Einstein and the Development of Twentieth-Century Philosophy of Science

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Introduction

What is Albert Einstein's place in the history of twentieth-century philosophy of science? Were one to consult the histories produced at mid-century from within the Vienna Circle and allied movements (e.g., von Mises 1938, 1939, Kraft 1950, Reichenbach 1951), then one would find, for the most part, two points of emphasis. First, Einstein was rightly remembered as the developer of the special and general theories of relativity, theories which, through their challenge to both scientific and philosophical orthodoxy made vivid the need for a new kind of empiricism (Schlick 1921) whereby one could defend the empirical integrity of the theory of relativity against challenges coming mainly from the defenders of Kant.[1] Second, the special and general theories of relativity were wrongly cited as straightforwardly validating central tenets of the logical empiricist program, such as verificationism, and Einstein was wrongly represented as having, himself, explicitly endorsed those same philosophical principles.

As we now know, logical empiricism was not the monolithic philosophical movement it was once taken to have been. Those associated with the movement disagreed deeply about fundamental issues concerning the structure and interpretation of scientific theories, as in the protocol sentence debate, and about the overall aims of the movement, as in the debate between the left and right wings of the Vienna Circle over the role of politics in science and philosophy.[2] Along with such differences went subtle differences in the assessment of Einstein's legacy to logical empiricism. Philipp Frank–himself a dissenter from central points of right-wing Vienna Circle doctrine–deserves particular mention for his more accurate reading of Einstein's position on such issues as the place of convention in scientific theory (Frank 1949b, 1949c). Still, on the whole, the Einstein legacy is rendered with an unhappy mixture of hagiography and persuasive misreading in the service of appropriating the mantle of his authority to legitimate logical empiricism as it sought at mid-century to establish itself as the authoritative philosophical voice on the nature and status of scientific knowledge.

A more accurate picture of Einstein's relationship to logical empiricism began to emerge at about the time in the mid-1960s when challenges to logical empiricist orthodoxy from the likes of Norwood Russell Hanson, Stephen Toulmin, Thomas Kuhn, and Paul Feyerabend began to create a space in which dissent was possible. The single most important early contribution to this new picture of Einstein's connection with logical empiricism was Gerald Holton's 1968 study, "Mach, Einstein, and the Search for Reality" (Holton 1968). Though one can argue with Holton's uninflected interpretation of Einstein's post-1915 view as a version of what later came to be termed "scientific realism" and with his missing the important theme of Einstein's sympathy for Pierre Duhem's brand of conventionalism (see Howard 1993), we nevertheless owe Holton a major debt for making clear the extent to which, already by the late 1920s, Einstein had explicitly forsworn any sympathy for verificationism and the anti-metaphysical agenda of logical empiricism, this at the very time when Vienna Circle pamphleteers were trumpeting his support for the movement (see, for example, Neurath, Hahn, and Carnap 1929).

Lacking, however, in both the original logical empiricist hagiography and in Holton's revisionist picture is a clear sense of what might well be Einstein's major legacy to twentieth-century philosophy of science, which is the fact that Einstein was, himself, one of the most important, active, constructive contributors to the development of a new empiricism in the 1910s and 1920s. Einstein did not merely provide theoretical grist for the philosophical mill of logical empiricism, that and possibly a benediction. On the contrary, virtually all of the major figures involved in the debates out of which logical empiricism grew worked out their ideas through an engagement not just with general relativity but with the author of the theory, himself. This was true of central founding figures in the logical empiricist camp, such as Moritz Schlick, Hans Reichenbach, Philipp Frank, and Rudolf Carnap. It was true, as well, of figures associated with other traditions, such as the neo-Kantian Ernst Cassirer and Hermann Weyl, whose chief philosophical debt was to the phenomenology of Edmund Husserl. In conversation, in correspondence, and in a wealth of published essays and reviews, Einstein tutored, criticized, and learned, his interventions concerning large questions of doctrine and fine points of detail.

Moreover, while Einstein produced no philosophical magnum opus to rival Schlick’s *Allgemeine Erkenntnislehre* (1918), Weyl’s *Philosophie der Mathematik und der Naturwissenchaft* (1927), or Reichenbach’s *Philosophie der Raum-Zeit-Lehre* (1928), the position that he defended from early to late–as mentioned, a variant of Duhemian conventionalism–has much to recommend it as, in its own right, a cogent, comprehensive philosophy of science, from which there is still much to be learned that is of relevance to contemporary philosophical controversies (see, for example, Howard 2004d). A full history of twentieth-century philosophy of science should, therefore, devote a whole chapter to Einstein as a figure at least as important and influential as those already canonized.

Background and Early Influences

Einstein was like many scientists of his generation in his early and regular engagement with philosophy. He is reported to have read Kant's first *Critique* already at the age of thirteen (Beller 2000). As a physics student at the Eidgenössische Technische Hochschule (ETH) in Zurich in the late 1890s, Einstein was required to attend lectures on "Theorie des wissenschaftlichen Denkens" given by August Stadler, a neo-Kantian philosopher who was Hermann Cohen's first doctoral student at Marburg, and Einstein elected to enroll also in Stadler's lectures on Kant (Beller 2000). On his own while a student and, a few years later with his friends in the discussion group they constituted in Bern around 1903, the so-called Akademie Olympia, Einstein read carefully many of the most important works of figures like Ernst Mach, Henri Poincaré, John Stuart Mill, Richard Avenarius, Karl Pearson, Richard Dedekind, and David Hume.[3] He read Friedrich Albert Lange's *Geschichte des Materialismus* (Lange 1873-1875), Eugen Dühring's *Kritische Geschichte der Principien der Mechanik* (Dühring 1887), and Ferdinand Rosenberger's *Isaac Newton und seine physikalischen Prinzipien* (Rosenberger 1895).[4] During his university years and repeatedly thereafter, Einstein read widely in the works of Arthur Schopenhauer (Howard 1997).

From the beginning, Einstein understood such reading in the history and philosophy of science as making an important difference to the way in which one does physics. Thus, in a 1916 memorial notice for Mach, Einstein wrote:

> How does it happen that a properly endowed natural scientist comes to concern himself with epistemology? Is there no more valuable work in his specialty? I hear many of my colleagues saying, and I sense it from many more, that they feel this way. I cannot share this sentiment. When I think about the ablest students whom I have encountered in my teaching, that is, those who distinguish themselves by their independence of judgment and not merely their

> quick-wittedness, I can affirm that they had a vigorous interest in epistemology. They happily began discussions about the goals and methods of science, and they showed unequivocally, through their tenacity in defending their views, that the subject seemed important to them. Indeed, one should not be surprised at this. (Einstein 1916, 101)

And in a 28 November 1944 letter to Robert Thornton he echoed those words of nearly thirty years earlier:

> I fully agree with you about the significance and educational value of methodology as well as history and philosophy of science. So many people today—and even professional scientists—seem to me like somebody who has seen thousands of trees but has never seen a forest. A knowledge of the historic and philosophical background gives that kind of independence from prejudices of his generation from which most scientists are suffering. This independence created by philosophical insight is—in my opinion—the mark of distinction between a mere artisan or specialist and a real seeker after truth. (Einstein to Thornton, 7 December 1944, EA 61-574)

Many of the mentioned authors had an important influence on the development of Einstein's thinking, foremost among them Kant, Poincaré, Mach, and Hume (Howard 2004a, Beller 2000, Norton 2004). Much has been made of the influence of Mach, in particular (see, for example, Frank 1949c). But the nature of that influence is complicated.

Einstein, himself, acknowledged an important debt to Mach in the genesis of both special relativity (Norton 2004 provides helpful detail; see also Holton 1968) and general relativity (see Howard 1999, Norton 1995, and Hoefer 1995). But on various later occasions Einstein stressed his dissent from Mach's doctrine of the elements of sensation as the basis of all scientific knowledge, as in a letter to Besso of 6 January 1948:

> I see his great service as residing in the fact that he dispelled the dogmatism that reigned in the foundations of physics in the 18th and 19th centuries. Especially in the *Mechanik* and the *Wärmelehre*, he sought to show how concepts grow up out of experience. He convincingly defended the view that these concepts, even the most fundamental ones, obtain their justification only from experience. . . . I see his weakness as residing in the fact that he more

> or less believed that science consists in the mere "ordering" of empirical materials; i.e., he misunderstood the free, constructive element in the formation of concepts. In a sense, he believed that scientific theories arise through *discovery* and not through *invention*. (Speziali 1972, 390-391)

More than anything else, the positive lesson that Einstein drew from Mach concerned the historicity of scientific concepts and theories. Both the *Mechanik* (Mach 1883) and the *Wärmelehre* (Mach 1896) advertise in their subtitles that they are essays in an "historical-critical" method, and, as in the contemporary Biblical hermeneutics literature that employed the same characterization of its critical method (see, for example, Strauss 1835-1836), the point was to exploit the historicity of texts, concepts, or theories–their having been authored in specific historical circumstances by specific individuals working in specific problematic settings–to deprive those texts, concepts, and theories of their authority as received wisdom. Precisely this aspect of Mach's legacy was stressed by Einstein in the just-quoted letter to Besso but also, over thirty years earlier, in the above-cited obituary notice for Mach:

> The fact is that Mach exercised a great influence upon our generation through his historical-critical writings. . . . Concepts that have proven useful in ordering things easily achieve such an authority over us that we forget their earthly origins and accept them as unalterable givens. Thus they come to be stamped as "necessities of thought," "a priori givens," etc. The path of scientific advance is often made impassable for a long time through such errors. For that reason, it is by no means an idle game if we become practiced in analyzing the long commonplace concepts and exhibiting those circumstances upon which their justification and usefulness depend, how they have grown up, individually, out of the givens of experience. By this means, their all-too-great authority will be broken. They will be removed if they cannot be properly legitimated, corrected if their correlation with given things be far too superfluous, replaced by others if a new system can be established that we prefer for whatever reason. (Einstein 1916, 102)

Mach's emphasis on the historicity of concepts and theories is continuous with his preferred description of his point of view as a "biological-economical" one, whereby he meant that the capacity for scientific knowledge, with its "economical" representations of experience through concepts and theories, should be viewed as an adaptive trait of the human species.

As great as was the influence of Mach on Einstein, there is another author whom Einstein seems also to have encountered at an early date, an author whose influence was, arguably, still more important, namely, Pierre Duhem. The story of Einstein's first acquaintance with Duhem is instructive not only as regards the content of Einstein's philosophy of science but also from the point of view of appreciating the manner in which Einstein and his contemporaries regarded the intermingling of physics and philosophy.[5]

When, in the fall of 1909, Einstein left his job at the patent office in Bern and returned to Zurich to take up his first formal academic position at the University of Zurich, he happened to rent an apartment at Moussonstraße 12, directly upstairs from his old friend and fellow Zurich physics student, Friedrich Adler. As chance would have it, Adler had been the other finalist for the very job Einstein was now assuming and–coincidence added to coincidence–just one year earlier Adler published his German translation of Duhem's *La Théorie physique: son objet et sa structure* (Duhem 1906, 1908). We know from Adler's letters to his father, Viktor Adler, co-founder of the Austrian Social-Democratic Party (see Ardelt 1984), that he and Einstein set up for themselves in the attic of the apartment building a shared study where they could work and converse, away from the noise of the children playing in the courtyard below. How did Adler recall their interactions?

> We stand on very good terms with Einstein, who lives above us, and indeed as it happens, among all of the academics, we are on the most intimate terms precisely with him. They have a bohemian household similar to ours, one boy of Assinka's age, who is very often at our place. . . . The more I speak with Einstein—and that happens fairly often—the more I see that my favorable opinion of him was justified. Among contemporary physicists he is not only

> one of the clearest, but also one of the most independent minds, and we are of one mind about questions whose place is generally not understood by the majority of other physicists. (Ardelt 1984, 166)

Adler later gained notariety for assassinating the Austrian Prime Minister, Graf Stürgkh, in 1916 as a protest against Austrian policies in World War I. In 1909, however, he was merely a Privatdozent at the University of Zurich, where he held the *venia legendi* for "experimental and theoretical physics, as well as their history and epistemological foundations" (Ardelt 1984, 157-166) and was well known in both philosophical and political circles for his writings on the philosophy of science, most of them written from a broadly Machian perspective, as with his intervention on Mach's side in the just then raging debate between Mach and Max Planck (see Planck 1909, 1910, Mach 1910, and Adler 1900). Adler's praise of Einstein's "independent" mind anticipates Einstein's own later stress on the independence of judgment that he thought the most valuable consequence of studying the history and philosophy of science.

The young Adler and the young Einstein were not at all unusual for the time in thus viewing the history and philosophy of science as an integral, critical part of the larger project of physics. Other examples are everywhere to be found, from Einstein's good friends Michele Besso (Speziali 1972), Maurice Solovine (Solovine 1956), and the Habicht brothers, Conrad and Paul (the latter three part of the *Akademie Olympia*), to future colleagues, like Philipp Frank (Frank 1949a, Howard 2004c), Erwin Schrödinger (Moore 1994), Wolfgang Pauli (Enz 2002), Paul Ehrenfest (Klein 1970), and Max Born (Born 1969 [*CPAE* 8, Doc. 575]). Many of this generation's teachers likewise evinced the same synthetic understanding of a philosophical physics, whether it be Mach (Banks 2003) or Planck (Heilbron 1986), Hermann von Helmholtz (Krüger 1994) or Ludwig Boltzmann (Broda

1983). Major journals promoted the integration of philosophy and physics, from Wilhelm Ostwald's *Annalen der Naturphilosophie* and *Zeitschrift für den physikalischen und chemischen Unterricht* to Arnold Berliner's *Die Naturwissenschaften*. The philosopher-physicist, if perhaps not quite the norm, was also far from being the exception.[6]

The main philosophical lesson that Einstein, Adler, and took from their reading of Duhem concerned the empirical content and warrant for physical theories. Duhem argued that it is not individual propositions but only whole theories that possess empirical content and that, in consequence of this holism, theory choice is underdetermined by empirical evidence, since the fit between theory and evidence can be maintained by making adjustments in various different places within the total body of theory. For Duhem, the choice among these options is a conventional one.

Duhem's position stands opposed to a stern empiricism, sometimes wrongly associated with Mach,[7] but rightly associated with the later verificationism of the Vienna Circle, according to which each proposition or even each scientific concept must have its own determinate empirical content. Duhem's position stands equally opposed to Poincaré's view that some scientific laws are not at risk of empirical refutation as a result of their playing a uniquely definitional role in theory, as with the law of free fall (alleged to define the notion of an inertial trajectory), for Duhem argues that there is no principled basis upon which to parse a theory into propositions that function as definitions and propositions that function as synthetic, empirical assertions.

That Einstein learned this lesson from Duhem is evident from the way in which he explained to his students in the winter semester of 1910-1911 how one can make good empirical sense of the notion of electrical charge within a solid charged body even though one cannot introduce a test particle whereby to gauge the electrostatic field in the interior of that solid:

> 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[ectrical] 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 exp[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*. (Einstein 1910-1911, 325)

One could not ask for a more succinct statement of the central epistemological lesson of Duhem’s *La Théorie Physique*.

A New Empiricism As an Answer to the Neo-Kantians

Logical empiricism was the philosophical movement with which Einstein later came to be most closely associated, this in no small measure because of Einstein’s personal connections to several of its chief architects. For example, as early as 1907, Einstein began a correspondence with Philipp Frank, then a central member of what is now known as the “first Vienna Circle” (Haller 1985, Frank 1949a) and to the end of his life prominently associated with the Vienna Circle and its descendent institutions (Stadler 1997, 681-687). Einstein had read Frank’s 1907 paper on causality (Frank 1907) and wrote to offer some suggestions about the manner in which causality might be seen as having a conventional and an empirical character (Frank 1949a, 22). In 1912 Frank succeeded Einstein to the chair in theoretical physics at the German university in Prague, this upon a recommendation from Einstein that included mention of Frank's epistemological writings, and in later years he became Einstein's biographer (Frank 1947) and an important commentator on Einstein's philosophy of science (Frank 1949b, 1949c; see also Howard 2004c).

Moritz Schlick became, after 1922, the guiding spirit of the Vienna Circle. Schlick and Einstein were in correspondence by late 1915, when Schlick sent Einstein a copy of his paper on the philosophical significance of the theory of relativity (Schlick 1915), a paper that Einstein commended in part because of Schlick's having appreciated the importance of Mach and Hume for the development of special relativity.[8] Theirs was an especially close relationship through the early 1920s, Einstein working to promote Schlick's career and the dissemination of his writings, as with his helping to arrange an English translation of Schlick's 1917 monograph, *Raum und Zeit in der gegenwärtigen Physik* (Schlick 1917), the main aim of which, according to Schlick, was to explain the chief philosophical implications of the general theory of relativity.[9]

Einstein first met Hans Reichenbach in the late 1910s, when, as a student, Reichenbach audited Einstein's lectures on relativity at the University of Berlin. Einstein did not wholeheartedly commend Reichenbach's early efforts to develop an interpretation of the theory of relativity as demonstrating a central role in physics for a contingent, constitutive a priori (Reichenbach 1920), a view that Reichenbach himself quickly modified in the direction of the metric conventionalism for which he later was famous. Nevertheless, in the mid-1920s, Einstein's esteem for Reichenbach was such that he collaborated with Planck to create for Reichenbach a chair in the philosophy of science in the physics department at Berlin (Hecht and Hoffmann 1982), from which position Reichenbach went on to build the Berlin outpost of the Vienna Circle.

Schlick and Reichenbach played the leading role in the early- to mid-1920s in shaping the philosophy of science that we remember as logical empiricism, much of the fine structure of which reflects their efforts, together with Einstein, to craft a form of empiricism adequate to the task of defending the empirical integrity of general relativity against various challenges, including most prominently that issuing from neo-Kantians such as Cassirer (Cassirer 1921). The Kantian challenge to general relativity came in many forms, ranging from those who simply rejected as impossible general relativity's assertion of a non-Euclidean metrical structure for space-time, and those who took refuge in an arguably quiet un-Kantian distinction between physical space and psychological space, to those technically more able philosophers who, like Cassirer, sought to save Kant by retreating from ascribing a priori status to a specific metrical geometry of space-time, locating some weaker a priori structure in, say, local topological structure.[10] As mentioned, in his first book on relativity Reichenbach sought to defend Kant by teasing apart the apodictic and constitutive aspects of the a priori, rejecting the former and retaining the latter—now regarded as a contingent a priori—as that which effects the needed coordination between theory and world and thereby endows a theory with empirical content (Reichenbach 1920).

Schlick was not enthusiastic about any of the Kantian responses to general relativity, Reichenbach's included. Schlick's own earlier exposition of the central philosophical implications of general relativity in his *Raum und Zeit in der gegenwärtigen Physik* (1917) emphasized the theory's positing a real space-time event ontology and, thereby, its incompatibility with a philosophy of science that would accord reality only to observables, such as Mach's elements of sensation. But Schlick also stressed the ineluctable moment of convention in scientific theorizing, inspired both by his reading of Poincaré and, even more so, by his own still earlier and then quite influential theory of truth as involving only a univocal, many-to-one coordination of theory to world (Schlick 1910), which entailed an underdetermination of theory choice by evidence not unlike that central to Duhem's philosophy of science. Einstein, who first read the book in manuscript, wholeheartedly commended both the critique of Mach and Schlick's views on the empirical underdetermination of theory choice. In the first edition of his *Allgemeine Erkenntnislehre* (1918), Schlick again gave center stage to the role of convention in scientific cognition, basing the argument once more on his view of truth as univocal coordination but employing also David Hilbert's idea of a theory's implicitly defining its primitive terms via the systematic role they play in the theory's axioms–as opposed to each term's being given an explicit empirical definition–for the purpose of stressing that the coordination relates theory to world only as a whole and not one concept or one proposition at a time.

When, by the early 1920s, the gathering neo-Kantian reaction to relativity finally elicited a focused and thoughtful reply from Schlick, it was the notion of convention that provided Schlick an alternative to the a priori. In private correspondence with Reichenbach and in a published review of Cassirer's book on Einstein's theory of relativity (Cassirer 1921, Schlick 1921), Schlick suggested that the "constitutive principles" whereby experience is ordered and interpreted are more helpfully characterized as conventions than as elements of either a contingent or an apodictic a priori component of scientific cognition.[11] Einstein–with whom Schlick was then in close and regular contact–had been suggesting much the same thing for several years. Thus, in a letter to Max Born of July 1918, Einstein wrote:

> 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, 25-26)

Einstein repeated the same point–that what Kant regards as a priori is more properly seen as conventional–in virtually every one of the many comments he penned on the subject through the mid-1920s (see Howard 1994).

As the discussion involving Einstein, Schlick, Reichenbach, and Cassirer progressed in the early 1920s, the effort to craft an empiricist analysis of general relativity that would force the point with the neo-Kantians led Schlick and Reichenbach to an historically important refinement in their understanding of where the conventional moment in science was to be located, and it led to a parting of the ways, ironically, with Einstein. Schlick and Reichenbach wanted to be able to say to the neo-Kantian that the attribution of a specific metrical structure to space-time was, in the end, an empirical matter, this in spite of there being a conventional aspect to ascriptions of metrical structure. They took their cue from Poincaré's well known view of conventions as disguised definitions (Poincaré 1902).

As first clearly presented in Reichenbach's 1924 book on the axiomatization of space-time theory (Reichenbach 1924) and then more famously in Reichenbach's 1928 *Philosophie der Raum-Zeit Lehre* (Reichenbach 1928) or Schlick's 1935 "Sind die Naturgesetze Konventionen?" (Schlick

1935), the idea was to restrict the moment of convention to the coordinating definitions that are assumed to effect links between a theory's primitive empirical concepts and relevant aspects of the world that the theory seeks to describe. Paradigmatic coordinating definitions would be the associations of basic notions like "spatial interval" and "temporal interval" with, respectively, a specific kind of practically rigid measuring rod and a specific kind of regular clock. Fix the empirical meaning of a theory's primitive terms by such conventional coordinating definitions and each of the synthetic empirical propositions in a theory thereby acquires a determinate empirical content such that its truth or falsity is, in principle, univocally determined by the corresponding experience. Differences do arise through our choosing different coordinating definitions. But such differences are inconsequential, for they depend, at root, only on arbitrary, conventional definitions, and so can be no more significant than the difference between choosing English or metric units. Moreover, for such empiricists empirical content is the only content, which implies that, since empirically equivalent theories have the same empirical content, they can only be alternative ways of expressing that same content, just as "il pleut" and "es regnet" are merely alternative ways of expressing the same meteorological proposition, "it is raining."

If this were the right way to think about how theories acquire empirical content, it would have represented a persuasive reply to the neo-Kantian critics of general relativity, for it would have made the choice of a metrical space-time geometry a straightforwardly empirical matter. But the maneuver rests upon one crucial assumption, namely, that one can in a principled manner distinguish within a theory among those propositions that, by their very nature, function as mere conventional definitions and those that function as synthetic, empirical claims. In another guise, this is the analytic-synthetic distinction.

Schlick and Reichenbach might have thought that they were simply elaborating a view about empirical content already put forward by Einstein in a widely-read lecture from 1921, *Geometrie und Erfahrung* (Einstein 1921). But when that lecture's discussion of the empirical interpretation of general relativity is read against the background of Einstein's then at least decade-old sympathy for Duhemian holism, it becomes clear that Einstein had in mind a very different picture of the place of convention in scientific theories.

In *Geometrie und Erfahrung*, Einstein distinguished "pure" and "practical" geometry. Pure geometry is an uninterpreted mathematical formalism the primitive terms of which–e.g., "point" and "line"–are defined implicitly, à la Hilbert, via the systematic role they play in the formalism's axioms. Practical geometry differs by associating or coordinating those primitive terms with some empirical or physical objects whereby the geometry becomes, in effect, a physical theory, a theory about the behavior of those objects. The crucial question concerns the manner in which the primitive terms are thus associated with empirical or physical objects.

Consider the primitive term, "segment of a straight line." Our intuition tells us to associate this with a physical structure–say the path of a ray of light or the edge of a rigid measuring rod–of such kind that information gleaned from the behavior of those structures concerns exclusively the geometrical properties of the space or space-time in which the objects live and not physical facts about the objects themselves. We all know, however, that real measuring rods are not perfectly rigid, suffering thermal deformations and mechanical stresses. Should measurements conducted with such rods appear to show deviations from Euclidean geometry, how much of that "deviant" behavior should be attributed to the geometry of space-time and how much to the physics of the rod itself? In "L'expérience et la géométrie" (1902) Poincaré had, in effect, argued that the defender of Euclidean geometry can always exploit this ambiguity by ascribing the deviance to the physics and that one would want to do so, in order to save Euclidean geometry, because Euclidean geometry is simpler than its non-Euclidean rivals. Of course, Einstein disagreed, arguing that we should choose not what yields the simpler geometry, but what maximizes the simplicity of geometry plus physics, as does, by his lights, general relativity.

There is, however, a serious obstacle to our maximizing the simplicity of geometry plus physics, for while we know, at one level, that various aspects of the behavior of the rod are physical effects, as with thermal deformations, we lack at present a complete, fundamental, physical theory of structures like measuring rods and clocks and so cannot, in fact, actually pose the simplicity question on the physical side. Under such circumstances, the best that we can do is, by stipulation and with a modicum of scientific good sense, to pick some practically rigid body as the physical or empirical interpretation of "segment of a straight line," likewise for some practically regular clock and the primitive term "temporal interval," and then to let measurements carried out with these rods and clocks settle the question of the metrical structure of space-time.

The resulting picture looks much like Reichenbach's. Geometrical primitives are assigned empirical interpretations via stipulative, hence conventional, coordinating definitions, whereafter the truth or falsity of synthetic, empirical propositions such as that concerning the space-time metric is univocally determined on the basis of the relevant experience. But what for Reichenbach and Schlick was a first principles story about how all theories get their empirical content was, for Einstein, but a provisional, stop-gap procedure forced upon us by the lack of a sufficiently complete fundamental theory:

> 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. (Einstein 1921, 8)

For Einstein, the correct first principles story was very different. It was essentially Duhem's holism.

This was not the first time that Einstein had contrasted an in-principles story akin to Duhem's with an in-practice story portraying a tighter link between theory and experience. In a 1918 address celebrating Planck's sixtieth birthday, "Motive des Forschens," Einstein compared the underdetermination of theory choice in principle with the well nigh universal impression of physicists that, in practice, a single theory always stands out as superior:

> The supreme task of the physicist is . . . the search for those most general, elementary laws from which the world picture is to be obtained through pure deduction. No logical path leads to these elementary laws; it is instead just the intuition that rests on an empathic understanding of experience. In this state of methodological uncertainty one can think that arbitrarily many, in themselves equally justified systems of theoretical principles were possible; and this opinion is, *in principle*, certainly correct. But the development of physics has shown that of all the conceivable theoretical constructions a single one has, at any given time, proved itself unconditionally superior to all others. No one who has really gone deeply into the subject will deny that, in practice, the world of perceptions determines the theoretical system unambiguously, even though no logical path leads from the perceptions to the basic principles of the theory. (Einstein 1918a, p. 31)

But between 1918 and 1921, the question of the extent to which and manner in which straightforward empirical warrant could be claimed for a space-time theory had become, for Einstein, a most acute one, thanks mainly to the challenge posed by Weyl's attempt at a unified field theory (Weyl 1918a, 1918b).

Weyl's attempt to unify gravitation with electricity and magnetism employed a geometrical framework that was more strictly "local" than that of Einstein's general relativity in the sense that it did not assume the cogency of direct distant comparisons of length. In Weyl's geometry, length was path dependent: two bodies congruent at one space-time point need not be congruent at another if they followed different paths (world-lines) to the latter. Einstein objected that this feature of Weyl's space-time geometry left it without determinate empirical content, since in a Weyl space the geometrical concept of the space-time interval could, therefore, not be given a univocal empirical interpretation (Einstein 1918b, 1918c; see also Einstein to Walter Dällenbach, after 15 June 1918 [*CPAE* 8, Doc. 565]). In effect, Einstein was arguing that, in a Weyl space, there could be no univocal definition of the interval via a practically rigid rod.[12] At the time, Einstein seemed to regard the impossibility of our giving such a direct empirical interpretation to the interval to be a nearly fatal methodological flaw, though he recognized that Weyl would not grant the point, since Weyl would insist on viewing rods and clocks as structures derived from within one's fundamental theory, as opposed to their being specified from without by a coordinating definition. By 1921, however, Einstein seems to have come to regard Weyl's point of view as, in principle, the correct one. Had Einstein really changed his mind?

The clearest statement of Einstein's view on the empirical interpretation of general relativity is found in a 1924 review of a book on Kant and relativity by Alfred Elsbach (Elsbach 1924). Elsbach had commended several arguments originally adduced in Paul Natorp's *Die logischen Grundlagen der exakten Wissenschaften* (1910), among them the claims (i) that if deviations from Euclidean geometry were discovered they could be accounted for by making changes in physical laws and (ii) that the metrical structure of space cannot be determined experimentally because space is not real but ideal. Einstein comments:

> The position that one takes on these claims depends on whether one grants reality to the practically-rigid body. If yes, then the concept of the interval corresponds to something experiential. Geometry then contains assertions about possible experiments; it is a physical science that is directly underpinned by experimental testing (standpoint A). If the practically-rigid measuring body is accorded no reality, then geometry alone contains no assertions about experiences (experiments), but instead only geometry with physical sciences taken together (standpoint B). Until now physics has always availed itself of the simpler standpoint A and, for the most part, is indebted to it for its fruitfulness; physics employs it in all of its measurements. Viewed from this standpoint, all of Natorp's assertions are incorrect. . . . But if one adopts standpoint B, which seems overly cautious at the present stage of the development of physics, then geometry alone is not experimentally testable. There are then no geometrical measurements whatsoever. But one must not, for that reason, speak of the "ideality of space." "Ideality" pertains to all concepts, those referring to space and time no less and no more than all others. Only a complete scientific conceptual system comes to be univocally coordinated with sensory experience. On my view, Kant has influenced the development of our thinking in an unfavorable way, in that he has ascribed a special status to spatio-temporal concepts and their relations in contrast to other concepts.
>
> Viewed from standpoint B, the choice of geometrical concepts and relations is, indeed, determined only on the grounds of simplicity and instrumental utility. . . . Concerning the metrical determination of space, nothing can then be made out empirically, but not "because is not real," but because, on this choice of a standpoint, geometry is not a *complete* physical conceptual system, but only a part of one such. (Einstein 1924, 1690-1691)

When Einstein speaks here of granting "reality" to the practically rigid rod, he means simply our taking it as the putative referent of the geometrical primitive, "segment of a straight line." According no "reality" to the practically rigid rod means simply that there is none such that can stand on the other side of a conventional coordinating definition of "segment of a straight line," for the reason earlier mentioned, namely, that real rods (and clocks) are not perfectly rigid (or regular). If there are no such rigid rods and regular clocks, then the alternative is to regard such structures as being defined implicitly within the theory (or some future, complete, fundamental theory), not stipulated from without, which, in turn, implies, as Einstein notes, that it is only the whole theory, "the complete scientific conceptual system" that has determinate empirical content.[13]

Reichenbach and Schlick argue that empirical content attaches to theories one proposition at a time. Einstein, following Duhem, argues that, in principle, it attaches only to whole theories, even if, for practical reasons, we proceed as if the Reichenbach-Schlick picture were the right one. What, then, of the empiricist reply to the neo-Kantians that Reichenbach and Schlick were seeking? Einstein, no less than they, wanted a reply to the neo-Kantians, but he found it precisely in Duhemian holism. Earlier in his review of Elsbach, after asserting that, if one thought relativity a reasonable theory, then one must reject the Kantian doctrine of the a priori character of space and time, Einstein added:

> 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 which 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 1924, 1688-1689)

Einstein's named target here is Kant. But Einstein's argument bears just as well against the kind of conventionalism defended by Reichenbach and Schlick, which, recall, arose out of Schlick's telling the Reichenbach of 1921 that his a priori principles of coordination were more helpfully regarded as conventional coordinating definitions. Call them a priori principles or conventional definitions. The fact remains that one requires a principled basis upon which to distinguish propositions of that kind from the synthetic propositions thereby allegedly endowed with empirical meaning. Einstein's point is that there is no principled basis for thus distinguishing the components of a theory, any such parsing of theory being, from a principled point of view, purely arbitrary.

We see here in germ an argument better known to most philosophers in the form famously given it by Quine in "Two Dogmas of Empiricism" (Quine 1951). The point of view concerning empirical content being developed by Schlick and Reichenbach, with its assumption of some kin to the analytic-synthetic distinction as the basis upon which to distinguish the non-empirical from the empirical components of a theory is the progenitor of the verificationism mainly targeted by Quine ("reductionism" in Quine's vocabulary),[14] and Einstein's deploying Duhemian holism against Schlick and Reichenbach anticipates Quine's principal critical arguments against both verificationism and the analytic-synthetic distinction.

Just how close Einstein comes to anticipating Quine's more famous argument is evident from Einstein's "Reply" to Reichenbach's contribution to the Einstein volume in the Library of Living

Philosophers. In his essay, Reichenbach had defended a first-principles view of the empirical character of geometry much like the provisional, stop-gap view that Einstein presented in *Geometrie und Erfahrung*, with the exception that Reichenbach, invoked explicitly his distinction between coordinating definitions and empirical hypotheses, portraying Einstein's identification of the geometer's "rigid body" with the physicist's "practically rigid rod" as an instance of the former (this is what is meant by a definition of "congruence"):

> 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 physical space. . . . 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. (Reichenbach 1949, 297)

In his reply, Einstein constructs an imaginary dialogue bewteen "Reichenbach" and "Poincaré," about which Ernest Nagel remarked in his review, "the spokesman for Poincaré does not obviously have the worst of the argument" (Nagel 1950, 293-294).[15]

The dialogue begins with "Poincaré" asserting that geometrical theorems are not, by themselves, testable because there are, in fact, no rigid bodies by which to interpret them. "Reichenbach" replies, à la the Einstein of 1921, that we can do well enough with the almost rigid bodies of our experience, as long as we make obvious corrections for such factors as changing temperature. Einstein's "Poincaré" notes that in making these corrections we must use physical laws that presuppose Euclidean geometry, and concludes that what is at stake in an experiment is, thus, the entire body of law consisting of physics and geometry. Here Einstein interrupts with the following parenthetical remark: "(The conversation cannot be continued in this fashion because the respect of the writer for Poincaré’s superiority as thinker and author does not permit it; in what follows therefore, an anonymous non-positivist is substituted for Poincaré—)” (Einstein 1949, 677).

As the dialogue resumes, “Reichenbach” grants a certain attractiveness to “Poincaré’s” point of view, while insisting that in fact, if not in theory, it would have been impossible for Einstein to have developed general relativity “if he had not adhered to the objective meaning of length,” that is, to the concept of the practically rigid body regarded as an unanalyzed primitive. “Reichenbach” goes on to argue that, if physics and geometry *are* tested together, then our aim should he the development of the simplest possible total system, not the simplest geometry alone.

Thus far the dialogue has proceeded in a predictable fashion. But now it takes an unexpected turn. Having wrung from “Reichenbach” the grudging admission that “Poincaré” might be correct, in theory, about physics and geometry being tested together, though with the stipulation that it is then the simplicity of this whole–physics plus geometry–that must be judged, the “Non-Positivist” points out that “Reichenbach” has thereby contravened one of his own fundamental postulates–the equation of meaning with verification:

> *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)? Do you not have to reach the point where you must deny the meaning of geometrical concepts and theorems and to acknowledge meaning only within the completely developed theory of relativity (which, however, does not yet exist at all as a finished product)? Do you not have to admit that, in your sense of the word, no “meaning” can be attributed to the individual concepts and assertions of a physical theory at all, and to the entire system only insofar as it makes what is given in experience “intelligible?” Why do the individual concepts that occur in a theory require any specific justification anyway, if they are only indispensable within the framework of the logical structure of the theory, and the theory only in its entirety validates itself. (Einstein 1949, 678)

Notice that Einstein here asserts not just the epistemological holism of Duhem but also the semantic holism fundamental to Quine's critique of logical empiricism.[16]

Not a "Machian," But Not a "Realist" Either

By the late 1920s Einstein, once Schlick and Reichenbach's close philosophical friend, had become an open critic of logical empiricist orthodoxy, notwithstanding repeated efforts by members of the Vienna Circle to claim him as an ally (see, for example, Neurath, Hahn, and Carnap et al. 1929).[17] There were many points of disagreement, logical empiricism's antimetaphysical bias prominent among them. Thus, in a letter to Schlick from November 1930, commenting on a manuscript in which Schlick opposed his own neo-Humean view of causality to more robustly metaphysical notions of causality, 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 it 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; as quoted in Howard 1994)

Einstein might well have been puzzled over how his old friend, Schlick, could have strayed so far from the realism central to the argument of *Raum und Zeit in der gegenwärtigen Physik* (Schlick 1917). Einstein had, himself, repeatedly stressed his preference for realism, as when he wrote in the above-cited review of Elsbach:

> Is there not an experiential reality [Erlebnis-Realität] that one encounters directly and that is also, indirectly, the source of that which science designates as real [wirklich]? Moreover, are the realists and, with them, all natural scientists (those who do not philosophize from the very start) not right if they allow themselves to be led by the startling possibility of ordering all experience in a (spatio-temporal-causal) conceptual system to postulate something real that exists independently of their own thought and being? (Einstein 1924, 1685)

A disagreement over metaphysics or realism was not, however, the main issue, or at least surely not the only issue, that led to Einstein's estrangement from logical empiricism. More fundamental, in my view, was the disagreement with Schlick and Reichenbach over the role of convention in scientific theory. They held that the moment of convention is confined to conventional choices of coordinating definitions, the stipulation of which thereby endows each empirical proposition with its own, determinate, empirical content. Einstein, the student of Duhem, held that only whole theories have empirical content and so regarded every component of a scientific theory to be equally conventional and empirical. But can a disagreement over such a recondite question about the semantics of scientific theories really have been the main difference separating Einstein from mainstream logical empiricism?

Recall Einstein's major complaint about Mach. It was not his denying the reality of unobservables, however much that had been an issue in Schlick and Einstein's discussion of the philosophical implications of general relativity when Schlick was writing *Raum und Zeit in der gegenwärtigen Physik* (Schlick 1917). As Einstein, himself, recalled in his "Autobiographical Notes" (Einstein 1946, 48), Mach turned out not to be dogmatic on this point, readily granting the reality of atoms after experimental confirmation ca. 1909 of Einstein's own earlier theoretical work on

Brownian motion. No, what chiefly bothered Einstein was Mach's neglect of the moment of invention or free creation in the development of scientific theories.

That theories are the "free creations of the human intellect" (Einstein 1921, 5), was a recurring theme in Einstein's writings (see also, for example, Einstein 1936, 314). It was always opposed to overly strict empiricist accounts of the relation between concepts or theories and experience: "In general, the weakness of the positivists appears to be that, on their view, the logical independence of concepts with respect to sense experiences does not stand out clearly" (Einstein 1924, 1687). Einstein was not alone in seeing this tension. Philipp Frank saw it as well, contrasting Mach's emphasis upon the empirical legitimation of theories with what he took to be Poincaré's stress on their character as free creations, and credited both Duhem (Frank 1949a, 26) and Einstein (1949b) with having shown us how to reconcile the two by asserting that, since it is only whole theories that possess empirical content, not individual concepts or propositions, the theorist is, therefore, free to make changes anywhere in the total body of theory, as long as the whole theory conforms with empirical fact.

For neither Duhem nor Einstein was the freedom such as to be an obstacle to progress in science. Duhem emphasized that all creative scientific activity takes place in an historical context that constrains, because of contingent historical circumstance, the freedom that the theorist enjoys as a matter of logic alone (Duhem 1906, ch. 7). Einstein contrasted the fact that, in principle, there is no univocal, logically determined path from experience to theory with the nearly universal recognition among scientists, themselves, that, in practice, theory choice seems univocally determined (see Einstein 1918b, 1933). Yet both Einstein and Duhem saw in the fact that the theorist is, thus, in principle, a free epistemic agent as that which secures for theory, as such, its central role in science. Were theoretical concepts eliminable via definitions in terms of phenomenalist protocol terms, or were there, in some other form, an empirical theory choice algorithm, then science could be left to the experimentalist.

That realism was not the main issue in Einstein's dispute with mainstream logical empiricism is evident also from the fact that Einstein was, in fact, rather skeptical about blanket claims of the metaphysical realist kind. Surely the most amusing example of this skepticism is found in a letter of 25 September 1918 from Einstein to the Bonn mathematician, Eduard Study, commenting on Study's book *Die realistische Weltansicht und die Lehre vom Raume* (1914). About Study's defense of realism against both positivists and conventionalists, Einstein wrote:

> "The physical world is real." That is supposed to be the fundamental hypothesis. What does "hypothesis" mean here? For me, a hypothesis is a statement, whose *truth* must be assumed for the moment, *but whose meaning must be raised above all ambiguity*. The above statement appears to me, however, to be, in itself, meaningless, as if one said: "The physical world is cock-a-doodle-doo." It appears to me that the "real" is an intrinsically empty, meaningless category (pigeon hole), whose monstrous importance lies only in the fact that I can do certain things in it and not certain others. This division is, to be sure, not an *arbitrary* one, but instead . . . . . .
>
> I concede that the natural sciences concern the "real," but I am still not a realist. (*CPAE* 8, Doc. 624)

In what sense might science, nonetheless, concern the real on Einstein's view? Arthur Fine has made the insightful suggestion that it was typical of Einstein to "entheorize" a methodological notion like realism, turning it into a physical thesis, more specifically, the thesis of determinism (Fine 1986). But whereas Fine sees Einstein entheorizing realism as the thesis of determinism, I see him entheorizing it as the thesis of spatial or spatio-temporal separability. Consider what he wrote to Max Born on 18 March 1948:

> I just want to explain what I mean when I say that we should try to hold on to physical reality. We are, to be sure, all of us aware of the situation regarding what will turn out to be the basic foundational concepts in physics: the point-mass or the particle is surely not among them; the field, in the Faraday-Maxwell sense, might be, but not with certainty. But that which we conceive as existing ("real") should somehow be localized in time and space. That is, the real in one part of space, *A*, should (in theory) somehow "exist" independently of that which is thought of as real in another part of space, *B*. If a physical system stretches over the parts of space *A and B*, then what is present in *B* should some-how have an existence independent of what is present in *A*. What is actually present in *B* should thus not depend upon the type of measurement carried out in the part of space, *A*; it should also be independent of whether or not, after all, a measurement is made in *A*.
>
> If one adheres to this program, then one can hardly view the quantum-theoretical description as a *complete* representation of the physically real. If one attempts, nevertheless, so to view it, then one must assume that the physically real in *B* undergoes a sudden change because of a measurement in *A*. My physical instincts bristle at that suggestion.
>
> However, if one renounces the assumption that what is present in different parts of space has an independent, real existence, then I do not at all see what physics is supposed to describe. For what is thought to be a "system" is, after all, just conventional, and I do not see how one is supposed to divide up the world objectively so that one can make statements about the parts. (Born 1969, 223-224)

That separability was central to Einstein's conception of physical reality makes sense when it is understood that it was Einstein's commitment to separability that underlay his decades long opposition to quantum mechanics as a preferred framework for fundamental physics, this because quantum entanglement seems hard to reconcile with the assumption that spatial separation is a sufficient condition for the individuation of physical systems.[18]

Conclusion

The development of Einstein's philosophy and the development of logical empiricism were both driven in crucial ways by the quest for an empiricism that could defend the empirical integrity of general relativity in the face of neo-Kantian critiques. But logical empiricism was more than a philosophy of relativity theory, and Einstein's philosophy of science was more than an answer to Kant.

A fuller account of Einstein's philosophy of science would have to include discussion of his belief in simplicity as a guide to truth, especially in areas of physics comparatively far removed from extensive and direct contact with experiment, as in his own long search for a unified field theory (see Howard 1998, Norton 2000, Janssen and Renn 2005, and van Dongen 2002). A fuller account would also investigate Einstein's largely original and, I think, quite profound distinction between "principle theories" and "constructive theories," the former constituted of mid-level, empirically well-grounded generalizations like the light principle and the relativity principle, which, by constraining the search for constructive models, often facilitate progress in science, as Einstein thought was the case in his discovery of special relativity (Einstein 1919; see also Howard 2004a). And a fuller account would examine Einstein's appropriation of what Joseph Petzoldt dubbed "the law of univocalness" (Petzoldt 1895), in effect the requirement that theories determine for themselves unique models of the phenomena they aim to describe, for this idea was central to Einstein's thinking about a permissible space-time event ontology, his solution of the "hole argument" via the "point-coincidence argument" in the genesis of general relativity, and his more general attitude toward physical reality and objectivity (see Howard 1992 and 1999). And partly through its influence on

Einstein, this idea of Petzoldt's also played a significant role in the history of logical empiricism, especially in the development of Carnap's thinking (see Howard 1996).

Still, the struggle to craft a compelling response to neo-Kantian critiques of general relativity was, in my view, the single most important factor shaping the development of the story about empirical content that defined mainstream logical empiricism, and Einstein's central role in this development through his personal and intellectual relationships with logical empiricism's chief architects has yet to receive the attention that it is due in the historical literature. Moreover, understanding Einstein's role in this history helps us to put into context Einstein's late and often quoted characterization of himself as an "epistemological opportunist" (Einstein 1949, 684). Yes, Einstein's philosophy of science borrowed from realism, positivism, idealism, and even Platonism. It might appear to the "systematic epistemologist" to be mere opportunism. But when viewed in its proper historical setting, it emerges as an original synthesis of a profound and coherent philosophy of science that is of continuing relevance today, the unifying thread of which is, from early to late, the assimilation of Duhem's holistic version of conventionalism.

Notes

1. For a helpful survey of philosophical reactions to relativity, see Hentschel 1990.

2. Stadler 1997 is a compendious source of information on all aspects of the history of logical empiricism. On the protocol sentence debate, see Zhai 1900. Howard 2003 provides background on the role of politics in the development of logical empiricism.

3. For a complete list of the readings of the Akademie Olympia, see Einstein 1989, xxiv-xxv.

4. Lange 1873-1875, Dühring 1873, and Rosenberger 1895 are among the many titles preserved in Einstein's personal library, now housed with the Einstein Archive at the Hebrew University and

National Library in Jerusalem.

5. For a fuller discussion of Einstein and Duhem, see Howard 1990a.

6. For helpful discussions of the late-nineteenth and early-twentieth century phenomenon of the "philosopher-physicist," see Stöltzner 2003 and Howard 2004b.

7. That such could not have been Mach's view is evident from his praise for Duhem both in his preface to Adler's translation of Duhem 1906 and in the laudatory comments he added to the 1906 second edition of his *Erkenntnis und Irrtum* (Mach 1906).

8. See Einstein to Schlick, 14 December 1915, EA 21-611, *CPAE* 8, Doc. 165.

9. For more on Einstein's relationship with Schlick, see Howard 1984 and 1994.

10. Hentschel 1990 surveys the variety of Kantian reactions to relativity.

11. See Schlick to Einstein, 9 October 1920, EA 21-580, and Schlick to Reichenbach, 26 November 1920, HR 015-63-22 (Hans Reichenbach Papers, Archive of Scientific Philosophy, University of Pittsburgh).

12. See Ryckman 2005, especially ch. 3, for further helpful discussion of Einstein's objection to Weyl's unified field theory.

13. More or less the same argument, indeed, using almost exactly the same formulations, is repeated in Einstein 1925.

14. Michael Friedman gives a very different reading of the history of verificationism, locating its birth in Einstein's point-coincidence argument (Friedman 1983, 24). For a critical evaluation of Friedman's argument, see Howard 1999.

15. An enduring puzzle is why Einstein so frequently attaches the name of Poincaré to the holistic version of conventionalism that was famously defended by Duhem in explicit opposition to the views of Poincaré, another well-known example of his doing so being Einstein 1921. For further discussion, see Howard 1990a.

16. Quine reports that he had not known of Einstein's quoted remarks from Schilpp 1949 when he wrote "Two Dogmas of Empiricism" (private communication). But given Quine's close connections with Philipp Frank at Harvard in the late 1940s and the latter's deep immersion in Einstein's philosophy of science at that very time (see, e.g., Frank 1947, 1949b, 1949c), one suspects that there is more to the story than Quine, himself, remembered.

17. While Einstein disagreed, as indicated, with came to be regarded as mainstream logical empiricism, his philosophy of science actually had much in common with the views prevalent among the members of what is now dubbed the "left wing" of the Vienna Circle, including Neurath, Hahn, and Frank, all of whom shared with Einstein the experience of being deeply influenced by Duhem. For more on the Vienna Circle's left wing, see Howard 2003.

18. For more on the role of separability in Einstein's critique of quantum mechanics, see Howard 1985, 1990b, and 1997.

References

Ardelt, Rudolf G. (1984). *Friedrich Adler. Probleme einer Persönlichkeitsentwicklung um die Jahrhundertwende*. Vienna: Österreichischer Bundesverlag.

Banks, Erik C. (2003). *Ernst Mach's World Elements. A Study in Natural Philosophy*. Dordrecht: Kluwer.

Barbour, Julian and Pfister, Herbert, eds. (1995). *Mach's Principle: From Newton's Bucket to Quantum Gravity*. Boston: Birkhäuser.

Beller, Mara (2000). "Kant's Impact on Einstein's Thought." In *Einstein: The Formative Years, 1879–1909*. Don Howard and John Stachel, eds. Boston: Birkhäuser, 83–106.

Born, Max, ed. (1969). *Albert Einstein—Hedwig und Max Born. Briefwechsel, 1916-1955*. Munich: Nymphenburger.

Broda, Engelbert (1983). *Ludwig Boltzmann: Man • Physicist • Philosopher*. Larry Gay and Engelbert Broda, trans. Woodbridge, CT: Ox Box Press.

Cassirer, Ernst (1921). *Zur Einsteinschen Relativitätstheorie. Erkenntnistheoretische Betrachtungen*. Berlin: Bruno Cassirer.

Duhem, Pierre (1906). *La Théorie physique, son objet et sa structure*. Paris: Chevalier & Rivière.

——— (1908). *Ziel und Struktur der physikalischen Theorien*. Friedrich Adler, trans. Foreword by Ernst Mach. Leipzig: Johann Ambrosius Barth.

Dühring, Eugen (1873). *Kritische Geschichte der allgemeinen Principien der Mechanik*. Berlin: Theobald Grieben.

Einstein, Albert (1910-1911). "Lecture Notes for Course on Electricity and Magnetism, University of Zurich, Winter Semester 1910-1911." *CPAE* 3, Doc. 3.

——— (1916). "Ernst Mach." *Physikalische Zeitschrift* 17, 101–104. *CPAE* 6, Doc. 29.

——— (1918a). "Motive des Forschens." In *Zu Max Plancks sechzigstem Geburtstag. Ansprachen, gehalten am 26. April 1918 in der Deutschen Physikalischen Gesellschaft*. C. F. Müller, Karlsruhe, 29-32. *CPAE* 7, Doc. 7.

——— (1918b). "Nachtrag." *Königlich Preußische Akademie der Wissenschaften* (Berlin). *Sitzungsberichte*, 478. *CPAE* 7, Doc. 8.

——— (1918c). Review of Weyl 1918. *Die Naturwissenschaften* 6, 373. *CPAE* 7, Doc. 10.

——— (1919). "Time, Space, and Gravitation." *Times* (London). 28 November 1919, 13–14. *CPAE* 7, Doc. 26.

——— (1921). *Geometrie und Erfahrung. Erweiterte Fassung des Festvortrages gehalten an der Preussischen Akademie der Wissenschaften zu Berlin am 27. Januar 1921.* Berlin: Julius Springer. *CPAE* 7, Doc. 52.

——— (1924). Review of Elsbach 1924. *Deutsche Literaturzeitung* 45: 1685-1692.

——— (1925). "Nichteuklidische Geometrie und Physik." *Die Neue Rundschau*. 1, 16-20.

——— (1933). "Zur Methode der theoretischen Physik." The Herbert Spencer Lecture, delivered at Oxford University, 10 June 1933. In *Mein Weltbild.* Amsterdam: Querido Verlag, 1934, 176-187.

——— (1936). "Physik und Realität." *Journal of The Franklin Institute* 221, 313-347.

——— (1946). "Autobiographical Notes." In Schilpp 1949, 1-94.

——— (1949). "Remarks Concerning the Essays Brought together in this Co-operative Volume." In Schilpp 1949, 665–688.

——— (1989). *The Collected Papers of Albert Einstein*. Vol. 2, *The Swiss Years: Writings, 1900-1909.* John Stachel et al., eds. Princeton, NJ: Princeton University Press.

Elsbach, Alfred (1924). *Kant und Einstein. Untersuchungen über das Verhältnis der modernen Erkenntnistheorie zur Relativitätstheorie*. Berlin and Leipzig: Walter de Gruyter.

Enz, Charles P. (2002). *No Time to Be Brief: A Scientific Biography of Wolfgang Pauli*. Oxford and New York: Oxford University Press.

Fine, Arthur (1986). "Einstein's Realism." In *The Shaky Game: Einstein, Realism, and the Quantum Theory*. Chicago: University of Chicago Press, 86-111.

Frank, Philipp (1907). "Kausalgesetz und Erfahrung." *Annalen der Naturphilosophie* 6, 443-450.

——— (1947). *Einstein: His Life and Times*. New York: Alfred A. Knopf, 1947.

——— (1949a). "Historical Background." In *Modern Science and Its Philosophy*. Cambridge, MA: Harvard University Press, 1–52.

——— (1949b). "Einstein's Philosophy of Science." *Reviews of Modern Physics* 21 (1949), 349-355.

——— (1949c). "Einstein, Mach, and Logical Positivism." In Schilpp 1949, 271-286.

Friedman, Michael (1983). *Foundations of Space-time Theories: Relativistic Physics and Philosophy of Science.* Princeton, NJ: Princeton University Press.

Haller, Rudolf (1985). "Der erste Wiener Kreis." *Erkenntnis* 22: 341–358.

Hecht, Hartmut and Hoffmann, Dieter (1982). "Die Berufung Hans Reichenbachs an die Berliner Universität." *Deutsche Zeitschrift für Philosophie* 30: 651-662.

Heilbron, John L. (1986). *The Dilemmas of an Upright Man: Max Planck as Spokesman for German Science*. Berkeley: University of California Press.

Hentschel, Klaus (1990). *Interpretationen und Fehlinterpretationen der speziellen und der allgemeinen Relativitätstheorie durch Zeitgenossen Albert Einsteins*. Basel and Boston: Birkhäuser.

Hoefer, Carl (1995). "Einstein's Formulations of Mach's Principle." In Barbour and Pfister 1995, 67-90.

Holton, Gerald (1968). "Mach, Einstein, and the Search for Reality." *Daedalus* 97, 636-673.

Howard, Don (1984). "Realism and Conventionalism in Einstein's Philosophy of Science: The Einstein-Schlick Correspondence." *Philosophia Naturalis* 21, 616-629.

——— (1985). "Einstein on Locality and Separability." *Studies in History and Philosophy of Science* 16, 171–201.

——— (1990a). "Einstein and Duhem." *Synthese* 83: 363–384.

——— (1990b). "'Nicht sein kann was nicht sein darf,' or the Prehistory of EPR, 1909-1935: Einstein's Early Worries about the Quantum Mechanics of Composite Systems." In *Sixty-two Years of Uncertainty: Historical, Philosophical, and Physical Inquiries into the Foundations of Quantum Mechanics.* Arthur I. Miller, ed. New York and London: Plenum, 61-111.

——— (1992). "Einstein and *Eindeutigkeit*: A Neglected Theme in the Philosophical Background to General Relativity." In Jean Eisenstaedt and A. J. Kox, eds. *Studies in the History of General Relativity.* Boston: Birkhäuser, 154–243.

——— (1993). "Was Einstein Really a Realist?" *Perspectives on Science: Historical, Philosophical, Social* 1, 204-251.

——— (1994). "Einstein, Kant, and the Origins of Logical Empiricism." In *Language, Logic, and the Structure of Scientific Theories*. Wesley Salmon and Gereon Wolters, eds. Pittsburgh: University of Pittsburgh Press; Konstanz: Universitätsverlag, 45-105.

——— (1996). "Relativity, *Eindeutigkeit,* and Monomorphism: Rudolf Carnap and the Development of the Categoricity Concept in Formal Semantics." In *Origins of Logical Empiricism*. Ronald N. Giere and Alan Richardson, eds. Minneapolis and London: University of Minnesota Press, 115–164.

——— (1997). "A Peek behind the Veil of Maya: Einstein, Schopenhauer, and the Historical Background of the Conception of Space as a Ground for the Individuation of Physical Systems." In *The Cosmos of Science: Essays of Exploration*. John Earman and John D. Norton, eds. Pittsburgh: University of Pittsburgh Press; Konstanz: Universitätsverlag, 87-150.

——— (1998). "Astride the Divided Line: Platonism, Empiricism, and Einstein's Epistemological Opportunism." In *Idealization in Contemporary Physics*. Niall Shanks, ed. Amsterdam and Atlanta: Rodopi, 143-163.

——— (1999). "Point Coincidences and Pointer Coincidences: Einstein on Invariant Structure in Spacetime Theories." In *History of General Relativity IV: The Expanding Worlds of General Relativity*. Hubert Goenner, Jürgen Renn, Jim Ritter, and Tilman Sauer, eds. Boston: Birkhäuser, 463–500.

——— (2003). "Two Left Turns Make a Right: On the Curious Political Career of North American Philosophy of Science at Mid-century." In *Logical Empiricism in North America*. Alan Richardson and Gary Hardcastle, eds. Mnnneapolis: University of Minnesota Press, 25-93.

——— (2004a). "Einstein's Philosophy of Science." *The Stanford Encyclopedia of Philosophy (Spring 2004 Edition)*, Edward N. Zalta, ed. URL = <http://plato.stanford.edu/archives/-spr2004/entries/einstein-philscience/>.

——— (2004b). "Fisica e filosofia della scienza all'alba del XX secolo." *Enciclopedia Italiana di Storia della Scienza*. Vol. 8, *La Seconda Rivoluzione Scientifica*. (Forthcoming.)

——— (2004c). "Einstein and Frank." Paper Presented at the symposium, "Philipp Frank: Wien • Prag • Boston," 27-28 September 2004, Vienna, Austria.

——— (2004d). "'Let me briefly indicate why I do not find this standpoint natural.' Einstein, General Relativity, and the Contingent A Priori." Paper presented at the conference, "Synthesis and the Growth of Knowledge," October 1-3, 2004, University of South Carolina.

Janssen, Michel and Renn, Jürgen (2005). "Untying the Knot: How Einstein Found His Way Back to Field Equations Discarded in the Zurich Notebook." In *The Genesis of General Relativity. Sources and Interpretations*. Vols. 1 and 2, *General Relativity in the Making: Einstein's Zurich Notebook*. Dordrecht: Kluwer (forthcoming).

Klein, Martin J. (1970). *Paul Ehrenfest*. New York: American Elsevier.

Kraft, Victor (1950). *Der Wiener Kreis. Der Ursprung des Neopositivismus. Ein Kapitel der jüngsten Philosophiegeschichte*. Vienna: Springer.

Krüger, Lorenz, ed. (1994). *Universalgenie Helmholtz. Rückblick nach 100 Jahren*. Berlin: Akademie Verlag.

Lange, Friedrich Albert (1873-1875). *Geschichte des Materialismus und Kritik seiner Bedeutung in der Gegenwart*. 2nd ed. Iserlohn and Leipzig: J. Baedeker.

Mach, Ernst (1883). *Die Mechanik in ihrer Entwickelung historisch-kritisch dargestellt*. Leipzig: Brockhaus.

——— (1896). *Die Principien der Wärmelehre. Historisch-kritisch entwickelt*. Leipzig: Johann Ambrosius Barth.

——— (1906). *Erkenntnis und Irrtum. Skizzen zur Psychologie der Forschung*, 2nd ed. Leipzig: Johann Ambrosius Barth.

——— (1910). "Die Leitgedanken meiner naturwissenschaftlichen Erkenntnislehre und ihre Aufnahme durch die Zeitgenossen." *Scientia* 7, 2ff. Reprinted in *Physikalische Zeitschrift* 11 (1910), 599-606.

Moore, Walter (1994). *A Life of Erwin Schrödinger*. Cambridge and New York: Cambridge University Press.

Nagel, Ernest (1950). "Einstein's Philosophy of Science." *The Kenyan review* 12 (Summer 1950). Review of Schilpp 1949. Reprinted in *Logic Without Metaphysics and Other Essays in the Philosophy of Science*. Glencoe, IL: The Free Press, 1956, 287-301.

Natorp, Paul (1910). *Die logischen Grundlagen der exakten Wissenschaften*. Leipzig and Berlin: B. G. Teubner.

Neurath, Otto; Hahn, Hans; and Carnap, Rudolf (1929). *Wissenschaftliche Weltauffassung: Der Wiener Kreis*. Vienna: Artur Wolf.

Norton, John (1995). "Mach's Principle before Einstein." In Barbour and Pfister 1995, 9-57.

——— (2000). "'Nature is the Realisation of the Simplest Conceivable Mathematical Ideas': Einstein and the Canon of Mathematical Simplicity." *Studies in History and Philosophy of Modern Physics* 31B, 135-170.

——— (2004). "How Hume and Mach Helped Einstein Find Special Relativity." Paper presented at the conference, "Synthesis and the Growth of Knowledge," October 1-3, 2004, University of South Carolina.

Petzoldt, Joseph (1895). "Das Gesetz der Eindeutigkeit." *Vierteljahrsschrift für wissenschaftliche Philosophie und Soziologie* 19, 146-203.

Planck, Max (1909). "Die Einheit des physikalischen Weltbildes." *Physikalische Zeitschrift* 10, 62-75.

——— (1910). "Zur Machschen Theorie der physikalischen Erkenntnis. Eine Erwiderung." *Physikalische Zeitschrift* 11, 1186-1190.

Poincaré, Henri (1902). "L'expérience et la géométrie." In *La Science et l'hypothèse*. Paris: Ernest Flammarion, 92-109.

Quine, Willard Van Orman (1951). "Two Dogmas of Empiricism." *Philosophical Review* 60, 29-43.

Reichenbach, Hans (1920). *Relativitätstheorie und Erkenntnis Apriori*. Berlin: Julius Springer.

——— (1924). *Axiomatik der relativistischen Raum-Zeit-Lehre*. Braunschweig: Friedrich Vieweg und Sohn.

——— (1928). *Philosophie der Raum-Zeit-Lehre*. Berlin and Leipzig: Walter de Gruyter.

——— (1949). "The Philosophical Significance of the Theory of Relativity." In Schilpp 1949, 289-311.

——— (1951). *The Rise of Scientific Philosophy*. Berkeley and Los Angeles: University of California Press.

Rosenberger, Ferdinand (1895). *Isaac Newton und seine physikalischen Prinzipien. Ein Hauptstück aus der Entwickelungsgeschichte der modernen Physik*. Leipzig: Johann Ambrosius Barth.

Ryckman, Thomas (2005). *The Reign of Relativity*. New York: Oxford University Press.

Schilpp, Paul Arthur, ed. (1949). *Albert Einstein: Philosopher-Scientist* Evanston, IL: The Library of Living Philosophers..

Schlick, Moritz (1910). "Das Wesen der Wahrheit nach der modernen Logik." *Vierteljarhsschrift für wissenschaftliche Philosophie und Soziologie* 34, 386-477.

——— (1915). "Die philosophische Bedeutung des Relativitätsprinzips." *Zeitschrift für Philosophie und philosophische Kritik* 159, 129-175.

——— (1917). *Raum und Zeit in der gegenwärtigen Physik. Zur Einführung in das Verständnis der allgemeinen Relativitätstheorie*. Berlin: Julius Springer.

——— (1918). *Allgemeine Erkenntnislehre*. Berlin: Julius Springer.

——— (1921). "Kritizistische oder empiristische Deutung der neuen Physik." *Kant-Studien* 26, 96-111.

——— (1925). *Allgemeine Erkenntnislehre*, 2nd ed. Berlin: Julius Springer.

——— (1935). “Sind die Naturgesetze Konventionen?” In *Actes du Congrès International de Philosophie Scientifique, Paris 1935*. Vol. 4, *Induction et Probabilité*. Paris: Hermann, 1936, 8-17.

Solovine, Maurice, ed. (1956). *Albert Einstein. Lettres à Maurice Solovine*. Paris: Gauthier-Villars,

Speziali, Pierre, ed. (1972). *Albert Einstein-Michele Besso. Correspondance 1903-1955*. Paris: Hermann.

Stadler, Friedrich (1997). *Studien zum Wiener Kreis: Ursprung, Entwicklung, und Wirkung des logischen Empirismus im Kontext*. Frankfurt am Main: Suhrkamp.

Stöltzner Michael (2003). “Vienna Indeterminism: Causality, Realism, and the Two Strands of Boltzmann’s Legacy (1896-1936).” Ph.D. Dissertation. University of Bielefeld.

Strauss, David Friedrich (1835-1836). *Das Leben Jesu*, 2 vols. Tübingen: C.F. Osiander.

Study, Eduard (1914). *Die realistische Weltansicht und die Lehre vom Raume*. Braunschweig: Friedrich Vieweg & Sohn.

van Dongen, Jeroen (2002). *Einstein’s Unification: General Relativity and the Quest for Mathematical Naturalness*. Ph.D. Dissertation. University of Amsterdam.

von Mises, Richard (1938). *Ernst Mach und die empiristische Weltauffassung*. ‘s-Gravenhage, The Netherlands: W.P. van Stockum & Zoon.

——— (1939). *Kleines Lehrbuch des Positivismus. Einführing in die empiristische Wissenschaftsauffassung*. The Hague, W. P. van Stockum & Son.

Weyl, Hermann (1918a). "Gravitation und Elektrizität." *Königlich Preußische Akademie der Wissenschaften* (Berlin). *Sitzungsberichte*, 465-478, 478-480.

——— (1918b). *Raum. Zeit. Materie. Vorlesungen über allgemeine Relativitätstheorie*. Berlin: Springer.

——— (1927). *Philosophie der Mathematik und Naturwissenschaft.* Munich and Berlin: R. Oldenbourg.

Zhai, Zhenming (1990). "The Problem of Protocol Statements and Schlick's Concept of 'Konstatierungen.'" *PSA 1990: Proceedings of the 1990 Biennial Meeting of the Philosophy of Science Association*, vol. 1. Arthur Fine, Micky Forbes, and Linda Wessels, eds. East Lansing, MI: Philosophy of Science Assn., 15-23.

The Cambridge Companion to Einstein. Einstein and the Development of Twentieth-Century The Cambridge Companion to Einstein. The Cambridge Companion to Einstein. Chapter.Â Stevens, Syman 2015. The Dynamical Approach as Practical Geometry. Philosophy of Science, Vol. 82, Issue. 5, p. 1152. CrossRef.