Physics and the Philosophy of Science at the Turn of the Twentieth Century

(Forthcoming in the *Enciclopedia Italiana di Storia della Scienza* under the title, "Fisica e Filosofia della Scienza all'Alba del XX Secolo")

> I believe that philosophy can be helped to its feet again only if it devotes itself seriously and fervently to investigations of cognitive processes and the methods of science. There it has a real and legitimate task . . . . Philosophy has obviously come to a standstill because it . . . still has taken no new life from the vigorous development of the natural sciences.
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> — Hermann von Helmholtz to Adolf Fick, ca. 1875
> (as quoted in Koenigsberger 1902–1903, 243)

Introduction: Disciplinary Symbiosis

Theoretical physics and the philosophy of science are among the most important fields of research in the twentieth century, this as gauged both by their prominence within their respective disciplines and by their broader social and intellectual impact. Yet in 1850 neither field, as we know it today, would have been recognized in the academy or elsewhere as constituting an autonomous mode of inquiry with associated institutional structures. With hindsight, each might be glimpsed in germ. Some would read Hermann von Helmholtz's 1847 lecture, *Über die Erhaltung der Kraft* (Helmholtz 1848) as marking the advent of the search for generalizable explanatory structures whose deployment is a distinguishing mark of theoretical physics. Some would read Auguste Comte's *Cours de philosophie positive* (1830–1842) or William Whewell's *The Philosophy of the Inductive Sciences* (1840) as inaugurating the systematic study of those general questions about scientific method, the nature and limits of scientific knowledge, and the structure and interpretation of scientific theories whose focal significance later defined the field in the form made famous by the members of the Vienna Circle. But even the most astute observer in 1850 would likely not have recognized in these few books and papers the birth of new fields of inquiry that would reshape their parent disciplines. How different from that of the twentieth century was and remained the disciplinary landscape until well into the later nineteenth century is evidenced by the fact that as late as the turn of the century the standard, beginning, university-level textbook on analytical mechanics in the English-speaking world bore the title *Treatise on Natural Philosophy* (Thomson and Tait 1879–1903).

By 1935, thanks to the rise of relativity theory and quantum mechanics, theoretical physics was recognized has having wrought the most profound and pervasive change in our understanding of nature since at least the century of Galileo and Newton, and the philosophy of science had become in centers like Vienna and Berlin a self-assertive and exciting new field drawing scholars from afar and promising to export the benefits of a scientific way of knowing modeled mainly upon theoretical physics to many other disciplines and other cultural domains. The history of late-nineteenth and early-twentieth century theoretical physics is a subject long and thoroughly studied.[1] The history of the philosophy of science during the same period is only of late receiving comparable attention.[2] The history of the connection between these other two histories has received surprisingly scant attention— a deficit surprising because of the crucial importance of each to the other—and, so, will receive our main attention in what follows.

Symbiosis is an apt metaphor for the manner in which, from roughly 1850 to 1935, theoretical physics and the philosophy of science nurtured one another's growth. Physics provided the philosopher both a subject for analysis and a model of scientific cognition. Philosophy gave the physical theorist legitimation by defending the formal and empirical integrity of physical theory in the face of doubts expressed from the side of the experimentalists. In manifold ways, the two fields grew in tandem. Theoretical developments in physics, most notably in relativity theory, drove a rethinking of the relationship between the a priori and the contingent, empirical elements in scientific cognition. Philosophical critique deepened and refined the foundations of physical theory especially as regards basic concepts such as space, time, and causality. New journals served both communities, students moved back and forth between fields, new chairs and institutes were established in collaboration, and new professional organizations drew membership from both disciplines.

A full history of this symbiosis would explore relations between the philosophy of science and not only theoretical physics, but also other scientific fields then sprouting a more self-consciously theoretical branch. Physical chemistry is one such field and will be discussed briefly below. Theoretical biology is another, but will not receive separate attention here, even though its filiations with physics, especially in areas like molecular genetics, are important and themselves intertwined with developments in the philosophy of science.[3] Psychology, too, grew a philosophy of science as, ironically, it sought to distinguish itself from philosophy in the disciplinary structure of the academy.[4] Still, it was theoretical physics whose growing significance made the greatest difference in the way philosophers theorized science.

Hermann von Helmholtz, the Return to Kant, and the Birth of Scientific Philosophy

In 1922, the Berlin critical realist and neo-Kantian, Alois Riehl, dubbed the previous half century “the epoch of scientific philosophy” (Riehl 1922, 224), a period distinguished by self-conscious, critical reflection on the nature and limits of scientific knowledge. Riehl, like others, accorded Hermann von Helmholtz (1821–1894) a leading role in promoting scientific philosophy, many dating the movement’s inception to Helmholtz’s 1855 *Ehrenrede* for Kant (Helmholtz 1855; see Köhnke 1986, 151–157). How Helmholtz, himself, understood the respective roles of science and philosophy in this synthesis was explained in his 1878 Berlin *Rektoratsrede*:

> The fundamental problem which that age placed at the beginning of all science was that of epistemology: “What is truth in our intuition and thought? In what sense do our representations correspond to reality?” Philosophy and natural science encounter this problem from two opposed sides; it is the common task of both. The former, which considers the mental side of the problem, seeks to separate out from our knowledge and representation what originates in the influences of the corporeal world, in order to set forth unalloyed what appertains to the mind’s own activity. By contrast, natural science seeks to separate off whatever is definition, symbolism, representational form, or hypothesis, in order to retain unalloyed what appertains to the world of reality, whose laws it seeks. Both seek to accomplish the same separation, even if each is interested in a different part of what is separated. In the theory of sense perceptions, and in investigations into the fundamental principles of geometry, mechanics, and physics, the natural scientist, too, cannot evade these questions. (Helmholtz 1878, 218)

Helmholtz’s own philosophy was an idiosyncratic version of Kantianism, wherein physiological findings—his teacher Johannes Müller’s law of specific energies—are adduced as evidence for the subject’s active role in cognition, and causality is still regarded as a priori, but not the specific metrical properties of space. Helmholtz proffers a transcendental argument for the postulate that spatial congruence is preserved under arbitrary continuous spatial translations and rotations—the postulate of free mobility—spatial measurements being held impossible were our measuring rods not to retain their length while moved about in space. That spatial geometry is thereby constrained to be a geometry of constant curvature is, thus, a necessary a priori judgment, but the choice of a specific spatial metric rests upon both empirical and conventional considerations (see Helmholtz 1868, 1870).

Helmholtz’s views on the epistemic status of geometry became an important part of the background for later debates about the epistemology of geometry following the introduction of the general theory of relativity (see Friedman 2002). He also played an important role in promoting reflection on the epistemic status of fundamental physical principles such as the conservation of energy, a topic of widespread interest at the end of the nineteenth century (see Giedymin 1982).

Helmholtz's most significant contribution to the philosophy of science was, however, simply his public championing of scientific philosophy in the form of critical reflection on the scope and limits of scientific knowledge and his exemplifying the same knowledge-critical perspective in his own scientific work. Some of the most profoundly philosophical physicists of the next generation, most notably Heinrich Hertz (1857–1894) and Max Planck (1858–1947), were, themselves, students of Helmholtz, and he also attracted to his lectures young philosophers-to-be, such as Friedrich Albert Lange (1828–1875; see Köhnke 1986, 152), Hermann Cohen (1842–1918), and August Stadler (1850–1910), who was Cohen's first doctoral student and later the teacher from whom Albert Einstein learned about Kant and the philosophy of science at the ETH in Zurich (Beller 2000).

Helmholtz was not alone in turning "back to Kant" for philosophical guidance. "Zurück zu Kant" (Liebmann 1865) was the rallying cry under which a much broader and influential neo-Kantian movement developed in the later nineteenth century. Neo-Kantianism came in many varieties, but three stand out by virtue of their especially close ties to scientific philosophy: (1) the critical idealist Marburg school of Hermann Cohen, Paul Natorp (1854–1924), and Ernst Cassirer (1874–1945); (2) the Berlin critical realist school of Alois Riehl (1824–1944); and (3) the "Philosophie des als ob" ["philosophy of 'as-if'] of Hans Vaihinger (1852–1933) in Halle.

Vaihinger was important less for his once widely-read *Die Philosophie des als ob* (1911) than for his work as founding editor of the *Kant-Studien* (1896) and the short-lived but engaging *Annalen der Philosophie und philosophischen Kritik* (1919–1929), which latter was taken over in 1929 by the Vienna Circle and turned into the journal *Erkenntnis*, thereafter the main voice of logical empiricism. Both of Vaihinger's journals devoted considerable space to philosophical engagement with the best science of the day, as with the publication of Rudolf Carnap's doctoral dissertation, *Der Raum* (1921), as a supplementary issue of the *Kant-Studien*. From his influential position in Berlin, Riehl promoted among his many students his view of philosophy as "Wissenschaftslehre" (Riehl 1876), and after 1915 he encouraged a number of them to begin serious study of Einstein's general theory of relativity for the purpose of assessing its implications for the epistemic status of metrical geometry (see, for example, Sellien 1919 and Schneider 1921).

The Marburg school was, however, the dominant Kantian voice in late-nineteenth and early-twentieth century scientific philosophy, this in no small measure because of the comparative technical sophistication that it brought to the task, a trait already in evidence in Cohen's early *Das Princip der Infinitesimal-Methode* (1883). The central philosophical task that the school set itself in such works as Cohen's *Kants Theorie der Erfahrung* (1871) was to free the Kantian project of its dependence on a problematic and vulnerable doctrine of intuition by finding purely conceptual means whereby to effect the univocal contact with the world in its particularity that was, for Kant, the distinguishing responsibility of intuition. Their idea, perhaps most clearly and powerfully expressed in Cassirer's *Substanzbegriff und Funktionsbegriff* (1910), was that through the accumulation of sufficiently many conceptual determinations one might constrain the class of possible objects of cognition up to the point of uniqueness or, failing that, isomorphism. This ambition was to be frustrated by the ever clearer appreciation in the late 1920s and early 1930s of the pervasiveness of non-categoricity in formal theories, any formal theory as powerful as or more powerful than Peano arithmetic (in first-order formulation) necessarily admitting, as Gödel demonstrated in a corollary to his first incompleteness theorem, non-isomorphic models (see Howard 1992). But throughout the first two decades of the twentieth century the pursuit of the Marburg program yielded a rich harvest of insights into contemporary physical theory, in works such as Natorp's *Die logischen Grundlagen der exakten Wissenschaften* (1910) and Cassirer's *Zur Einsteinschen Relativitätstheorie* (1921), and their work set the stage for a portentous rethinking of the nature of the a priori in the form of the introduction of the notion of the contingent or relativized a priori in Hans Reichenbach's *Relativitätstheorie und Erkenntnis Apriori* (1920).[5]

Positivism, Energetics, and the Reality of Atoms

No field of physics was a scene of more protracted and intense philosophical debate in the late nineteenth century than was the theory of heat.[6] The question was whether macroscopic thermodynamics could be given a complete and consistent molecular-kinetic interpretation, a question that implicated, in turn, the question of the ontic status of the putative molecular and atomic constituents of macroscopic thermodynamical systems. Technical questions about the explanatory achievements of kinetic theory and statistical mechanics became entangled with philosophical questions about the circumstances under which it was reasonable to postulate subvisible physical structure. Two famous debates concerning the reality of atoms epitomized the conflict, one between Wilhelm Ostwald (1853–1932) and Ludwig Boltzmann (1844–1906), the other between Planck and Ernst Mach (1838–1916).

Wilhelm Ostwald was a dominating figure in the physical sciences at the turn of the century. The most prominent physical chemist of his day, author of many of the most widely used inorganic and analytical chemistry textbooks, and a well-known spokesperson for the monist movement (Ostwald 1911), Ostwald was also the founder and editor of journals such as the *Zeitschrift für den physikalischen und chemischen Unterricht* (1887) and *Ostwalds Annalen der Naturphilosophie* (1903–1912), which were venues in which scientists and philosophers regularly interacted, as well as the highly successful book series *Ostwalds Klassiker der exakten Wissenschaften* (1889), which republished works of Kant alongside those of scientists like Kepler, Gauss, and Maxwell.[7]

Ostwald was well known, along with his ally, Georg Helm (1851–1923) as a defender of energetics, a point of view according to which not only the theory of heat but all of physical theory can be developed as the study of energy and its modes of transformation, abstaining from any assumptions about the microstructure of systems thus described and implying a profound sketpicism about atomism (see Helm 1898). Though Boltzmann had by the mid-1890s brought the program of statistical mechanics close to the goal of providing a molecular-kinetic grounding of macroscopic thermodynamics (see Broda 1983 and Cercignani 1998), and though Planck, himself at the time no friend of atomism, was soon to deliver what later was appreciated as a telling critique of energetics (Planck 1896), the energeticist program was still thriving at the time of a famous encounter between Boltzmann and Helm at the 1895 *Naturforscherversammlung* in Lübeck. Powerful philosophical voices, such as that of Mach, joined a worry about the epistemic status of unobservable atoms to more purely technical doubts about explanatory failures of the atomic hypothesis, prominent among which was the ever more embarrassing problem of anomalous specific heats.

The encounter in Lübeck elicited from Boltzmann a series of papers providing a thoughtful philosophical defense of atomism (see, for example, Boltzmann 1896a, 1896b, and 1897). One should not be too quick, however, to find here an anticipation of later-twentieth century philosophical debates over realism and antirealism, in part because the issue is as much one local to kinetic theory and thermodynamics as it is a general issue about the interpretation of scientific theories, but also because the arguments adduced by Boltzmann are not exactly those central to that later debate. To be sure, Boltzmann appeals to the explanatory potential of the atomic hypothesis, as would any sensible physicist. But he also deploys the model-theoretic view of knowledge that was shortly before famously defended by Hertz in the Introduction to his *Prinzipien der Mechanik* (1894) and even more importantly, for Boltzmann, employed by James Clerk Maxwell (1831–1879) in his works on electrodynamics and kinetic theory. Boltzmann knew this side of Maxwell's thinking quite well, thanks to his work in editing and annotating for the *Ostwalds Klassiker* series two of Maxwell's classic essays, "On Faraday's Lines of Force" (1895) and "On Physical Lines of Force" (1898), in the notes for which Boltzmann stresses Maxwell's views on models.

In Maxwell's thinking, the issue arises mainly in the context of debates about mechanical models for the electromagnetic ether, and for many of his ideas about the role of such models in science Maxwell was deeply indebted to the Scots "common sense" tradition, the philosophy that he had learned as a young student in Ediburgh (see Olson 1975). While stressing the heuristic and psychological significance of constructing models or pictures of underlying physical structure as something essential to a clear, scientific understanding of the phenomena under investigation, Maxwell went out of his way to caution against premature assertions of the physical reality of models, even going so far as to argue that there is sometimes a benefit in employing mutually incompatible models to illuminate different aspects of the phenomena. Maxwell placed special emphasis on the point that the mere explanatory success of a model did not license an inference to the truth of that model.

Hertz was the best-known proponent of the model-theoretic view of scientific knowledge. The major influence on his thinking in this respect was his teacher, Helmholtz, who espoused a "semantic" view of knowledge as nothing more than the unambiguous functional coordination between things in the mind and things in the world. Helmholtz meant, thereby, to oppose the view that there is any kind of pictorial resemblance between ideas and that which they represent (for which reason "picture" is a seriously misleading if common translation of the German word "Bild" that Hertz, Boltzmann, and others employ). In Hertz's words:

> We form for ourselves internal models [Scheinbilder = (literally) phantasms] or symbols of external objects, and the form that we give them is such that the necessary consequents in thought of the models [Bilder] are always the models of the necessary consequents in nature of the objects thus modeled. (Hertz 1894, 1)

For Hertz, predictive or explanatory success implies some measure of conformity between thought and world but only in the one respect just specified, from which it follows that there can be more than one successful way of modeling the phenomena, choice among the alternatives being based, in the end, on simplicity, which means a minimum of superfluous elements in the model.

In a 1902 article on "Model" for the *Encyclopedia Britannica*, Boltzmann cited Maxwell, Helmholtz, Hertz, and Mach as sources for the view just sketched, which Boltzmann described thusly:

> On this view our thoughts stand to things in the same relation as models to the objects they represent. The essence of the process is the attachment of one concept having a definite content to each thing, but without implying complete similarity between thing and thought. . . . What resemblance there is lies principally in the nature of the connexion, the correlation being analogous to that which obtains between thought and language, language and writing, the notes on the stave and musical sounds, &c. (Boltzmann 1902).

What is implied for the ontic status of models? Boltzmann explains that while, formerly, one took the success of a model as evidence for the high probability of the actual existence of the mechanisms featured in the model, "nowadays philosophers postulate no more than a partial resemblance between the phenomena visible in such mechanisms and those which appear in nature" (Boltzmann 1902).

Boltzmann was a realist about atoms, but his was a modest and nuanced realism, as his mention of Mach as a proponent of a similar view of scientific knowledge should make clear. That the realist Boltzmann so cites Mach also suggests that it might also be wrong to read Mach as a straightforward anti-realist, however much propagandists on behalf of the Vienna Circle later strove to interpret him that way and thereby claim the mantle of his authority for their much sterner strictures on the scientist's employment of terms putatively referring to unobservable entities (see, for example, von Mises 1938 and Kraft 1950). Indeed, Boltzmann was Mach's successor in Vienna in 1902, and many of their students and contemporaries took them to be engaged in a common philosophical project, namely, achieving the maximum clarity about the epistemic credentials and ontic implications of the best contemporary scientific theory (see Stadler 1997).

It was not long after the start of his physics career that Mach's interests began to broaden in the direction of the history and philosophy of science. An early book on the history of the energy conservation principle (Mach 1872) was followed by a series of masterful works including his *Mechanik* (1883), *Analyse der Empfindungen* (1886), *Wärmelehre* (1896), and *Erkenntnis und Irrtum* (1905). Mach became in 1896 the first occupant of the first academic chair explicitly devoted to the philosophy of science, the chair for Geschichte und Theorie der induktiven Wissenschaften in Vienna. His major writings went through many editions and were translated into many languages, he was a member of Austrian parliament, and he became a figure of such broad public influence, well beyond the narrow confines of physics and the philosophy of science, as even to become the principal target of one of Lenin's major works, his *Materialism and Empirio-Criticism* (Lenin 1909).[8]

The core of Mach's philosophy is his doctrine of the "elements." Sometimes unhelpfully styled the "elements of sensation," Mach's elements are, in fact, neutral, neither in the mind nor in the world, the innner-outer distinction itself being a construction within the realm of the elements. Whether specific objects constructed out of the elements are to be assigned to the physical or mental realm depends exclusively on the pattern of functional relations between those objects and other elements. Mach's doctrine of the elements is, thus, a version of what has since come to be known as neutral monism.

Many have read Mach's philosophy as a successor to the empiricism of Berkeley and Hume, his main philosophical argument being, on this analysis, that the introduction of scientific concepts is warranted only if the manner of their construction out of the elements can be exhibited clearly (see, for example, Popper 1953). On this view the elements play a privileged role for epistemic reasons, because of the warrant accompanying our unmediated cognitive contact with such basic units of experience. Opposed to this phenomenalist, epistemic reading of Mach's doctrine of the elements is a genetic one whereupon the point is less to stipulate criteria of admissibility for scientific concepts and more simply to understand the manner in which such concepts arise in cognition. On this view, the elements are prized for psychological (or even biological) reasons, because of the basic functional role that they play in cognition.

That the genetic reading brings us closer to Mach's own understanding of his program is indicated by the fact that Mach preferred to describe his as a "biological-economical" point of view. It is biological because it takes seriously the lessons of Darwin for the natural origins of human cognitive capacities, capacities to be evaluated, therefore, partly from the point of view of the advantages they confer in the struggle for survival. It is economical because it sees one of the chief biological advantages of the introduction of concepts to lie in the economies of thought and effort that they afford. The genetic reading is reinforced by Mach's historical works, in all of which he brought to bear techniques then well known from the Biblical hermeneutics literature to effect a historical-critical analysis of central scientific concepts, like the Newtonian concepts of absolute space and time. By thus historicizing scientific concepts, Mach hoped to exhibit the contingent grounds of their previous successful employment and thereby open the question of whether even once important concepts like absolute space and time are still suited to perform comparable work in a changed problematic setting. Precisely here was to be found Mach's chief legacy to the science of the next generation, a point clearly understood and stressed by one of Mach's most important philosophical legatees, Einstein (see Einstein 1916).

More than anyone else, Planck is to be credited with cementing in the public mind the view that Mach's philosophy of science was a kind of science-unfriendly anti-realism. Like his teacher, Helmholtz, Planck crafted a philosophy of science shaped strongly by his reading of Kant, but he interpreted Kant in the realist manner of his Berlin colleague, Riehl. Planck launched his attack on Mach in an address to a student group at Leyden in 1908, "Die Einheit des physikalischen Weltbildes," where he wrote:

> When the great masters of the exact sciences introduced their ideas into science: when Nicolaus Corpernicus removed the earth from the center of the world, when Johannes Kepler formulated the laws named after him, when Isaac Newton discovered universal gravitation, when your great compatriot Christian Hugens established his wave theory of light, when Michael Faraday created the foundations of electrodynamics—the list could be continued still further—economical points of view were then certainly the last to steel these men in their battle against traditional attitudes and overriding authorities. No: It was their rock-solid faith, whether based on aesthetic or religious foundations, in the reality of their world-picture. In the face of this indisputable fact, we cannot brush aside the suspicion that, if the Machian principle of economy were ever to become central to the theory of knowledge, the thought processes of such leading minds would be disturbed, the flights of their imagination would be paralyzed, and the progress of science might, thereby, be seriously impeded. (Planck 1909, 74).

That Mach was hurt and puzzled is evident from his reply (Mach 1910). Appearing in the pages of the *Physikalische Zeitschrift*, one of Europe's most widely-circulated and widely-read scientific periodicals, the whole debate, including Planck's rejoinder (Planck 1910), was a major event in the philosophical history of early twentieth-century physics. Mach had many defenders, but Planck never retreated, reiterating much the same assault on Machian positivism on a number of later occasions (see, for example, Planck 1931).[9]

Like many of his contemporaries, Mach had skeptical doubts about the reality of atoms, but his was not a principled anti-realism about all unobservables in physics.[10] On the contrary, by 1910, after Jean Perrin's experimental confirmation of Einstein's earlier theoretical work on Brownian motion, Mach graciously conceded that he was wrong, not because atoms had become observable, but simply because the evidence coming from Perrin's laboratory, and evidence of other kinds, such as Ernest Rutherford's work on atomic structure, tipped the balance the other way.[11]

As with atoms, so too with absolute space and time. Mach's complaint against Newton was not that unobservables had no place in physics but that posited unobservable structure had to do significant explanatory work that could not be done as well by observable structure. His much discussed critique of Newton's bucket experiment in the *Mechanik* (Mach 1883, ch. 2, sec. 6) asserts not that motion with respect to absolute space cannot explain the dynamical effects of rotation, but that in the universe as we find it, a universe populated with stellar masses, it is not clear how we could ever know that motion with respect to absolute space alone was the right explanation.

At the turn of the twentieth century, positivism came in many forms. Nearly as famous and influential as Mach was the Zurich philosopher Richard Avenarius (1843–1896), this thanks not only to the large audience for his books, including his imposing *Kritik der reinen Erfahrung* (Avenarius 1888, 1890; see also Avenarius 1876), in which he, too, defended a kind of neutral monism and a principle of mental economy, but also thanks to his having co-founded in 1877 (with Wilhelm Wundt and others) and for many years helped edit the first journal devoted principally to the philosophy of science, the *Vierteljahrsschrift für wissenschaftliche Philosophie*.[12] One measure of Avenarius's standing is that he, like Mach, was a principle target of Lenin's *Materialism and Empirio-Criticism* (Lenin 1909).

Among the veritable army of more minor positivists, Joseph Petzoldt (1862–1929) is the one most deserving of mention. Petzoldt taught for many years at the Technische Hochschule in Berlin, which location brought him into direct contact with many leading figures in philosophy and physics.

A friend to both Mach and Avenarius, Petzoldt began his career with a dissertation in which he advanced a critique of the Mach-Avenarius doctrine of mental economy, seeking to replace this with a more general principle of "stability" (Petzoldt 1890). This was followed by a later widely cited 1895 paper, "Das Gesetz der Eindeutigkeit" (Petzoldt 1895), in which Petzoldt, inspired by Mach's influential doctrine of causality as merely univocal functional correlation, promulgated a general methodological thesis that asserts, in effect, that a necessary condition on any acceptable scientific theory is that it provide an unambiguous or univocal representation of that for which it aims to account. This principle came to play a major role in many contexts, including the Marburg neo-Kantians' progressive refinement of their idea of purely conceptual determinations providing a univocal characterization of the objects of cognition (see Cassirer 1910), debates about whether general relativity permits a univocal characterization of some aspects of space-time structure (see Howard 1992 and 1996), and the elaboration of the concept of categoricity in formal semantics (see Howard 1999).

For a while, Petzoldt was more closely associated with Avenarius, publishing a two-volume "introduction" to Avenarius's *Philosophie der reinen Erfahrung* (Petzoldt 1900, 1904) that was more widely because more accessible than the daunting and opaque original. Advocate of a variant that he termed, "relativistic positivism" (Petzoldt 1906), in later years Petzoldt became quite well known for the many books and papers in which he sought to show, sometimes with more zeal than insight, that Einstein's theory of relativity was a confirmation of the basic claims of positivism thanks to its allegedly relativizing everything to the observer (see, for example, Petzoldt 1912a, 1912b, 1914, 1921a, and 1921b). Petzoldt seems never to have understood that reference frames in relativity have nothing to do with the epistemic perspective of a human observer. Petzoldt nevertheless played a leading role in the institutional development of positivism, this as founder in 1912 of both the

Gesellschaft für positivistische Philosohpie (whose founding members included Mach, Einstein, Helm, David Hilbert, Felix Klein, and Sigmund Freud!) and its journal, the short-lived *Zeitschrift für positivistische Philosophie.*

Duhem, Poincaré, and Conventionalism

Philipp Frank (1884–1966) was a physicist trained in Vienna, where he heard lectures by Mach and Boltzmann. He was Einstein's successor in theoretical physics at the Charles University in Prague, a core member of the Vienna Circle, and the last surviving representative of the left-leaning Neurath wing of the Vienna Circle (see Cartwright et al. 1996). In a lovely memoir recalling the intellectual environment of the Vienna of his youth, he describes his teacher Mach as a representative of a movement called the "new positivism," whose other members include the French conventionalists, Pierre Duhem (1861–1916), Henri Poincaré (1854–1912), and Abel Rey (1873–1940). Frank's immediate concern is Mach's doctrine of causality as univocal functional coordination, which view, Frank thinks, is incompatible with the Kantian view of causality as an a priori element in scientific cognition and strongly suggests a conventional aspect to our ascription of causal relations between events (Frank 1949, 14). But Frank's assimilation of Mach's philosophy of science to those of Duhem and Poincaré will surprise those who were taught to read Mach as a reductionist phenomenalist and thus as espousing a view strongly opposed to the holism of Duhem.[13] The point is simply that Duhem takes only whole theories to have content, a view incompatible with verificationist idea often ascribed to Mach, the view that each individual scientific concept must have its own empirical content via its construction out of the elements.

Surprise only grows when one learns that it was Mach who championed the translation in German of Duhem's *La Théorie physique: Son objet et sa structure* (Duhem 1906, 1908), to which he added a sympathetic foreword, that he expressed high praise for Duhem in the preface to the second edition of his *Erkenntnis und Irrtum* (Mach 1906), and that he added to that second edition several footnotes specifically commending the very aspect of Duhem's philosophy, namely, the holism, that is rightly seen as incompatible with reductionist phenomenalism or verificationism (Mach 1906, 202, 244).

Mach and Duhem did have much in common. They both advocated an anti-metaphysical conception of scientific knowledge, and they were both critics of the molecular-kinetic approach in the theory of heat. But it is something of an overstatement to describe them as representatives of a unitary "new positivist" movement, even more so when Poincaré is added to the group, for the disagreements between Poincaré and Duhem were at least as significant as any between Duhem and Mach.

Duhem was trained, like Ostwald, as a physical chemist, and, again, like Ostwald, he was a proponent of energetics.[14] Like Mach, his interest first broadened in the direction of the history of science, writing major studies of medieval mechanics, cosmology, and other fields, after which he turned his attention to the philosophy of science. Duhem's model for physical theory was the highly formalized general thermodynamics that stood at the center of his own work. In *La Théorie physique* he mocked the "English" need to construct picturizable models of the phenomena, a la Maxwell, and denied to physical theory the ability to get behind the phenomena and discover ultimate metaphysical truth. For Duhem, a very devout Catholic, metaphysics was the domain of theology, not science. Description, not explanation, was held to be the aim of science.

How science is thus limited in its explanatory ambitions was the point of the central epistemological argument of *La Théorie physique*. Duhem argued that especially in instrumentally dense sciences like physics, one never tests a proposition in isolation. Since predictions are inferred from theory only in conjunction with all manner of auxiliary hypotheses, chief among them assumptions about the physics, chemistry, etc. of one's instrumentation, it is only whole bodies of theory that are tested as a whole, and thus only whole bodies of theory possess empirical content. It follows that when a prediction is falsified, that does not automatically entail the falsity of the hypothesis in question, for, as a matter of logic, the problem could have lain any of the auxiliary assumptions as well. Thus, it is generally the case that more than one body of physical theory will be compatible with a given body of experimental and observational data, meaning that theory choice is underdetermined by considerations of logic and evidence alone. To be sure, with a healthy *bon sens* guiding the theorist's choices, science eventually approaches what Duhem terms the "natural classification." But from a logical point of view, the choice among alternative theories is still a matter of convention.

Poincaré arrived at his version of conventionalism via a different route.[15] Working mainly in mathematics and mathematical physics, where he made fundamental contributions to mechanics and cosmology, Poincaré was very nearly a co-discoverer of the special theory of relativity. He wrote widely, however, on more philosophical topics (see the essays collected in Poincaré 1902, 1905, and 1913), and he was especially interested in the epistemic status of geometry. Much like Helmholtz, Poincaré believed that the postulate of free mobility had an a priori grounding and that the only admissible metrical geometries, therefore, corresponded to spaces of constant curvature. Which specific metrical geometry we choose is a matter of convention, though simplicity considerations almost guarantee the choice of Euclidean geometry (see Friedman 2002). More so than Helmholtz, however, Poincaré examined the manner in which that choice rested upon stipulative and hence conventional definitions of geometrical primitives, such as "segment of a straight line," via physical structures, such as a practically rigid rod or the path of a ray of light (see especially ch. 5, “L’expérience et la géométrie,” in Poincaré 1902, 92–109).

In effect, Poincaré restricts the conventional moment in physical theory to those parts of a theory that function as definitions, and therein lies a crucial difference between Poincaré’s conventionalism and the holistic conventionalism of Duhem, a point that Duhem, himself, stressed. Duhem discussed, in particular, the analogous point made by Poincaré concerning the status of mechanical principles, such as the principle of inertia (see ch. 6, “La mécanique classique,” in Poincaré 1902, 110–134). Poincaré held that the principle of inertia, though not a priori, could also not be refuted by experiment, since if a putatively inertial motion were to appear otherwise, one could always find a frame of reference with respect to which the body in question could be regarded as moving in a uniform, rectilinear fashion. The principle functions, thus, as a stipulative, conventional definition of the concept of an inertial trajectory. To this Duhem responds that Poincaré is wrong in assuming that such principles are tested in isolation from other parts of physical theory:

> In truth, hypotheses which by themselves have no physical meaning undergo experimental testing in exactly the same manner as other hypotheses. Whatever the nature of a hypothesis, we have seen . . . that it is never in isolation contradicted by experiment; experimental contradiction always bears as a whole on the entire theoretical ensemble without it being possible to designate which, in this ensemble, is the proposition that should be rejected. (Poincaré 1906, 328–329).

The question whether the moment of convention can be restricted to those parts of theory that function as definitions, or whether it is only whole theories that are the units of conventional choice, will become a question of major significance when logical empiricists tackle the question of the empirical status of the space-time metric.

Conventionalism was as important a movement in early-twentieth century philosophy of science as was positivism, and it came in as many forms. To the names already mentioned, one should add at least Édouard Le Roy (1870–1954) and Émile Meyerson (1859–1933) as important representatives. In the French context, it stood in a complicated dialogical relationship with the neo-Kantianism of thinkers like Émile Boutroux (1845–1921), and it likewise became deeply entangled with neo-Kantianism when it was taken up in Germany and Austria by the thinkers out of whose work grew the movement known as the Vienna Circle.

The Vienna Circle, Scientific Philosophy, and Logical Empiricism

There were two Vienna Circles. The one that we know by that name, the one to whose philosophy of science we attach the name "logical empiricism," came into being in 1922 when Moritz Schlick (1882–1936) arrived in Vienna to take up Mach and Boltzmann's old chair, now titled the chair for Philosophie der induktiven Wissenschaften. The first Vienna Circle grew up in the first decade of the twentieth century around a group that included the mathematician Hans Hahn (1879–1934) and the sociologist and economist Otto Neurath (1882–1945), and it was their activity that created the setting out of which Schlick-circle emerged in the 1920s.[16] But Vienna was not the only place where the new discipline of the philosophy of science was developing. Berlin, for example, became home to the Gesellschaft für wissenschaftliche Philosophie in the later 1920s, continuing a tradition that went back to Riehl and Petzoldt, but including now Hans Reichenbach (1891–1953) at its core.

It is a noteworthy fact that three of the major figures in the development of logical empiricism and the new scientific philosophy grew to philosophical maturity with work on the theory of relativity. Schlick was a physicist by training, having done a dissertation under Planck in Berlin in 1904. A few years later he turned to philosophy, studying in Zurich with Wundt's student, Gustav Störring (1860–1949). Soon after taking up his first academic position in Rostock in 1910, he began a serious study of the philosophical implications of relativity (Schlick 1915), this on the advice of his friend, Max von Laue, and his first important book, highly praised by Einstein, was his widely-read *Raum und Zeit in der gegenwärtigen Physik* (1917), whose purpose he described as being to explicate the essential philosophical lessons of the general theory of relativity. He and Einstein became, for a time, close professional friends, their unusually productive intellectual exchange having important implications for the further development of each one's thinking (see Howard 1984).

Reichenbach was a student in Berlin in the late 1910s, where he attended Einstein's lectures on general relativity. After completing a dissertation on the foundations of probability, he published his first major book—now, once again much discussed (see Friedman 2003)—*Relativitätstheorie und Erkenntnis Apriori* (1920), in which he sought to reconcile Kant with Einstein by distinguishing the apodictic and constitutive aspects of the a priori and arguing that while the lesson of relativity was that apodicticity had to be abandoned, since the geometry of space-time is not Euclidean, relativity theory itself reaffirmed the central importance of the constitutive role of the a priori. His next two books also concerned the philosophical analysis of relativity theory, *Axiomatik der relativistischen Raum-Zeit-Lehre* (1924) and *Philosophie der Raum-Zeit-Lehre* (1928), and these were followed in 1944 by his *Philosophic Foundations of Quantum Mechanics*.

Rudolf Carnap (1891–1970) was a student in Jena. In his dissertation, *Der Raum* (1921), written under the direction of the neo-Kantian Bruno Bauch (1877–1942), he sought to identify the a priori and contingent elements in psychological, formal, and physical space. Most of his philosophical work prior to the *Aufbau* (1928), also concerned problems in physics (see, for example, Carnap 1925, 1926), including an unfinished project on axiomatizing space-time theory (see Howard 1996), and late in his career he published a more comprehensive *Philosophical Foundations of Physics* (1966), this based on earlier lectures.

Given the extent of Schlick, Reichenbach, and Carnap's early engagement with relativity theory, it is to be expected that this work had a major impact on the fine structure of emergent logical empiricism. General relativity's challenge to the Kantian doctrine of the a priori character of Euclidean geometry was the major issue. As Schlick put the point in his review of Cassirer's *Die Einsteinschen Relativitätstheorie* (Cassirer 1921), the task was to create a new form of empiricism capable of defending the empirical integrity of general relativity (Schlick 1921), a task in which Einstein, himself, was, initially, an equal collaborator. Precisely how to articulate the empirical credentials of general relativity was, however, something about which opinion differed.[17]

As mentioned, Reichenbach had suggested that Kant was wrong to insist upon the apodictic character of the a priori but right to insist upon its constitutive role in scientific cognition. But what kind of constitution is involved? It is not the constitution of objects in intuition as understood by Kant. Following Einstein's lead, Schlick suggested that those elements of theory that Reichenbach regarded as constitutive a priori components were more properly characterized as conventional, most of them being like in kind to the conventional, stipulative definitions to which Poincaré had assigned a fundamental role in geometry, such as the association of the geometrical notion of a segment of a straight line with a practically rigid measuring rod. Schlick understood the work of such definitions to be that of effecting the univocal coordination between theory and world that, for him, was the mark of a theory's truth.

When Schlick first made this suggestion in 1920, he had in mind the view of the role of conventions that he had most extensively described in his *Allgemeine Erkenntnislehre* (1918). That there would be an unavoidable conventional aspect to scientific theory was, for Schlick, a direct consequence of view of truth as univocal coordination between theory and world (Schlick 1910). As Schlick had repeatedly stressed, if truth is mere univocal coordination, then more than one theory can be thus coordinated with the same body of empirical fact:

> The totality of our scientific propositions, in word and formula, is in fact nothing else but a system of symbols *correlated* to the facts of reality; and that is equally certain, whether we declare reality to be a transcendent being or merely the totality and interconnection of the immediately "given." The system of symbols is called "true," however, if the correlation is completely univocal. Certain features of this symbol system are left to our arbitrary choice; we can select them in this way or that without damaging the univocal character of the correlation. It is therefore no contradiction, but lies, rather, in the nature of the matter, that under certain circumstances, several theories may be true at the same time, in that they achieve indeed a different, but each for itself completely univocal designation of the facts. (Schlick 1915, 149)

But Schlick's conventionalism of this period, though not explicitly derived from that of Duhem, nevertheless breathes the spirit of Duhemian holism, as did the conventionalism of Schlick's friend Einstein (see Howard 1990). Schlick's early holism was more a product of his having adopted Hilbert's view of implicit definitions as that aspect of theory whereby the referring terms in a theory's vocabulary acquire their content, from which doctrine it follows that it is a term's systematic role in the entire theory whereby its meaning is fixed.

In the ensuing discussion, Reichenbach quickly acceded to Schlick's suggestion, but Schlick's own thinking also underwent a significant development, moving away from his earlier, more holistic, Duhemian version of conventionalism and toward the view that we recognize as the signature position of mature logical empiricism. On this latter view, famously espoused by Schlick in the second edition of the *Allgemeine Erkenntnislehre* (1925) and later publications (see, for example, Schlick 1935), as well as by Reichenbach in his *Axiomatik der relativistischen Raum-Zeit-Lehre* (1924) and *Philosophie der Raum-Zeit-Lehre* (1928), the conventional moment in scientific theory is confined to definitions, most importantly the coordinating definitions that link theoretical primitives to physical structures. Such definitions, being analytic propositions, can be distinguished in a principled manner from synthetic, empirical propositions, and once all of the conventional coordinating definitions are established, each empirical proposition, and each empirical term, thereby acquires its own, determinate empirical content, such that the truth or falsity of individual empirical propositions can be univocally determined on the basis of the corresponding experience. Moreover, the variant theories that arise from different choices of coordinating definitions are held not really to differ, for since empirical content is the only content, empirically equivalent theories are just different ways of saying the same thing, no more different in kind than the assertions "il pleut" and "es regnet." It was thought that only thus, by fixing a determinate empirical content for each empirical proposition, could one counter the Kantian challenge to general relativity's claims about the geometry of space-time. Here, in the struggle with neo-Kantianism, one therefore finds the birth of the verificationist theory of meaning.

Einstein remained a Duhemian holist throughout his life, and his growing disagreement with Schlick over the role of conventions in science was as important to their eventual parting of the ways by the late 1920s as was their disagreement over logical empiricism's anti-metaphysical strictures. Einstein believed that holism, itself, held the key to a more cogent reply to the neo-Kantian. He argued that it was precisely the arbitrariness in our decision about which elements of theory to designate as a priori that was the major problem with the Kantian program, there being no systematic, principled basis upon which to distinguish the analytic and the synthetic (Einstein 1924; see also Howard 1990, 1994a).[18]

One of the major fault lines within logical empiricism also concerned a disagreement over holism and conventionalism. The so-called right wing of the Vienna Circle and its allies was composed mainly of those—Schlick and Reichenbach—who had been driven to deny holism by the efforts to counter neo-Kantian critiques of relativity. The left wing, centered around the socialist Neurath and including Philipp Frank, defended an explicitly Duhemian, underdeterminationist version of conventionalism, though their motivations were as much political as they were scientific, for they held that, especially in areas like the social sciences and economics, social and political values do and should fill the gap left when experience and logic do not determine a uniquely correct theory. When making social or economic policy, choose we must among empirically equivalent alternative theories. On Neurath's view we should be honest about the basis upon which we choose, and, other things being equal, we should choose those theories most likely to be conducive to the achievement of progressive social ends. When, in the early 1930s, Neurath took on Schlick in the famous protocol-sentence debate, it was not just the choice of a physicalist or phenomenalist protocol language that was at issue. It was equally a debate over the holist and non-holist versions of conventionalism and the political consequences of the choice between those two philosophies of science (see Uebel 1992).

To be sure, logical empiricism developed in interaction with many currents of scientific thought, not just the theory of relativity. For example, there was a protracted discussion of the status of the principle of causality, this prompted largely by the quantum theory's suggestion of radical physical contingency (see Schlick 1931 and Frank 1932). Other sciences played a role as well, including biology and, especially in Neurath's case, the social sciences and economics. There was also the enormous impact of the developments in logic and the philosophy of language brought about by the work of thinkers like Hilbert, Bertrand Russell, and Ludwig Wittgenstein. But physical theory, most prominently the theory of general relativity, remained the model of scientific knowledge upon which the logical empiricist project was mainly grounded.

Philosophy and Physics in the Work of Albert Einstein

Pervasive connections between physics and philosophy in the early twentieth century had important implications for the development of the latter. But the arrow of influence pointed in the opposite direction as well, for the physics of the early twentieth century was unusual in the extent to which it was suffused with a distinctively philosophical way of thinking. No one individual better illustrates this phenomenon than Einstein (1879–1935).[19]

Mention was just made of Einstein's role in crafting an empiricist response to neo-Kantian critiques of general relativity. But Einstein's engagement with the philosophy of science was not just a late and defensive maneuver. Einstein had been deeply engaged with philosophy and the philosophy of science from a very early date. He read Kant as a teen and again in the late 1910s. As a student at the ETH in Zurich, he enrolled for August Stadler's lectures on Kant and on "Theorie des wissenschaftlichen Denkens." With his friends in the Olympia Akademie in Bern he read Mach, Avenarius, Mill, Hume, Poincaré, and Spinoza. During his university days and repeatedly during his entire life he read Schopenhauer (see Howard 1997). He reflected profoundly on what he read, and it made a difference, both by way of his deploying in his physics specific bits of philosophical doctrine, but also by way of his having early on developed a philosophical temperament or habit of mind. This latter, especially, made the crucial difference, as Einstein himself observed. In a letter to a young philosopher of science in 1944, Einstein wrote:

> 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)

This had been Einstein's view for many years, as one sees from the way in which he recorded the nature of his debt to Mach in a 1916 obituary note:

> 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)

How does philosophy endow one with such "independence of judgment"? Einstein explains:

> 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," " 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)

As noted, Einstein's own philosophy of science was a version of Duhemian holistic conventionalism.[20] Einstein most likely first encountered Duhem in Zurich in the fall of 1909 through the agency of his friend, Friedrich Adler, who was the translator for the German edition of Duhem's La Théorie physique (Duhem, 1908). Einstein was quick to employ Duhem's point of view in lectures on electricity and magnetism at the University of Zurich in the winter term of 1910–1911, explaining that even though a test body cannot be introduced within a solid charged body, a theory positing charge within such a body could, nevertheless, be well grounded, since it is only whole theories that have to possess empirical content (Einstein 1992, 325). It was this prior adoption of a holistic form of conventionalism that prepared Einstein to be so receptive to the similar view he found in Schlick's writings on the philosophical implications of relativity when he first encountered them in December 1915. It was the Duhemian holistic version of converntionalism to which Einstein, himself, appealed in trying to answer the Kantian challenge. And near the end of his life he was still employing this point of view to try to explain to Reichenbach why it was wrong to think that only coordinating definitions are conventional and why the resulting verificationist theory of meaning is the wrong way to think about the semantics of physical theories (Einstein 1949, 678), two years before Quine more famously advanced a similar argument (Quine 1951).

The context within which Einstein's sympathy for Duhemian holistic conventionalism probably made the greatest difference were the very debates over the empirical content of space-time geometry that were so important in shaping early logical empiricism. Einstein's best known published intervention in those debates was his 1921 lecture, "Geometrie und Erfahrung," Here Einstein explained that the stipulation of conventional coordinating definitions, in the form of a specification of what practically rigid physical body will be coordinated with the geometrical notion of a segment of a straight line and the analogous coordinating definition for an ideal clock, are necessary at the present stage in the development of physical theory, but only because fundamental physics has not yet advanced to the point where complicated structures such as rods and clocks are given as solutions to our theory's basic equations. The latter is, however, the ideal, and for such a complete fundamental theory there will be no need for coordinating definitions, all of the elements of the theory's ontology being, in effect, implicitly defined by the systematic role played by the corresponding terms in the whole theory.

Duhem is usually interpreted as an anti-realist. Einstein's attitude toward realism was more complicated.[21] He was a realist about the point-events that formed the foundation of a space-time ontology, even though he thought such an ontology to be systematically underdetermined by empirical evidence (see Howard 1999), but he mocked realism as a general thesis about the interpretation of scientific theories, writing to one correspondent in 1918:

> "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. (Einstein to Eduard Study, 25 September 1918; as quoted in Howard 1993)

Was Einstein really a realist?

Einstein's most profound statement of his view on physical reality appears in a perhaps unexpected place, namely, a discussion of his reasons for thinking that quantum mechanics is incomplete. He wrote the following to Max Born in 1949:

> 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 ('actual') 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 somehow 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 by 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; my translation)

What, then, is realism for Einstein? It is, surprisingly, a commitment to the principle of spatial or spatio-temporal separability, the assumption that a non-null spatial or spatio-temporal separation is a sufficient condition for the individuation of physical systems (see Howard 1985), a principle that he seems to have learned from a perhaps improbable source, namely Schopenhauer (Howard 1997). What most troubled Einstein about quantum mechanics was not, in the final analysis, its denial of strict microphysical determinism. It was, instead, quantum entanglement, which seems to deny separability, for according to quantum mechanics, the joint state of two previously interacting systems cannot be expressed as the product of two individual states. In what sense can separability be thought to capture the essential idea of physical reality? Surely at least in this sense, namely, that, for Einstein, the metaphor of independence deployed in talk of the real as being independent of the knower is not a metaphor but a literal physical claim about the physics of the interaction between knower and known in perception and cognition, both knower and known being regarded primarily as physical systems.

Novel as it might be, Einstein's "entheorizing" of the concept of realism as the physical principle of separability was not his only original contribution to the philosophy of science.[22] Arguably his most original contribution was his introduction of the distinction between "principle theories" and "constructive theories" (Einstein 1919). Principle theories consist of principles like energy conservation, the relativity principle, or the light principle, all of them distinguished on Einstein's view by their being empirically well-grounded high-level generalizations. Constructive theories, by contrast, consist of constructive models purporting to explain the phenomena. Ultimate understanding, says Einstein, is provided by constructive theories, which are the goal of all science. But—and here is Einstein's real insight—progress in physics is too often impeded by the premature search for constructive models in situations where we lack sufficient empirical guidance. Better to proceed as Einstein said he did in the search for special and general relativity, namely, by seeking principle theories whose establishment would then later constrain the search for an ultimate, fundamental, explanatory theory.

Conclusion: The Philosopher-Physicist

The Einstein volume in the Library of Living Philosophers series is titled *Albert Einstein: Philosopher-Physicist* (Schilpp 1949). The title aptly characterizes the kind of thinker Einstein was. But in this respect, Einstein was not as unusual as one might think. For Einstein was part of a whole generation of scientists who could have been equally well described as philosopher-physicists. This blending of the scientific and philosophical temperaments is to be found in many of Einstein's contemporaries. Consider a few other noteworthy cases. Niels Bohr (1885–1962) was a good friend of the Danish philosopher Harald Høffding, from whom he acquired the distinctively Kantian vocabulary that marked his many explanations of the complementarity interpretation of quantum mechanics (see Howard 1994b). Werner Heisenberg (1901–1976) was a careful student of ancient Greek philosophy, Aristotle's metaphysics playing a crucial role in his attempts to understanding the ontological lessons of the quantum theory (see Heisenberg 1989). Wolfgang Pauli (1900–1958) had as his Godfather none other than Ernst Mach (Enz 2002). Erwin Schrödinger (1887–1961) was a serious student of the writings of Schopenhauer, through whom he became so enamored of the Indian Vedantic tradition that he taught himself Sanskrit so as to be able to read the Vedas in the original (Moore 1994), and the fundamental metaphysical holism that he encountered there importantly conditioned his understanding of quantum mechanical entanglement (see Howard 1997).

This list could be extended with many other names. Perhaps the best example, after Einstein, is Hermann Weyl (1885–1955). Deeply influenced both by Hilbert's axiomatics and the phenomenology of Edmund Husserl, Weyl was not only one of the most important physicists and mathematicians of his generation, but also a profound and original philosopher of science, whose *Philosophie der Mathematik und Naturwissenschaft* (1927) is one of most important, if also surprisingly underappreciated, works in the philosophy of science of the twentieth century.

It was not just accidents of biography that produced in all of these thinkers a scientific-philosophical habit of mind, for the larger scientific and philosophical culture was suffused with that spirit. They all read Kant and Schopenhauer in their youth. Some, like Einstein, were required to study the philosophy of science as part of their physics training. In the Vienna of Mach, Boltzmann, and Franz Exner, the philosophy of science was so much a part of the atmosphere that young physics students inhaled it with every intellectual breath they took. In the Zurich of Avenarius and the Berlin of Helmholtz, Planck, and Riehl, to be a sophisticated young physics or philosophy student like Schlick or Reichenbach meant being informed about the latest debates in the arena of scientific philosophy and being prepared to defend one’s views on, say, Kant versus Mach. The pages of prominent scientific journals such as *Die Naturwissenschaften*, the *Physikalische Zeitschrift*, and the *Naturwissenschaftliche Rundschau* were filled with articles by the leading philosopher-physicists of the day and reviewed every major book in the philosophy of science.

The fifty years from 1880 to 1930 was the era of the philosopher-physicist. Was it just a coincidence that this was also the period in which was wrought the most profound and far-reaching changes in physics since at least the time of Newton? As noted above, Einstein thought it no coincidence. He believed that the cultivation of a philosophical attitude was the key to achieving the independence of thought necessary for genuine progress in science. Who are we to disagree?

Notes

1. On the development of theoretical physics in the late-nineteenth and early-twentieth centuries, see Jungnickel and McCormmach 1986a and 1986b. A helpful recent comprehensive history of theoretical physics in the twentieth century is Kragh 1999.

2. There are no commendable comprehensive histories of either late-nineteenth century or early-twentieth century philosophy of science. Important parts of the nineteenth and early twentieth century story are told in Coffa 1991, with special attention being paid to the rise of logical empiricism. Some helpful material on other aspects of the nineteenth century will be found in Giere and Westfall 1973. The early-twentieth century has been more thoroughly investigated, the background and rise of logical empiricism receiving, again, the bulk of the attention. An almost encyclopedic source of information on the Vienna Circle, in particular, is Stadler 1997. For more analytical perspectives, see Friedman 1999 and Parrini 2002. Gillies 1993 provides a broad overview of several currents now recognized as important in twentieth century philosophy of science.

3. Judson 1996 is good place to begin for understanding relations among biology, physics, and philosophy in the early twentieth century.

4. Wilhelm Wundt (1832–1920), for example, was a major figure in the philosophy of science, as was Gustav Fechner (1801–1887). Large parts of Wundt's massive, two-volume *Logik* could be and were read as a textbook in the philosophy of science. On Fechner, see Heidelberger 1993.

5. For more on the history of the early years of the Marburg school, see Holzhey 1986. A helpful recent study of Cohen, with special emphasis on his philosophy of science, is Patton 2004. For a definitive, new intellectual biography of Cassirer, see Ferrari 2003; see also Krois 1987. Cassirer also wrote what is perhaps the best early philosophical study of the quantum theory, *Determinismus und Indeterminismus in der modernen Physik* (Cassirer 1936).

6. Brush 1976 remains the best, self-contained history of theremodynamics and the kinetic theory of heat in the late nineteenth century.

7. For more on Ostwald and the debate over energeticism, see Deltete 1983.

8. Blackmore 1972 is still the only comprehensive intellectual biography of Mach. A useful recent study is Banks 2003.

9. A good biography of Planck, one alert to his philosophical commitments, is Heilbron 1986. Not much has been written specifically concerning Planck's philosophy of science, but see Kretzschmar 1967 and Vogel 1961.

10. A similar reading of Mach as not staunchly opposed to granting the reality of unobservables is persuasively presented in Wolters 1987.

11. For more the philosophical implications of work on Brownian motion, see Nye 1972.

12. The journal later continued as the *Vierteljarhsschrift für Wissenschaftliche Philosophie und Soziologie.*

13. Quine 1951 is a famous and influential rehearsal of this way of putting the views of Mach and Duhem in opposition to one another.

14. For more on the life and work of Duhem, see Jaki 1984, Maiocchi 1985, Brenner 1900, and Martin 1991. Brenner 2003 provides a very helpful overview of the entire history of early-twentieth century French philosophy of science.

15. In the absence of a good intellectual biography of Poincaré, Stump 1998 is a helpful starting point for the further study of Poincaré's life and work. Giedymin 1982 situates Poincaré's conventionalism in a broader historical context.

16. On the first Vienna Circle, see Frank 1949, Haller 1983, and Stadler 1997, which latter is a good source on the history of the entire movement, as is Haller 1993.

17. For a fuller account of the role played in the development of logical empiricism by reactions to neo-Kantian critiques of relativity, see Howard 1994a.

18. Friedman 1999 offers a different perspective on the impact of neo-Kantianism on the development of logical empiricism, emphasizing the role that the notions of constitution and construction played especially in Carnap's work.

19. For a more comprehensive account of Einstein's philosophy of science, and for detailed references, see Howard 2004.

20. For a thorough study of Einstein's assimilation of Duhem's version of conventionalism, see Howard 1990.

21. Howard 1993 presents a careful assessment of Einstein's realism; see also Fine 1986 and Holton 1968.

22. I borrow the term "entheorizing" from Fine 1986, who argues, however, that Einstein's realism is entheorized via the principle of causality, not separability.

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