

Chapter 14

Carl Hempel: Whose Philosopher?

Nikolay Milkov

For most academics, even most philosophers, the individual who best personified logical empiricism in North America was neither Carnap nor Reichenbach, but Carl Hempel. . . . Hempel's early papers, "Studies in the Logic of Confirmation" (1945) and "Studies in the Logic of Explanation" (1948, with Paul Oppenheim), effectively defined what by 1960 were arguably the two most active areas of research in North American philosophy of science.

(Giere 1996, pp. 339–340)

14.1 Michael Friedman's Thesis

Recently, Michael Friedman has claimed that virtually all the seeds of Hempel's philosophical development trace back to his early encounter with the Vienna Circle (Friedman 2003, 94). Hempel, it is true, spent the fall term of 1929 as a student at the University of Vienna, and, thanks to a letter of recommendation from Hans Reichenbach, he even attended some sessions of the Vienna Circle. This gave the young Hempel the opportunity to witness firsthand what was called in the literature "stage one" of the debate that saw Moritz Schlick and Friedrich Waismann go head-to-head with Otto Neurath and Rudolf Carnap on the "protocol sentences."

As opposed, however, to Friedman's view of the principal early influences on Hempel, we shall see that those formative influences originated rather with the Berlin Group. The evidenced adduced here against Friedman on this score concentrates on his contention that Hempel's entire philosophical development, as well as the major themes that were his special concern, were colored by (i) the

N. Milkov (✉)

Department of Philosophy, University of Paderborn, Paderborn, Germany
e-mail: nikolay.milkov@upb.de

Neurath–Schlick debate over the nature of truth (1932–1934), in which Hempel sided with Neurath; and (ii) Neurath’s disputation with Carnap of the mid-1930s. This latter contest saw Neurath defending a naturalist position on the issue of scientific investigation, whereas Carnap, who won Hempel’s support, championed the philosophical logic pioneered by Frege and Wittgenstein. Carnap adopted the latter as a so-called “logic of science”. Friedman himself alludes to Hempel’s confession, in the 1980s, that he ultimately abandoned the practice of “Carnapian explications” (cf. § 5, below). In the end, Hempel turned to a variation of Neurath’s naturalism, which took the form of Kuhnian historical and sociological studies that foreground the “pragmatic” factors of science.¹

Hempel actually spent much less time in Vienna than in Berlin, where he studied under Reichenbach from 1926 till 1933 and wrote a dissertation on probability,² (Hempel 1935–36) Reichenbach’s specialty. Hempel also attended seminars conducted by Walter Dubislav, another member of the Berlin Group. The seriousness of Hempel’s involvement in the Dubislav’s work is evidenced by the fact that together with Olaf Helmer, Hempel read the proofs of Dubislav’s book *Contemporary Philosophy of Mathematics* (Dubislav 1932, p. v). As late as 1934, immediately prior to leaving for Brussels in April, Hempel wrote Reichenbach that he continued to find Dubislav’s colloquium “very stimulating.”³

Besides Hempel’s presence at some Vienna Circle meetings, another factor that Friedman adduces in support of his thesis that Hempel’s philosophy of science has its roots in the Vienna Circle is that Hempel first won his reputation as an author with his 1935–1936 *Analysis* papers on the Vienna-Circle theory of truth (Cf. Hempel 1935a, b, 1936). In reality, however, these publications moot topics that in the 1930s were being widely debated in the analytic literature and Hempel weighed in on them, as Friedman (2003, p. 99) himself informs us, only after Susan Stebbing invited him in January 1935 “to present a lecture in London on the latest developments within the Vienna Circle and in particular on the exchange between Neurath and Schlick that had just appeared in *Erkenntnis*.” (The first of the three *Analysis* articles, “On the Logical Positivists’ Theory of Truth” (1935a), is merely a revised version of the London lecture.) Consequently, that Hempel wrote the articles does not unequivocally support the view that the ideas of the Vienna Circle alone were the source of his interest in explicating the Vienna-Circle theory of truth in the three papers.

Tellingly, Hempel’s crystal clear and comprehensive critical treatment of the Neurath–Schlick debate is characteristic of the Berlin Group. In form, Hempel’s analysis closely approximates that of Kurt Grelling in the latter’s reviews of Carnap’s *Aufbau* (Grelling 1929) and Reichenbach’s *The Philosophy of Space and Time* (Grelling 1930). Grelling himself, under Leonard Nelson, had mastered a discursive style distinguished for its high degree of clarity in thought and exposition,

¹On the “pragmatism” of Hempel’s later position see Wolters (2003).

²Since Reichenbach left Germany for Turkey in the summer of 1933, formally, Wolfgang Köhler, not Reichenbach, was the supervisor of Carl Hempel’s dissertation.

³Carl Hempel’s letter to Hans Reichenbach of 19.03.1934 [HR 013-46-30].

widely recognized as signature skills of Nelson's group of neo-Friesians. It is not accidental that it was Hempel as the *Berliner*—not as the member of the Vienna Circle—who produced the first and most perspicuous account of the dispute between Schlick and Neurath. This was thanks to training in Berlin that enabled Hempel to lay out the arguments and the issues at stake in the debate with great accuracy and lucidity.

Most importantly, from 1934 through 1936 Hempel also worked on the book *Der Typusbegriff im Lichte der neuen Logik* (Hempel and Oppenheim 1936b). As we shall see, at that point in time Hempel's real interest lay in these studies rather than in the Neurath–Schlick debate.

Friedman's finding is that in these years “the tension between a Carnapian and a Neurathian conception of philosophy of science, which had fundamentally shaped Hempel's earliest work but had long since lay dormant, was stimulated and came to life once again” in the last years of his philosophical development (Friedman, 110). On this account, Hempel, the “logician of science” and anti-naturalist, finally woke up from his “dogmatic slumber.” The present chapter demonstrates that Friedman's reading of the facts is misleadingly one-sided.

14.2 Methodological Remark: Carl Hempel as a Historian of Philosophy

A major challenge to any effort at determining Carl Hempel's place on the map of the history of philosophy of science is that Hempel himself was a reluctant and unreliable historian of philosophy, his own philosophy in particular. Only in the early 1980s did friends manage to persuade him to grant Richard Nollan an interview and in that way leave us something of an autobiographical record. The interview shows Hempel to be an inexact chronicler. He reports, for example, that Herbert Feigl was “the first, or one of the first” of the Vienna Circle to leave Austria for the United States (Hempel 2000, p. 14). Historians of the philosophy of science, however, all know that Feigl immigrated to the USA a full 5 years before Carnap, the next member of the Circle to flee. Furthermore, in his recollections about the Vienna Circle and the Berlin Group, published in 1993, Hempel often mistakenly identifies the latter with the “Society for Empirical/Scientific Philosophy,” which was an independent entity (Cf. Gerner 1997).⁴ Occasionally Hempel says that the Society was a partner of the Vienna Circle, and sometimes he states that it was affiliated with the “Ernst Mach Association” (cf. Hempel 1993, pp. 3 and 4). The latter two, however, were different entities as well.

Besides a hazy historical memory, Hempel had no well-developed sense of “philosophical loyalty:” he never felt obliged to identify himself with a particular philosophical school—not the Berlin Group, not the Vienna Circle, nor any other philosophical coterie. In his last days, however, Hempel did concede that he was

⁴Cf. Chapter One, § 1.3.

“closely associated” with both Vienna and Berlin, and hence doubtless significantly influenced by them (cf. Wolters 2003, p. 111). One does also find only a few references to Neurath, the member of the Circle whom Hempel closely followed in the 1930s—and to whose position, according to Friedman, he returned in the 1980s: and these appear only in the first paper (published 1945) of a collection of 12 of Hempel’s most distinguished essays, published under the title *Aspects of Scientific Explanation* (1965).

As the evidence suggests, then, we cannot take Hempel as a reliable source of the history of his own philosophical development. The objective here is thus to trace the verifiable lines of influence on Hempel in their wide variety.

14.3 Paul Oppenheim and Carl Hempel

When trying to determine Hempel’s relation to the Berlin Group, one must bear in mind that the latter was neither limited to the city of Berlin, nor to the years 1926–1933 (cf. § 1.3). The Berlin Group has roots that extend to South Germany. Indeed, Reichenbach first formulated many of the ideas that appear in his mature thinking while serving as an Associate Professor (*Privatdozent*) in Stuttgart (1920–1926). In 1922 he began corresponding with Carnap, who was then living in Buchenbach, in the Black Forest, some 180 km (ca. 110 miles) southwest of Stuttgart. It was at Erlangen (Bavaria), at the cutting-edge conference on exact philosophy, that in March of 1923 Reichenbach and Carnap first met (cf. Thiel 1993).

The Berlin Group attracted other non-Berliners, as well, something that Hempel’s late recollections confirm:⁵

The discussion [of the Society] lasted for 4 h, the final two of them at a nearby café, where the excited participants—among them Reichenbach, Dubislav, Grelling, Heinrich Scholtz (who had come from Kiel, I believe), Kurt Lewin, and the very gentle Paul Bernays⁶ [from Göttingen]—had become so agitated and noisy they almost caused a public nuisance and made young couples at neighboring tables break off their tender exchanges. (Hempel 1993, p. 4)

One of the external (or associate) members of the Berlin Group was Paul Oppenheim (1885–1977) of Frankfurt on Main. He was the product of the cross-fertilization of business, industry and scientific philosophy that was typical in Germany at the beginning of twentieth-century. (Another such “product” was Count Georg von Arco, one of the co-founders of the Society for Empiric Philosophy.) Around the turn of the twentieth century, Oppenheim was a student in Giessen, where his professor was Hermann Ernst Grassmann, a son and follower of Hermann

⁵This story refers to Hempel’s letter to his friend, written in November 1929, and is thus reliable.

⁶David Hilbert’s assistant Paul Bernays was sometime a member of the Leonard Nelson’s “Jakob Friedrich Fries Society” in Göttingen (active between 1913 and 1921). In the mid-1930s Heinrich Scholtz set up what was later called the “Münster Group” of exact philosophy.

Heinrich Grassmann, the mathematician who authored important works in universal algebra (cf. Graßmann 1844). By the late 1920s Oppenheim also held an adjunct lectureship at the University of Frankfurt.⁷

Oppenheim started to work together with Reichenbach as early as 1921—a contact probably facilitated by Einstein, with whom Oppenheim was on good terms. This collaboration was intensive and lasted till the end of the 1920s. In the “Acknowledgements” of his book *Die natürliche Anordnung der Wissenschaft: Grundgesetze der vergleichenden Wissenschaftslehre* (1926), Oppenheim thanked Reichenbach for “his constant and most effective help by putting [his] ideas into a book form.” The title of this volume itself reveals that in the 1920s, Oppenheim also worked with Kurt Lewin, who extensively explored the “comparative theory of sciences [*die vergleichende Wissenschaftslehre*]” (cf. Lewin 1920, 1925). Reflective of this relationship is the extended and highly laudatory review of Oppenheim’s book which Lewin published in *Kant-Studien* (cf. Lewin 1929).⁸

During these years, Lewin worked on a program somewhat akin to that of Reichenbach.⁹ Under the influence of Ernst Cassirer, Lewin strove to replace mainstream scientific concepts, such as causality, with other theoretical notions that served various complex functions. Lewin’s concept of *genetic series*, for example, together with the related notion of *genidentity* as applied to biology and physics, elicited wide interest.¹⁰ The concept of “genidentity” identifies the relation that secures the continuity of an object from one point in time to another; in other words, it explores the way in which objects preserve their identity over time.¹¹ Lewin’s aim in formulating novel scientific concepts was to recast the epistemology of science, and with it scientific classification, along new lines.

⁷This point betrays Oppenheim’s connection with another person close to the ideas of the Berlin Group—Franz Oppenheimer (1864–1943). Oppenheimer was the first professor of sociology in Germany and a close friend of Leonard Nelson: in the mid-twenties Oppenheimer invited Nelson’s former doctoral student Julius Kraft to become his assistant. (Kraft was also close friend of Karl Popper with whom he launched in 1957 the journal *Ratio* o.s. Cf. Popper 1962) Among Oppenheimer’s students were Theodor Adorno and Ludwig Eckhart (the “father” of the West-German *Wirtschaftswunder* after World War Two). Interestingly enough, Oppenheimer spoke about “united science [*Einheitswissenschaft*]” much before either the Berlin Group or the Vienna Circle did so. (Cf. Oppenheimer 1922, pp. xiv f., 10 f) This point was noted in Neurath 1932, p. 271, with reference to Kurt Lewin as a source of information.

⁸Reviews of Oppenheim’s book were also published by Hempel (cf. Hempel 1931) and the mathematician of the Hilbert’s group in Göttingen, Richard Courant (cf. Courant 1927), who was sometime also a member of the Jakob Friedrich Fries Society around Leonard Nelson.

⁹In the already mentioned paper of Kurt Grelling, “Philosophy of the Exact Sciences: Its Present Status in Germany,” he presented Reichenbach and Lewin as two alternative philosophers of exact science. Cf. Grelling (1928), p. 98.

¹⁰That concept was used in Reichenbach (1928, 1956), Carnap (1928a) and Hermes (1938). See Chap. 5.

¹¹Cf. with the theory of rigid designators of Hilary Putnam (one of Reichenbach’s students at the University of California at Berkeley) and Saul Kripke.

What drew Reichenbach to Lewin was the interdisciplinary character of Lewin's thinking, something reflected in his conviction that every branch of science produces knowledge that can be of philosophical value. There was an important difference between Lewin and Reichenbach, though. Whereas the former was most interested in the ordering of the new scientific theories and their concepts, Reichenbach concentrated his efforts on bringing to light the ever-changing principles that mark the evolution of scientific theory. Moreover, whereas Lewin conceived of his comparative science of the sciences as a discrete discipline, Reichenbach's philosophy merged with the sciences.¹² More precisely, Reichenbach was convinced that philosophy and science focus on different facets of one and the same subject: nature.

Particularly noteworthy for our concern here is that in the late 1930s Carl Hempel more closely followed Lewin's Cassirer-inspired project than he did that of Hans Reichenbach. Still, Hempel never lost sight of what he learned from Reichenbach, Dubislav and Grelling in Berlin. All three continued to influence Hempel's thinking over the course of a long academic career.

Oppenheim first met Hempel about 1930 through Reichenbach.¹³ After playing instrumental role in the Berlin Group's takeover of the Society for Empirical Philosophy in the summer of 1929, Reichenbach (who began to lose interest in Lewin's program) ceased working with Oppenheim and referred him to his promising student and follower Carl Hempel.

From 1934 through 1939 Hempel worked with Oppenheim in Brussels as his "scientific secretary." The issues they explored were clearly closer to Lewin's program than to that of Reichenbach, ranging as they did from the logic of classification and the systematic ordering of science, to taxonomy and the theory of ordering concepts that reflects conceptual isomorphism among different sciences.

In 1938, Kurt Grelling joined Oppenheim and Hempel in Brussels. Together Oppenheim and Grelling explored Lewin's theme of gestalt-theory. In effect, the trio of Oppenheim, Hempel, and Grelling thus constituted an independent satellite unit of the Berlin Group. As already said, along with its varying alignments of affiliated thinkers, the Group was clearly not limited geographically or to a particular time-frame.

In the fall of 1939 Oppenheim and Hempel immigrated to the USA. (Hempel had previously traveled to America, working in Chicago for 9 months in 1937–1938 as an assistant of Carnap's.) It was in America that a new cohort of the Berlin Group came to life in the years 1942 through 1944, this time at Princeton, where Hempel joined Oppenheim and Helmer. Hempel's most influential papers, "Studies in the Logic of Confirmation" (1945) and "Studies in the Logic of Explanation" (1948,

¹²This difference is underlined in Grelling (1928), p. 98.

¹³Hempel himself remembers that he first met Oppenheim immediately after the former returned from Vienna, i.e. in Spring 1930, while Oppenheim dated this event in 1933 (Oppenheim 1969, p. 1).

with Paul Oppenheim), reflect this collaboration. After a period of intensive joint study, Hempel stopped working together with Oppenheim and instead served as his “philosophical advisor, talent scout, and professional agent” (Rescher 1997, p. 157).

Nicholas Rescher provides a firsthand description of the way that Oppenheim worked with his Berlin colleagues. Rescher joined Oppenheim in 1952 to investigate the logic of Gestalt theory, a topic that Oppenheim had earlier explored with Grelling in Brussels.¹⁴

Typically, Oppenheim raised

(1) the topic of the investigation, and (2) [evinced] a guiding concern for structural issues that reflected a conceptual isomorphism among different scientific disciplines, specifically the view that there is a concept (e.g. that of *gestalt*) which, despite its origin in one particular science, was in principle a versatile instrumentality with useful applications in other branches of science. (Rescher 1997, p. 159)

In view of this, it seems evident that the topic of “scientific explanation” for which both Oppenheim and Hempel became famous in the philosophy of science originated with Oppenheim.

We should say a word at this juncture about Oppenheim’s academic project from the 1930s through 1950s. In contrast to the “encyclopedic” program of the Vienna Circle, Oppenheim was not reductionist but

looked to a more stylistic and structural unity of science, . . . [thus he] proposed to search for shared elements of epistemic process among substantively diverse sciences . . . for commonalities among the sciences that abstracted from substantive differences and looked at structural uniformities. . . .

Oppenheim, in sum, was convinced that various guiding concepts of scientific thought (classification, confirmation, explanation) reflected a fundamental structural community—an isomorphism of concepts of order—that runs across different branches of science. (ibid., pp. 161–162)

Hempel’s idea of a generalized “logic of confirmation,” which formalized evidential processes of thought common to all forms of scientific reasoning, appears to have been closely connected to this programmatic vision. It was not simply a further development of the Vienna Circle idea of epistemic significance. The same goes for the generalized “logic of explanation.”

To be sure, in the early 1940, Hempel and Oppenheim concentrated their efforts on providing a definition of “degree of confirmation” measure for simple formalized languages as the quotient of two range measures. According to Rescher the issue of confirmatory strength of a theory soon transmuted into one of assessing the explanatory adequacy of a theory. The disadvantage of the old approach was the enormous “gap between the inevitably fragmentary observational evidence we actually have and the vast (literally unending) claims that are implicit in any general theory” (ibid., p. 168). The new approach claimed that “the best standard of theory assessment is one that proceeds not in terms of evidential support, but rather in terms

¹⁴This work resulted in Oppenheim and Rescher (1955).

of the extent to which the theory correctly directs and canalizes our observational expectations” (ibid., p. 169).

In fact, however, theoretical interest in the concept of scientific explanation was long an element of Oppenheim’s thinking. It already played an important role in his two books, *Die natürliche Anordnung der Wissenschaft* (1926), and *Die Denkfläche* (1928). Oppenheim later recollected having “worked for years on the possibility of a systematic ordering of the sciences” in which the concept of explanation also played a role (cf. Oppenheim 1969, pp. 1, 3). Discussions of explanation also appear in Dubislav’s *Naturphilosophie* (Dubislav 1933, pp. 93 ff.) and, more notably, in Hempel and Oppenheim’s book *Der Typusbegriff im Lichte der neuen Logik* (1936b), where the antithesis between description and explanation plays an important role. Moreover, by 1936, these authors connected explanations with the covering laws that group together the empirical data that are to be explained (cf., esp., pp. 102 ff.). Roughly in the same period, Reichenbach, too, spoke about explanation as “summarizing the data under one law” (Reichenbach 1930, p. 55).

Years before Oppenheim, Dubislav and Reichenbach, however, the Southwestern neo-Kantian Heinrich Rickert (1863–1936) treated at length the factors distinguishing descriptions from explanations. Rickert held that there is an important difference (albeit not one in principle) between explanation and description: When we “explain,” declared Rickert, we refer to the “generality of the necessity” that has no empirical sources (Rickert 1896, p. 81)—this in contrast with descriptions, which, according to Rickert, are limited to the empirical domain.

The foregoing history establishes how Hempel and Oppenheim’s theory of explanation developed not only along the route from “confirmation” to “explanation” but also the other way round: from “explanation” to “confirmation.” In other words, when Hempel and Oppenheim first explored the “logic of confirmation,” they already had in mind the option to assess scientific theories through their explanatory power.

14.4 Hempel and Carnap

We turn now to the relationship between Hempel and Carnap. Michael Friedman rightly notes that “of all the leading members of the logical empiricist movement, Hempel had always been on closest terms, from a personal point of view, with Carnap” (Friedman 2003, p. 109). The concern here, however, is whether this supports Friedman’s claim that the Vienna Circle exercised a continuing influence on Hempel. That it does not becomes apparent when one takes into account Carnap’s philosophical character.

Among Carnap’s distinguishing traits as a thinker was his predisposition readily to assimilate alternative theoretical doctrines. This is not to suggest that as a scientific philosopher Carnap was an indecisive thinker. Rather, as the memoir left by one of his students at the University of Chicago makes clear, the changeability of his theoretical position was a function of

his almost selfless drive for truth. He really took seriously the idea that there is progress in human knowledge, that science is a cooperative enterprise whose protagonists share a common goal. He absolutely submerged his ego in that enterprise, more than anyone . . . , and he would generally give others the benefit of the doubt—assuming that they too were joining in a selfless and disinterested search for truth.¹⁵ (Sharpless 2009)

The profile that Seth Sharpless sketches above finds substantiation virtually throughout Carnap's career. In the *Aufbau* (1928a), for instance, besides the profound impact of Russell's program for the logical construction of physical objects, neo-Kantian and Husserlian currents also inform Carnap's discussion. Additionally, the work discloses anti-Kantian influences originating with Greifswald Realists, Hans Driesch, and others (cf. Milkov 2004). The reassessments of the logical positivism, which appeared in the 1990s (Friedman 1999; Richardson 1998), drew attention to only one aspect of Carnap's *Aufbau*, namely its connections with the Marburg neo-Kantians.

Further substantiating Sharpless's sketch are events that occurred in the early 1930s, when Carnap readily followed Neurath's lead in subscribing to physicalism (cf. Carnap 1932, p. 338) and promoting the project for an encyclopedia of the sciences. Indeed, the pair worked in close collaboration for about 2 years, with Neurath clearly setting the agenda. A final example of evidence that confirms the accuracy of Sharpless's portrait of his distinguished teacher is the formative impact that Russell's logicism had on Carnap at the very outset of his professional career. Following his move first to Vienna and later to Prague, however, Carnap increasingly viewed that logicism from the perspective of Ludwig Wittgenstein's pan-linguisticism—Wittgenstein insisted that all problems of philosophy are problems of language. Carnap's scholarship at the time featured this pan-linguisticism with such literal fidelity that in 1932 Wittgenstein accused him of downright plagiarism (Nedo and Ranchetti 1983, p. 381). This orientation finds its culminating expression in Carnap's *Logical Syntax of Language* (1934).

In addition to being susceptible to modes of thought originating in widely divergent philosophical currents, and in some respects *in spite* of it, Carnap exhibited an unmistakable "Berlin side." As the last living member of the Berlin Group, Olaf Helmer recalled, "The most prominent members of that group, aside from Hans Reichenbach himself, were Hempel, Dubislav, and (when he came to Berlin on a lecture visit) Rudolf Carnap."¹⁶ Until 1926, Carnap's ideas owed a great deal to the thinking of Reichenbach. Even when, in Vienna, he came under the influence of Wittgenstein, Schlick, Waismann, and Neurath, particularly between 1926 and 1929, Carnap continued to pursue projects associated with Reichenbach, Dubislav and Lewin. Among other research initiatives that evidence this continuing affinity with the Berlin Group are Carnap's explorations in axiomatic (Carnap 1928b)

¹⁵In this kind of selfless pursuit of truth, Carnap is reminiscent of Bertrand Russell and strongly opposed Husserl and Wittgenstein who insisted that the truth they discovered are "eternal" and thus cannot be corrected or supplemented by their critics. Cf. Milkov (2012).

¹⁶Email communication of Olaf Helmer to the author from July 27, 2009.

(Dubislav's and Reichenbach's subject), even as he called axiomatic "applied logic" (Carnap 1929). Further, he investigated the topic of "definitions" (Carnap 1927) (Dubislav's theme), and he wrote about genidentity (Lewin's brainchild) in *Aufbau*.

This Berlin side of Carnap's scientific philosophy is what led Friedman to propose that the *Aufbau* is above all a book in philosophy of science, its aim being

to redefine its [philosophy's] own task in the light of the recent revolutionary scientific advances that have made all previous philosophies untenable; . . . to use recent advances in the science of logic together with advances in the empirical sciences (Gestalt psychology, in particular) to fashion a scientific replacement for traditional epistemology.¹⁷ (Friedman 1991, pp. 508–509)

Be this as it may, between 1929 and 1936 Carnap and the Berlin Group were at odds over many points in philosophy of science. This surfaced dramatically in Carnap's debate with Grelling and Reichenbach on probability, which took place in September 1929 (cf. § 1.6 (b)). The venue was the "Conference of Epistemology of exact Sciences" in Prague, where the Berlin philosophers declared war on the "principle of verification."¹⁸ Another sign of the conflict between Carnap and the Berliners was Reichenbach's attack on Carnap's "logical positivism" in the early 1930s. This challenge found its most incisive expression in the former's first book in English, *Experience and Prediction* (1938). In yet another symptom of fundamental disagreement with the Berliners, Carnap sharply questioned Reichenbach's naturalistic stance. As Carnap saw it, the philosopher of science is tasked with logically analyzing the language of science. Reichenbach, by contrast, understood his purpose as analyzing the facts of science. Carnap regarded this as overstepping the bounds proper to philosophy and unwarrantably proposing to engage in the practice of science (Carnap 1936a, b).

From the time shortly before he left Prague at the end of 1935, however, Carnap drifted closer to the Berlin Group's philosophical program. Two constellations of developments bear witness to this philosophical realignment:

- (i) First and foremost, Carnap gradually shifted his attention away from the theory of verification as applied to the *sentences* of science and more toward exploring the confirmation of scientific *theories*. Two papers that he wrote at this time document this change: "Wahrheit und Bewahrung" (1936) and "Testability and Meaning" (1936–1937). Reichenbach saw this development as marking Carnap's transition from "dogmatic" positivist of the Vienna Circle to scientific philosopher, one who critically analyzes the latest advances in the sciences (Reichenbach 1938, pp. 76 f.).

¹⁷In support of this claim we would like to note that between 1926 and 1935 Carnap taught philosophy at the University of Vienna and then at the University of Prague. When he started to teach at the University of Chicago, however, he invited (in 1937) Reichenbach's students Hempel and Helmer, and not some of his own students, to become his assistants. This also explains why Hempel and Helmer so easily started to work together with Carnap.

¹⁸Cf. Diskussion uber Wahrscheinlichkeit, *Erkenntnis* 1 (1930): 260–287.

- (ii) From 1942 through 1944, Carnap's thinking came under the spell of a new Berliner current through his contacts with the "H₂O philosophers:" Hempel, Helmer and Oppenheim, whom as we've seen for all intents and purposes constituted a later variant of the Berlin Group. Carnap's interest in induction and the later focus on "comparative concepts"¹⁹ (cf. Carnap 1950, §§ 4 f.) clearly betray the Group's impact on him. Furthermore, during this period Carnap's studies in the logical foundation of probability reflect the thinking of Reichenbach. This is not to suggest that Carnap followed Reichenbach in the treatment of probability—he did not. Rather, Carnap simply began serious work on a topic—probability—that happened to have preoccupied Reichenbach from the beginning of his philosophical career. (Carnap himself reported that his interest in probability originated with lectures he audited by Richard von Mises.)

Among other things, what obscures the Berlin Group's impact on Carnap is that Hempel, that unreliable historian of philosophy and reluctant autobiographer, stressed Carnap's continuing influence on *himself*. Evidently Hempel could not entertain the thought that in many instances it was he and Oppenheim who significantly influenced *Carnap*. Lamentably, it is Hempel's account that has become the accepted view in the literature.

14.5 The Method of Explication

A good deal of evidence unquestionably points to Carnap's influence on Hempel. This influence, however, did not invariably reflect ideas that preoccupied the Vienna Circle. One example is Carnap's so-called "method of explication" (cf. Carnap 1945) which he propounded while he was writing *Meaning and Necessity* (1942–1944). He described it as the process of "transforming a given more or less inexact concept into an exact one" (Carnap 1950, p. 3). Hempel adopted this method, first thematizing "explication" in his *Fundamentals of Concept Formation in Empirical Science* (1952).

Actually, both Carnap and Hempel employed the method prior to taking it up as a formal theme of philosophical analysis. It is notable in this connection that Carnap's conception of explications underwent considerable transformation. While he formulates it in *Meaning and Necessity* (1947) along roughly Fregean/Russellian lines, in *Introduction to Semantic Theory* (1950) Carnap presents it more from a late-Wittgenstein standpoint. To be more exact, while in 1947 Carnap sought to develop a formal means of replacing vague concepts with precise ones by applying the exactitude of the scientific method, by 1950 he concluded that explication cannot

¹⁹Comparative concepts were already discussed in Hempel and Oppenheim (1936a, b). Cf. also Tegtmeier (1981).

be decided in an exact way. Carnap thus ultimately arrived at the position that the theory of explications, rather than being able to invoke the standard of scientific exactitude, is perforce limited to the simply satisfactory (cf. *ibid.*, p. 4).

Historians of the philosophy of science tend to assume that it was the thinking of British ordinary-language philosophers that was largely behind the Wittgensteinian shift in Carnap's approach to the method of explication. But this assumption rests solely upon references Carnap made in *Meaning and Necessity* (pp. 8, 42, and 63n. 7) to C.H. Langford's paper on G.E. Moore's "paradox of analysis," which appeared in Schilpp's *G.E. Moore* volume. The influence of Wittgenstein is much more long standing and direct, however, tracing back to the early 1930s and Carnap's wholehearted subscription to Wittgenstein's "enchantment with words." Under this influence, Carnap held that language is philosophy's subject matter and that the subject-matter of scientific philosophy is the language of science. Wittgenstein himself developed the technique of conceptual analysis, arguably his main preoccupation in those of his later works that investigate the necessary and sufficient conditions of language applications. And exactly Wittgenstein's method of conceptual analysis is what grounds Carnap's (and thus Hempel's) method of explication.

Moreover, in the 1930s, this method figured as the principal topic of debate among British philosophers such as Susan Stebbing, John Wisdom and Max Black. Hempel readily joined this discussion. In fact the first paper he published after relocating to the USA focuses upon Max Black's treatment of vagueness, not philosophy of science (cf. Hempel 1939). This redirection of his thinking set the stage for Hempel readily to adopt, some 10 years later, Carnap's method of explication.

14.6 Carl Hempel Between External and Internal Philosophy of Science

Oppenheim's Lewin-inspired approach to structural issues of science at once postulated a conceptual isomorphism among different scientific disciplines and so helped to introduce the method of explication. This doctrine had two contrasting consequences by the mid 1960s. On the one hand, it led Hempel to conceive a discrete new discipline, *philosophy of science*, with clear-cut themes and a compelling program of theoretical research. This innovation of Hempel's found its classic expression in his *Philosophy of Natural Sciences* (1966). It signaled the birth of the philosophy of science as a discipline and was a significant development of modern intellectual history, effectively narrowing the gap between modern science and philosophy.

On the other hand, however, by the mid 1960s, it became clear that the method of explications which Carnap and Hempel employed²⁰ was not interdisciplinary but

²⁰As already seen, Carnap and Hempel practiced it from the beginning of the 1940s onward.

rather “non-disciplinary” (Rescher 1997, p. 162). What’s more, it had no “connection to any scientific theory. The concepts to be analyzed [by it] were general, methodological concepts supposedly common to all the sciences.” (Giere 1996, p. 340) In other words, they patently belonged to what Ernan McMullin termed “external philosophy of science.”²¹ The result for pedagogy and scholarly praxis was “an increasing separation between philosophy of science and the content of the sciences. People trained in philosophy, but with little knowledge of any science, could write article after article with titles like ‘The Paradoxes of Confirmation’ or ‘The Symmetry between Explanation and Prediction’.” (ibid., p. 341)

The prevalent view today is that the situation started to change only with the appearance and the assimilation of the ideas of Thomas Kuhn’s *Structure of Scientific Revolutions* (1962). Kuhn’s work impelled authors of philosophical studies to acquire detailed knowledge in the special sciences and exhibit it in their writings. Arguably, this was a turn to “internal philosophy of science.”

In truth, however, Reichenbach had already acquired and worked with such detailed knowledge back in the 1920s. On this score, he was the first contemporary philosopher of science—or, more precisely, the first philosopher of physics, a discipline that presupposes “detailed investigations into the particular aspects or interpretations of physical theories” (Ryckman 2007, p. 193). The claim in these pages is that the philosophy of science, a vital and thriving sub-discipline today, was born in Berlin in the 1920s and early 1930s.

14.7 Epilogue

By way of conclusion, we should note that when Hempel abandoned Carnapian explications and embraced naturalism in the 1980s, he was not, *pace* Friedman (cf. § 14.1, above), simply following Neurath. Substantiating this fact is unimpeachable evidence that in his last years Hempel, returning to his philosophical roots, propounded a modified form of Reichenbach’s Berlin naturalism. In fact, Friedman’s reading of the history hinges on four matters with respect to which Neurath, curiously enough, happened to be close to Reichenbach and his colleagues in Berlin:

- Like Reichenbach, Neurath had called for philosophers strongly interested in scientific theories as instruments for prediction. That said, however, while Neurath discussed sciences in quite general terms (*vis-à-vis* the unity-of-science project), Reichenbach undertook detailed investigations into particular themes of physical theories.

²¹On Ernan McMullin’s terms “external” and “internal” philosophy of science see Chapter One, § 1.9.

- Like Reichenbach, Neurath (after 1931) fought logical *positivism*, criticizing it for straying from scientific praxis. To differentiate their positions from the positivists, both Reichenbach and Neurath began characterizing their work and defending it as *logical empiricism*. (Reichenbach also used the term *logistic empiricism*.)
- Like Reichenbach, Neurath embraced a program for the unity of science. But whereas Neurath championed an “encyclopedia of sciences” that employed unific concepts, Reichenbach advocated parallel investigations of the “relativised a priori” principles in different sciences.
- Lastly, like Reichenbach, Neurath embraced, after 1931, the program for physicalism (Carnap following suit in 1932).

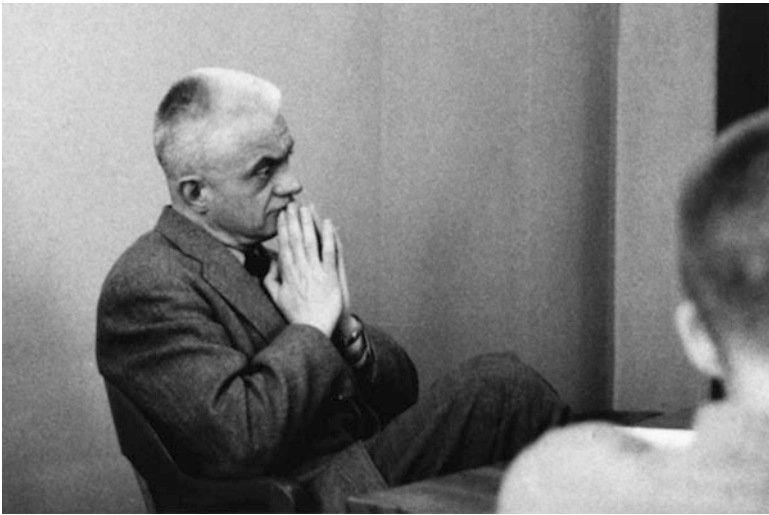
It should be clear, then, that while Friedman rightly sees Carl Hempel as at last siding, in the 1980s, with Neurath, the evidence suggests that what motivated Hempel’s move was not Neurath himself but rather Reichenbach and the spirit of the Berlin Group in general, the milieu in which Hempel served his philosophical apprenticeship.

References

- Carnap, Rudolf. 1927. Über eigentliche und uneigentliche Begriffe. *Symposion* 1: 355–374.
- Carnap, Rudolf. 1928a. *Der logische Aufbau der Welt*. Berlin: Weltkreis-Verlag.
- Carnap, Rudolf. 1928b. *Untersuchungen zur allgemeinen Axiomatik*. Hrsg. von Thomas Bonk. Darmstadt: Wissenschaftliche Buchgesellschaft, 2000.
- Carnap, Rudolf. 1929. *Abriss der Logistik*. Wien: Springer.
- Carnap, Rudolf. 1932. Die physikalische Sprache als Universalsprache der Wissenschaft. *Erkenntnis* 2: 432–465.
- Carnap, Rudolf. 1936a. Von der Erkenntnistheorie zur Wissenschaftslogik. In: *Actes du Congrès international de philosophie scientifique, Paris 1935*, Fasc. 1, *Philosophie scientifique et l’empirisme logique*, 36–41. Paris: Hermann.
- Carnap, Rudolf. 1936b. Wahrheit und Bewährung, in: *Actes du Congrès international de philosophie scientifique, Paris 1935, Fasc. 4, Induction et probabilité*, Paris: Hermann, pp. 18–23.
- Carnap, Rudolf. 1936–1937. Testability and meaning. *Philosophy of Science* 3 (1936): 419 ff.; 4 (1937): 1 ff.
- Carnap, Rudolf. 1945. The two concepts of probability. *Philosophy and Phenomenological Research* 5(1945): 513–532.
- Carnap, Rudolf. 1947. *Meaning and necessity: A study in semantics and modal logic*. Chicago: University of Chicago Press.
- Carnap, Rudolf. 1950. *Logical Foundations of Probability*. Chicago: University of Chicago Press.
- Courant, Richard. 1927. Paul Oppenheim. *Die Naturwissenschaften Die natürliche Ordnung der Wissenschaft* 15: 655.
- Dubislav, Walter. 1932. *Die Philosophie der Mathematik in der Gegenwart*. Berlin: Junker & Dünnhaupt.
- Dubislav, Walter. 1933. *Naturphilosophie*. Berlin: Junker und Dünnhaupt.
- Friedman, Michael. 1991. The re-evaluation of logical positivism. *The Journal of Philosophy* 88: 505–519.
- Friedman, Michael. 1999. *Reconsidering logical positivism*. Cambridge: Cambridge University Press.

- Friedman, Michael. 2003. Hempel and the Vienna circle. *Minnesota Studies in the Philosophy of Science* 18: 94–114.
- Gerner, Karin. 1997. *Hans Reichenbach: sein Leben und Wirken*. Osnabrück: Phoebé.
- Giere, Ronald. 1996. From *wissenschaftliche Philosophie* to philosophy of science. *Minnesota Studies in the Philosophy of Science* 16: 335–354.
- Graßmann, Hermann Günther. 1844. *Die Wissenschaft der extensiven Größe oder die Ausdehnungslehre*. Leipzig: Wigand.
- Grelling, Kurt. 1928. Philosophy of the exact sciences: Its present status in Germany. *The Monist* 38: 97–119.
- Grelling, Kurt. 1929. Realism and logic: An investigation of Russell's metaphysics. *The Monist* 39: 501–520.
- Grelling, Kurt. 1930. Die Philosophie der Raum-Zeit-Lehre. *Philosophischer Anzeiger* 4: 101–128.
- Hempel, Carl. 1931. Review of Oppenheim 1926. *Erkenntnis* 2: 473–474.
- Hempel, Carl. 1935a. On the logical positivists' theory of truth. *Analysis* 2: 49–59.
- Hempel, Carl. 1935–1936. Über den Gehalt von Wahrscheinlichkeitsaussagen. *Erkenntnis* 5: 228–260.
- Hempel, Carl. 1935b. Some remarks on 'Facts' and propositions. *Analysis* 2: 93–96.
- Hempel, Carl. 1936. Some remarks on empiricism. *Analysis* 3: 33–40.
- Hempel, Carl. 1939. Vagueness and logic. *Philosophy of Science* 6: 163–180.
- Hempel, Carl. 1945. Studies in the logic of confirmation. *Mind* 54: 1–26 and 97–121.
- Hempel, Carl. 1952. *Fundamentals of concept formation*. Chicago: University of Chicago Press.
- Hempel, Carl. 1993. Empiricism in the Vienna circle and in the Berlin society for scientific philosophy. Recollections and reflections. *Institute of the Vienna Circle Studies* 1: 1–9.
- Hempel, Carl. 2000. Intellectual autobiography—The interview with Richard Nollan. In *Science, explanation, and rationality*, ed. J.H. Fetzer, 3–35. Oxford: Oxford University Press.
- Hempel, C., and Oppenheim, P. 1936a. *L'importance logique de la notion de type*. *Actes du Congrès International de Philosophie Scientifique*, vol. 2, 41–49. Paris: Hermann.
- Hempel, C., and Oppenheim, P. 1936b. *Der Typusbegriff im Lichte der neuen Logik*. Leiden: Sijthoff.
- Hempel, C., and Oppenheim, P. 1948. Studies in the logic of explanation. *Philosophy of Science* 15: 135–175.
- Hermes, Hans. 1938. *Eine Axiomatisierung der allgemeinen Mechanik*. Leipzig: Hirzel.
- Lewin, Kurt. 1920. *Die Verwandtschaftsbegriffe in Biologie und Physik und die Darstellung vollständiger Stammbäume*. Berlin: Bornträger.
- Lewin, Kurt. 1925. Über Idee und Aufgabe der vergleichenden Wissenschaftslehre. *Symposion* 1: 61–94.
- Lewin, Kurt. 1929. Review of Oppenheim 1926. *Kant-Studien* 34: 461–464.
- Milkov, Nikolay. 2004. G. E. Moore and the Greifswald objectivists on the given, and the beginning of analytic philosophy. *Axiomathes* 14: 361–379.
- Milkov, Nikolay. 2012. The construction of the logical world: Frege and Wittgenstein on fixing boundaries of human thought. In: *Crossing borders*, eds. Alfred Dunshirn et al., 151–161. Vienna: University of Vienna.
- Nedo, Michael, and Michele Ranchetti. 1983. *Ludwig Wittgenstein: Sein Leben in Bildern und Texten*. Frankfurt: Suhrkamp.
- Neurath, Otto. 1932. In *Sociology in physicalisms*, ed. M. Stölzner and T. Uebel, 269–314. Wiener Kreis/Hamburg: Meiner, 2006.
- Oppenheim, Paul. 1926. *Die natürliche Anordnung der Wissenschaft: Grundgesetze der vergleichenden Wissenschaftslehre*. Jena: Fischer.
- Oppenheim, Paul. 1928. *Die Denkfläche: Statische und dynamische Grundgesetze der wissenschaftlichen Begriffsbildung*. Berlin/Charlottenburg: Pan.
- Oppenheim, Paul. 1969. Reminiscences of Peter. In *Essays in Honour of Carl G. Hempel*, ed. N. Rescher, 1–4. Dordrecht: Reidel.
- Oppenheim, P., and N. Rescher. 1955. Logical analysis of Gestalt concepts. *The British Journal for the Philosophy of Science* 6: 89–106.

- Oppenheimer, Franz. 1922. *Allgemeine Soziologie*. Stuttgart: Fischer, 1964.
- Popper, Karl. 1962. Julius Kraft. *Ratio* 4: 2–10.
- Reichenbach, Hans. 1928. *Philosophie der Raum-Zeit-Lehre*. Berlin: de Gruyter.
- Reichenbach, Hans. 1930. Die philosophische Bedeutung der modernen Physik. *Erkenntnis* 1: 49–71.
- Reichenbach, Hans. 1938. *Experience and prediction*. Chicago: Chicago University Press.
- Reichenbach, Hans. 1956. In *The direction of time*, ed. M. Reichenbach. Berkeley: University of California Press.
- Rescher, Nicholas. 1997. H₂O: Hempel–Helmer–Oppenheim: An episode in the history of scientific philosophy in the 20th century. *Philosophy of Science* 64: 779–805.
- Richardson, Alan. 1998. *Carnap's reconstruction of the world: The Aufbau and the emergence of logical positivism*. Cambridge: Cambridge University Press.
- Rickert, Heinrich. 1896. *Die Grenzen der naturwissenschaftlichen Begriffsbildung*. Tübingen: J.C.B. Mohr, 1921.
- Ryckman, Thomas. 2007. Logical empiricism and the philosophy of physics. In *The Cambridge companion to logical empiricism*, ed. A. Richardson and T. Uebel, 193–227. Cambridge: Cambridge University Press.
- Sharpless, Seth. 2009. Reminiscences about Carnap at Chicago (1945–1951). URL = <http://www.sethsharpless.com/papers/Reminiscences.htm>
- Tegtmeier, Erwin. 1981. *Komparative Begriffe: Eine Kritik der Lehre von Carnap und Hempel*. Berlin: Dunker & Humblot.
- Thiel, Christian. 1993. Carnap und die wissenschaftliche Philosophie auf der Erlanger Tagung. In *Wien, Berlin, Prag. Der Aufstieg der wissenschaftlichen Philosophie*, ed. R. Haller and F. Stadler, 175–188. Vienna: Hölder–Pichler–Tempsky.
- Wolters, Gereon. 2003. Carl Gustav Hempel—Pragmatic empiricist. In *Logical empiricism: Historical and contemporary perspectives*, ed. P. Parrini et al., 109–122. Pittsburgh: University of Pittsburgh Press.



Carl Hempel, Spring 1958, at the University of Yale (by Veli Valpola)

The editors and publishes would like to thank the following for permission to use photographs: Fred Stein Archive, Stanfordville, NY (for 2); Fotoagentur Ullstein Bild, Berlin (for 3); Nicholas Rescher (for 1 and 5); Karin Gimple-Grelling, Zürich (for 4); The Special Collections Department, University Library System of the University of Pittsburgh (for 6).