Vienna Circle would not merit such interest today. Schlick was more oriented towards supporting the work of others than revamping his own profile. This is a pity, because the study of the Circle's history urgently needs to focus on Schlick. The edition of complete works of Schlick, now underway, and the accompanying Schlick-Studien are the desired correction to this situation. On the other hand, it was Neurath who was the "big locomotive" of the Unity of Science Movement. Unfortunately, most writers on the Vienna Circle do not know his extensive correspondence with Carnap. In addition, Neurath's archive was lost to the Austrian authorities in 1934, then to Gestapo, and it can now be found in the Moscow War Archives. Nothing about it has been published so far. Crucial shorthand manuscripts by Waismann, illuminating Wittgenstein's connection with the Vienna Circle, still await transcription. The rediscovery of the Vienna

Circle has only reached a halfway point. If it continues there will be a number of surprises. Since the focus is on the legacy of unique philosophical pioneers, there is bound to be an impact on contemporary thought.

It is good to read Uebel's book together with the historical parts of A. W. Carus' *Carnap and Twentieth-Century Thought* (2007). Both of them provide new insights, although the picture is only partially similar. New sources are being uncovered and interpreted. The book by Uebel is the first extensive work drawing on Neurath's and Carnap's unpublished works immediately preceding their well-known writings on physicalism, with observations such as Carnap's short-time belief in two universal languages. Carus, on the other hand, reports on the highly valuable yet thus far completely neglected Carnap collection at UCLA and discusses, among a series of new interpretations, Carnap's Davosian sketch for a new system of logic, which is indebted to Wittgenstein. The time for a definitive book on the Vienna Circle has apparently not arrived.

I will not even try to recapitulate the rich contents of Uebel's book which will be recommended reading for a long time, especially as concerns the development of Neurath's thought, but also for the background of Carnap's physicalism. The book is more than a synthesis of Uebel's many earlier publications. It turns out to be a highly recommendable revision of Uebel's earlier views. But there are continuities, of course. For some reason Uebel's suggestion that private language arguments were quite common during the 1930s and especially important for Neurath has not caught fire among Wittgenstein scholars, although Wittgenstein should be discussed within this context, which is presented clearly by Uebel. And special attention should be given to Uebel's rich discussion of Neurath's theory of testimony.

Uebel is now able to give plausible evidence for Heinrich Neider's suggested defence of inter-subjective controllability in science, which is important for Uebel's interpretation of Neurath's and Carnap's development. Unfortunately, there is still no convincing document, and so the discussion may go on. One could add that Neider's dissertation opposed the idea of "understanding" as a specific cognitive mode. In his evaluation dated 26 June 1930, Schlick praised the work, but he also pointed out what he considered to be a shortcoming: "... when he says that it should actually not be permissible at all to speak about other minds (vom Fremdpsychischen) and derives from this his main argument against Dilthey and his followers." (Archives of the Vienna University, Philosophical Faculty, Rigorosenakt Heinrich Neider, 1930).

This does not mean that Schlick would have been opposed to naturalism or even physicalism. In his *General Theory of Knowledge* Schlick had written: "... spatio-temporal concepts may be used to describe any arbitrary reality, without exception, including the reality of consciousness." Further: "Physics is the system of exact concepts that our knowledge correlates to all reality. I say to all reality, since according to our hypothesis the entire world is in principle open to designation by that conceptual system. Nature is all; all that is real is natural. Mind, the

life of consciousness, is not the opposite of nature, but a sector of the totality of the natural.” (A. E. Blumberg’s translation of the 2nd edition, Open Court 1985, p. 295-6). When Schlick later referred to this as his acceptance of physicalism, Carnap quoted these passages and commented on them in a letter to Neurath on 15 May 1935: “This is not a vague anticipation; this is in itself the thesis of physicalism.” (Vienna Circle Archive, Noord-Hollands Archief, Haarlem, Otto Neurath: Korrespondenz, 220).

“Physicalism” was a word used by Schlick’s colleague Karl Bühler in his book *Die Krise der Psychologie* (1927) in order to refer to a standpoint which he did not find congenial. Neurath adopted it to replace his earlier self-made Marxist talk about the “materialistic basis” of all science, when Schlick had rejected the manuscript of his book *Der wissenschaftliche Gehalt der Geschichte und der Nationalökonomie* or the “Proto-Sociology” as Uebel calls it; a more militant draft than the one that was later printed.

Late in his life Neurath received from Carnap a letter commenting the quarrels surrounding the book rejection. On 23 August 1945, Carnap explained: “... since you ask so insistently what I meant when I spoke of your violent emotional reactions, I will mention the two occasions uppermost in my mind: your quarrel with Schlick about your manuscript, the second, your quarrel with me when I was in Prague and you sent the long wires from Moscow.” (VCA, Otto Neurath: Korrespondenz, 223). Carnap was especially referring to Neurath’s wish not to appear as a plagiarist of Carnap, much like Wittgenstein later on concerning the very same publication on physicalism by Carnap, though for different reasons. Carnap concluded:

... you deserved credit and I was glad to give it to you. What I minded was only the violent emotional way with outbursts and moral pressure by which you induced me to give you what seemed to me an exaggerated amount of credit. I gave it for the sake of peace and preservation of friendship. But I resent to the present day that this one time in my life I was bullied by another man into saying something not in accord with my conviction. (Ib.).

Uebel has great difficulty in describing Schlick’s standpoint in the protocol-sentence debate. In this he is not alone. I believe that the ongoing publication of Schlick’s complete works will clarify the matter, although Schlick’s views were in transition because of Wittgenstein’s continuing influence, as the archives in particular reveal. Still, a longer perspective than the one opened up by the intervention in 1934 is needed to understand what Schlick meant at that moment. Uebel reads Schlick as a foundationalist of some kind, because Schlick introduced to the debate something he called “affirmations”. One of Uebel’s summaries of this puzzling doctrine is the following: “... the epistemological problems of science cannot be solved by structural means: justification needs appeal to personal experience.” (Uebel, *ib.*, p. 450; cf. p. 442-445). The final evidence had to be something immediately given, incorrigible and certain, understood phenomenally.

I think that some continuity in Schlick's views can be found. The affirmations were not something new that was introduced in 1934. In his *General Theory of Knowledge* (Blumberg's translation, p. 165) Schlick wrote:

... the pragmatists (Peirce, Dewey in America, F. C. S. Schiller in England and others) did perform a genuine service by pointing out (specifically for assertions about reality) that there is indeed no other way to establish truth except through verification. This is actually of great importance. We add, however, the likewise important finding that verification always ends up in establishing the identity of two judgements. The moment it turns out that in designating a perceived fact we arrive at the same judgment that we had already on logical ground deduced for this fact, we become convinced of the truth of the tested proposition.

The original German expression for "to establish" was "zu konstatieren", i.e. to affirm. Did Schlick talk about "affirmations" in 1934 in a logical or epistemological sense? They were psychological for him. The affirmations were an answer to the question why an individual accepted something as true, but nothing more. This psychological question was different from the question of constructing a system of science and accepting it as true.

What did Schlick mean with the "logical grounds deduced for this fact"? Definitely not anything concerned with the immediately given. As I read him, he meant the very same as already in his dissertation on truth, when he was discussing the discovery of the planet Neptunus:

The inference which led to this discovery was drawn from two groups of premises. The first consisted of the principles of mechanics and the Newtonian law; the second was made up of a series of judgements about the 'perturbed' orbit of Uranus, and thus about observed facts. By purely mathematical transformations it emerged from these premises that as yet unknown attractive forces must be influencing the motion of Uranus, and once Leverrier had added the further premise: this attraction comes from a planet circling the sun beyond the orbit of Uranus, he could draw the conclusion: At a certain time, a planet will be found at a certain point in space; and from this proposition he could appropriately derive a new judgement of the form: "An observer who at a certain time and place looks through a telescope aligned in approximately such and such direction, will have a visual perception of such and such a kind." Some time later, as we know, Galle thereupon made an observation, on the strength of which he was able to assert this same judgement as a judgement of perception. Now because this perceptual judgement was identical with the judgement deduced by Leverrier, the premise he had hypothetically established concerning the existence of the new planet was held to be verified. Since then, by innumerable observations of the same kind, new verifications of the same truth have repeatedly taken place. (M. Schlick, *Philosophical Papers*, Vol. I, ed. by H. L. Mulder and B. F. B. van de Velde-Schlick, D. Reidel 1979, p. 75.)

For Schlick, all of science was hypothetical and corrigible, and the matter was not changed by his view that ultimately perception was also needed, a judgement "whereby an actual experience is immediately expressed". Uebel agrees with this, but then he draws the strange conclusion that in Schlick's epistemology it was

“all about the subject” (p. 454). Had Schlick changed his views radically on this matter? No such change can be seen in his book on general epistemology—see for instance p. 163 which actually repeats the early description taken from the history of science, only in more formal terms. When we turn to Schlick’s lecture on the foundation of knowledge from the winter term of 1933/34, we find him saying:

Thus observation plays the part of absolute certain knowledge, not when it is taken as a starting-point and stands at the beginning, but when it is arrived at through science ... The essence of science does indeed consist in making predictions. But the foundation and link with reality is not the predicting, but the fact of its success. Science makes contact with the real, not at the base, but at the apex; what matters in science is not what it rests on, but what it leads to. (M. Schlick, *The Problems of Philosophy in their Interconnection*, ed. by H. L. Mulder, A. Kox and R. Hegselmann, Kluwer 1987, p. 92).

This passage is quoted by Uebel, but in my opinion he makes too much of the rhetoric of “absolute certainty”, directed against Neurath. Downshifting this terminology we get exactly the same picture as in the earlier works, and actually Schlick said “plays the part”, not “is”. I agree that Schlick had a difficulty in formulating the affirmations, but this definitively does not mean that all was about the subject. There is no observation of dark matter or energy. It remains a speculative concept until something new is observed—or fundamental theories have changed. Both of these alternatives are possible from Schlick’s point of view. Despite the rhetoric of “immediacy” at the apex, Schlick remained somehow a realist in a specific sense. Here is his difference to the other empiricists he was opposing. And what is missing from inter-subjectivity, when “innumerable observations of the same kind” are made?

The strengths of Uebel’s book are to be found in his detailed discussions of the evolution of Carnap’s views through several different stages and in his reconstruction of Neurath’s theory of testimony. He tries to make the best of Schlick, but in my opinion not quite successfully. In any case, the book will be a treasure trove for some time to come.

Juha Manninen (Helsinki)

The Cambridge Companion to Carnap, edited by Michael Friedman and Richard Creath, Cambridge University Press, 2007, 371 pp.

The volume contains a collection of fourteen articles on Carnap’s philosophy by leading scholars in the field. The contributions follow (with two exceptions) a chronological order that begins with Carnap’s early work on the philosophy of geometry and concludes with his long-term project on inductive logic and probability. Thematically, the essays can be grouped as follows. (1) several papers

examining the details of Carnap's formative intellectual influences and his interaction with other philosophers (Carus, Gabriel, Ryckman, Pincock, Uebel, and Creath); (2) several essays concerned with more general philosophical themes in Carnap's work and their theoretical evolution (Mormann, Friedman, and Richardson); (3) several papers discussing more technical details of Carnap's philosophy of logic, mathematics, and general science (Reck, Ricketts, Awodey, Demopoulos, and Zabell). Friedman's excellent introductory essay gives an overview of several received views of Carnap's philosophy (promoted mainly by Ayer and Quine) as well as the more recent renaissance in scholarship that has led to a "more balanced and dispassionate understanding of Carnap's place within twentieth-century philosophy." He portrays Carnap's intellectual development from his early philosophical work on logic and mathematics to his mature *Wissenschaftslogik* by highlighting several unifying themes: a general anti-metaphysical and, at some point, anti-epistemological spirit, the analytic/synthetic distinction as well as an overall pragmatist orientation in Carnap's work.

Carus' article is closely related to his recent book *Carnap in Twentieth-Century Thought: Explication as Enlightenment* (2007) and presents a general account of Carnap's early intellectual background (in particular the German Youth Movement) as well as of his theoretical development from the "Aufbau project" to his later work on formal semantics and inductive logic. Carus concludes with a discussion of Carnap's "ideal of explication" in his work after 1935. Mormann presents a detailed account of Carnap's early philosophy of geometry with the main focus on latter's dissertation *Der Raum* (1922). He argues that several of the dominant topics in Carnap's later philosophy—in particular his conventionalism—are already present in the dissertation. With respect to his account of physical space, this "geometrical leitmotif" concerns a conventionalist understanding of the metrical structures of a space (with a given topological structure). Gabriel presents a detailed account of Frege's influence on Carnap's philosophy of logic. Carnap attended several of Frege's seminars on the *Begriffsschrift* and on *Logic in Mathematics* in Jena between 1910 and 1914. Gabriel outlines Frege's logic as documented in Carnap's lecture notes and discusses several traces of Frege's influence in Carnap's later work. Ryckman's article draws a number of parallels between Husserl's and Carnap's work on logic and mathematics and the Husserlian background for Carnap's phenomenalist constitutional system in *Der logische Aufbau der Welt* (1928).

Uebel presents a detailed discussion of Carnap's work from the 1920s and 1930s in the intellectual context of the Vienna Circle. The paper investigates the philosophical differences between Carnap's program of rational reconstruction in *Aufbau* and parallel work by Schlick as well as the protocol sentence debate between Carnap and Neurath. The articles by Pincock and Friedman discuss Carnap's *Aufbau*. Pincock surveys Carnap's "philosophical relationship" with Russell with respect to the similarities and differences in their accounts of scientific philosophy. He shows convincingly that neither Carnap in the *Aufbau* nor Russell

in *The Analysis of Matter* (1927) is a reductive empiricist in any strong sense. Instead both promoted a “structuralist theory” of scientific knowledge according to which scientific concepts refer to purely formal relations between the things of the object domain. Pincock points out that the real bone of contention between Russell and Carnap concerns the former’s metaphysical and theoretical realism. Friedman’s paper also challenges the traditional reception of Carnap’s *Aufbau* as suggesting a kind of empiricist reductionism and as being anti-metaphysical as a consequence of this. Friedman discusses “the *Aufbau*’s critical rejection of metaphysics on its own terms”, by focusing on its concluding section V, which is devoted to the extensive discussion of different metaphysical positions and several “point(s) of agreement” with Carnap’s constitutional system. Friedman argues that Carnap’s anti-metaphysical attitude in *Aufbau* is motivated not by epistemological reductionism but by a novel conception of “scientific philosophy” according to which the constitutional theory is conceived as a neutral standpoint compatible with realism, idealism, and phenomenalism. Demopoulos examines Carnap’s later “Ramsey-sentence reconstruction” of scientific theories. Briefly, for a given theory TC (and given the distinction between observation O-terms and theoretical T-terms used in it), the Ramsey sentence $R(TC)$ is obtained by replacing the theoretical predicates of TC by existentially bound variables. It is supposed to present the factual part of a theory. The so-called Carnap Sentence $C(TC)$ of the form “If $R(TC)$ then TC” then expresses the analytical part of TC. Demopoulos discusses several strengths of Carnap’s account and then turns to a “basic difficulty” concerning the “almost analytic” status of the factual (and thus a posteriori and synthetic) T-sentences. This throws into doubt whether the factual theoretical statements of a scientific theory can actually be expressed by Ramsey sentences.

The articles by Reck, Ricketts, and Awodey each investigate a certain aspect of Carnap’s evolving views on the philosophy of logic and mathematics. Reck presents a historically sensitive discussion of the intellectual background of Carnap’s early contributions to modern logic, in particular Frege’s lectures in Jena as well as Carnap’s correspondence with Russell on the theory of types. Reck then turns to a detailed survey of Carnap’s work on general axiomatics in the late 1920s and his treatment of three metatheoretic notions of completeness in the manuscript “Untersuchungen zur allgemeinen Axiomatik” (2000). Reck holds that Carnap’s formal explication of these notions can be viewed as an attempt to reconcile the “universalist” conception of logic of Frege and Russell with a Hilbertian conception of formal axiomatics. He shows that while Carnap’s main result in *Untersuchungen*, the so-called *Gabelbarkeitssatz* (stating the general equivalence of the categoricity, semantic completeness, and syntactic completeness of a given theory) is incorrect, it contains an interesting and still unresolved question concerning the metatheory of axiomatics. Ricketts’ paper investigates the intricate connections between Carnap’s specific version of logicism, empiricism, and the principle of tolerance in *Logical Syntax of Language* (1934). He argues that the principle of tolerance concerning the choice of logical calculi implies a revised conception of

logicism, i.e., a new understanding of the distinctive role of logic and mathematics as analytical and content-free “auxiliary devices” in scientific languages. Ricketts critically discusses an objection against Logical Syntax originally formulated by Gödel, namely that Carnap’s syntactic view of mathematics and the need to give a consistency proof for his logical calculi is effectively undermined by Gödel’s second incompleteness theorem. Finally, he addresses the question whether Carnap’s principle of tolerance is compatible with this revised logicism when applied to the informal syntax language in which different calculi are to be investigated. Awodey’s article surveys Carnap’s attempts in his subsequent work on formal semantics to find a “satisfactory general characterization” of the notions of analyticity and L-truth (logical truth). Following a discussion of Carnap’s essentially semantic definition of “analytic in LII” and of a demarcation criterion for logical and non-logical constants in *Logical Syntax*, the paper investigates in closer detail Carnap’s attempts to define L-truth and logical constancy semantically, in particular in the three-volume book project *Series in Semantics* (Carnap 1942, 1943, 1947). Awodey argues that the attempts given there do not meet the requirements of the modern model-theoretical notion of logical truth mainly due to Carnap’s tacit assumption that (logical) languages come equipped with a fixed interpretation. Consequently, the difference between “truth in a particular model” and “truth in all models” (and thus a modern model-theoretic account of model variation) cannot be expressed within his pre-modern conception of semantics.

Zabell presents a detailed and informed discussion of Carnap’s work on probability and inductive logic from the 1940s onwards. Richardson discusses Carnap’s pragmatism as a “fundamental philosophical commitment” in his scientific philosophy. It is manifest in the principle of tolerance as well as in Carnap’s general distinction between “practical decisions” and “theoretical questions” in *Logical Syntax* and in later work, e.g., in “Empiricism, Semantics, and Ontology” (1950). Richardson also surveys the relation and differences of Carnap’s philosophy with American pragmatism. Creath’s concluding article investigates Carnap’s logical pluralism and the notion of analyticity in *Logical Syntax*. Based on this, he presents a critical discussion of Quine’s notorious objections to the analytic/synthetic distinction in “Two dogmas of empiricism” (1951), specifically of Quine’s demand for an “empiricist criterion of significance” for the term “analytic”.

Overall, this is an excellent volume that presents the state of the art in Carnap scholarship. Many of the articles are based on, and comment on, recent research on specialized topics concerning Carnap’s work. The companion therefore provides an invaluable source of reference for scholars working in the field. At the same time, the contributions are intended to be accessible to a larger non-expert audience. As such, the volume also functions as a compact and accessible introduction to the broad spectrum of Carnap’s work and his central position in twentieth century philosophy.

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