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Einstein, Kant, and the Origins of Logical Empiricism

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Many influences combined to shape what came to be known, in its Vienna form, as logical empiricism, or, in its Berlin form, as scientific philosophy: the empiricism of Mill, Mach, and Avenarius; the logicism of Frege and Russell; the conventionalism of Poincaré and Duhem; and the neo-Kantianism of Cohen, Natorp, and Cassirer (for more on the history of the Vienna Circle and its allies, see Coffa 1991; Friedman 1983; Haller 1982, 1985; Kraft 1950; and Proust 1986, 1989). Without question, however, the crucial, formative, early intellectual experience of at least Schlick, Reichenbach, and Carnap, the experience that did most to give form and content to their emergent philosophies of science, was their engagement with relativity theory. Thus, after a few early writings on more general philosophical themes, Schlick first caught the attention of a broader philosophical public with his 1915 essay, "Die philosophische Bedeutung des Relativitätsprinzips," and his 1917 monograph, *Raum und Zeit in der gegenwärtigen Physik,* these to be followed by at lest four more major papers on relativity by 1922. Reichenbach first appeared on the public philosophical stage in 1920 with his *Relativitätstheorie und Erkenntnis Apriori,* which was also to be followed by a long series of books and papers on relativity. And it was relativity, again, that was the focus of Carnap's 1921 Jena dissertation, *Der Raum,* and, in one way or another, every one of his published writings prior to his 1928 *Der logische Aufbau der Welt.*

It is, of course, not surprising that three bright, technically sophisticated, young philosophers would be excited by relativity, especially in the wake of the wave of public interest after Eddington's 1919 eclipse expedition yielded confirmation of the predicted bending of light near the sun. With its radical challenge to received views about the nature of space, time, and gravitation, with its implicit challenge even to the method whereby physics had earlier been done, general relativity suited the rebellious temperaments of young thinkers who were coming of age at a time of political and cultural upheaval and eager to lead a revolt against the philosophy of their elders similar to the revolt then underway against the politics of their elders (for biographical information, see Herbert Feigl's memoir in Schlick 1979, vol. 1, xv–xxxviii; the various memoirs and autobiographical essays in Reichenbach 1978, vol. 1, 1–86; and Carnap 1963). Certainly Schlick and Reichenbach were also attracted not only to the theory, but to its author. Einstein was more than just a world-famous scientific genius. He was a pacifist who was notorious in Germany for his early expression of doubts about German war aims. He was a socialist who was sympathetic to the aims of young Berlin student radicals, like Reichenbach (see Nathan and Norden 1968, 1–57). He was a Jew in a Germany already showing the first signs of a vindictive anti-Semitism. How could one not be drawn to such a man and his science? How could one not seek to develop a philosophy of science that would legitimate the relativity theory's claims to superiority over its predecessors, a philosophy of science that would be legitmated in turn by its success in thus rationalizing the achievements of relativity?

We should not be surprised, then, to see this early engagement with relativity leave its trace in logical empiricism. But relativity turns out to have influenced more than just the broad features of logical empiricism. It was more than just an exemplar of good science to which a new philosophy of empirical scientific method would have to be adequated. As we will see, this early engagement with relativity also determined in crucial ways what might be termed the fine structure of emergent logical empiricism.

More specifically, I want to argue that, in the case of all three thinkers, what spurred the maturation of their philosophies of science was their efforts to clarify the precise sense in which the general theory of relativity, with its novel implications about the geometry of

space, can be said to be an empirical theory, and, in doing so, to defend the theory's claim to good empirical credentials against two kinds of neo-Kantians, those who repudiated the general theory for its apparent denial of the Kantian doctrine of the a priori character of time and Euclidean space as categories of internal and external intuition, and those who sought to reconcile general relativity with Kant. What was required for this purpose was the articulation of a new kind of empiricism, one that negotiates a careful path between a crudely reductive Machian positivism and the excesses of Kantian a priorism. It would have to be an empiricism that acknowledged the need for a constitutive element in cognition, but without granting synthetic a priori status to this constitutive element.

The key to this new empiricism was to be found in what Schlick, Reichenbach, and—to some extent—Carnap had to say about the role of convention in science. In my opinion, this effect was, on the whole, deleterious. To show why, I will tell my story by contrasting the positions of Schlick, Reichenbach, and Carnap on the role of convention with the rather different and more salubrious position of Einstein.

The chief difference, in brief, is this. By the middle 1920s, Schlick, Reichenbach, and Carnap were all defending the view that the propositions constituting a scientific theory could be divided into two mutually exclusive and jointly exhaustive categories—empirical propositions and coordinative definitions—and that only the latter had the status of conventions, so that, once the latter are fixed by conventional stipulation, the truth or falsity of the remaining empirical propositions would be determined unambiguously by empirical evidence, each such proposition having its own, determinate empirical content. Such a view nicely forced the issue with the neo-Kantians. It conceded the need for a constitutive element in human intellection—the conventional coordinative definitions—while denying that any one manner of constitution is privileged, since any of several different conventional coordinations are possible. More importantly, it entailed that once a few rather innocent coordinative definitions were established by convention, the rest of our science, including all the most interesting propositions about the geometry of physical space, would literally be forced upon us by experience. There would be no way for the neo-Kantian to defend the claim that our geometry is true a priori, that is, necessarily and independently of experience. By the early 1930s, Carnap had modified his views in the direction of a more nuanced conception of the role of convention in science, but Schlick and Reichenbach adhered to more or less the same view until the end of their careers.

Einstein was just as eager as Schlick, Reichenbach, and Carnap to defend the empirical integrity of general relativity, and he was no less suspicious than they of facile reconciliations with Kant. But from early on he developed serious reservations about the logical empiricist way of doing this. In particular, he doubted that one could effect such a clean, principled distinction between the empirical and the conventional. Heavily influenced by his reading of Duhem, his view of the structure of scientific theories was all along more holistic; he regarded all of the propositions constituting a scientific theory as being equally capable of revision, and, hence, as being equally conventional. Indeed, this is, I think, at the heart of what he meant by his famous dictum that theories are the "free creations of the human intellect." His truly original answer to the neo-Kantians, a profound answer, in my opinion, also makes ingenious use of a holistic conception of theories.

Einstein's reasons for thus parting company with Schlick, Reichenbach, and Carnap over the role of convention in science were rooted in his earlier critical reading of Mach and Duhem. By 1910, Einstein had come to understand that an empiricism requiring every empirical concept (and, thus, by construction, every empirical proposition) to have its own, determinate, empirical content would cripple theoretical physics. He understood that a freely creative theoretical physics was possible only if one's scientific epistemology envisioned a somewhat looser connection between theory and experience, of the kind recommended by Duhem, wherein only whole bodies of theory, and not individual concepts or propositions, are tested against experience. Einstein's Duhemian empiricism still demanded that theory be grounded in experience, but it permitted the theorist the luxury of introducing constructions that conduced to the simplicity of the total body of theory and thereby to the more adequate representation of deep physical reality. Einstein thus anticipated already in 1910 some of the objections that would later be raised against verificationism, which perhaps explains the affect in Einstein's almost formal declaration of his parting of the ways with Schlick in 1929, when he admonished Schlick for having become too much of a positivist to suit the taste of an old "metaphysician" (and theoretician) like himself.

One must savor the irony of this history. Einstein, on the one hand, and Schlick, Reichenbach, and Carnap, on the other, set out around 1920 to defend the empirical integrity of the new century's most revolutionary physical theory against their common neo-Kantian foe. And they agreed that the answer to the neo-Kantians would require the articulation of a new kind of empiricism incorporating a careful delineation of the role of convention in science. In the cases of Schlick and Reichenbach, at least, this common intellectual enterprise was intertwined with close personal relationships with Einstein, which were marked by warmth, admiration, respect, and more than a little help from Einstein for the careers of Schlick and Reichenbach. By the middle of the decade, however, this common enterprise led them down ever more divergent paths, and by the late 1920s, the growing intellectual separation was mirrored by a noticeable cooling of the personal relationships as well.

The Neo-Kantians and Relativity: Einstein's Early Reaction

Einstein was thirteen when he first read Kant, a study of the first *Kritik* having been recommended to him by his older friend, Max Talmey (1932, 164). As far as we know, he next encountered Kant at the Eidgenössische Technische Hochschule (ETH), where he was enrolled in August Stadler's lectures,[1] though Hans Byland, a school friend from the Aargau Kantonsschule, recalls his reading Kant again around the age of sixteen (see Seelig 1960, 22). And he is claimed to have read and discussed Kant also with the members of Bertha Fanta's salon, the so-called *Fanta-Kreis,* during his year teaching at the Charles University in Prague, 1911–1912.[2] Unfortunately, we have no record of Einstein's reaction to these early encounters, though one might guess that the Einstein who was drawn to Mach while a student at the ETH would have been critical of Kant, at least from the time of his university days. In any case, no evidence shows that Einstein's reading of Kant had any direct, positive influence, either on his physics itself, relativity included, or upon his slowly maturing philosophy of science. On the contrary, what evidence we have suggests that Kant did not leave much of an impression on the young Einstein, "Hume really made a powerful impact on me. Compared to him, Kant seems to me truly weak, but in order to save time I have given up defending this thesis" (Einstein to Paul Ehrenfest, 24 October 1916, EA 9-388).

Paul Natorp was the first major neo-Kantian to publish his thoughts on relativity, these concerning special relativity, in his influential *Die logischen Grundlagen der exakten Wissenschaften* (1910). Writing a few years before general relativity's challenge to the Kantian doctrine of the a priori character of Euclidean geometry would become clear, Natorp saw no essential conflict between relativity and the Kantian critical philosophy, arguing that relativity merely confirmed Kant's thesis, taken over from Newton, that all empirical determinations of space and time must be relative and that, being so, they necessarily presuppose an underlying absolute space and time for their intelligibility.

We have no direct evidence of Einstein's having read Natorp, and certainly no record of his reaction to Natorp. He is likely to have been familiar with Natorp's views on relativity, though, if only through the intermediary of Otto Buek, one of the favorite students of Natorp and Cohen, who struck up something of a friendship with Einstein during the latter's first couple of years in Berlin, 1914–1915 (for more on Einstein's acquaintance with Buek and with Natorp's writings, see Howard 1993).

Discussions with Buek may have awakened Einstein's interest in thinking about connections between Kant and relativity. But the need to think carefully about neo-Kantian reactions to relativity would certainly have been brought home to Einstein by his reading in December 1915, just weeks after the completion of general relativity, of Schlick's first essay on relativity, "Die philosophische Bedeutung des Relativitätsprinzips" (1915; for more on Einstein's relationship with Schlick, see Howard 1984).

Elaborating on remarks in his earlier review of Natorp's *Die logischen Grundlagen der exakten Wissenschaften* (Schlick 1911), Schlick first notes some of Natorp's confusions about the physics of relativity, then explained how, especially in its repudiation of the Newtonian concept of absolute time, relativity really could not be reconciled with the Kantian doctrine of absolute Newtonian time as the a priori category of inner intuition unless one were to restrict the Kantian doctrine to a claim about the qualitative features of our subjective experience of time. But this would not be true to Kant, who regarded the time thus intuited as not merely subjective time but physical time.

Schlick also discusses, at some length, another attempt to reconcile Kant and relativity, this by the Breslau philosopher, Richard Hönigswald, in his *Zum Streit über die Grundlagen der Mathematik* (1912). Schlick concedes Hönigswald's claim that we can distinguish time itself from its measurement, but he rightly disputes the further claim that time itself must be the absolute time of Newton and Kant, noting that "it is quite impossible to get from the relative times to an absolute one by seeking out some common element" (1915; quoted from Schlick 1979, vol. 1, 176). Where does this leave us? Schlick sums up the new situation: "As to the relation of the historical Kantian system to the new theory, we are therefore obliged to conclude that the latter can neither be predicted from it, nor does it find therein a ready-made place into which it can be inserted without gaps. The Kantian concept of time proves to be too narrow to accommodate the advances of principle made by natural science" (ibid., 177).

That Einstein paid careful attention to Schlick's remarks about the neo-Kantian response to relativity is clear from Einstein's letter to Schlick of 14 December 1915, in which Einstein reports that he has just finished reading Schlick's essay, commenting, "It is among the best that have so far been written about relativity. From the philosophical side, nothing at all appears to have been written on the subject that is nearly so clear" (EA 21-611). Einstein then records his more detailed reactions: "The relationship of the relativity theory to Lorentz's theory is explained excellently, and truly masterfully its relationship to the theory of Kant and his followers. Trust in the 'apodictic certainty' of 'synthetic judgments a priori' is seriously shaken by the recognition of the invalidity of just a single one of these judgments" (ibid.).

This letter is the start of what was to become for the next ten years one of Einstein's closest and most important intellectual relationships, certainly his most important relationship with someone who could be characterized as a philosopher of science. Einstein's comment about Kant is the first of what were to be ever more frequent and ever more biting remarks on the Kantian reaction to relativity.

It was to be Schlick with whom Einstein would most commonly share his thoughts about Kant, but not always Schlick. One of Einstein's most revealing early comments—for my argument about how the attitudes of Einstein, Schlick, Reichenbach, and Carnap toward the role of convention in science shaped their differing responses to the neo-Kantians—is found in his correspondence with Max Born. Writing from the resort of Ahrenshoop while on vacation in (most probably) July 1918, Einstein reports:

I am reading Kant's *Prolegomena* here, among other things, and am beginning to comprehend the enormous suggestive power that emanated from the fellow and still does. Once you concede to him merely the existence of synthetic a priori judgments, you are trapped. I have to water down the "a priori" to "conventional," so as not to have to contradict him, but even then the details do not fit. Anyway it is very nice to read, even if it is not as good as his predecessor Hume's work. Hume also had a far sounder instinct. (Born [1969] 1971, 25–26)

Since we will see the idea of reconceiving the "a priori" as the "conventional" again in a few years, in the context of Schlick's reaction to Reichenbach's *Relativitätstheorie und Erkenntnis Apriori,* it is important to note its first appearance here in Einstein's thinking about Kant. Of course, given the close relationship that had already developed between Schlick and Einstein, Schlick may well have suggested this way of regarding the a priori. But the order of influence may just as easily have been the opposite, a conjecture strengthened by noting the absence of any such idea in the discussions of conventions in either the first edition of Schlick's *Raum und Zeit in der gegenwärtigen Physik* (1917) or the first edition of the *Allgemeine Erkenntnislehre* (1918), which appeared shortly after the time of Einstein's letter to Born from which I just quoted.

The neo-Kantian reaction to Einstein began to gather force around 1919, from which time there began to appear a growing number of books and articles seeking either to criticize relativity or to reconcile it with Kant; this flood did not begin to abate until the middle 1920s (for more on the neo-Kantian reactions to relativity, see Hentschel 1990). Einstein paid attention to this literature, usually dismayed by what he found. Typical of his early reaction is a remark in a letter to Schlick of 17 October 1919, where Einstein adds as a postscript, "The philosophers are already eagerly striving to cram the general theory of relativity into the Kantian system. Have you seen the rather foolish dissertation by Sellien [1919] (a member of Riehl's school)?" (EA 21-623). The reference is to the dissertation, *Die erkenntnistheoretische Bedeutung der Relativitätstheorie* (1919), by Ewald Sellien, a student of Henrich Scholz's at Kiel, and a follower of Einstein's Berlin colleague, the leader of the critical realist school, Alois Riehl.

One of Riehl's own students, Ilse Schneider, also produced a forgettable dissertation on Kant and relativity, *Das Raum-Zeit Problem bei Kant und Einstein* (1921). She audited Einstein's lectures around 1919, which led to several conservations with Einstein about Kant and relativity, and her recollections of these conversations give us more of the flavor of Einstein's thinking at this time. On 15 September 1919, Einstein wrote to Schneider, again stimulated by Sellien's dissertation, "I have received the mentioned dissertation by S. (Epistemology and Relativity Theory). . . . Kant's celebrated view on time reminds me of Andersen's tale about the emperor's new clothes, except that instead of the emperor's clothes, it concerns the form of intuition" (Rosenthal-Schneider 1980, 83–84). Surely the most amusing of Einstein's remarks around this time is one for which we have only Rosenthal-Schneider's report:

Once when we had debated for a long time some of Kant's intricate questions and had mentioned the various widely differing interpretations by the Kantians in their schools of philosophy (of which there were about as many as there were universities in the German-speaking countries, sometimes several different ones at the same university) Einstein illustrated his views in the following way: "Kant is a sort of highway with lots and lots of milestones. Then all the little dogs come and each deposits his contribution at the milestones." Pretending to feel indignation I said, "But what a comparison!" But Einstein, laughing loudly, only remarked, "But what will you have? Your Kant is the highway, after all, and that is there to stay." (Ibid., 90)

A few months after he read the Sellien dissertation, Einstein read the first draft of what was to prove the best of the neo-Kantian reactions to relativity, this being Ernst Cassirer's *Zur Einsteinschen Relativitätstheorie* (1921). As he indicated in a letter to Cassirer of 5 June 1920, Einstein was impressed by the way Cassirer had mastered what Einstein called the "spirit" [*Geist*] of the relativity theory, but he was careful to note exactly where he disagreed with Cassirer's philosophical interpretation of the theory and its consequences:

I can understand your idealistic way of thinking about space and time, and I even believe that one can thus achieve a consistent point of view. To me, as a non-philosopher, philosophical contrarieties appear more contrarieties of emphasis than contrarieties of a principled kind. What Mach calls *connec-*

tions, are for you ideal names, which experience first makes possible. But you emphasize this side of knowledge, whereas Mach wants to make them appear as insignificant as possible. I acknowledge that one must approach the experiences with some sort of conceptual functions, in order for science to be possible; but I do not believe that we are placed under any constraint in the choice of these functions *by virtue of the nature of our intellect*. Conceptual systems appear empty to me, if the manner in which they are to be referred to experience is not established. This appears most essential to me, even if, to our advantage, we often isolate in thought the purely conceptual relations, in order to permit the *logically* secure connections to emerge more purely. (EA 8-386)

Cassirer had sought to salvage something of the Kantian doctrine by arguing that while the full Euclidean structure of space may not have a synthetic a priori status, at least some weaker topological structure would have to be assigned this status in order for us to understand what exactly permits the various, more specific metrical determinations that distinguish the different geometries our experiments might reveal. This topological structure would be an instance of the kind of "conceptual function" that Einstein concedes we must bring to our experience of the world. What is interesting is Einstein's denial that *any specific* conceptual function, even such a weak structure as this topology, is required to ground the possibility of knowledge. *Some* conceptual functions are required, he grants, but not any particular ones.

Though he does not say it explicitly in the letter to Cassirer, Einstein believed that the needed conceptual functions, not forced upon us by the nature of the intellect, are matters of convention. That was his point about the *Prolegomena* in his previously quoted letter to Born of July 1918, and it was a view that he continued to affirm. Thus, for example, on 27 September 1922, he wrote to Eberhard Zschimmer:

That which you work out in your essay seems correct to me, at least from the physical side, which is all that I can judge with certainty. In my opinion, though, the important question for the opposition of relativity theory and Kantian philosophy does not emerge sharply enough: are the spatio-temporal, etc. forms, which also ground "a priori" the relativity theory, only convenient tools of descriptions—to be appraised as conventions—or are they givens, necessitated simply by the character of human thought, and inalterable in detail? I, myself, occupy the former standpoint, represented also, e.g., by Helmholtz and Poincaré, whereas it appears to me that Kant's standpoint was more the latter. (EA 24-160)

The essay mentioned, Zschimmer (1923), was intended as a reply to one of the more inferior neo-Kantian assaults upon relativity, Ripke-Kühn (1920). Thirty-seven years later, Einstein was still making the same point in his replies to the essays collected in the Schilpp (1940) volume:

The theoretical attitude here advocated is distinct from that of Kant only by the fact that we do not conceive of the "categories" as unalterable (conditioned by the nature of the understanding) but as (in the logical sense) free conventions. They appear to be *a priori* only insofar as thinking without the positing of categories and of concepts in general would be as impossible as breathing in a vacuum. (Einstein 1949, 674)

Einstein's letter to Cassirer of June 1920 contains the germ of Einstein's later, more considered, critical reactions to neo-Kantianism. We find in it, as well, an anticipation of the principal criticism leveled against Cassirer one year later by Schlick in his review of *Zur Einsteinschen Relativitätstheorie*. Schlick frames the issue in the same way, if not so concisely, as Einstein:

All exact science, whose philosophical justification undoubtedly forms the prime goal of the theory of knowledge founded by Kant, rests upon observations and measurements. But mere sensations and perceptions are not yet observations and measurements; they only become so by being ordered and interpreted. Thus the forming of concepts of physical objects unquestionably presupposes certain principles of ordering and interpretation. Now I see the essence of the critical viewpoint in the claim that these constitutive principles are *synthetic a priori judgments*, in which the concept of the *a priori* has the property of apodeicticity (of universal, necessary and inevitable validity) inseparably attached to it. . . . The most important consequence of the view just elaborated is that a thinker who simply perceives the necessity of constitutive principles for scientific experience should not yet be called a critical philosopher on that account. An empiricist, for example, can very well acknowledge the presence of such principles; he will deny only that they are synthetic and *a priori* in the sense defined above. (1921; quoted from Schlick 1979, vol. 1, 323–24)

Cassirer's error, according to Schlick, is his not realizing that a consistent empiricism can accommodate the need for constitutive principles since he assumes that the only viable empiricism is a phenomenalist positivism of the Machian kind:

He quite rightly condemns the attempt sometimes made by Mach, to treat even analytico-mathematical laws like things "whose properties one can read off by immediate perception," but that does not prove the truth of logical idealism, it merely refutes the sensualist theory. Between the two we still have the empiricist viewpoint, according to which these constitutive principles are either *hypotheses* or *conventions;* in the first case they are not *a priori* (since they lack apodeicticity), and in the second they are not synthetic. (Ibid., 324)

Thus, like Einstein one year earlier, Schlick grants the necessity of constitutive principles, but denies to them an a priori status, viewing them instead either as hypotheses or (like the Einstein of the 1918 letter to Born) as conventions. It is hardly surprising, then, that on 10 August 1921, Einstein wrote to Schlick, "It was nice to be with you. This morning I have read your essay on Cassirer with true enthusiasm. For a long time I have read nothing so sharpwitted and right" (EA 21-638).[3]

To concede the need for constitutive principles while denying to those principles the status of the a priori is a clever response to the neo-Kantians. The elaboration of a complete epistemology of scientific knowledge, however, requires at least a more specific account of the character and status of the needed constitutive principles. If they are not synthetic a priori judgments, what are they? Schlick and Einstein agree that they may be conventions (though, interestingly, Schlick wavers between calling them conventions or hypotheses). But to say that is still not to say enough, since there were even at this time several different ways of regarding conventions. And it was over precisely this question—in what sense are the constitutive principles conventions?—that the once close relationship between Einstein and Schlick was to come apart. Reichenbach, unintentionally, was to begin driving the wedge between them.

Constitution, Coordination, and Convention: Schlick and Reichenbach Create a New Kind of Empiricism

It was most probably in the winter semester 1918–1919 and the summer semester 1919 that Reichenbach attended Einstein's lectures on special and general relativity at the University of Berlin. (See Reichenbach's notes from the lectures, HR 028-01-04 and 028-01-03.) Within a year, he had completed the manuscript of his first book on relativity, *Realtivitätstheorie und Erkenntnis Apriori* (1920), which Einstein announced to Schlick in a postcard of 19 May 1920, "The young Reichenbach has written an interesting treatise on Kant and general relativity in which he also adduces your analogy of the calculating machine" ((EA 21-632)).

According to Reichenbach, Kant ascribed to a priori judgments two chief characteristics. They are, first, necessarily true independently of any experience, and they are, second, the constitutive principles by means of which we construct the concepts of objects manifested to us in intuition. According to Kant, the axioms of Euclidean geometry are synthetic a priori truths defining the categorical form of the space in which the objects of external intuition are constituted for us. Reichenbach argues that if we take this doctrine to apply to physical space as it is described in the general theory of relativity, then we must deny to the a priori the first of the two mentioned characteristics, its apodictic character. But he insists that constitutive principles are still required in order to ground the possibility of experience and knowledge, even if they are not determined once and forever by the nature of the human intellect.

The similarity between Reichenbach's position and that expressed by Einstein to Cassirer just three weeks after announcing Reichenbach's work to Schlick is obvious, so much so that one cannot help but conjecture that Einstein's views had been shaped by discussions with Reichenbach, even if one cannot be sure in which direction the influence ran. However, Reichenbach works out this position with detail and subtlety of a kind not to be found in any of Einstein's writings or correspondence from this time, and the details were to prove most interesting to Schlick.

In explaining how the a priori does its work of constitution, and in explaining why this constitutive function must be separated from the alleged apodicticity of the a priori, Reichenbach made essential use of an idea that Schlick had introduced in 1910, namely, that the truth of a proposition or set of propositions consists solely in its being coordinated unambiguously to a fact or set of facts ((see Schlick 1910; for more on Schlick's conception of truth as unambiguous coordination, Howard 1993; and Ryckman 1991)). But Reichenbach gave his own interpretation to the doctrine of truth as unambiguous coordination. He argued that empirical knowledge is distinguished from rational knowledge by the fact that, in empirical knowledge, perceptions furnish the criterion for the desired uniqueness of the coordination by enabling us to determine whether numerical values derived from different measurements are the same.[4]

Reichenbach uses as his example Eddington's test of Einstein's prediction that light passing near the sun will be deflected by the sun's gravitational field. Einstein's theory of gravitation, plus a variety of measurements (perceptions), yields a prediction of 1.7 seconds of arc for the observed deflection; if Eddington's measurements (perceptions) and the calculations performed on them yield the same numerical value, then the consistency of the two values confirms that the theory is uniquely coordinated with reality, and thus true. If the two values are inconsistent, the coordination is shown not to be unique in the sense that *different* numbers were obtained by the two different routes, and not the same one, and thus the theory would be accounted false: "If the values obtained by the measurements are consistently the same, then the coordination possesses that property which we call truth or objective validity. Therefore, we define: *Uniqueness* of a cognitive coordination means that a physical variable of state is represented by the *same value* resulting from *different empirical data*" (Reichenbach 1920; quoted from 1965, 45).

How do we know that consistent coordinations can ever be achieved? According to Reichenbach, "this question . . . is equivalent to Kant's question: 'How is natural science possible?' " (ibid., 46). What does "possible" mean here?

> "Possible" is not meant in a psycho-physical, but in a logical sense: it pertains to the logical conditions of a coordination. We have seen in our example that conditions specifying a coordination must exist; these conditions are principles of a general sort such as those of direction, metric relations, and so forth. . . . We may therefore formulate the critical question in the following way: *By means of which principles will a coordination of equations to physical reality become unique?*
>
> Before we answer this question, we must characterize the epistemological position of the principles of coordination. They are equivalent to Kant's synthetic a priori judgments. (Ibid., 47)

In other words, the possibility of a unique coordination of theory with reality, the possibility of the existence of true theories, is grounded in principles of coordination that do the same work as Kant's synthetic a priori judgments.

When Reichenbach argues that the a priori must be denied an apodictic character, but that its constitutive function remains, he is thus making a claim about principles of coordination whose existence secures the possibility of empirical knowledge. That is, the principles of coordination effect the needed coordination by their constituting the objects of knowledge, "By determining the coordination, they define the individual elements of reality and in this sense *constitute* the real object" (ibid., 53). Constitution is required because the real is not itself given in experience; the objects of empirical knowledge are not real, *noumenal* objects, but *phenomenal* objects constituted in intuition.

Reichenbach's argument for denying apodicticity to constitutive principles turns upon a feature of the Kantian doctrine of the a priori not usually given its due. According to Kant, empirical knowledge is possible only by virtue of the ordering of perceptual material effected by the a priori principles of constitution or coordination, hence "*experiences* cannot effect a change of human reason, because experience presupposes reason. . . . 'necessarily true' must be understood in this sense" (ibid., 56). But is Kant right? Only on one condition:

> We had established previously that the coordinating principles must be distinguished by the fact that they permit unique coordinations; this appeared to be the significance of the critical question. But there is no guarantee that these principles that originate in reason possess this property, because the criterion of uniqueness, that is, perception, is independent of reason. It would be a strange accident of nature if those principles originating in reason were also those determining uniqueness. There is only one possibility for explaining this coincidence: if the principles of coordination are irrelevant for the requirement of uniqueness; if, in other words, a unique coordination is always possible for any arbitrary system of coordinating principles. (Ibid., 57)

Does Kant think that there is such arbitrariness in the a priori? That Kant would have answered "yes" is indicated, according to Reichenbach, by Kant's allowing for the possibility of other beings employing constitutive principles different from ours. Thus, whether the a priori constitutive principles of coordination are necessary turns upon the question of whether or not any arbitrary system of principles can be consistently deployed:

> We have reached the conclusion that the validity of Kant's theory of knowledge can be made dependent upon the validity of a clearly formulated hypothesis. Kant's theory contains the hypothesis *that there are no implicitly*

contradictory systems of coordinating principles for the knowledge of reality. Since this hypothesis is equivalent to the statement that any arbitrary, explicitly consistent system of coordinating principles can arrive at a unique coordination of equations to reality, we shall call it the *hypothesis of the arbitrariness of coordination.* Only if this hypothesis is correct, are the two meanings of the concept of a priori compatible: only then are the constitutive principles independent of experience and necessary, that is, true for all times. (Ibid., 60)

Of course, Reichenbach will deny the hypothesis of the arbitrariness of coordination, and thus the necessity of the a priori. He will argue that certain systems of coordinating principles are shown by the special and general theories of relativity to be inconsistent, but it does not follow that there is only one consistent system of coordinating principles. Indeed, Reichenbach's empiricism at this time consists precisely in his claim that various possible systems of coordinating principles are consistent, but not all of them.

To speak of systems of coordinating principles, however, is still not to speak of what is thereby coordinated with reality, that is, physical theory. Reichenbach contrasts the principles of coordination with what he calls the "axioms of connection" (ibid., 54) exemplified by Einstein's equations of gravitation from the general theory of relativity. The principles of coordination would be exemplified by the assumed form of the spacetime manifold itself since the latter indicates "that four numbers are necessary to define a single real point" (ibid., 53). Aside from giving us examples, Reichenbach has little to say here about the distinction between the principles of coordination and the axioms of connection, except to note that the principles of coordination differ from the axioms of connection "in that they do not connect certain variables of state with others but contain general rules according to which connections take place" (ibid., 54), and that the principles of coordination belong to "a class of principles that precede the most general laws of connection as presuppositions of knowledge though they hold as conceptual formulas only for the conceptual side of the coordination. These principles are so important because they define the otherwise completely undefined problem of cognitive coordination" (ibid., 55). Whether or not Reichenbach intends any further, fundamental differences in kind between the principles of coordination and the axioms of connection is unclear.

Schlick found much to like in Reichenbach's *Relativitätstheorie und Erkenntnis Apriori,* which is not surprising given Reichenbach's debt

to Schlick. But Schlick had reservations about one point, which he expressed for the first time in a letter to Einstein of 9 October 1920:

In the last few days I have read with the greatest pleasure the booklet by Reichenbach on relativity theory and a priori knowledge. The work really appears to me to be a quite splendid contribution to the axiomatics of the theory and of physical knowledge in general. . . . Of course, in a few points I still cannot entirely support Reichenbach. . . . Reichenbach seems to me not to be fair with regard to the theory of conventions of Poincaré; what he calls a priori principles of coordination, and rightly distinguishes from the empirical principles of connection, seem to me to be wholly identical with Poincaré's "conventions" and to have no significance beyond that. R.'s reliance upon Kant seems to me to be, carefully considered, only purely terminological. (EA 21-580; quoted by permission of the Vienna Circle Archive; all rights reserved)

Schlick made essentially the same point about the conventional character of the principles of coordination in a long and detailed letter to Reichenbach of 26 November 1920, from which I quote at length:

For me the presupposition of object-constituting principles is so self-evident that I have not pointed it out emphatically enough, above all in the Allg. Erkenntnisl. . . . It is quite clear to me that a perception can become an "observation" or even a "measurement" only through certain principles being presupposed by means of which the observed or measured object is then constructed. In this sense the principles are to be called a priori. . . . But there are indeed, moreover, two possibilities, that those principles are hypotheses or that they are conventions. In my opinion, precisely this turns out to be the case, and it is the central point of my letter, that I cannot discern wherein your a priori propositions are actually distinguished from conventions. That you passed over Poincaré's theory of conventions with so few words is what most amazed me about your essay. . . . The crucial places where you describe the character of your a priori principles of coordination appear to me, frankly, as quite successful definitions of the concept of convention. . . . I do not fear that you can object that conventionalism must also make use of the hypothesis that you find implicit in Kant's philosophy (p. 57) [there are no contradictory systems of principles, the hypothesis of the arbitrariness of coordinations]. Indeed, only such conventions are permitted that fit into a certain system of principles, and this system *as a whole* will be determined by experience; the arbitrariness only enters in the manner of its construction and is steered by the principle of the minimum of concepts. Here there appears to me to be a small gap in your essay, which is not without consequences: In the concept of knowledge you consider explicitly only the *one* side, the coordination, and you slight a little bit the other side, that the coordination should be accomplished with the fewest and consequently the most general possible concepts. (HR 015-63-22; quoted by permission of the Vienna Circle Archive; all rights reserved)

Just three days later, on 29 November, Reichenbach answered with an even longer and more detailed reply:

You ask me why I do not call my a priori principles *conventions*. I believe that we will easily come to agreement about this question. Even though several systems of principles are possible, nevertheless, only one *group* of principle-systems is always possible; and precisely in this restriction there lies some knowledge. Every possible system signifies in its possibility a *property* of reality. I miss in Poincaré an emphasis on the fact that the arbitrariness of the principles is restricted, in the way one *combines* principles. For that reason I cannot adopt the name "convention." Also, we are never certain that two principles that we today allow to exist alongside one another as constitutive principles, and which are therefore both *conventions*, according to Poincaré, might not tomorrow have to be separated because of new experiences, so that between the two conventions the alternative appears as synthetic. (HR 015-63-21)

The exchange on this issue concluded with Schlick writing on 11 December:

1) on the question of the "conventions." If Poincaré did not explicitly emphasize that conventions are not independent of one another, but are always possible only as groups, still one would naturally do him quite an injustice, if one believed that he was not aware of this circumstance. This was obviously the case, and he would have repudiated with mockery the nonsense that, e.g., Dingler has perpetrated with the concept of conventions while misunderstanding this circumstance. Thus, in my view, nothing stands in the way of the retention of the term. (HR 015-63-19; quoted by permission of the Vienna Circle Archive; all rights reserved)

That the constitutive coordinating principles should be regarded as conventions was evidently a point of considerable importance for Schlick, so much so that it became the central critical point in both of the reviews he published of *Relativitätstheorie und Erkenntnis Apriori*, the first a note appended to his review of Cassirer (Schlick 1921) and the second a brief review in *Die Naturwissenschaften* (Schlick 1922).

We have no way of knowing whether or not Schlick was being truthful in claiming that the constitutive function of coordinating principles had always been self-evident to him. Such a claim had not been emphasized in any of Schlick's earlier writings, including the *Allgemeine Erkenntnislehre* (1918). But certain other ingredients of the view emerging in the Schlick-Reichenbach correspondence are discernible in some of Schlick's earlier writings, aside from Schlick's obvious contribution of the idea of truth as unambiguous coordination.

Most interesting in this connection are sections eight, "Das Wesen des Urteils," nine, "Urteilen und Erkennen," and ten, "Was ist Wahrheit," of the first edition of the *Allgemeine Erkenntnislehre* (1918). The *Allgemeine Erkenntnislehre* recapitulates the chief features of what by 1918 had become Schlick's "semiotic" epistemology: Concepts are nothing more than signs for objects; judgments are statements of identity between concepts, and they stand as signs for facts, which are relations between objects; judgments are true if and only if they are unambiguously coordinated with facts. Schlick draws a distinction between two kinds of judgments, which resembles Reichenbach's distinction between axioms of connection and principles of coordination. Schlick distinguishes judgments that correlate a new sign (concept) to a state of affairs, which he calls *definitions*, from judgments that correlate an existing sign to a state of affairs, which he calls a *knowledge claim* [*eine Erkenntnis*] (Schlick 1918; see Schlick 1985, 55). The idea that constitutive principles are necessary for knowledge is foreshadowed in Schlick's argument that space and time, "the great uniters and dividers," are necessary for the individuation of that of which, ultimately, one asserts identity in any judgment that counts as knowledge;[5] Reichenbach had cited space and time as examples of Kantian a priori constitutive categories that he would now have us regard as principles of coordination.

We also find Schlick writing frequently about the role of conventions in science. It is usually in connection with his doctrine of truth as unambiguous coordination, and it always occurs in the context of his asserting that in the nature of the sought-for unambiguous coordination more than one system of judgments, more than one theory, can always be coordinated unambiguously with one and the same set of facts. A representative remark is that found in his monograph, *Raum und Zeit in der gegenwärtigen Physik*, where he writes:

It is, however, possible to indicate identically the *same* set of facts by means of *various* systems of judgments; and consequently there can be various theories in which the criterion of truth is equally well satisfied, and which then do equal justice to the observed facts, and lead to the same predictions. They are merely different systems of symbols, which are allocated to the same objective reality: different modes of expression that reproduce the same set of facts. (Schlick 1917; quoted from 1979, vol. 1, 266)

As Schlick indicates, such underdetermination is a simple consequence of the semiotic epistemology. The choice among the underdetermined alternatives, insofar as it is not dictated by considerations of simplicity, is a matter of convention. As to where, exactly, the element of convention is found in a body of theory, Schlick says, "Only the primitive concepts and judgments—those to which knowledge reduces all the others—depend on conventions and have to be learned as arbitrary signs" (1918; quoted from 1985, 62); and it is "definitions," the most basic category of judgment, "which accomplish a coordination through *arbitrary* stipulation" (ibid., 63).

Looking back from the vantage point of 1920, one is tempted to read Schlick's comments about the conventional character of definitions as a clear anticipation of the claim made in his correspondence with Reichenbach that principles of coordination, now read as "definitions," are the sole conventional element in physical theories. In a substantially new section eleven, "Definitionen, Konventionen, Erfahrungsurteile," added to the second edition of the *Allgemeine Erkenntnislehre* (Schlick 1925; see 1985, 69–79),[6] Schlick says more or less exactly this, as if he were merely clarifying what he had said about definitions and conventions in the earlier sections from which I quoted, "By a suitable choice it is always possible under certain circumstances to obtain an unambiguous designation of the real by means of the concept. Conceptual definitions and coordinations that come into being in this fashion we call *conventions*" (ibid., 71). But the view that Schlick elaborates here, in the wake of his reading of and discussion with Reichenbach, represents a significant modification of what Schlick had said in the 1918 first edition.

What is new in section eleven of the second edition is, as it were, a hardening of the 1918 distinction between definitions and knowledge claims (*Erkenntnisse*), where the distinction was not fixed, but relative, into a principled distinction between what are now styled analytic definitions and synthetic empirical propositions. In 1918 Schlick wrote:

In a completely self-contained, deductively connected scientific system, genuine judgments can be distinguished from definitions only in a practical or

psychological sense, not in a purely logical or epistemological one. This we see very clearly in the case of the fixed, rigorous systems of judgments offered most notably by mathematics. There we can, under certain assumptions, select arbitrary theorems, treat them as definitions, and derive from them as consequences those judgments that serve ordinarily as the definitions of the concepts. In such purely conceptual systems the distinction between definition and judgment is thus a relative one. Which properties of a concept I had best employ in defining it depends only on considerations of expediency. . . . When we carry such considerations over to the factual sciences, we must be mindful that these sciences are never strictly self-contained. On the contrary, as we study real objects, we constantly become acquainted with new properties, so that the concepts of these objects acquire in time an ever richer content. Thus concepts change, whereas the words with which we designate them still remain the same. The word stands for the real object in the full abundance of its properties and relations; the concept stands only for whatever is allotted to it by definition. For this reason, definitions and genuine judgments are strictly separated from one another in the thinking of the factual sciences; yet one and the same sentence may, depending on the particular state of the inquiry, serve either as a definition or as an instance of knowledge [*Erkenntnis*]. Hence, so far as the linguistic formulations are concerned—and ultimately it is only in these that judgments can be fixed—in the factual sciences too the difference in kinds of judgments is a relative one. (Quoted from 1985, 46–47)

Schlick says more about the relativity of the distinction in the factual sciences a few pages later:

Once a science has developed into a rounded-out more or less closed structure, what is to count in its systematic exposition as definition and what as knowledge [*Erkenntnis*] is no longer determined by the accidental sequence of human experiences. Rather, those judgments will be taken as definitions that resolve a concept into the characteristics from which one can construct the greatest possible number (possibly all) of the concepts of the given science in the simplest possible manner. (Ibid., 50)

In the new section eleven of the 1925 second edition, the distinction is handled somewhat differently. Schlick asserts categorically that "all judgments in science are either definitions or hypotheses" (1925; quoted from 1985, 73).[7] He asserts, as well, that the definitions—which are conventional—are analytic (and thus a priori), whereas the hypotheses are synthetic (and thus a posteriori) (ibid.,75–77). The distinction thus established is *not* a relative one:

We might be tempted to think that the distinction between analytic and synthetic judgments cannot be drawn sharply, since one and the same judgment may be synthetic or analytic depending on what we include in the subject concept. But this opinion ignores the fact that the judgment is really *not* the same in the two cases. In the first case, we define the concept *body* in "All bodies are heavy" so that being heavy is one of its features; in the second case, we do not. True, the sentence contains the same *words* each time, but they designate different judgments, for the word "body" has a different meaning in each. We explained above (§ 8) that one and the same (linguistic) sentence can express both a definition and a piece of knowledge [*Erkenntnis*]. It all depends on what concepts we connect to the words. The partitioning of judgments into analytic and synthetic is thus something quite well defined and objectively valid, and does not depend, say, on the subjective standpoint or mode of comprehension of the one who judges. (Ibid., 76)

The backward reference to the passage I just quoted from section eight should not be allowed to obscure the essential shift in Schlick's thinking.

Schlick did say in 1918 in section eight that the distinction between definitions and knowledge claims is relative "so far as the linguistic formulations are concerned," but he did not mean that the distinction in the underlying judgments clothed in those linguistic formulations is, as a matter of epistemological principle, an objective one, as he now asserts. Rather, the difference in the judgments was, in a manner of speaking, a *genetic* one. One and the same linguistic formulation clothes now a definition and now a knowledge claim [*Erkenntnis*], not because definitions and knowledge claims are fundamentally different kinds of judgments, one analytic and one synthetic, but because a characteristic of an object whereby we first became acquainted with it, a characteristic that we once regarded as defining the object—say the malleability of gold—has now come to be regarded as a consequence of the object's possessing some other, now defining characteristic—say the atomic structure of gold. As Schlick said in 1918:

Every judgment places a concept in relation to other concepts and designates the fact that this relation exists. If the concept in question is already familiar and defined, then we have an ordinary judgment. If this is not the case, then the concept is to be regarded as having been created by the judgment. The latter thus becomes the definition, which constructs the concept out of its characteristics. It therefore seems quite proper to grant the status of judgments to definitions as well; *theoretically, definitions do not occupy a special position.* (Quoted from 1985, 47; emphasis added)

What is the culmination of this process of development? In its maturity, a science evolves "into a rounded-out more or less closed structure," in which "what is to count in its systematic exposition as definition and what as knowledge is no longer determined by the accidental sequence of human experiences" (ibid., 50). In such a mature science, in a "completely self-contained, deductively connected scientific system," the distinction between two kinds of judgments lapses, "Genuine judgments can be distinguished from definitions only in a practical or psychological sense, not in a purely logical or epistemological sense" (ibid., 46). In these "purely conceptual" scientific theories, "the distinction between definition and judgment is thus a relative one" (ibid.).

A mature science is one in which theory has achieved complete formalization or deductive systematization, and in such a theory the need for "concrete definitions"—later styled coordinative definitions—is reduced to a minimum, if it does not altogether disappear, the only definitions being "implicit definitions," whose role in mathematics had been clarified by Hilbert. Indeed, the logical ideal of a formal theory is one in which the meanings of the fundamental concepts is so thoroughly constrained by the network of axioms providing implicit definitions of those concepts as to guarantee that only one kind of thing in reality could correspond to each concept, thus eliminating any need for concrete definitions. I need not define "line" by pointing to an exemplar if the meaning of "line" is so constrained that only one kind of thing could correspond to it. We now call theories that so constrain the meanings of their primitives "categorical theories," and we know, thanks to Gödel—it is a corollary to the first incompleteness theorem—that such an ideal is unattainable for any but the most primitive theories. But this impossibility was not recognized in 1918. (For more on the ideal of a system of implicit definitions uniquely determining the objects satisfying those definitions, with specific reference to Cassirer's earlier development of this idea, see Howard 1993. For a discussion of Carnap's 1927 criticism of concepts introduced thus as "improper concepts," see the section entitled "Carnap's Early Contribution to the New Empiricism" later in this essay.)

The idea that the need for coordinative definitions will disappear in a mature science appears prominently in Einstein's important 1921 lecture, *Geometrie und Erfahrung*, a work that betrays the clear influence of the Schlick of the 1918 first edition of the *Allgemeine*

Erkenntnislehre. Einstein asserts that a coordinative definition for geometrical primitives like "rigid body" is required only because our total theory of physics plus geometry has not yet matured to the point where the appropriate *physical* concept of a "practically rigid body" emerges deductively as a consequence of our physico-geometrical axioms, being thus implicitly defined by those axioms.

In the second edition of Schlick's text, the idea that an empirical science matures into a formal one survives:

> Now the remarkable thing is that for a suitable choice of objects (singled out by means of concrete definitions), we can find implicit definitions such that the concepts defined by them may be used to designate uniquely those same real objects. That is, the concepts will then be connected to one another by a system of judgments coinciding fully with the network of judgments that on the basis of experience had been uniquely coordinated to the system of facts. Whereas we had to obtain this network of judgments empirically mesh by mesh through laborious single acts of knowledge, the system of judgments that coincides with the network can be derived *in toto* by pure logic from the implicit definitions of its basic concepts. (1925; quoted from 1985, 70)

Schlick even adds that the maturation of a theory into such a form is necessary if the theory is to be able to make predictions about future, as-yet-unobserved events. He goes so far as to say that "to suppose that the world is intelligible is to assume the existence of a system of implicit definitions that corresponds exactly to the system of empirical judgments" (ibid.). Yet, in 1925, Schlick denies fundamental epistemological significance to the relativity of the definition-knowledge distinction in such mature sciences. He writes, in the introductory paragraph of the new section eleven:

> Every judgment we make is either definitional or cognitive. This distinction, as we noted above (§ 8), has only a relative significance in the conceptual or "ideal" sciences. It emerges all the more sharply, however, in the empirical or "real" sciences. In these sciences it has a fundamental importance; and a prime task of epistemology is to make use of this distinction in order to clarify the kinds of validity possessed by various judgments. (Ibid., 69)

Why did Schlick feel the need to harden what was formerly a relative distinction into a fixed, objective one (and to try to cover his tracks in doing so)? Or, rather, why did he now, in 1925, attach "fundamental" epistemological importance only to the distinction between definition and knowledge in *immature* theories, where this distinction is now allegedly absolute, even though the form taken by

a system of knowledge in the "ideal" sciences, where implicit definitions are the norm, is still deemed necessary for achieving "the task for which science was invented" (ibid., 70)? It was in order to be able to reply as forcefully as possible to the neo-Kantians.

That the motive was the desire to answer the neo-Kantians becomes clear when we appreciate the work that is supposed to be done by the purportedly objective distinction between analytic, a priori, conventional definitions and synthetic, a posteriori empirical propositions. Through fixing, by conventions, the definitions, one accomplishes two things. First, one guarantees that every empirical proposition in a true theory is thereby coordinated unambiguously with a *single fact,* that is, that one invests every synthetic empirical proposition with its own empirical content. Second, one guarantees that the truth or falsity of the nonconventional empirical propositions will be determined unambiguously by experience of the empirical facts constituting their empirical content, "Once a certain number of concepts are fixed by convention, the relations that hold between the objects so designated are not conventional. They must be determined through experience" (ibid., 72). And:

> The system of definitions and cognitive judgments, which constitutes any real science, is brought into congruence at individual points with the system of reality, and is so constructed that congruence then follows automatically at all remaining points. . . . If the whole edifice is correctly built, then a set of real facts corresponds not only to each of the starting points—the fundamental judgments—but also to each member of the system generated deductively. Every individual judgment in the entire structure is uniquely coordinated to a set of real facts. (Ibid., 78)

(The two sentences after the ellipsis are taken over from the end of section ten of the 1918 edition. The idea that the coordination between theory and experience should obtain judgment by judgment had been there all along; what is new is the hardening of the distinction between definitions and empirical propositions in order to guarantee the point-by-point coordination.)

Kant is clearly the target; the following is also new in 1925:

> According to [Kant], besides the two classes of judgments we have described—definitions in the widest sense (Kant calls them analytic judgments) and empirical judgments or hypotheses (these he calls synthetic judgments *a posteriori*)—there is a third class, the so-called synthetic judgments *a priori*. . . . The fact of the matter is that no one has as yet succeeded in ex-

hibiting a synthetic judgment *a priori* in any science. That Kant and his followers nevertheless believed in their existence may be explained quite naturally by the fact that among both the definitions and the empirical propositions of the exact sciences we find statements that are deceptively similar to synthetic judgments *a priori*. In the class of definitions, which by their very nature possess a validity independent of experience and thus are *a priori*, there are a great many conventions that, viewed superficially, seem not to be derivable from definitions and hence to be synthetic. Their true character as conventions is revealed only by a most painstaking analysis. An example would be the axioms of the science of space. In the class of empirical judgments, which are clearly synthetic since their validity for reality does not follow from the definitions, there are many propositions (for example, the principle of causality) of such seemingly unconditional validity that in the absence of a more penetrating examination it is easy to mistake them for *a priori* judgments.

Once we demonstrate . . . that the judgments held to be synthetic and *a priori* are in fact not synthetic or not *a priori*, there is no reason whatever to suppose that judgments of this strange sort might yet exist in some obscure corner of the sciences. And this is sufficient ground for us to try in what follows to explain all knowledge of reality as a system built up exclusively of judgments belonging to the two classes described above. (Ibid., 73–75)

If all of the judgments or propositions constituting a scientific theory are either analytic definitions or synthetic empirical propositions, and if, our having fixed by convention a sufficient number of definitions, the truth or falsity of the remaining empirical propositions is determined unambiguously by experience of the empirical facts coordinated, one by one, with the empirical propositions, then there is simply no room for the Kantian synthetic a priori. In the context of what the theory of general relativity says about the geometry of spacetime, once you fix your rods and clocks by convention, the value of the metric is determined unambiguously by experimental results.

What explains the change from the Schlick of 1918, for whom the distinction between definitions and knowledge was a relative one, for whom "theoretically, definitions do not occupy a special position," to the Schlick of 1925, for whom the distinction is an objective one? I think the change occurred because the Schlick of 1925 realized that if the distinction is relative, if, therefore, *any* judgment can be regarded as being fixed by convention, then it follows that we cannot assign to each empirical proposition its own, unique empirical content. A true theory, as a whole, will be coordinated unambiguously with reality, but the coordination cannot be regarded as occurring empirical proposition by empirical proposition. If the distinction between definition and empirical proposition is "not . . . purely logical or epistemological" (1918; quoted from 1985, 46) but "only . . . practical or psychological" (ibid.), then nothing privileges the empirical propositions alone as the bearers of empirical content. This is a view of theories that exudes the spirit of holism and implicit definition, the spirit of Duhem and Hilbert (for more on implicit definition and holism in the thinking of Schlick before 1920, see Ryckman 1991b).

This view of the structure of theories leaves a door open for the neo-Kantian.[8] Because the neo-Kantian can argue that since the coordination does not occur empirical proposition by empirical proposition, since only whole theories are coordinated unambiguously with reality, and since, therefore, many different theories can equally well be coordinated with reality from the point of view of epistemological principle, which is to say that experience alone does not determine unambiguously our choice among all possible theories, it is the function of synthetic a priori judgments to resolve the ambiguity. We can have either a simple Euclidean geometry with a rather complicated physics, or a more complicated non-Euclidean geometry with a rather simpler physics. Experience does not force us to choose. That we pick the former option is a consequence of the synthetic a priori status of our geometry. If experience did decide unambiguously which geometry we had to choose, then no room would be available for the neo-Kantian to assert a role for synthetic a priori judgments. But that is precisely the trick attempted by the Schlick of 1925. Rigorously distinguish definitions from empirical propositions, fix the former by convention, and the truth or falsity of the latter is determined unambiguously by experience of their unique, individual empirical contents. There is no room for the synthetic a priori.

Reichenbach seems to have understood that simple underdetermination leaves open such a door for the neo-Kantian, and that one closes this door by turning such judgments as those about the metric (Euclidean or non-Euclidean geometry) into straightforwardly empirical judgments. Consider the penultimate paragraph in his discussion of neo-Kantianism in his 1922 review article, "Der gegenwärtige Stand der Relativitätsdiskussion":

One should not contend that, since the logical equivalence of all geometries leaves the internal consistency of Euclidean geometry unaffected, the validity of synthetic *a priori* judgments remains unchallenged; such an appeal to conventionalism is denied to Kantianism. The internal consistency of geometry is *analytic*. Conventionalism would admit all *logically consistent* conceptual systems as possible structural forms of empirical knowledge, but the significance of synthetic *a priori* principles consists in the fact that they constitute a specific choice among the *logical* possibilities. Similarly, the law of causality is not *logically* necessary since uncaused events are logically possible; according to Kant, it is a synthetic *a priori* principle which excludes uncaused events. If 'synthetic *a priori*' meant nothing but 'internally consistent,' Kantians would have to admit that, at some future time, we might gain knowledge of uncaused events. But on such an interpretation, synthetic *a priori* principles would ultimately degenerate into empty formulas that would impose no limits upon experience. This is the reason why eliminating the metric from pure intuition leads to a denial of synthetic judgments *a priori*. (Quoted from Reichenbach 1978, vol. 2, 30)

We eliminate the metric from pure intuition not by making it an analytic, a priori definition, but by making it, so far as possible, a synthetic, empirical proposition whose truth or falsity is determined unambiguously by experience of its unique empirical content.

Mention of Reichenbach again leads us to recall what happened to the distinction between axioms of connection and principles of coordination (in Reichenbach's 1920 idiom) or between definitions and empirical propositions (in Schlick's 1925 idiom). What happened is that it became the centerpiece of the emerging logical empiricist view of the structure and empirical significance of scientific theories, containing, in germ, the crucial idea of the verificationist theory of meaning.

Along with the new section eleven of the 1925 second edition of Schlick's *Allgemeine Erkenntnislehre*, it was Reichenbach's two other books on relativity that cemented the place of this doctrine in logical empiricism. Thus, in his *Axiomatik der relativistischen Raum-Zeit-Lehre* (1924), Reichenbach begins with a careful discussion of the distinction between axioms and what are now called, for the first time, "coordinative definitions," an expression that combines the earlier distinctively Reichenbachian and Schlickian variations on the notion of principles of coordination or definitions:

It is characteristic of the axiomatization of physics compared to that of mathematics that there exists such a distinction between axioms and definitions; an essential task of the axiomatization consists in tracing this distinction within the theoretical system.

However, even definitions in physics are different from definitions in mathematics. The mathematical definition is a *conceptual definition*, that is, it clarifies the meaning of a concept by means of other concepts. The physical definition takes the meaning of the concept for granted and coordinates to it a physical thing; it is a *coordinative definition*. Physical definitions, therefore, consist in the coordination of a mathematical definition to a "piece of reality"; one might call them *real definitions*. (Quoted from Reichenbach 1969, 8)

And essentially the same conception is presented early in his classic *Philosophie der Raum-Zeit-Lehre*, where he writes:

Defining usually means reducing a concept to other concepts. In physics, as in all other fields of inquiry, wide use is made of this procedure. There is a second kind of definition, however, which is also employed and which derives from the fact that physics, in contradistinction to mathematics, deals with real objects. Physical knowledge is characterized by the fact that concepts are not only defined by other concepts, but are also coordinated to real objects. This coordination cannot be replaced by an explanation of meaning, it simply states that *this concept* is coordinated to *this particular thing*. In general this coordination is not arbitrary. Since the concepts are interconnected by testable relations, the coordination may be verified as true or false, if the requirement of uniqueness is added, i.e., the rule that the same concept must always denote the same object. The method of physics consists in establishing the uniqueness of this coordination, as Schlick has clearly shown. But certain preliminary coordinations must be determined before the method of coordination can be carried through any further; these first coordinations are therefore definitions which we shall call *coordinative definitions*. They are *arbitrary*, like all definitions; on their choice depends the conceptual system which develops with the progress of science. (Reichenbach 1928; quoted from Reichenbach 1957, 14)

Once fix the arbitrary coordinative definitions by convention, and the truth or falsity of the remaining coordinations (read empirical propositions or axioms) is not arbitrary, it is determined unambiguously by experience.

Consider finally a typical very late remark, this from Schlick's 1935 essay, "Sind die Naturgesetze Konventionen?":

What is arbitrary are, first, the rules which determine the mutual relations of the symbols used, the mathematical axioms, and the explicit definitions of the derived concepts of natural science and, secondly, the ostensive definitions by means of which, in the last analysis, the meanings of the fundamental concepts of natural science are determined. These rules in their totality form the grammar of the scientific language, i.e., the complete inventory of rules according to which the symbols (letters, words, sentences, etc.) are to be used in the description of facts. All these "grammatical" rules, and these alone, together determine the meaning of the propositions of science. . . . *They* are the only conventions, not the natural laws. It is those rules which turn mere sentences into genuine propositions, for they determine their significance.

Once the rules are fixed, i.e., once agreement is reached concerning the grammar of the scientific language, then there is no longer any choice about how to formulate any facts of nature. After this there is only one possibility, only one way of formulating the sentence which will fulfill the purpose. A natural law can then be represented in only one quite definite form and not in any other. . . .

Thus we see that all genuine propositions, as for instance natural laws, are something objective, something invariant with respect to the manner of representation, and not dependent in any way upon convention. What is conventional and, hence, arbitrary, is only the form of expression, the symbols, the sentences, thus only something external or superficial which is immaterial to the empirical scientist. (Quoted from Schlick 1979, vol. 2, 443–44)

Much has, of course, happened to Schlick and logical empiricism between 1928 and 1935, including the assimilation of Wittgenstein's *Tractatus* (1992) and the somewhat new turn represented by Carnap's *Logische Syntax der Sprache* (1934). Some of these developments were for the good, such as Schlick's having learned to talk of sentences and propositions, rather than judgments. Some of them were for the bad, such as the grotesque assertion that ostensive definitions of fundamental concepts are part of the grammar of a scientific language. These developments will not be considered here. For now, let us note that the basic distinction between conventional coordinative definitions and empirical propositions (here the natural laws) has survived, along with, most importantly, the idea that once the conventional coordinative definitions are fixed, all else is unambiguously determined by experience.

My main point is that Schlick and Reichenbach were self-consciously engaged in the development of a new form of empiricism that would force the issue with the neo-Kantians, and that they were doing this chiefly to save relativity—the theory with which both had grown to professional and intellectual maturity—from the clutches of the neo-Kantians. In his 1921 review of Cassirer's *Zur Einsteinschen Relativitätstheorie,* Schlick explicitly stated as much:

Cassirer, in his book, has made it his object to prove that the philosophical foundations of relativity theory can be found only in the field of critical philosophy, and more precisely in that form of the critical viewpoint which he is pleased to label logical idealism. He sets himself the task of deciding, by epistemological analysis, "whether the theory in its origin and development is to be taken as an example and witness of the *critical* or of the *sensualistic* concept of experience."

But in light of this formulation, doubts are immediately bound to arise: Is the problem really reducible to these alternatives? Do we have a *tertium non datur*? There is certainly an empiricism that is distinct from sensualism and cannot be reduced to it, as can easily be discerned both historically and in terms of subject-matter. So if it is shown (as it is not hard to do) that the theory of relativity cannot be made out upon purely sensualistic premisses, this alone does not prove either the necessity or even the admissibility of the critical interpretation of the theory. (Quoted from Schlick 1979, vol. 1, 323)

Of course, what will define this new empiricism will be its attitude toward constitutive principles of coordination:

The most important consequences of the view just elaborated is that a thinker who simply perceives the necessity of constitutive principles for scientific experience should not yet be called a critical philosopher on that account. An empiricist, for example, can very well acknowledge the presence of such principles; he will deny only that they are synthetic *a priori.* (Ibid., 324)

Schlick argues that there are three options, which we might call "Kant," "Mach," and "Schlick-Reichenbach." I argue that a fourth option, called "Einstein," is available and that understanding how "Einstein" differs from "Schlick-Reichenbach," while still making possible a cogent reply to "Kant," will tell us a lot about what is right or wrong with "Schlick-Reichenbach."

Carnap's Early Contribution to the New Empiricism

Like Schlick and Reichenbach, Carnap first appeared on the public philosophical stage with his dissertation, *Der Raum* (1921), a work chiefly concerned with relativity. He had been interested in relativity for several years, already, as evidenced by the "Rundbrief über Relativitätstheorie" that he and three friends circulated in the spring of 1918, while Carnap was serving in the army, assigned to a military research institute in Berlin (see Carnap 1963, 10, for the background to the "Rundbrief"). The subject matter proposed for discussion in the "Rundbrief" included, in addition to original works by Einstein, Lorentz, and Minkowski, Hans Witte's *Raum und Zeit im Lichte der neueren Physik* (1914) and Schlick's *Raum und Zeit in der gegenwärtigen Physik* (1917). In 1919 Carnap first contemplated a dissertation in the form of an axiomatization of spacetime theories starting from two primitives, the notion of the coincidence (*C*) of world points of two physical systems and the temporal relationship (*T*) between the world points of one and the same physical system. The resulting axiom system (*C-T*) was never published, even though Carnap returned to it several times in later years (for more on the *C-T* axiom system, see the account given in Carnap 1925b; see also Carnap 1963, 11, 13).

Kant exerted a powerful influence on the young Carnap's thinking about space and time, in particular Kant as interpreted by the Marburg critical idealists. Carnap credits the influence of Natorp and Cassirer on his dissertation, where Carnap was chiefly concerned to distinguish three kinds of space: (a) the formal space of the mathematicians, knowledge of which would be purely logical in character, embodied in abstract axiom systems; (b) intuitive space, knowledge of which would be synthetic a priori, comprising, however, only certain topological properties and not either metrical properties or even the number of dimensions of space; and (c) physical space, knowledge of which would be empirical.

In the spirit of Cassirer's *Zur Einsteinschen Relativitätstheorie* (1921), Carnap's main conclusion is that the purely topological intuitive space, knowledge of which is synthetic a priori, is the condition for the possibility of objects of experience. Thus, among the variety of neo-Kantian responses to relativity theory, Carnap's has to be classed somewhere between those "immunization" strategies that defend the Kantian doctrine of space by making it a doctrine about intuitive or psychological space, rather than physical space, and those "revision" strategies that defend kant by weakening the Kantian position to a claim on behalf of the synthetic a priori status not of a geometry, including even the metrical properties of space, but only a weaker topology.

Simply to class Carnap's view among the variety of neo-Kantian responses to relativity is, however, to miss the originality and subtlety of Carnap's work. Most important is Carnap's effort to distinguish those features of physical space that are forced upon us by experience and those that result from a "free choice" or "stipulation" (*Setzung*). Interest in this problem was already evident in the "Rundbrief," where Carnap wrote in a note of 9 May 1918:

> I deny the "impossibility" of saying something physic[al] about space a[nd] time. It is justified a[nd] obligatory for the physicist to indicate which requirements are to be placed on the *definition of the measures of space and time*, so that, with the help of this def[inition], one may be able to describe natural processes in a unified manner and avoiding arbitrary elements.*
>
> *) e.g., an arbitrarily chosen measuring rod or reference system. (RC 004-01-01)[9]

But the idea is not further developed here.

In his dissertation, Carnap concludes that once we freely choose something to serve as a standard measuring rod (*Maßsetzung*), the question of the metric or curvature of physical space becomes, thereby, a straightforwardly empirical question; precisely which physical object to choose as our standard measuring rod is determined by considerations of simplicity. Carnap seems, thus, essentially in agreement with the point of view then emerging in the work of Schlick and Reichenbach, wherein conventional and thus (more or less) freely-chosen constitutive coordinative definitions, such as the determination of a standard measuring rod, are distinguished from the remaining empirical propositions, such as a proposition about the curvature of spacetime. But Carnap's position differs from that of Schlick and Reichenbach in crucial ways.

To begin with, Carnap notes that we could equally well proceed in the opposite direction from that just indicated, more or less freely choosing a metric to ascribe to physical space (the constraint being, again, one of simplicity), with the result that our determination of a standard measuring rod is forced upon us by experience. In this regard, Carnap's position resembles more that of the 1918 Schlick, for whom the definition-empirical proposition distinction was a relative one, than the 1925 Schlick. The difference with the later position of Schlick and Reichenbach goes even further, however, for Carnap asks which of the two possible procedures is to be preferred: choosing a measuring rod on the basis of simplicity considerations, or choosing a metric for reasons of simplicity. His answer is interesting.

Carnap notes that, historically, the latter option has been preferred, meaning that we have chosen to retain a Euclidean geometry for reasons of its intrinsic simplicity, with virtually no one having opted for the former procedure. What about those cases where a non-Euclidean geometry has actually been adopted or considered? Was this consequent upon a simplest possible choice of measuring rod? No. Gauss, Riemann, Helmholtz, and Einstein did not make the simplest possible choice of measuring rod, meaning that they did not pick a rod without considering the influence of temperature and other physical factors. Instead, they complicated the choice of the rod, using physics to correct for the influence of temperature and other factors:

> This fact must call forth doubts about whether the correct scientific procedure may consist in either of these paths: to adopt a free choice of the simplest R [*Raumform*—metric] or the simplest M [*Maß*stab—measuring rod]. If we recall that the simplicity requirement applicable to the procedure of scientific representation refers to the total representation of a state of affairs, then we recognize that maximal simplicity for elective determinations independent of the state of affairs is to be demanded only insofar as the greater simplicity of the structure ensuing on the basis of these determinations is thereby achieved. The latter always remains the measure: Simplicity of the structure precedes simplicity of the construction and its auxiliary means.
>
> Thus, neither R nor M should be freely chosen without consideration of T [*Tatbestand*—state of affairs], even though the conceptual possibility of this choice must always be retained. Rather, an, as it were, mediating path is to be followed, that starts neither from the simplest R nor from the simplest M, and whose justification is manifested only when it achieves the goal of leading to the simplest construction out of what is currently known. (Carnap 1922, 55–56)

Let us carefully consider what Carnap is saying, and appreciate how it differs from the position of Schlick and Reichenbach. His point is that if the measuring rod is freely chosen (by convention, presumably, though he does not use that designation), then the metric of physical space is unambiguously determined by experience and if the metric is freely chosen (by convention), then the measuring rod is unambiguously determined by experience. But he does not recommend our doing either. Rather, inspired by the examples of Gauss, Riemann, Helmholtz, and Einstein, he urges us to follow a "mediating" path that would presumably involve our adjusting *both* the choice of the rod and the choice of the metric so as to achieve the simplest total theory.

Carnap's suggestion to seek the simplest total body of theory is similar to, and was perhaps inspired by, the view advocated by Einstein himself in his famous and influential *Geometrie und Erfahrung* (1921), which was first delivered as an address to the Prussian Academy of Sciences at the end of January 1921, soon thereafter appearing as a widely-circulated pamphlet. Like Einstein's position, to be considered more carefully below, Carnap's recommendation tends in the direction of a holistic view of the structure and empirical interpretation of theories that is hard to reconcile with the position of Schlick and Reichenbach. For if *both* R and M are to be adjusted in the search for the simplest total body of theory, then the truth-value of neither R nor M *alone* can be regarded as being determined unambiguously by experience. What is thus lacking in Carnap's position, by contrast with that of Schlick and Reichenbach, is an emphasis on some kind of principled distinction between definitions and empirical propositions. For the Carnap of 1921, all of the propositions constituting a theory are on the same epistemological footing, every one of them being a candidate for rejection, revision, or fine tuning in the search for the simplest total theory.

The holistic implications of Carnap's position had receded into the background, however, when, two years later, Carnap returned to the question of the manner in which the simplicity criterion should be deployed in the choice of a physical theory, this in his essay, "Über die Aufgabe der Physik und die Anwendung des Grundsatzes der Einfachstheit" (1923). Here Carnap argues that a scientific theory should be seen as consisting of three "volumes." "Volume 1" comprises the axiomatic structure of mathematical physics, including fundamental dynamical laws. "Volume 2" is "a kind of dictionary" (ibid., 99), providing connections between complexes of sensations and the objects constituting the ontology of a physical theory; it is essentially a catalogue of coordinative definitions. "Volume 3" consists of a comprehensive description of the physical state of the world at two arbitrary points of time, yielding, in effect, boundary conditions for calculations and predictions. In order to do physics, one derives from the state descriptions of volume 3 and the laws of volume 1 a physical description of the state of the universe in some spacetime region, which is then translated into the language of sensations by means of the dictionary in volume 2.

In choosing a total physical theory, one can seek the maximal simplicity of either volume 1 or 2, although, as Carnap notes, the meaning of "simplicity" differs in the two cases. In a departure from the program of 1921, Carnap now presents these as the only two options, the "mediating" path of *Der Raum* not being mentioned. Carnap refuses to express a preference for one path over the other, saying that his purpose is merely "to indicate the problematic" (1923, 104).

The tendency of Carnap's thinking here is clearly away from the holism so prominent in *Der Raum*. It is hard to resist the conjecture that his growing friendship with Reichenbach, with whom he had begun corresponding sometime not too long before their first personal meeting at a conference in Erlangen in March of 1923, helps to explain the shift of emphasis, especially given the obvious connection between Carnap's "volume 2" and Reichenbach's coordinative definitions (see Carnap 1963, 14, for details; their acquaintance probably does not date from earlier than 1922, for Reichenbach's 1922 major review article includes no mention of Carnap's dissertation). Another hint that Carnap was feeling the influence of Reichenbach (or perhaps Schlick) is a shift away from Kant, for no mention of intuitive space is made in the 1923 essay. Still, there is nothing like an explicit, wholehearted endorsement of the Schlick-Reichenbach conception of the distinction between conventional coordinative definitions and empirical propositions, leading one to wonder just what Carnap thought about their emerging empiricist response to Kant.

Two years later, in a review of Reichenbach's *Axiomatik der relativistischen Raum-Zeit-Lehre* (1924), Carnap comes close to an endorsement, writing that the book "gives a synoptical system for the logical structure of the R.-T., in that, starting from the individual empirical facts as axioms, it constructs the theory so that it becomes evident, for each of its propositions, upon which empirical facts and upon which methodological principles that proposition rests" (Carnap 1925a, 34). First among those points in the work representing "an advance in our knowledge" and giving "fruitful directions for further research," Carnap lists "the clarification of the role of physical definitions (as 'coordinative definitions,' as distinct from the conceptual definitions of mathematics, they establish a coordination between a 'piece of reality' and a mathematical concept)" (ibid.). But

the appearance of an endorsement is qualified by Carnap's pointed remark that "the strength of Reichenbach's work lies not in the epistemological domain, where many important problems remain open, but rather in the logical" (ibid.).

Even Carnap's later recollections fail to resolve the quandary. Here is what Carnap wrote in the preface to the English translation of Reichenbach's *Philosophie der Raum-Zeit-Lehre* (1928):

> Physical geometry describes the structure of physical space; it is a part of physics. The validity of its statements is to be established empirically—as it has to be in any other part of physics—after rules for measuring the magnitudes involved, especially length, have been stated. (In Kantian terminology, mathematical geometry holds indeed *a priori*, as Kant asserted, but only because it is analytic. Physical geometry is indeed synthetic; but it is based on experience and hence does not hold *a priori*. In neither of the two branches of science which are called "geometry" do synthetic judgments *a priori* occur. Thus Kant's doctrine must be abandoned.)
>
> In physical geometry, there are two possible procedures for establishing a theory of physical space. First, the physicist may freely choose the rules for measuring length. After this choice is made, the question of the geometrical structure of physical space becomes empirical; it is to be answered on the basis of the results of experiments. Alternatively, the physicist may freely choose the structure of physical space; but then he must adjust the rules of measurement in view of the observational facts. . . .
>
> The view just outlined concerning the nature of geometry in physics stresses, on the one hand, the empirical character of physical geometry and, on the other hand, recognizes the important function of conventions. This view was developed in the twenties of our century by those philosophers who studied the logical and methodological problems connected with the theory of relativity, among them Schlick, Reichenbach, and myself. The first comprehensive and systematic representation of this conception was given by Reichenbach in 1928 in his *Philosophie der Raum-Zeit-Lehre* (the original of the present translation). This work was an important landmark in the development of the empiricist conception of geometry. ([1956], 1957, vi)

The common opposition to Kant is there. The defense of an empiricist conception of physical geometry is there. But in spite of Carnap's assertion that "*this*" view of the nature of geometry in physics (a singular term) was developed in common with Schlick and Reichenbach, and in spite of the fact that this is an introduction to *Reichenbach's* major work on the subject, Carnap avoids using the vocabulary of coordinative definitions that is characteristic of the Schlick-Reichenbach point of view and prominently employed in the book being introduced, speaking instead of "rules for measuring length." Most importantly, Carnap insists, as he has since 1921, that one can choose freely *either* the measuring rod, in which case experience dictates a metric, *or* the metric, in which case the physicist must "adjust the rules of measurement in view of the observational facts." The tendency in the writings of Schlick and Reichenbach had, all along, been to view the metric as that which is empirically determined, once a standard rod, a definition of congruence, or the like has been stipulated by a coordinative definition (see, for example, Reichenbach 1949, 294, 297).

Carnap's seemingly deliberate avoidance of an explicit endorsement of the Schlick-Reichenbach distinction between coordinative definitions and empirical propositions is possibly a consequence of Carnap's having pursued from the early 1920s a somewhat different approach to the problem of the empirical significance of theories. The "Konstitutionstheorie" of Carnap's 1928 *Der logische Aufbau der Welt* represents the culmination of this line of investigation, but important parts of the program emerge also in two earlier works, his 1926 monograph, *Physikalische Begriffsbildung,* and his 1927 essay, "Eigentliche und uneigentliche Begriffe." The latter is, perhaps, the more revealing regarding the tendency of Carnap's thinking.

Carnap's "Eigentliche und uneigentliche Begriffe" is a remarkable and historically important essay. Among other things, it clearly defines for the first time several concepts later to become important in formal semantics and metatheory, including the concept of categoricity (Carnap's term is "monomorphism"), decidability (Carnap's "*entscheidungsdefinit*"), and completeness. But, for our purposes, it should be read as a methodological introduction to the project of elaborating a *Konstitutionstheorie* in the *Aufbau* (1928) since it examines the ways in which concepts are introduced in the sciences, recommending the employment of what Carnap terms "proper concepts" (*eigentliche Begriffe*).

"Proper concepts" are ones introduced by means of explicit definitions in terms of "perceivable characteristics" (*wahrnehmbare Kennzeichen*). They are contrasted with "improper concepts" (*uneigentliche Begriffe*), which are introduced through systems of implicit definitions. Whereas the Schlick of 1918 had seen a system of implicit definitions as the ideal form achieved by mature sciences, and the Schlick of 1925 had argued that the possibility of casting a theory

into the form of a system of implicit definitions is necessary for achieving the aim of science by making predictions about unobserved events, Carnap argues that systems of implicit definitions are defective, from the point of view of empirical science, because the improper concepts thus defined are "indeterminate" (*unbestimmt*) in their application.

Even in the case of monomorphic (categorical) theories, whose models are all isomorphic to one another, there can be no determinate answer to the question whether or not a given real object corresponds to one of the improper concepts defined by the theory. If a single improper concept is defined by a system of implicit definitions, as "number" is defined by the Peano axioms, to decide whether a given entity is a number is still impossible because its being a number would depend upon whether it stands in the correct relationship to other objects, the whole set of objects representing a structure of the kind determined by the implicit definitions (a "progression" in the case of the Peano axioms). The situation is even worse with systems of implicit definitions for more than one concept, such as Hilbert's axioms for Euclidean geometry, which take "point" and "line" as primitives. In such cases, one encounters the added problem typified by the "duality" of Hilbert's axioms: One can systematically interchange the terms "point" and "line" everywhere they occur in the axioms without any net change in the axiom system.

The application of proper concepts is, by comparison, always determinate. Thus, Carnap characterizes improper concepts as "variables" and proper concepts as "constants," remarking, "The sign for a constant has a determinate meaning: a propositional sign-complex, in which only such signs are present, has a determinate truth-value (truth or falsity). By contrast, the sign for a variable has no determinate meaning" (1927, 371). So, just as for Schlick and Reichenbach, empirical propositions acquire a determinate truth-value once a set of coordinative definitions are fixed by convention, for Carnap a proposition built up out of properly defined concepts, defined in terms of perceivable characteristics, has a determinate truth-value.

Under certain circumstances, a system of implicit definitions can acquire empirical content, as long as the improper concepts they define can be linked to proper concepts, which is to say, in Carnap's way of speaking, that variables are replaced by constants satisfying the formal conditions constraining the variable improper concepts, "The blood of empirical reality streams in through this point of contact and flows into the most highly branched veins of the heretofore empty schema, which thus turns into a theory with content" (ibid., 373). But the proper concepts, contact with which invests a system of implicit definitions with empirical content, must themselves be "real concepts," constructed out of perceivable characteristics in a *Konstitutionstheorie,* of the kind to be given in the *Aufbau.* Therein lies a serious problem, for, as it turns out, the project of the *Aufbau* is a failure.

The fundamental flaw in the *Aufbau,* from the point of view of the epistemological ambitions that lay behind Carnap's project, is that the system of real concepts cannot be defined explicitly solely in terms of perceivable primitive characteristics. Much as the Russell-Whitehead logicist project failed, because the concept of set membership had to be taken as a primitive alongside the purely logical concepts, so too Carnap's project failed because he was forced to take as a primitive the concept of "recollection of similarity" (*Ähnlichkeitserinnerung*) (Carnap 1928, sec. 78), which is arguably not a perceivable characteristic of things designated by proper, real concepts, or of the basic concepts out of which they are constructed. As a consequence, Carnap's *Konstitutionstheorie* turns out to be, itself, just another system of implicit definitions.

The failure of Carnap's project in the *Aufbau,* together with Neurath's critique of phenomenalism in the famous debate over *Protokollsätze,* led to the reemergence of holism in Carnap's *Logische Syntax der Sprache* (1934). But it was still a curious kind of holism, because it managed to coexist comfortably with a strong analytic-synthetic distinction of the kind that otherwise was essential to the Schlick-Reichenbach distinction between coordinative definitions and empirical propositions. A more robust variety of holism was all along being defended by the other principal player in this drama, namely, Einstein, to whom we now turn.

Einstein's Later Response to the Neo-Kantians: Empiricism and Holism

In the early 1920s, Einstein, Schlick, and Reichenbach were self-consciously united in the effort to defend the empirical integrity of the relativity theory in the face of attempts by the neo-Kantians either to criticize the theory for its implicit denial that Euclidean geometry is true a priori, or to assimilate the theory to the critical philosophy by arguing that it really confirms the Kantian doctrine of space and time, when the latter is properly understood. They were united in the attempt to show how one could be an empiricist about space and time, thus repudiating the Kantian position, but without lapsing into a phenomenalistic, Machian positivism.

Beyond their being united in a common intellectual enterprise; they were also drawing closer both personally and professionally. Thus, from late 1915 through the early 1920s, Einstein and Schlick carried on a warm and regular correspondence, and visited one another whenever possible in Berlin, Rostock, and Kiel. After 1919, Einstein went to great lengths to use his influence to secure for Schlick a better position than the one in Rostock (for more on the relationship between Schlick and Einstein, see Howard 1984). Reichenbach had audited Einstein's lectures on relativity in Berlin in 1918–1919, and, living in the same suburb of Berlin, they would frequently ride the streetcar together to and from the university. They remained in regular contact between 1920, when Reichenbach left to take up a position in Stuttgart, and 1926, when he returned to take up a newly created chair in the philosophy of physics in Berlin, the creation of the chair and Reichenbach's appointment both due in no small part to Einstein's efforts (for more on the personal professional side of Einstein's relationship with Reichenbach, see Maria Reichenbach's introduction to Reichenbach 1965).

We will see, however, that by no later than 1924, the path taken by Einstein in responding to the neo-Kantians and that taken by Schlick and Reichenbach had begun to diverge, with the result that, in fact, two new kinds of empiricism were developed. What was to become logical empiricism featured the distinction between analytic coordinative definitions and synthetic empirical propositions previously described, with the associated claims that only the former are conventional, and that once they are fixed by convention, the truth or falsity of the empirical propositions is determined unambiguously by experience of the empirical content uniquely associated with each empirical proposition.

Einstein's empiricism will differ from logical empiricism on each of these points. He will deny the possibility of an objective distinction between coordinative definitions and empirical propositions, arguing instead that while some such division is needed whenever we want to assess the empirical truth of a theory, how we draw the distinction is completely arbitrary, much as Schlick had argued in the 1918 first edition of the *Allgemeine Erkenntnislehre.* Einstein will also deny, therefore, that the truth or falsity of any proposition is determined unambiguously by experience of its unique empirical content. He will argue, very much in the spirit of Duhem's holistic, underdeterminationist variety of conventionalism that only whole theories have empirical content, not the individual propositions constituting a theory, and that, therefore, any proposition is a candidate for revision in the event that a theory does not square with experience, which is to say that any proposition can be regarded as being established by convention, and not merely the coordinative definitions. And Einstein will deploy his holistic view of theories in a reply to the neo-Kantians quite different in kind from, but no less compelling than that offered by, logical empiricism.

These differences between nascent logical empiricism and Einstein's empiricism were slow to emerge clearly. Indeed, we can read Einstein's major statement on the epistemology of relativity and geometry from the early 1920s, *Geometrie und Erfahrung* (1921), as if it were merely an elaboration of the view being developed by Schlick and Reichenbach. Einstein makes a fundamental distinction here between what he calls "purely axiomatic geometry" and "practical geometry." The objects described by the former—"points," "lines," and "surfaces"—are implicitly defined by the axioms, and thus bear no necessary connection to empirical reality. "Practical geometry," however, is an empirical science. Einstein characterizes the distinction in words that betray an obvious debt to Schlick, which Einstein acknowledges; and they read uncannily like certain passages from the new section eleven in the 1925 second edition of the Schlick's *Allgemeine Erkenntnislehre:*

> It is clear that the system of concepts of axiomatic geometry alone cannot make any assertions as to the behavior of real objects of this kind, which we will call practically-rigid bodies. To be able to make such assertions, geometry must be stripped of its merely logical-formal character by the coordination of real objects of experience with the empty conceptual schemata of axiomatic geometry. To accomplish this, we need only add the proposition: solid bodies are related, with respect to their possible dispositions, as are bodies in Euclidean geometry of three dimensions. Then the propositions of Euclid contain affirmations as to the behavior of practically-rigid bodies. (Einstein 1921; quoted from Einstein 1954b, 234–35)

In other words, what turns purely axiomatic geometry into practical geometry, a geometry with empirical content, is a coordinative definition, equating the purely geometrical concept of a rigid body with some kind of real physical body. The truth or falsity of the empirical, practical geometry thus obtained is settled by experience: "The question whether the practical geometry of the universe is Euclidean or not has a clear meaning, and its answer can only be furnished by experience" (ibid., 235).

In 1924 we find Einstein still writing about these questions in a way that might suggest essential agreement with the Schlick-Reichenbach point of view. Thus, in a letter to André Metz of 27 November 1924, Einstein writes:

> The article of Herr Berche [1924] does not seem to me worthy of penetrating study. As you correctly indicate in your rejoinder, it has not become clear to the author of the article that, in addition to conventions (simultaneity), the foundations and contents of the relativity theory also contain premises that correspond to the character of physical hypotheses (statements about reality that either do or do not apply). (EA 18-255)

In a review of Reichenbach's *Philosophie der Raum-Zeit-Lehre,* Einstein stresses the distinction between coordinative definitions and empirical propositions as one of the chief contributions of the book:

> The author distinguishes sharply between conceptual systems (pure mathematics) that are founded exclusively on "implicit definitions," the propositions of which are merely logically valid implications and in themselves have no relation to reality, and conceptual systems that claim "truth," i.e., contain statements about the real world. The relating of a conceptual system to the world of the "real" is accomplished through the "coordinative definition." This is an indication of the content, what kind of experiential object a concept should correspond to. The clear working out of the role of the coordinative definition, especially in the domain of relativity theory, is one of the main goals that the author has attempted and achieved for the book. (1928b, 20)

On a more careful reading, however, the appearance of Einstein's thoroughgoing agreement with Schlick and Reichenbach begins to give way. Consider, again, *Geometrie und Erfahrung* (Einstein

1921). Immediately after the passages quoted from *Geometrie und Erfahrung*, which seemingly endorse the distinction between coordinative definitions and empirical propositions, presenting the identification of the practically-rigid body with the geometrical notion of a rigid body as an instance of a coordinative definition, Einstein writes:

Why is the equivalence of the practically-rigid body and the body of geometry—which suggests itself so readily—rejected by Poincaré and other investigators? Simply because under closer inspection the real solid bodies in nature are not rigid, because their geometrical behavior, that is, their possibilities of relative disposition, depend upon temperature, external forces, etc. Thus the original, immediate relation between geometry and physical reality appears destroyed, and we feel impelled toward the following more general point of view, which characterizes Poincaré's standpoint. Geometry (G) predicates nothing about the behavior of real things, but only geometry together with the totality (P) of physical laws can do so. Using symbols, we may say that only the sum of (G) + (P) is subject to experimental verification. Thus (G) may be chosen arbitrarily, and also parts of (P); all these laws are conventions. All that is necessary to avoid contradiction is to choose the remainder of (P) so that (G) and the whole of (P) are together in accord with experience. Envisaged in this way, axiomatic geometry and the part of natural law which has been given a conventional status appear as epistemologically equivalent.

Sub specie aeterni Poincaré, in my opinion, is right. The idea of the measuring-rod and the idea of the clock coordinated with it in the theory of relativity do not find their exact correspondence in the real world. It is also clear that the solid body and the clock do not in the conceptual edifice of physics play the part of irreducible elements, but that of composite structures, which must not play any independent part in theoretical physics. But it is my conviction that in the present stage of development of theoretical physics these concepts must still be employed as independent concepts; for we are still far from possessing such certain knowledge of the theoretical principles of atomic structure as to be able to construct solid bodies and clocks theoretically from elementary concepts. (Quoted from Einstein 1954b, 236–37)

What he gives with one hand, he takes away with the other. The need for coordinative definitions is a consequence not of some deep principled fact about the structure and interpretation of scientific theories. It is, rather, a reflection of the primitive state of development of our fundamental physics, primitive in the sense that we are not now capable of deriving the concept of the practically-rigid body from

physical first principles, and so must introduce it independently into our total body of theory through a coordinative definition. In other words, ostension is necessary because our physics is not yet well-enough developed to yield the requisite unambiguous implicit definition.

One is reminded here of the previously quoted remark from the 1918 first edition of the *Allgemeine Erkenntnislehre* in which Schlick said that as scientific theories develop, they grow beyond the need for "concrete definitions," and that, in consequence, any distinction between definitions and knowledge claims is wholly arbitrary (see Schlick 1985, 50). But, as we will see shortly, the deeper roots of the epistemological holism we are beginning to see here lie in Einstein's assimilation as early as 1909 or 1910 of the lessons of Duhem's holistic, underdeterminationist conventionalism (and note that the standpoint Einstein identifies with Poincaré is really more Duhem than Poincaré).

Einstein's disagreement with the Schlick-Reichenbach reply to the neo-Kantians, and with the logical empiricism they were developing as part of that reply began to emerge more clearly in 1924. In that year, Einstein for the first time published reviews of two neo-Kantian works on relativity. The first, a review of Joseph Winternitz's *Relativitätstheorie und Erkenntnislehre* (1923), does little more than reiterate Einstein's now-standard position that categories are necessary, but that they have the status of conventions, and thus are not fixed a priori:

Thus Winternitz asserts with Kant that science is a mental construction on the basis of a priori principles. That the edifice of our science rests and must rest on principles that are not themselves derived from experience, will be acknowledged without doubt. For me, doubt only arises if one asks about the dignity of those principles, that is, about their irreplaceability. Are those principles at least in part so constituted that their modification would be incompatible with science, or are they collectively mere conventions, like the ordering principle of the words of the words in a lexicon? W. inclines toward the former view, I to the latter. (Einstein 1924b, 21–22)

Later that year, however, Einstein published a much longer and more detailed review of Alfred Elsbach's *Kant und Einstein* (1924), which gives the first clear sign of Einstein's distinctively different approach to the defense of relativity against neo-Kantianism. Indeed, this review seems, by its scope and detail, to have been intended by

Einstein as his major statement on the matter. The central part of Einstein's argument deserves to be read with great care. After asserting that relativity theory is incompatible with the Kantian doctrine of the a priori, Einstein writes:

This does not, at first, preclude one's holding at least to the Kantian *problematic,* as, e.g., Cassirer has done. I am even of the opinion that this standpoint can be rigorously refuted by no development of natural science. For one will always be able to say that critical philosophers have until now erred in the establishment of the a priori elements, and one will always be able to establish a system of a priori elements that does not contradict a given physical system. Let me briefly indicate why I do not find this standpoint natural. A physical theory consists of the parts (elements) A, B, C, D, that together constitute a logical whole that correctly connects the pertinent experiments (sense experiences). Then it tends to be the case that the aggregate of fewer than all four elements, e.g., A, B, D, *without* C, no longer says anything about these experiences, and just as well A, B, C without D. One is then free to regard the aggregate of three of these elements, e.g., A, B, C as a priori, and only D as empirically conditioned. But what remains unsatisfactory in this is always the *arbitrariness in the choice* of those elements that one designates as a priori, entirely apart from the fact that the theory could one day be replaced by another that replaces certain of these elements (or all four) by others. (Einstein 1924a, 1688–89)

Notice what Einstein does and does not say here. He does say that what, from the Kantian point of view, is a priori is, from his point of view, a matter of convention. He does not say that what the Kantian regarded as a priori are now, as a matter of principle, to be regarded as coordinative definitions. He also says that if we fix all but one of the components of the theory, presumably by convention, the remaining component by itself is "empirically conditioned." But which component is regarded as thus empirically conditioned, and which are fixed by stipulation, is arbitrary. And why is it arbitrary? Because, as he says, it is really the theory *as a whole* that "correctly connects the pertinent experiments" or "sense experiences."

Schlick and Reichenbach answer the neo-Kantian by arguing that, once the analytic coordinative definitions are fixed by convention, the truth or falsity of each remaining synthetic proposition constituting a physical theory is fixed unambiguously by experience corresponding to that proposition's individual empirical content. By contrast, Einstein answers the neo-Kantian by arguing simply that while the distinction between the a priori and the a posteriori must be made in order to evaluate a theory's empirical warrant, the way in which one draws the distinction is arbitrary, so that, contrary to the Kantian position, a proposition's being dubbed "a priori" carries with it no principled epistemological distinction, such as conferring on it an immunity from prosecution on the basis of contrary experience. Einstein's stress on the arbitrariness of the a priori–a posteriori distinction is equally a challenge to Schlick and Reichenbach, who would make the distinction between conventional coordinative definitions and empirical propositions a principled one. The root of this latter difference lies in Einstein's epistemological holism.

Notice that the kind of arbitrariness here highlighted by Einstein, an arbitrariness in choosing what part of a theory to regard as a priori and which as a posteriori, is quite unlike the kind of arbitrariness characteristic of coordinative definitions as understood by Schlick and Reichenbach. The latter is merely an arbitrariness with regard to the choice of a coordinative definition—should I choose this or that as my measuring rod? It does not extend to the determination of the *kind* of proposition that counts as a coordinative definition. A coordinative definition, as its name is intended to imply, is the kind of proposition that "coordinates" theory with experience.

Duhemian epistemological holism will become the chief characteristic distinguishing Einstein's empiricism from the empiricism of Schlick and Reichenbach in later years. Einstein seems first to have assimilated the lessons of Duhemian holism around 1909 or 1910, when he was a neighbor and colleague (at the University of Zurich) of Duhem's German translator, Friedrich Adler, an old friend of Einstein's from their student days in Zurich (for more on Einstein's early reading of Duhem and his relationship with Adler, see Howard 1990). Possibly as early as the winter semester of 1910–1911, in his lectures on electricity and magnetism at the University of Zurich, Einstein invoked a holistic conception of the relation between theory and experience to allay the misgivings every beginning physics student has about the empirical significance of ascribing a determinate magnitude to the electrical charge at every point within a material body, even though such points are not accessible to test particles:

We have seen how experience led to the introduction of the concept of electrical charge. It was defined with the help of forces that electrified bodies exert on each other. But now we extend the application of the concept to cases in which the definition finds no direct application as soon as we conceive electrical forces as forces that are exerted not on material particles but *on electricity.* We establish a conceptual system whose individual parts do not correspond immediately to experiential facts. Only a certain totality of theoretical materials corresponds again to a certain totality of experimental facts.

We find that such an el[ectrical] continuum is always applicable only for representing relations inside ponderable bodies. Here again we define the vector o[f] el[electrical] field strength as the vector of the mech[anical] force that is exerted on a unit of pos[istive] electr[ical] charge inside a ponderable body. But the force thus defined is no longer immediately accessible to ex-p[eriment]. It is a part of a theoretical construction that is true or false, i.e., corresponding or not corresponding to experience, only *as a whole.* (1993, 325)

Einstein's strategy here is exactly that of Duhem in his *La Théorie physique: son objet et sa structure* (Duhem 1906, chap. 6, secs. 8–9; chap. 7), where a narrow (Machian) positivist conception of empirical significance—each admissible scientific term must have its own, individual empirical content as a result of its being constructed out of the elements of sensation—is repudiated as unduly constraining the introduction of theoretical terms not individually grounded in experience; if only whole theories are viewed as possessing empirical content, then theoretical concepts that are not individually grounded in experience are perfectly acceptable as long as the whole theories to which they belong are so grounded.

That Einstein so understood the import of Duhem's position is clear from an interlineated remark about Duhem in a letter to the Bonn mathematician, Eduard Study, of 25 September 1918, "The positivist or pragmatist is strong as long as he battles against the opinion that there [are] concepts that are anchored in the 'A priori.' When, in his enthusiasm, [he] forgets that all knowledge consists [in] concepts and judgments, then that is a weakness that lies not in the nature of things but in his personal disposition /just as with the senseless battle against hypotheses, cf. the clear book by Duhem/. In any case, the railing against atoms rests upon this weakness" (EA 22-307).

After the mid-1920s, Einstein's expressions of support for the holistic views of theories became ever more common and more emphatic. Thus, for example, in a 1928 review of Émile Meyerson's *La Déduction Relativiste* (1925), of which Einstein described as "one of the most valuable contributions on the theory of relativity which has

been written from the viewpoint of epistemology" (Einstein 1928a; quoted from Meyerson 1985, 256), Einstein gave a summary of Meyerson's position so positive and so unlike Meyerson's in points of detail that one must take it as revealing something of Einstein's own views:

Pure positivism and pragmatism are rejected, indeed ardently combatted. Subjective experiences or facts of experience are indeed the basis of every science, but they do not make up its content, its essence; rather they are merely the given to which science refers. Mere affirmation [*Konstatierung*] of empirical relationships between experimental facts cannot, according to the author, be represented as the only goal of science. First of all, such general relationships as are expressed in our laws of nature are not at all the mere affirmation [*Konstatierung*] of the experiential; they can be formulated and derived only on the basis of a conceptual construction which cannot be extracted for us from experience as such. Secondly, science by no means contents itself with formulating laws of experience. It seeks, on the contrary, to build up a logical system, based on as few premises as possible, which contains all laws of nature as logical consequences. This system, or rather the structures occurring in this system, is coordinated with the objects of experience; reason seeks to arrange this system, which is supposed to correspond to the world of real things of prescientific Weltanschauung, in such a way that it corresponds to the totality of facts of experience or (subjective) experiences. Thus, at the base of all natural science lies a philosophical realism. . . . In this sense Meyerson is a rationalist, not an empiricist. But he also differs from critical idealism in Kant's sense. For there is no feature, no characteristic, of the system we are seeking, about which we can know a priori that it must necessarily belong to this system due to the nature of our thought. This also holds for the forms of logic and causality. We can only ask how the system of science (in its states of development thus far) is composed, but not how it *must* be composed. The logical foundations of the system as well as its structure are thus (from a logical point of view) conventional; their only justification lies in the performance of the system vis-à-vis the facts, in its unified character, and in the small number of its premises. (Ibid., 252–53; the translation has been corrected on the basis of the original German manuscript EA 1-068)

At the same time that Einstein is thus commending Meyerson for his (or Einstein's own) epistemological holism and his philosophical realism, he is growing ever more disenchanted with the direction being taken by what he regards as the positivism of Schlick and Reichenbach. Earlier there had been a near complete agreement between Schlick and Einstein on questions of the structure and interpretation of scientific theories, an agreement betokened by numerous remarks like the one Einstein directed to Schlick in a note of 17 October 1919, "Tomorrow I travel to Holland for two weeks and am taking along your *Erkenntnistheorie* as my only reading. This as proof of how gladly I read around in it. Born also much loves your book" (EA 21-623). The parting of the philosophical ways, however, was nearly complete by 28 November 1930, when, after reading the manuscript of an essay in which Schlick argued that causality means nothing more than predictability (see Schlick 1931), Einstein wrote:

> From a general point of view, your presentation does not correspond to my way of viewing things, inasmuch as I find your whole conception, so to speak, too positivistic. Indeed, physics *supplies* relations between sense experiences, but only indirectly. For me *its essence* is by no means exhaustively characterized by this assertion. I put it to you bluntly: Physics is an attempt to construct conceptually a model of the *real world* as well as of its law-governed structure. To be sure, it must represent exactly the empirical relations between those sense experiences accessible to us; but *only* thus it is chained to the latter. . . . You will be surprised at the "metaphysician" Einstein. But every four- and two-legged animal is de facto in this sense a metaphysician. (EA 21-603)

The distinction between conventional coordinative definitions and empirical propositions is an essential ingredient of the positivism that Einstein is here condemning, for, as we have seen, it is what licenses the characteristically late-Schlickian view that "it is possible to test only the individual statements that are derived from a law of nature" (Schlick 1931; quoted from Schlick 1979, vol. 2, 188), as opposed to testing an entire body of theory.

Einstein returned to the question of holism and the distinction between coordinative definitions and empirical propositions in 1936, in an early section of his classic essay, "Physik und Realität," where he wrote:

> We shall call "primary concepts" such concepts as are directly and intuitively connected with typical complexes of sense experiences. All other concepts are—from the physical point of view—meaningful only insofar as they are brought into connection with the "primary concepts" through statements. These statements are partly definitions of the concepts (and of the statements logically derivable from them) and partly statements that are not derivable from the definitions, and that express at least indirect relations between the "primary concepts" and thereby between sense experiences. Statements of the latter kind are "statements about reality" or "laws of nature," i.e., statements that have to prove themselves on the sense experiences that are comprehended in the primary concepts. *Which of the statements are to be regarded as definitions and which as laws of nature depends largely upon the chosen representation; in general it is only necessary to carry through such a distinction when one wants to investigate to what extent the whole conceptual system under consideration really possesses content from a physical standpoint.* (P. 316; emphasis added)

As Schlick had written in 1918, as Carnap had written in 1921, the distinction between definitions and laws is an arbitrary or relative one, depending "largely on the chosen representation," and the distinction need be drawn "only . . . when one wants to investigate to what extent the *whole* conceptual system . . . possesses content from a physical standpoint." The distinction is not a principled one.

Another indication of Einstein's dislike for the Schlick-Reichenbach conception of the structure and interpretation of theories is Einstein's repeated emphasis in this and later writings upon the "intuitive," meaning not "logically determinable" character of the connection between primary concepts and sense experience:

> Concerning the manner in which we are to build and connect concepts, and the manner in which we are to coordinate them with sense experiences, not the least can be said a priori, in my view. Only success in regard to establishing an order among sense experiences is decisive. The rules for connecting concepts must be stipulated [*festgelegt*] only in general, for otherwise knowledge of the kind for which we strive would be impossible. One has compared these rules with the rules of a game, rules that are in themselves arbitrary but whose determinateness first makes the game possible. However, this stipulation can never be a final one, rather they can only claim validity for an intended domain of application (i.e., there are no ultimate categories in Kant's sense).
>
> The connection of the elementary concepts of everyday thought with complexes of sense experiences is only intuitively understandable and is inaccessible to scientific, logical fixation. The totality of these connections—itself not conceptually understandable—is all that distinguishes the edifice of science from an empty conceptual schema. (Ibid., 315–16)

He stresses that even the most basic concepts, those in the closest contact with experience, are like the more refined concepts of science in their being, like all theory, "free creations of the human mind":

> The first step toward the postulation [*Setzung*] of a "real external world" lies in my view in the construction of the concept of the corporeal object, or cor-

poreal objects of various kinds. Certain repeating complexes of sense experiences . . . are, in thought, lifted out of the fullness of sense experience in an arbitrary manner and coordinated with a concept, the concept of the corporeal object. From a logical point of view, this concept is not identical with the totality of those sense experiences, it is rather a free creation of the human (or animal) mind. On the other hand, this concept owes its meaning and justification exclusively to the totality of those sense experiences to which it is coordinated. (Ibid., 314)

As Einstein elsewhere explains all such "free creation" is, in practice, apparently rigidly constrained by experience, though it is, in principle, not logically so constrained. (For more on Einstein's distinction between "practical" determination by experience and underdetermination "in principle," see Howard 1990.)

Other remarks emphasizing the holistic character of the relationship between theory and experiment are scattered throughout Einstein's writings over the next nineteen years. Typical is this remark from the *Autobiographical Notes:*

I see on the one side the totality of sense experiences and, on the other, the totality of the concepts and propositions that are laid down in books. . . . The concepts and propositions get "meaning," or "content," only through their connection with sense experiences. The connection of the latter with the former is purely intuitive, not itself of a logical nature. . . . The system of concepts is a creation of man, together with the rules of syntax, which constitute the structure of the conceptual systems. Although the conceptual systems are logically entirely arbitrary, they are restricted by the aim of permitting the most nearly possible certain (intuitive) and complete coordination with the totality of sense experiences. . . . A proposition is correct if, with a logical system, it is deduced according to the accepted logical rules. A system has truth-content according to the certainty and completeness of its possibility of coordination with the totality of experience. A correct proposition borrows its "truth" from the truth-content of the system to which it belongs. ([1946] 1979, 11, 13)

For our purposes surely the clearest evidence of Einstein's having understood how his holistic conception of the relation between theory and experience runs counter to the view of Schlick and Reichenbach comes from Einstein's reply to Reichenbach in the Schilpp (1949) volume. In his essay, Reichenbach had invoked the distinction between coordinative definitions and empirical propositions in order to explain why he did not regard the whole of geometry as conventional:

Another confusion must be ascribed to the theory of conventionalism, which goes back to Poincaré. According to this theory, geometry is a matter of convention, and no empirical meaning can be assigned to a statement about the geometry of physical space. Now it is true that physical space can be described by both a Euclidean and a non-Euclidean geometry; but it is an erroneous interpretation of this relativity of geometry to call a statement about the geometrical structure of physical space meaningless. The choice of a geometry is arbitrary only so long as no definition of congruence is specified. Once this definition is set up, it becomes an empirical question *which* geometry holds for a physical space. . . . The combination of a statement about a geometry with a statement of the co-ordinative definition of congruence employed is subject to empirical test and thus expresses a property of the physical world. The conventionalist overlooks the fact that only the incomplete statement of a geometry, in which a reference to the definition of congruence is omitted, is arbitrary; if the statement is made complete by the addition of a reference to the definition of congruence, it becomes empirically verifiable and thus has physical content. (Reichenbach 1949, 297)

Einstein's reply takes the form of a fictional dialogue between "Reichenbach" and "Poincaré," in which, after a few paragraphs, Einstein replaces "Poincaré" by an "anonymous non-positivist," owing to his "respect . . . for Poincaré's superiority as thinker and author" (1949, 677). After getting "Reichenbach" to agree that geometry is not tested by itself, but only together with physics, the "Non-Positivist" draws the inevitable holistic conclusion:

Non-Positivist: If, under the stated circumstances, you hold distance to be a legitimate concept, how then is it with your basic principle (meaning = verifiability)? Must you not come to the point where you deny the meaning of geometrical statements and concede meaning only to the completely developed theory of relativity (which still does not exist at all as a finished product)? Must you not grant that no "meaning" whatsoever, in your sense, belongs to the individual concepts and statements of a physical theory, such meaning belonging instead to the whole system insofar as it makes "intelligible" what is given in experience? Why do the individual concepts that occur in a theory require any separate justification after all, if they are indispensable only within the framework of the logical structure of the theory, and if it is the theory as a whole that stands the test? (Ibid., 678; I have corrected the translation on the basis of Einstein's 1954a original German text)

Notice that this extension of the consequences of holism from the epistemology of scientific theories, where the empirical truth-value of individual propositions is at issue, to the semantics of scientific vocabularies, where the empirical meaning of individual concepts and terms is at issue, was penned two years before Quine's more famous elaboration of the identical argument in "Two Dogmas of Empiricism" (Quine [1951] 1953; for more on Einstein's holism, see Fine [1984] 1986, 107–10 and Howard 1990).

Conclusion

The aim of this essay has been to trace the emergence of a cluster of doctrines central to the mature logical empiricist conception of the structure and interpretation of scientific theories: (1) the distinction between analytic coordinative definitions and synthetic empirical propositions; (2) the assertion that the former are, alone, conventional; and (3) the claim that once the former are fixed by convention, each of the remaining empirical propositions is invested with its own, determinate, individual empirical content, such that the truth or falsity of each empirical proposition is determined unambiguously by experience corresponding to that empirical content. What we have seen is that these doctrines emerged in the course of efforts by Schlick, Reichenbach, and, to some extent, Carnap to articulate a new kind of empiricism—not identical to Machian phenomenalistic positivism—that would be sufficient to defend the empirical integrity of the general theory of relativity in the face of efforts by a variety of neo-Kantians either to repudiate the theory or to claim that it offers no threat to the Kantian doctrine of the synthetic a priori character of the geometry of space. Our reconstruction of this history has been helped by comparing the new empiricism of Schlick and Reichenbach to another new empiricism, developed for similar reasons, by their friend and colleague, Einstein, whose own view stressed a holistic view of the structure and interpretation of theories, denying any principled distinction between coordinative definitions and empirical propositions, and, with it, the associated verificationist conception of meaning.

If there has been any other aim to the essay, it is to suggest that in studying the history of the philosophy of science, at least that of the early part of our century, we must always remember the context in which that philosophy of science was emerging. More often than not the relevant context was constituted by specific controversies concerning the most important new physical theories of the day. Crucial to an understanding of this context and its influence on the development of the philosophy of science is a familiarity with what the leading physical scientists of the day were themselves saying about the problems of method, metaphysics, and epistemology raised by those controversies. For we learn that physicists like Einstein, Planck, and Bohr made contributions to philosophy that were equal in importance to their contributions to physics. They were regarded by thinkers like Schlick, Reichenbach, and Carnap not merely as the authors of the scientific theories serving as touchstones for the new philosophy of science, but also as fellow philosophers of science whose opinions on matters philosophical were studied with the utmost seriousness.

NOTES

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1. See Einstein's ETH record and transcript and the outline of his ETH curriculum, both in Einstein (1987, 45–50, 362–69). A representative of Marburg neo-Kantianism, Stadler was Hermann Cohen's first student (see Cohen 1910), and he wrote extensively on questions concerning Kant and the Natural sciences (see Stadler 1874, 1876, 1883). For more on Stadler's interpretation of Kant, see Beller (1990).

2. See Seelig (1960, 208). While there is some reason to believe that the systematic reading of Kant's first *Kritik* in the *Fanta-Kreis* came after Einstein's departure from Prague (see Brod 1979, 171), Einstein is said by contemporary witnesses to have defended, on occasion, a Kantian position, "He took a lively part in the debates about Kant. . . . His train of thought sometimes took quite surprising, under certain circumstances pro-Kantian turns" (ibid.).

3. That Einstein wrote from Kiel, presumably after a visit with Schlick in Rostock, is significant. Schlick was ready to move to Kiel (one year before moving on to Vienna), a step up the professional ladder that he owed almost entirely to Einstein's having urged his appointment upon Scholz, a student of Riehl's who had been Ordinarius at Kiel since 1919. See Scholz to Einstein, 9 March 1921 (EA 21-532) and Einstein to Scholz, 13 March 1921 (EA 21-534).

4. There are important differences between Schlick's original conception of truth as unambiguous coordination and Reichenbach's interpretation. Thus, unambiguous coordination is more a matter of internal consistency for Reichenbach, not correspondence; and Reichenbach regards it as a definition of knowledge, not just truth. Schlick and Reichenbach discussed these differences in their correspondence from the fall of 1920 (see Schlick to Reichenbach, 26 November 1920, HR 015-63-22; Reichenbach to Schlick, 29 November 1920, HR 015-63-20; and Schlick to Reichenbach, 11 December 1920, HR 015-63-19; see also the discussion in Reichenbach 1922).

5. Thus, when I assert that "snow is white," I am asserting the identity of two regions of spacetime, a snowy region and a white region, see Schlick (1918; 1985, 38–39, 48–49).

6. Not all of this section 11 is new, a few parts of it having been taken over from the end of section 10 of the 1918 first edition; see, for example, the passage from the translation 1985, 78, quoted later in this essay.

7. Strictly speaking, there are three classes of judgments, the broad class of pieces of knowledge (*Erkenntnisse*) being subdivided into *historical* judgments, which are said to designate observed facts, and *hypotheses*, which hold also for unobserved facts. But the subclass of historical judgments is effectively empty since such judgments would have to hold only for "facts that are immediately experienced in the present moment"; thus the class of pieces of knowledge becomes coextensive with the class of hypotheses. See Schlick (1925; translation in 1985, 73).

8. The Schlick of 1918 was of a different opinion, believing that the establishment of the semiotic epistemology was alone sufficient to refute the Kantian position: "If we have succeeded through our previous efforts in establishing beyond any doubt the designating or semiotic character of thinking and knowing, then the critical conception of knowledge is thereby disposed of. All the possibilities contained in that conception, all the consequences that flow from it, must be recognized as untenable. On the basis of our earlier positive findings, we may therefore regard the whole question as settled against the Kantian philosophy" (Schlick 1918; quoted from 1985, 306).

9. The alleged "impossibility" was asserted by Carnap's friend, Kurt Frankenberger (see RC 004-01-01, 20).

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