Logical Empiricism*

Christopher Pincock, chrispincock@gmail.com

October 29, 2013 – 10522 words

"Neatness and clarity are striven for, and dark distances and unfathomable depths rejected. In science there are no 'depths'; there is surface everywhere: all experience forms a complex network, which cannot always be surveyed and can often be grasped only in parts. Everything is accessible to man; and man is the measure of all things." – Neurath, Hahn and Carnap, "The Scientific Conception of the World: The Vienna Circle" (1929)

Logical empiricism was a movement in philosophy that sought to transform many aspects of philosophy. This transformation aimed to change not only the philosophical claims that philosophers debated, but the very methodology of philosophy itself. By "methodology" I mean the choice of problems for philosophers to pursue and the acceptable methods for solving these problems. A methodological claim is of course a philosophical claim, but it is a claim about philosophy itself in a special way that distinguishes it from more ordinary philosophical claims about knowledge or existence. One of the more remarkable features of logical empiricism is its explicit methodological debates. Many logical empiricists showed a keen awareness of the need for a new conception of philosophical problems and methods, and this prompted several sharp disputes among the logical empiricists. A reconsideration of these discussions may be productive today precisely when a new round of methodological debates is underway among philosophers. While it is too much to hope that the logical empiricists' philosophical methodologies will provide a viable solution to the methodological problems of our own time, their debates remain instructive for philosophers today. Logical empiricism shows the value of a healthy skepticism towards the depth and profundity of philosophy itself, and it seems that this skepticism is largely absent in contemporary philosophy.

I will organize my exposition around three problems that preoccupied logical empiricism. Each of these problems was more or less ill-defined in its original form. The refinement of the problem itself and associated claims about the methods needed to resolve the problem prompted three rounds of methodological debate. The primary problem is commonly referred to as the coordination problem. Given that the sciences successfully deploy increasingly abstract tools, how should our scientific claims be coordinated with what we can experience and test? Debates about the coordination problem resulted in two additional problems. First, there is a problem about logic. Logic may provide the resources to account for the

* To appear in H. Cappelen, T. Gender and J. Hawthorne (eds.), *Oxford Handbook of Philosophical Methodology*.

coordination of abstract structures with concrete reality, but what is logic and how should this sort of logical coordination be effected? Second, there is an alternative range of issues tied to the role of context in science. Scientific claims are the products of human activity carried out in a broader social and political context. What role should this context play in coordinating abstract structures with concrete reality?

Schlick and Reichenbach offered similar proposals for how to resolve the coordination problem in the earliest stages of logical empiricism. But they quickly parted ways based on their differing views of the significance of logic for coordination. Under the influence of Wittgenstein's *Tractatus*, Schlick came to believe that logic revealed the essential features of all representations and that logical tools were both necessary and sufficient to resolve the basic coordination problem. Reichenbach, by contrast, appealed instead to substantial claims about causality and probability. Carnap in the *Logical Syntax of Language* developed yet another methodological option based on his logical pluralism and conception of scientific knowledge as tied to a formally articulated language. Finally, Neurath and Frank responded by appealing to features of the context in which science is done. This required a different conception of the coordination problem and its solution. While logic was assigned some role in coordination, greater emphasis was placed on the actions of individuals and their social context. A reply to this expansive conception of philosophy can be found in Reichenbach's celebrated distinction between the contexts of discovery and justification. After surveying these debates, I will conclude with some remarks about the methodological proposals offered by Hempel in his later work.[1]

I. Coordination

Nineteenth century successes in physics and chemistry, combined with a new experimental psychology, prompted many to worry that it was no longer clear what the sciences were studying or how our scientific knowledge was to be justified. Several writers traced these worries to an unreasonable tendency to hold on to traditional common sense and religious beliefs in the face of their lack of scientific standing. A forceful instance of this trend is Ernst Mach. Mach sought to recast the scientific claims of both psychology and physics in terms of neutral

[1] The contemporary scholarship on logical empiricism is considerable. Important introductions are Stadler 2001, Richardson 2003, Rescher 2006, Richardson and Uebel 2007 and Friedman and Creath 2007. I am especially indebted to the work of Michael Friedman and Thomas Uebel. See in particular Friedman 1999, Friedman 2000 and Uebel 2007. I am also grateful to Greg Frost-Arnold, Robert Kraut and Gregory Lavers for the opportunity to review some of their unpublished work on logical empiricism. See in particular Frost-Arnold 2013, Kraut forthcoming and Lavers forthcoming. I would also like to thank the editors and an anonymous referee for helpful suggestions.

elements. At the same time Mach recognized the need to bring to bear some conceptual organization of these elements if our cognition was to function. He insisted only that these concepts prove their scientific value. The assumption was clearly that most traditional beliefs would not survive this sort of scrutiny and be replaced by a new conception of the world: "In place of the traditional, instinctive ways of thought, a freer, fresher view, conforming to developed experience, and reaching out beyond the requirements of practical life, must be substituted throughout" (Mach 1897, p. 24).[2] It is important to see Mach's empiricism in this light. Mach's empiricism was not the philosophical claim that all knowledge is justified by experience. It was instead closer to what van Fraassen has termed the empirical stance: an attitude undertaken at a given time for a particular purpose (van Fraassen 2002). Mach's goal was to overcome the tensions between the sciences and our non-scientific beliefs.

Moritz Schlick arrived in Vienna in 1922 as the professor of philosophy of the inductive sciences, occupying a chair once held by Mach. Schlick embraced the idea that our beliefs must withstand critical scrutiny, especially in light of the development of Einstein's general theory of relativity. One theme of Schlick's *Space and Time in Contemporary Physics* is that our ordinary beliefs about space and time reflect inherited biases that must be swept away. At the same time, Schlick was highly critical of Mach's immanence philosophy: the view that all that exists is part of someone's experience. In *General Theory of Knowledge*, Schlick defended a form of scientific realism. The crucial means to this conclusion is Schlick's novel account of what scientific concepts are and how they might be connected with reality. He adapted Hilbert's account of the implicit definition of geometrical concepts by axioms to all mathematical and scientific concepts. On this approach, a scientific theory begins as a sequence of statements. The non-logical terms of these statements are uninterpreted, but the collection of statements is taken to define what those non-logical terms mean. This seemed to work especially well for terms for distances and times as well as fundamental physical quantities like mass and charge. To arrive at a genuine theory, the uninterpreted statements must be related or coordinated with empirical reality. Crucially, Schlick assumed that these coordinations must involve the stipulation that something observed is the referent of the implicitly defined terms. This comes through clearly in Schlick's emphasis on point coincidences: "The adjustment and reading of all measuring instruments of whatever variety ... are always accomplished by observing the space-time coincidence of two or more points" (Schlick 1963, pp. 49-50). For example, the use of a clock to measure a period of time involves the coincidence of the hands of the clock with marks on the clock. So, it is possible to use observations of a clock to stipulate what some theoretical terms for times refer to.

[2] This passage is partially given in Nemeth 2007.

Given this sparse observational basis Schlick was forced to coordinate his abstract terms with very simple observational phenomena. The coordination problem was to be solved, then, by finding suitable observational phenomena for sufficiently many abstract scientific concepts. On this approach, the philosopher of science has two primary tasks. First, she should clarify the different elements that must be addressed to solve the coordination problem. Second, she should specify how particular theories should be reconstructed in these terms and how those theories might fit together. A secondary task is to combat alternative approaches that invoked superfluous machinery. *General Theory of Knowledge* aims to accomplish both tasks by developing a conception of knowledge that goes beyond Machian positivism and yet takes seriously the conception of knowledge involved in this sort of coordination (Schlick 1985).

While Schlick was developing his account of the coordination problem, Hans Reichenbach tackled similar issues in Berlin. Also preoccupied with the special and general theories of relativity, Reichenbach distinguished axioms of connection and axioms of coordination. The former played the same role as Schlick's implicit definitions while the latter served to link uninterpreted non-logical terms to concrete reality. But Reichenbach sought to give more substance to his axioms of coordination. Early on, Reichenbach gave axioms of coordination a constitutive role that shared some features with Kant's synthetic a priori. These axioms were revisable, unlike Kant's synthetic a priori judgments, but at the same time they were "constitutive of the concept of the object" (given by Friedman 2007, p. 98). As with many Kantian claims, this notion of constitution can be given a more idealistic or more realistic interpretation. On the idealistic interpretation, constitution requires that the object be mind-dependent and somehow created by the scientific community. As axioms of coordination are revisable, this entails that what exists is also changing over time. The more plausible realistic interpretation is that it is only our concepts that are changing, but that the concepts at a given time will pick out genuine objects if our definitions succeed in matching the right features of those objects. On this approach we wind up with a genuine scientific theory if we supplement our axioms of connection with the right sort of coordinating axioms. The job of the philosopher of science is to figure out what these axioms should be and to reconstruct our scientific theories in these terms. This project is pursued in *The Philosophy of Space and Time*. While insisting that axioms of coordination are merely definitions, Reichenbach also grants the need to invoke approximations and limiting procedures when defining crucial notions like the concept of a rigid body (Reichenbach 1958, p. 22).

While Schlick's and Reichenbach's conception of the coordination problem are closely linked, even in the 1920s there were signs of a crucial disagreement. From Schlick's starting point, it is tempting to assume that coordination is an act of stipulation that lies at the basis of all of our scientific knowledge. It seems to follow that a particular act must involve only what is given independently of all scientific

knowledge. This restricts the primary referents of the non-logical scientific terms to Mach's neutral elements or some similarly unconceptualized experience. Reichenbach is able to avoid this extreme conclusion by conceiving of the coordinating axioms as substantial claims about a realistically construed physical world. The claims are substantial because they could be false. For example, the definition of rigid bodies makes use of a distinction between internal and external forces, and this in turn assumes that sense can be made of a closed system. Unfortunately, "a closed system can never be strictly realized" (Reichenbach 1958, p. 23), and so some limiting procedure must be invoked that assures us that our purported rigid body would behave in a certain way in a series of increasingly isolated systems. The validity of this limiting procedure is a risky claim about the world. The challenge for Reichenbach is to explain how we can support these sorts of claims. Ultimately Reichenbach must posit certain highly general regularities tied to causation and probability. These regularities lie at the heart of our scientific theories, but Reichenbach has difficulty accounting for our knowledge of them.

Rudolf Carnap's *Logical Structure of the World* (also known simply as the *Aufbau*) (Carnap 2003) is sometimes read as a proposed solution to Schlick's coordination problem. On this interpretation the *Aufbau* gets by with a single act of coordination between the relation symbol "Rs" and the relation of recollected similarity on the total momentary experiences of an individual. The definitions of scientific concepts that Carnap offers as part of his "autopsychological" constitution system are then analogous to Schlick's implicit definitions. On an alternative interpretation, which I prefer, Carnap's constitution system is offered instead as a rational reconstruction of our scientific knowledge.[3] Taking our scientific knowledge for granted, Carnap reorders it to reflect relationships of epistemic priority. The acts of coordination emphasized by Schlick and Reichenbach thus find a place within the constitution system itself (Friedman 2007, p. 208). There is no account of how our words hook on to reality as the notion of a system-independent reality is dismissed as metaphysical. This interpretation of Carnap's *Aufbau* suggests a somewhat different methodology for philosophers. Now the aim is to build a unified conceptual system within which all of our concepts can find a place. Some of these efforts will resemble the implicit definitions or coordinative definitions of Schlick and Reichenbach. But there is no attempt to transcend the system itself, and so coordinative definitions in their objectionable ostensive or realistic forms are avoided. Unfortunately, in the *Aufbau* itself Carnap devoted little space to indicating the nature of logic or how logical structure might be isolated. Without these clarifications, it remained unclear how merely arranging our concepts in a logical system could address the worries that motivated the original coordination problem. Investigations of the nature and philosophical significance of logic thus formed the next phase of logical empiricism's methodological development.

[3] See Pincock 2009 for a survey of different interpretations.

II. Logic

In its early stages, most logical empiricists were content to rely on the new logic of Frege, Russell and Hilbert without placing the nature of logic on the philosophical agenda. The central role of logic in discussions of coordination put pressure on this methodological position. The pressure is clear in the unsatisfactory discussions of logic in the works by Schlick, Reichenbach and Carnap just reviewed.[4] The situation changed considerably when logical empiricists turned to a careful study of Wittgenstein's *Tractatus* (Wittgenstein 1974). Schlick and Carnap developed quite different conceptions of the role of logic in philosophy in this period. These differences showed themselves clearly in a debate about the correct way to present the most basic elements of our knowledge. This is the notorious "protocol sentence" debate of the 1930s. We will return to Reichenbach at the end of the next section.

Schlick coopted a number of Tractarian doctrines and embraced a form of verificationism that displaced the limited scientific realism of *General Theory of Knowledge.* A concise formulation of this new position is to be found in "The Turning Point in Philosophy" (Schlick 1930), the paper that opens the newly launched journal *Erkenntnis*. Announcing the end of "the fruitless conflict of systems" (Schlick 1930, p. 54), Schlick extolled "methods which make every such conflict in principle unnecessary" (Schlick 1930, p. 54). According to Schlick, Wittgenstein has determined the nature of logic by focusing on what is essential to all representations. He agrees with Wittgenstein that this form cannot be represented. So, the task that is left to philosophy is merely one of clarification. This activity is ultimately the assignment of meanings to linguistic items. These meanings cannot be assigned by using statements, but the process "always comes to an end in actual pointings, in exhibiting what is meant, thus in real acts" (Schlick 1930, p. 57). It is clear that Schlick is here building on his earlier focus on coordinative definitions. But the new Wittgensteinian conception of logic closes off any further role for philosophy. Schlick is now also clear that these acts must involve only what is given to us in immediate experience. This makes his earlier realism unattainable. For our words can only mean what we point to, and Schlick assumes we can only point to our immediate experiences.

Schlick can be seen to illustrate one extreme in the methodological development of logical empiricism. It seems obvious that one role for philosophy is to clarify claims, but Schlick's view that this is the only possible philosophical activity clashes with the wide variety of activities that have been historically ascribed to philosophers. Furthermore, Schlick's insistence that the only material available in "actual pointings" is immediate experience is unmotivated. By the time of "The Foundation of Knowledge" (Schlick 1934) the untenability of Schlick's account of philosophy was becoming obvious. There Schlick places a special sort of

[4] Schlick 1985, part II, Carnap 2003, section 107, and Reichenbach 1958, section 7.

statement that he calls a "confirmation" at the foundation of our knowledge.[5] They are foundational because "I grasp their meaning at the same time as I grasp their truth" (Schlick 1934, p. 225). To play this role, they must pertain only to purely demonstrative matters. All ordinary statements are thus merely hypotheses that are confirmed to some extent by the confirmations that they generate. Scientific knowledge, then, rests on a slender basis of private experiences of satisfaction. Philosophy aims to isolate this slender basis and force scientific theories into this rigid Wittgensteinian framework.

Carnap proceeded in a completely different direction and developed what was to become the most lasting methodological innovation of the logical empiricists. This is the principle of tolerance and the corresponding conception of philosophy and its methods. Carnap began this phase of his development by embracing the view of the *Tractatus* that there was only one logic that reflected the essential features of any system of representation. This position is found in the influential "The Elimination of Metaphysics Through Logical Analysis of Language" (Carnap 1932). There Carnap criticized Heidegger for violating the rules of logical syntax. This criticism is only coherent if there actually is a single system of rules for logical syntax that we can hold up as a standard against which to measure the statements of others. But soon after this paper Carnap accepted the more tolerant position: "In logic, there are no morals. Everyone is at liberty to build up his own logic, i.e. his own form of language, as he wishes. All that is required of him is that, if he wishes to discuss it, he must state his methods clearly, and give syntactical rules instead of philosophical arguments" (Carnap 2002, p. 52). If one accepts the principle of tolerance, then it is still possible to criticize Heidegger for failing to "state his methods clearly" as the proposed logical syntax of Heidegger's language is far from clear. But this sort of criticism is different from the dogmatic insistence that one's opponent is engaged in meaningless metaphysics.

Carnap's path to tolerance is a complex one (Awodey and Carus 2007). Even in *Aufbau* Carnap had allowed several systems of definitions of our scientific concepts ("constitution systems"). He seems to have originally aimed for a single logical framework that would allow the comparison and investigation of these systems and the associated axiom systems of mathematics. However, Gödel's incompleteness theorems eventually convinced Carnap that this sort of all-embracing metalanguage was not achievable. The alternative was to allow that any language could be presented as a candidate for the language of science. To propose a language one must specify the vocabulary along with the formation rules for sentences and the transformation rules for inferences. In *Logical Syntax of Language* Carnap offers an elaborate procedure that will allow one to use these rules to isolate the logical expressions along with those sentences that are L-valid or analytic for a given language (Carnap 2002, sections 50-52). Carnap clearly hoped

[5] The German word "Konstatierung" is also translated as "affirmation".

that the logical and mathematical statements of his languages would wind up as analytic. Strictly speaking, though, there is no prior language-independent way to pick out the mathematical statements of a language to check whether this aim is realized. All that is available is the language-relative distinction that results from Carnap's definitions.

It is difficult to appreciate how Carnap sought to use these proposed languages to address anything like our original coordination problem. The challenge is to link a descriptive expression of some proposed language to an experience or object in the physical world. Without such a link, it seems that Carnap's languages are purely formal entities that are poor candidates to be vehicles for genuine scientific knowledge. Carnap encourages this interpretation by insisting that all of philosophy is merely the investigation of the logical syntax of these languages. In restricting the focus of philosophy to logical syntax, Carnap aims to exclude questions about reference, meaning and truth. To the extent that such semantic questions are legitimate, it must be possible to translate the "material mode" semantic question into an equivalent "formal mode" syntactic question. For example, a statement in the material mode about reference could be equivalent to a formal mode statement about the occurrence of a word or its synonyms, where synonymy is treated in terms of the syntactical rules of the language in question.

Carnap grants that one could present a syntactic definition of a kind of "protocol" sentence that would serve a special role in the testing and confirmation of scientific hypotheses. However, his position in *Logical Syntax* appears to be that the philosopher is not tasked with determining which protocol sentences one should assert: "the statement of the protocol-sentences is the affair of the physicist who is observing and making protocols" (Carnap 2002, p. 317). This is quite different from Schlick's attitude. Schlick sought a special connection to experiences that would privilege the protocol sentences and explain their special epistemic role. Carnap instead delegates the choice of protocol sentences to the scientist. It is only within a given language that we can make acceptable inferences, and so it appears that Carnap has nothing to say about which protocol sentences are correct. Carnap is more forthcoming in "Testability and Meaning" (Carnap 1936). There he supplements his "Logical Analysis of Confirmation and Testing" with an "Empirical Analysis of Confirmation and Testing". So it looks like he has decided that empirical matters are properly philosophical as he devotes 17 pages to discussing them in one of his philosophy papers. But when we turn to the contents of that section, we again find Carnap deferring to scientific findings. Two basic terms "observable" and "realizable" are assumed and used to define other terms like "confirmable". But the basic terms are defined "within psychology, and more precisely, within the behavioristic theory of language" (Carnap 1936, p. 454). This marks a clear boundary between the formal, logical studies of the philosopher and the related, though distinct, empirical work of psychologists and linguists.

Carnap's mature conception of philosophy retains tolerance while allowing semantic concepts to play a part in genuinely philosophical projects. The main impetus for this shift was Tarski's theory of truth. Tarski showed how the concept of truth for a given language L1 could be defined in a metalanguage L2 if L2 was granted certain expressive resources. This convinced Carnap that semantic notions such as reference, meaning and truth were no more problematic than the syntactic notions deployed in *Logical Syntax*. After this shift Carnap was forced to repeatedly make clear that his notions of meaning and truth had none of the traditional metaphysical overtones that critics worried about. Notoriously, Ryle complained in his review of *Meaning and Necessity* that Carnap had assumed a discredited "'Fido'-Fido" conception of language (Ryle 1949).[6] Carnap responded in "Empiricism, Semantics and Ontology" (Carnap 1950a) that his semantic notions are internal to a given metalanguage, and are not intended to take a stand on the traditional metaphysical question of how words hook on to objects in some mind-independent reality. Existence claims thus come in two flavors. If one asserts "There are Fs", then this could be meant as a claim internal to a given language. In the special case where the existential claim follows simply from the rules of the language in question, then the claim is analytic and the existence of these entities is guaranteed for anyone who adopts that language. This was Carnap's attitude towards numbers and semantic entities like propositions. In other cases where "There are Fs" is a well-formed sentence of the language, but neither it nor its negation follows from the rules, then we have an ordinary empirical claim whose status must be resolved by ordinary scientific methods. But if the claim "There are Fs" is meant as an external claim, then Carnap denies that such claims make any sense: "An alleged statement of the reality of the system of entities is a pseudo-statement without cognitive content" (Carnap 1950a, p. 214). All that can be accomplished by such an external claim is that the speaker makes a proposal for what sort of language to adopt.[7] And for Carnap the proper language to adopt is determined only by one's practical purposes such as building a rocket or feeding the inhabitants of a city. So, Carnap was not only opposed to the "'Fido'-Fido" theory of language that Ryle saw in *Meaning and Necessity*, but was of the opinion that any such theory was theoretically meaningless.

Even after the turn to semantics, then, the methods available to the philosopher qua philosopher are quite restricted. A philosopher can propose and investigate formal languages. These languages or linguistic frameworks are individuated by their syntactic and semantic rules. It is possible for the philosopher to distinguish certain terms as observable and the rest as theoretical, and to investigate the logical relationships between sentences formed using these terms. In large measure, these languages are developed with an eye on the needs of science. Ultimately the goal is to find a formal language that could serve as the

[6] On this view all meaningful terms are thought to be names. See also Eklund 2010.
[7] See Price 2010 and Kraut forthcoming for two elaborations of this view.

language of a unified science that would incorporate all the various branches of science into one precise system. Unsurprisingly Carnap never came close to achieving such a language himself. He spent much of the last phase of his career considering the problem of probability and confirmation. At the same time, he tried to refine the logical tools that the philosopher could use to develop new languages. Carnap insisted that the philosopher cooperate with the scientist when executing her philosophical tasks. This engagement takes two forms. First, the philosopher should find out from the scientists what sort of scientific theories are being developed, and use this information to focus her philosophical efforts on languages of the appropriate form. Second, the philosopher should cooperate with the linguist to see how the formal languages found in philosophy can be mapped on to the linguistic behavior of actual speakers. This sort of "pragmatics" of language is not part of philosophy, but without having an eye on what speakers actually do, and how their behavior could be reformed to better achieve practical goals, the formal activity of the philosopher risks being pointless (Ricketts 2003).[8]

Carnap's conception of philosophy and its methods achieved a kind of internal coherence that has continued to attract those skeptical of traditional metaphysics or impatient with never-ending philosophical disputes. For a dedicated Carnapian, there are no genuine philosophical disagreements, but only unclarities tied to the failure to articulate one's linguistic framework. The optimistic attitude is that if we were only to take the time to make these commitments explicit, then endless philosophical debates would be ended. For the non-Carnapian, however, the conception of philosophy offered by the Carnapian can seem unduly restrictive. For we typically intend to make factual claims, and the Carnapian cannot countenance such claims in the domain of philosophy. And most philosophers aim to deploy methods quite different from merely proposing and analyzing linguistic frameworks.

Two large challenges face the Carnapian conception of philosophy. They are what I will call the neutrality problem and the naturalism problem.[9] Recall that the principle of tolerance insisted that we are free to choose the logic of our language. This suggests that the Carnapian should occupy a neutral vantage point from which she can assess the available options for the logic of the proposed language of science. At the same time, it is clear that a metalanguage must be chosen for this sort of comparison to even get started. The neutrality problem is that the choice of

[8] Uebel 2013 ascribes a "bipartite metatheory" of science to Carnap that includes these pragmatic investigations. However, Uebel is in general agreement with our interpretation of Carnap as he is clear that pragmatic investigations, though important, lie outside of the scope of philosophy: "Carnap was more concerned to distinguish his logic of science from these empirical studies than to pursue them" (Uebel 2013, p. 526).

[9] A third problem tied to the limits of formalization will be discussed in section IV.

metalanguage will bias our choice for the language of science and thus deprive the Carnapian of their neutrality (Friedman 1999, ch. 9, esp. sections VI and VII). For example, if we choose a metalanguage with classical logic, then any worries about classical logic for the object language will be easy to address. What this neutrality problem highlights is that even the most dedicated Carnapian must recognize a role for decisions that are not subject to any further rational scrutiny. It might seem like everything is open to examination and stands to be tested by its efficiency as a means to our chosen ends. But this sort of neutrality is impossible to achieve.

The naturalism problem is known to most contemporary philosophers in the form raised by Quine. We have seen that Carnap's entire conception of philosophy depends on the distinction between statements that are licensed by the choice of the language and statements that are licensed by other reasons. The naturalist challenge rejects this distinction. For the naturalist (as I will use this term), at any given time our choice of which statements to accept is determined both by the language we have adopted and by our empirical evidence. It is not possible or desirable to try to separate our choices into the two factors that Carnap identifies. Quine raised the naturalism problem from the narrow perspective that insisted that only the physical sciences had standing in our philosophical deliberations (Creath 2007). However, there is no need for the naturalist to accept this restriction. One could instead begin with the broad assumption that both the physical and social sciences were equally deserving of our respect. The resulting conception of philosophy is to be found in Otto Neurath. We can take Neurath's naturalism to involve a different solution to our original correspondence problem.

III. Context

For Neurath, the solution to the correspondence problem lies not in the nature of logic, but in the activities of human agents engaged in the task of navigating a complex world. Scientists are already in contact with the world when they use language to communicate with one another and deploy measuring instruments to physically interact with the world. There is thus no need to coordinate the uninterpreted abstract statements of Schlick. Furthermore, Neurath was highly skeptical of the scientific significance of appeals to the private experiences of individuals. Science as observed proceeds by public statements and procedures whose reliability consists in the potential for scrutiny and checking by other scientists. Neurath's approach to philosophy emphasized all of these themes, although he often became side tracked by the need to engage in polemic exchanges with other philosophers, including Schlick and Carnap.

Much of Neurath's philosophical methodology can be gleaned from the famous boat metaphor from "Protocol Sentences" (Neurath 1932):

> *There is no way of taking conclusively established pure protocol sentences as the starting point of the sciences*. No *tabula rasa* exists. We are like sailors

> who must rebuild their ship on the open sea, never able to dismantle it in dry-dock and reconstruct it there out of the best materials. Only the metaphysical elements can be allowed to vanish without trace. Vague linguistic conglomerations [Ballungen] always remain in one way or another as components of the ship (Neurath 1932, p. 201).

A number of important points are combined in this one passage. To start, it is clear how hostile Neurath is to Schlick's approach to coordination. Schlick required an implicitly defined network of terms to be coordinated with the immediate experiences of individuals. This sets up Schlick's confirmations as "pure protocol sentences" that are entirely responsible for the empirical content of our scientific statements. By contrast, Neurath insists that we are already using a language whose content is more or less acceptable as it stands. It remains imperative to reform this language, clarify its overall structure and purify the statements we accept of needless accretions. But the reconstruction that Schlick offers is unable to assist in this process.

In his positive proposal for protocol sentences Neurath presented a stark alternative to Carnap's program as well. In the 1932 paper Neurath offered the following as a potential protocol: "Otto's protocol at 3:17 o'clock: [At 3:16 o'clock Otto said to himself: (at 3:15 o'clock there was a table in the room perceived by Otto)]" (Neurath 1932, p. 202). A charitable reconstruction of what Neurath intends here has been offered by Uebel (Uebel 2007, ch. 11). The basic idea of this interpretation is that Neurath is attempting to clarify the process of scientists offering and evaluating protocols or reports on their experimental observations.[10] A scientist might actually say something like "At 3:15 o'clock there was a table in the room". But Neurath notes that the scientific community does not accept this report without first carefully considering it. On Uebel's interpretation, the reformed protocol that Neurath offers is meant to tease out the various tests that the scientific community may impose before they will accept the embedded report. These tests are not merely formal, but also involve considerations tied to the social and political context. By making this critical process more explicit in the form of the proposed protocol statements themselves, Neurath hopes to aid the scientific process of testing and hypothesis refinement. At the same time, the proposal is meant to make clear that no appeal to private experiences or other philosophical refinements is needed to make sense of scientific testing and scientific knowledge more generally. Some philosophers might try to screen out the contextual factors that Neurath emphasizes, but Neurath shows no interest in removing these sorts of considerations. His hope seems to have been that the best kinds of tests that we

[10] So, Neurath is like a sailor attempting to rebuild his ship while at sea. He is a scientist who is trying to improve the practice of science by making these critical suggestions.

can hope for are just those scientific procedures that we already have, once they are made clear and open to community-wide participation.

The outermost layer reflects the need to check if Otto is really offering the statement as a scientific report. If Otto is not a scientist, his protocol might be rejected even at this stage. The next layer corresponding to "Otto said to himself" focuses on the sincerity of Otto's protocol. He might, for example, be lying, and so it would not be right to say that he said the rest of the protocol to himself. Finally, there is the question of what was actually perceived by Otto. Here it might be that Otto is hallucinating or making some other sort of error in reporting his perception. If all these tests are met, then it is appropriate to add the embedded report to the body of statements accepted by the scientific community. Even here Neurath allows that there might be good reason to reject the report. This might happen if accepting the report would undermine a valuable store of accepted statements.

It should clear how important the social context of the scientific process is to making sense of this procedure. At each stage the choice about how to proceed is not determined by any logical rules or formal procedure. There is ultimately only the judgments of individual scientists and their collective practices. As Neurath once cryptically put it, "a logically tenable multiplicity is reduced by life" (Neurath 1935, p. 117), where "life" includes our historical and social situation. This has left the impression that Neurath mandated some kind of coherence theory of truth or worse, as with Russell's unfortunate remark that according to Neurath "empirical truth can be determined by the police" (Russell 1940, p. 140). To start, just as Carnap did not offer any traditional account of how words refer to things, Neurath did not offer any traditional theory of truth. His considered view is that the notion of truth was just one of those metaphysical concepts that the ship of science should discard (Mancosu 2008). Instead, Neurath was offering an account of statement acceptance and in these terms he insisted that no algorithm for statement acceptance was available. This is because the factors that are relevant to accepting or rejecting a statement are not exhausted by elements that can be captured in the formally defined linguistic frameworks familiar from Carnap. As we saw earlier, Carnap attempted to consign these contextual matters to the "pragmatics" of language use and the engineering decision about which logical means are best to achieve our practical goals. Carnap separated out these contextual factors from the proper task of the philosopher. Neurath can be seen, then, as emphatically reintroducing these factors and arguing that they are indeed central to any appreciation of how science works. To the extent that this context includes history as well as social and political values, Neurath's proposal is quite radical.[11]

Neurath proposed a philosophical methodology, then, where the formal, logical studies of Carnap would live along side a "behavioristics" of science that

[11] Quine construed the relevant context much more narrowly, and this marks a decisive difference between Quine's and Neurath's philosophical methodologies.

would describe and reform the behavior of the scientific community. Neurath clearly thought that the latter task was the primary one for only it would make contact with scientific knowledge as it actually is. This is one way to understand Neurath's work on the unity of science project. For many years Neurath sought to create a kind of encyclopedia that would bring together in a series of volumes the scientific knowledge of his day.[12] It would obviously have made little sense to have these volumes written in the logical languages that Carnap was developing. They were written in a natural language so that the findings of the special sciences could be absorbed and used by other scientists and even the general public. The political dimension of this project is hard to ignore. For Neurath, the ultimate goal was a widely accessible summary of natural science and social science that could be used to better educate individuals and allow enlightened policies tied to central planning and social progress.

Neurath's main ally in this project was Philipp Frank. In "The Place of Logic and Metaphysics in the Advancement of Modern Science" (Frank 1948) Frank complements Neurath's approach by arguing that metaphysics results from a failure to understand how the problem of coordination is to be solved (Uebel 2011). As with other logical empiricists, Frank insists that contemporary scientific theories deploy concepts that have little contact with common sense or ordinary experience. Problems arise when philosophers and scientists fail to recognize this break, and try to force scientific hypotheses into a predetermined interpretive framework:

> What we call in a vague way "common sense" is actually an older system of science which was dropped because new discoveries demanded a new conceptual scheme, a new language of science. Therefore the attempt to interpret scientific principles by "common sense" means actually an attempt to formulate our actual science by the conceptual scheme which was adequate to an older stage of science which is now abandoned (Frank 1948, p. 285).

This attempt is bound to fail, and what we are left with are vague analogies between common sense terms and the terms of the new science. This not only complicates any understanding of what the scientific hypotheses are actually aiming at, but blocks the necessary testing and revisions that constitute scientific progress.

In this diagnosis of the nature of metaphysics, with all of its historical and psychological aspects, Frank is exemplifying the broad scope of philosophy as he envisioned it. The methodological program that Frank sought to execute is described in papers like "The Institute for the Unity of Science: Its Background and Purpose" (Frank 1947). The role of the philosophers goes far beyond merely articulating formal linguistic frameworks and seeing how these frameworks fit or

[12] See Cartwright, Cat, Fleck and Uebel 1996 and Reisch 2007 for additional discussion and references.

fail to fit with scientific practice. The philosopher must also engage with the history and political context of science as it is actually done. This allows a more effective intervention in the practice of science so that the logical insights of the more restricted formal studies can be actually used to make science work better. The Institute that Frank describes will instill a "new logic of science" (Frank 1947, p. 163) that will supersede the outdated old logic of science that sought to interpret all science in Newtonian terms. This traditional approach answered the special sciences' search for unity by an artificial or superficial interpretation that failed to do justice to what each special science had accomplished. The first part of this project is a "logico-empirical or semantical analysis" (Frank 1947, p. 165) that will render each of the special sciences in an appropriately unified language. Here Frank cites Neurath's unity of science volumes as a successful step in this direction. However, a second part is essential: "In order to understand a particular choice of symbols logico-empirical analysis has to be complemented by an investigation of the psychological and sociological causes which have led to this choice" (Frank 1947, p. 166). This "socio-psychological analysis" is critical because only it will allow a true understanding of why science has developed in the way that it has.

Few would recognize this sort of contextually-situated and politically-motivated scholarship as properly philosophical. A canonical response to the programs of Neurath and Frank is offered by Reichenbach in the 1930s. As Reichenbach's views developed he continued to defend a form of scientific realism that he argued was the natural development of logical empiricist ideals (Reichenbach 1936). The key move that Reichenbach argued for was the replacement of a verificationist theory of meaning with a theory of meaning based on probability assignments. While verificationism led to the sort of positivism defended by Schlick, the allowance for probability assignments as a form of meaning was a crucial first step for accepting meaningful claims about the future and unobservable entities. In the course of making this argument Reichenbach offered his famous distinction between the context of discovery and the context of justification. Epistemology aims to "construct justifiable sets of operations which can be intercalated between the starting-point and the issue of thought-processes, replacing the real intermediate links" (Reichenbach 1938, p. 5). Psychology aims merely to describe the actual processes of thinking without reference to their correctness. When we consider the context of justification of a given claim, though, we should consider only the evidence for this claim. With the distinction between discovery and justification in place, Reichenbach argues that the philosopher's ultimate focus is rational reconstruction, and any interest in social and political factors in scientific decision-making is not properly philosophical.

It is important to emphasize that this distinction is merely the first step in Reichenbach's mature theory of philosophical methodology. He goes on to emphasize that epistemology has a significant descriptive component in addition to its critical component (Reichenbach 1938, p. 11). The description of scientific

activity, when suitably reconstructed in the context of justification, reveals the need for many "volitional decisions" (Reichenbach 1938, p. 9) at the heart of science. The most important of these decisions fix the aims of science. Unfortunately, scientists and philosophers may misunderstand the relationship between the decisions that have been made, and so fall into a kind of incoherence. The "advisory task" (Reichenbach 1938, p. 13) of the epistemologist is thus to set forward coherent sets of decisions, principally in connection with the ends chosen for scientific activity.

This is how Reichenbach conceives of his theory of meaning in terms of probability assignments. It is a "given sociological phenomenon" (Reichenbach 1938, p. 146) that scientists make predictions about the future and act on these predictions by, for example, taking out life insurance policies. Reichenbach argues that a positivist theory of meaning in terms of conclusive verification cannot make sense of these actions. So, if we are to propose a language that might license these actions, then only a realistic language and the associated procedures for probability assignments are allowed. This is the set of coherent decisions that does the best job of making sense of our current use of words like "knowledge" and "prediction".

While Reichenbach's choice of language creates the space for justified beliefs about the future and unobservable entities, it is far from clear that he actually delivers the justifications. The main problem is that Reichenbach is not able to justify the probability assignments that go into his probability calculations. He is eager to defend a frequentist interpretation of probability. On this interpretation a probability claim is about the relative frequencies of certain outcomes over a repeated process. Reichenbach aims to use this very notion of probability in "projections" from the observed to the unobservable (Reichenbach 1938, pp. 124, 154-155). However, he is also aware that in many important cases the kinds of claims in question lack a sufficient track record to be placed in an appropriate reference class. Reichenbach's solution to this problem is to invoke what he calls a "blind posit" (Reichenbach 1938, p. 353). This is a bet or wager on a claim that is justified because it is the best means of achieving some end. These posits lie at the heart of Reichenbach's justification of induction and the associated probability assignments for scientific claims. The basic idea is that logical reasoning alone can assure us that a particular rule of induction is the best one available. It is not guaranteed to work, but will work if any such rule will work.

Reichenbach's project of vindicating scientific realism thus founders on two fronts. First, as the many discussions of his justification of induction have made clear, his choice of inductive procedure does not appear to have the privileged place he assigned it (Salmon 1991, Galavotti 2011). Second, even if the choice of the rule for induction could be defended, it remains unclear how this rule will translate into appropriate probability assignments for scientific theories. Until these

problems can be overcome, one must judge Reichenbach's defense of scientific realism as incomplete.[13]

Despite its limitations, Reichenbach's conception of philosophy was quite influential, especially in the philosophy of science. In *How the Cold War Transformed Philosophy of Science* and elsewhere, Reisch has documented the extent to which Neurath's and Frank's more expansive conception of the philosophy of science was marginalized by more or less political considerations (Reisch 2005, 2007). From this perspective, the more technical programs pursued by Reichenbach and Carnap flourished in part because of their less threatening and more specialized character. One can grant Reisch's historical point, though, and still ask how we should evaluate these developments today. I have suggested that the philosophical methodology of Carnap is too restrictive and that Reichenbach was unable to justify his conception of our scientific knowledge using the resources he allowed himself. But this by itself does not vindicate the expansive programs of Neurath and Frank. Just as few philosophers would agree to adhere to Carnap's methods, few contemporary philosophers would wish to take on, in their philosophical work, the historical and political burdens that Neurath and Frank carried. Reichenbach once praised logical empiricism in these terms: "The strength of this group lies in its common working program and not in a common doctrine – a program which distinguishes it from philosophical sects, and makes possible progress in research" (Reichenbach 1936, p. 142). It may be that a refusal to engage in political matters is actually a strength of this strand of logical empiricism for it allows a "common working program" that would be undermined by inevitable political disagreements. Supporters of Neurath and Frank would tend to view this judgment as a politically naïve preference for the "icy slopes of logic", or worse, as a sort of tacit support for unsavory political ideals.

IV. The End of Logical Empiricism

In the 1960s logical empiricism entered a late and somewhat moderated phase. Many of the more strident methodological pronouncements associated with Schlick, Reichenbach, Carnap, Neurath and Frank were qualified or combined with new methodological commitments that arguably mark the end of logical empiricism. We see these developments clearly in the work of Carl Hempel. In his early work Hempel sided more or less with Carnap and Reichenbach on the restricted character of philosophical investigations. In his later work he admitted a role for the context of science that marks a break with some of the core ideals of logical empiricism and aligns him more closely with contemporary philosophy.

Hempel is best known for his accounts of confirmation and explanation (Hempel 1965). Two papers along these lines are "Studies in the Logic of

[13] Psillos 2011 provides a recent, critical reconstruction of Reichenbach's argument for scientific realism.

Confirmation" and "Studies in the Logic of Explanation". Hempel offers little by way of methodological reflection in the course of carrying out these studies. One reasonable interpretation is that he takes himself to be revealing what confirmation and explanation really are. The tools for these projects are exclusively formal and logical. For example, Hempel distinguished a species of explanation that he dubbed deductive-nomological. A deductive-nomological explanation is one that involves a valid argument with true premises, where at least one premise is a scientific law. Hempel sought to complete this account by further providing formal criteria for a given statement to be a law. No appeal to the "material" of science such as its subject matter or broader context was permitted.

One can interpret this sort of work as a natural development of Reichenbach's procedure in *Experience and Prediction*. Reichenbach took his job to be to assemble coherent sets of decisions for matching the means and the ends of science. Once these sets of decisions were clear it was possible to see which set, if any, could match the current practice of science. However, Reichenbach seems to have given no special standing to science as it was done. A given set of decisions was not justified because it conformed to current practice. Hempel's formal studies suggest that he *did* take current practice to be more or less justified. All that was required was that one deploy more sophisticated logical tools and demand a level of precision and clarity that ordinary scientists failed to observe. From this methodological perspective, then, the philosopher proposes formal models for a given concept, and tests them by seeing to what degree they validate current scientific practice.

Although it is difficult to know if this is how Hempel conceived of this work at the time it was done, this is certainly how he describes it in his late essay "Valuation and Objectivity in Science" (Hempel 1983).[14] Hempel here uses Carnap's term "explication" and applies it to his earlier work. These explications "propose explicit and precise reconstructions of vague concepts that play an important role in philosophical theories of knowledge", including "confirmation, inductive reasoning, types of explanation, [and] theoretical reduction" (Hempel 1983, p. 379). Hempel emphasizes the extent to which Carnap's own explications depend on an empirical basis. This is because we must first examine how a given concept is deployed in contemporary science before we can propose an adequate, precise substitute for that concept. Carnap had himself granted the need for these preparatory studies. But in line with our earlier discussion of his mature methodology, I would argue that these observations are not responsible for the ultimate justification of Carnap's proposals. For example, Carnap did grant the importance of "our intuitive judgments concerning inductive validity, i.e., concerning inductive rationality of practical decisions (e.g., about bets)" (given at Hempel 1983, p. 380) as reasons to accept his proposals about probability. However, the only real justification for

[14] Wolters 2003 focuses on this later phase of Hempel's development.

accepting this account of probability is the practical goals that it allows us to achieve. Where we start or what is currently done has no evidential status whatsoever.[15]

For the late Hempel, things are quite different. How we do things now is part of the evidence that we should use to justify our philosophical proposals. This is why appeals to intuitive judgments and empirical observations of scientific procedures are legitimate. Invoking Goodman's celebrated methodology of reflective equilibrium, Hempel insists that explication requires the assumption that

> there is a body of widely *shared* intuitions and unwillingnesses, and that approximate conformity with them provides a justification for acknowledging as sound certain rules of deductive and inductive reasoning. Indeed, without such a body of shared ideas on sound reasoning, there would be no explicandum, and the question of an explicatory theory could not arise (Hempel 1983, p. 380).

Hempel then uses this starting point to relate his earlier formal studies to the "pragmatist" theory of scientific revolutions championed by Kuhn. Hempel takes Kuhn to have shown that scientific practice cannot be accurately reconstructed in formal, logical terms. What is needed, then, is a shift from a restricted, formal rational reconstruction to a "relaxed" rational reconstruction that tolerates some appeal to imprecision: "proper scientific procedures are governed by methodological norms some of which are explicit and precise, while others – including very important ones – are vague" (Hempel 1983, p. 389).

It is but a short step from this methodological proposal of Hempel's to the central methodological quandaries of our own time. For if we take for granted that proposals in the philosophy of science are justified by the current practice of science, then it is tempting to argue that proposals in other areas of philosophy are justified by current practices in other domains, including common sense. Two things are missing from this most recent methodological turn that highlight the gap between the logical empiricists and many contemporary philosophers. First, whatever their considerable methodological differences, the logical empiricists shared Mach's assumption that our current practices stand in need of scientific scrutiny and are ultimately justified only to the extent that they contribute to the ongoing production of genuinely scientific knowledge. Second, the logical empiricists did not hold out any hope of, or desire for, a substantial, autonomous body of philosophical knowledge. This is why the tools they deployed in their critical

[15] Hempel here draws on Carnap's response to Kemeny in Schilpp 1963, p. 978. However, while for Kemeny "our final definition really reproduces the original meaning" (Schilpp 1963, p. 713), in his original discussion of explication Carnap had emphasized the need to dramatically revise meanings in the process of explication (Carnap 1950b, esp. pp. 4, 12-13).

and constructive projects were no different from the tools deployed by scientists more generally. Logic and empirical observation were thought to be enough to get the job done. Any suggestion that genuine philosophical problems existed that had a depth that required special tools or insights would have been met with a great deal of skepticism. In contemporary philosophy, most of us have overcome that skepticism and I would certainly not urge it in the extreme form found in logical empiricism. At the same time, I take a lasting achievement of logical empiricism to be a healthy reminder that there is always one option available for overcoming the gap between the philosophical tools at our disposal and the philosophical knowledge that we aspire to. We may always elect to give up the quest for that elusive sort of knowledge and content ourselves with what science and other more established pursuits have to offer.

References

Awodey, S. and A. W. Carus (2007). Carnap's Dream: Gödel, Wittgenstein and "Logical Syntax". *Synthese* 159: 23—45.

Ayer, A. J. (ed.) (1959). *Logical Positivism*. New York: Free Press.

Carnap, R. (1932). The Elimination of Metaphysics Through Logical Analysis of Language. Reprinted in Ayer (ed.) (1959), pp. 60—81.

Carnap, R. (1936). Testability and Meaning. *Philosophy of Science* 3: 419—471, 4: 1—40.

Carnap, R. (1950a). Empiricism, Semantics and Ontology. Reprinted in R. Carnap, *Meaning and Necessity: A Study in Semantics and Modal Logic*. Chicago: University of Chicago Press, 1956, pp. 205—221.

Carnap, R. (1950b). *Logical Foundations of Probability*. Chicago: University of Chicago Press.

Carnap, R. (2002). *The Logical Syntax of Language*. A. Smeaton (trans.). Chicago: Open Court.

Carnap, R. (2003). *The Logical Structure of the World and Pseudoproblems in Philosophy*. R. A. George (trans.). Chicago: Open Court.

Cartwright, N., J. Cat, L. Fleck and T. Uebel (1996). *Otto Neurath: Philosophy Between Science and Politics*. Cambridge: Cambridge University Press.

Chalmers, D., D. Manley and R. Wasserman (eds.) (2010). *Metametaphysics: New Essays on the Foundations of Ontology*. Oxford: Oxford University Press.

Creath, R. (2007). Quine's Challenge to Carnap. In Friedman and Creath (eds.) (2007), pp. 316—335.

Eklund, M. (2010). Carnap and Ontological Pluralism. In Chalmers, Manley and Wasserman (eds.) (2010), pp. 130—156.

Galavotti, M. C. (2011). On Hans Reichenbach's Inductivism. *Synthese* 181: 95—111.

Frank, P. (1947). The Institute for the Unity of Science: Its Background and Purpose. *Synthese* 6: 160—167.

Frank, P. (1948). The Place of Logic and Metaphysics in the Advancement of Modern Science. *Philosophy of Science* 15: 275—286.

Friedman, M. (1999). *Reconsidering Logical Positivism*. Cambridge: Cambridge University Press.

Friedman, M. (2000). *A Parting of the Ways: Carnap, Cassirer, and Heidegger*. Chicago: Open Court.

Friedman, M. (2007). Coordination, Constitution and Convention: The Evolution of the A Priori in Logical Empiricism. In Richardson and Uebel (eds.) (2007), pp. 91—116.

Friedman, M. and R. Creath (eds.) (2007). *The Cambridge Companion to Carnap*. Cambridge: Cambridge University Press.

Frost-Arnold, G. (2013). *Carnap, Tarski, and Quine at Harvard: Conversations on Logic, Mathematics, and Science*. Chicago: Open Court.

Hempel, C. (1965). *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science*. New York: Free Press.

Hempel, C. (1983). Valuation and Objectivity in Science. Reprinted in J. Fetzer (ed.), *The Philosophy of Carl G. Hempel*, New York: Oxford University Press, 2001, pp. 372—395.

Kraut, R. (forthcoming). Three Carnaps on Ontology. In S. Blatti and S. Lapointe (eds.), *Ontology After Carnap*, Oxford: Oxford University Press.

Lavers, G. (forthcoming). Carnap on Abstract and Theoretical Objects. In S. Blatti and S. Lapointe (eds.), *Ontology After Carnap*, Oxford: Oxford University Press.

Mach, E. (1897). *Contributions to the Analysis of Sensations*. La Salle: Open Court.

Mancosu, P. (2008). Tarski, Neurath and Kokoszyńska on the Semantic Conception of Truth. Reprinted in P. Mancosu, *The Adventure of Reason: Interplay between Philosophy of Mathematics and Logic, 1900—1940*. Oxford: Oxford University Press, 2010, pp. 415—439.

Nemeth, E. (2007). Logical Empiricism and the History and Sociology of Science. In Richardson and Uebel (eds.), (2007), pp. 278—302.

Neurath, O. (1932). Protocol Sentences. Reprinted in Ayer (ed.) (1959), pp. 199—208.

Neurath, O. (1935). Unity of Science as Task. Reprinted in O. Neurath, *Philosophical Papers*, *1913—1946*, Boston: D. Riedel, 1983, pp. 52—57.

Neurath, O., H. Hahn, and R. Carnap (1929). The Scientific Conception of the World: The Vienna Circle. Reprinted in O. Neurath, *Empiricism and Sociology*, Boston: D. Riedel, 1973, pp. 299—318.

Pincock, C. (2009). Carnap's *Logical Structure of the World*. *Philosophy Compass* 4/6: 951—961.

Price, H. (2010). Metaphysics After Carnap: The Ghost Who Walks? In Chalmers, Manley and Wasserman (eds.) (2010), pp. 320—346.

Psillos, S. (2011). On Reichenbach's Argument for Scientific Realism. *Synthese* 181: 23—40.

Reichenbach, H. (1936). Logistic Empiricism in Germany and the Present State of its Problems. *Journal of Philosophy* 33: 141—160.

Reichenbach, H. (1938). *Experience and Prediction: An Analysis of the Foundations and the Structure of Knowledge*. Chicago: University of Chicago Press.

Reichenbach, H. (1958). *The Philosophy of Space and Time*. New York: Dover.

Reisch, G. (2005). *How the Cold War Transformed Philosophy of Science: To the Icy Slopes of Logic*. Cambridge: Cambridge University Press.

Reisch, G. (2007). From "the Life of the Present" to the "Icy Slopes of Logic": Logical Empiricism, the Unity of Science Movement and the Cold War. In Richardson and Uebel (eds.) (2007), pp. 58—87.

Rescher, N. (2006). The Berlin School of Logical Empiricism and Its Legacy. *Erkenntnis* 64: 281—304.

Richardson, A. (2003). The Scientific World Conception: Logical Positivism. In T. Baldwin (ed.), *The Cambridge History of Philosophy: 1870—1945*, Cambridge: Cambridge University Press, pp. 391—400.

Richardson, A. and T. Uebel (eds.) (2007). *The Cambridge Companion to Logical Empiricism*. Cambridge: Cambridge University Press.

Ricketts, T. (2003). Languages and Calculi. In G. Hardcastle and A. Richardson (eds.), *Logical Empiricism in North America*, Minneapolis: University of Minnesota Press, pp. 257—280.

Russell, B. (1940). *An Inquiry into Meaning and Truth*. London: Allen and Unwin.

Ryle, G. (1949). Discussion of Rudolf Carnap: Meaning and Necessity. Reprinted in G. Ryle, *Collected Papers*, vol. 1, London: Hutchinson, pp. 225—235.

Salmon, W. (1991). Hans Reichenbach's Vindication of Induction. *Erkenntnis* 35: 99—122.

Schilpp, P. A. (ed.) (1963). *The Philosophy of Rudolf Carnap*. La Salle, IL: Open Court.

Schlick, M. (1930). The Turning Point in Philosophy. Reprinted in Ayer (ed.) (1959), pp. 53—59.

Schlick, M. (1934). The Foundation of Knowledge. Reprinted in Ayer (ed.) (1959), pp. 209—227.

Schlick, M. (1963). *Space and Time in Contemporary Physics: An Introduction to the Theory of Relativity and Gravitation*. H. Brose (trans.). Mineola: Dover.

Schlick, M. (1985). *General Theory of Knowledge*. A. Blumberg (trans.). Chicago: Open Court.

Stadler, F. (2001). *The Vienna Circle: Studies in the Origins, Development and Influence of Logical Empiricism*. New York: Springer.

Uebel, T. (2007). *Empiricism at the Crossroads: The Vienna Circle's Protocol-Sentence Debate*. Chicago: Open Court.

Uebel, T. (2011). Beyond the Formalist Criterion of Cognitive Significance: Philipp Frank's Later Antimetaphysics. *HOPOS* 1: 47—71.

Uebel, T. (2013). Pragmatics in Carnap and Morris and the Bipartite Metatheory Conception. *Erkenntnis* 78: 523—546.

Van Fraassen, B. (2002). *The Empirical Stance*. New Haven: Yale University Press.

Wittgenstein, L. (1974). *Tractatus Logico-Philosophicus*. D. Pears and B. McGuinness (trans.). London: Routledge and Kegan Paul.

Wolters, G. (2003). Carl Gustav Hempel: Pragmatic Empiricist. In P. Parrini, W. Salmon and M. Salmon (eds.), *Logical Empiricism: Historical and Contemporary Perspectives*, Pittsburgh: University of Pittsburgh Press, pp. 109—122.