Thomas Kuhn

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1 Kuhn and Logical Empiricism

MICHAEL FRIEDMAN

Conventional wisdom concerning twentieth-century philosophical approaches to scientific knowledge has held that Kuhn's theory of scientific revolutions is diametrically opposed to the philosophical movement known as "logical positivism" or "logical empiricism." Logical positivism has been portrayed as a naive version of empiricist foundationalism, according to which all knowledge is to be reduced to an epistemically certain basis in observational reports. And it follows, on this view, that there can be no genuine scientific revolutions in the Kuhnian sense: scientific progress must rather follow the "development-by-accumulation" model (in this case, development by accumulation of observable facts) that Kuhn explicitly rejects at the outset.[1] If we accept Kuhn's theory, by contrast, it follows that the progress of science is marked by radical discontinuities quite incompatible with such naive empiricism. So it is no wonder that Kuhn's theory of scientific revolutions is standardly taken as a major factor in the demise of logical empiricism.[2]

Over the past twenty-five years, however, a growing body of active research has been devoted to detailed study of the rise and decline of the logical empiricist movement. And this research has shown, not surprisingly, that the accepted conventional wisdom concerning the relationship between Kuhn's theory of scientific revolutions and logical empiricist philosophy of science is seriously oversimplified and fundamentally misleading. Perhaps the most striking results of this research appear in an article by George Reisch (1991) entitled "Did Kuhn Kill Logical Empiricism?" Beginning with the well-known fact that Kuhn's *The Structure of Scientific Revolutions* first appeared, in 1962, as a volume of the *International Encyclopedia of Unified Science* (the official monograph series of the logical empiricist movement in exile), Reisch presents two previously unpublished letters written to Kuhn by Rudolf Carnap in the latter's capacity as editor of this series. There Carnap expresses enthusiastic approval of Kuhn's ideas, which, he says, "will be very stimulating for all those who are interested in the nature of scientific theories and especially the causes and forms of their changes."

Carnap also states, while admitting that his own "knowledge of the history of science is rather fragmentary," that he especially "liked your [Kuhn's] emphasis on the new conceptual frameworks which are proposed in revolutions in science, and, on their basis, the posing of new questions, not only answers to old problems."[3]

These expressions of approval by Rudolf Carnap – the generally acknowledged leading representative of logical empiricism – are certainly striking, and they must give serious pause to expositors of the conventional wisdom. But even more striking, as Reisch also explains, are the deep affinities between Carnap's underlying philosophical perspective and Kuhn's ideas. Natural science, for Carnap, is to be conceived as represented within a particular formal language or linguistic framework. And perhaps Carnap's most fundamental thought is that there are a plurality of essentially different, nonintertranslatable such frameworks. Thus, for example, there are linguistic frameworks in which the rules of classical logic are taken to be valid, and there are also linguistic frameworks in which we instead adopt the rules of intuitionistic logic (wherein the law of the excluded middle is no longer universally valid). For Carnap, moreover, there is no sense in which one such framework can be "correct" while another is "incorrect." Rather, all standards of logical correctness are relative or "internal" to a particular choice of linguistic framework. "External" questions concerning which linguistic framework to adopt are not similarly adjudicable by already established logical rules but rather require a "conventional" or "pragmatic" choice based on suitability or adaptedness for one or another given purpose.[4] Such external questions, involving the change from one linguistic framework to a different one, are precisely what is at issue, for Carnap, in scientific revolutions.[5]

The affinities between Carnap's philosophy of linguistic frameworks and Kuhn's theory of scientific revolutions are therefore pervasive indeed. According to Kuhn, there are two essentially different kinds of periods in the history of science: periods of normal science in which the relevant community operates unquestioningly within a generally accepted paradigm "committed to the same rules and standards for scientific practice" (1970, p. 11), and periods of revolutionary science in which precisely such an underlying consensus is then undercut. Similarly, for Carnap, there are two essentially different kinds of activities associated with the linguistic frameworks within which our theories in natural science are formulated: the adjudication of internal questions on the basis of the accepted logical rules of a single given linguistic framework and the adjudication of external questions that, by hypothesis, do not and cannot presuppose such logical rules.[6] Just as, for Carnap, the logical rules of a linguistic framework are constitutive of the notion of correctness or validity relative to this framework, a particular paradigm governing a given episode of normal science, for Kuhn, yields generally agreed upon – although largely tacit – rules constitutive of what counts as a valid or correct solution to a problem within this episode of normal science. Just as, for Carnap, external questions concerning which linguistic framework to adopt are not similarly governed by logical rules but rather require a much less definite appeal to conventional and/or pragmatic considerations, changes of paradigm in revolutionary science, for Kuhn, do not proceed in accordance with generally agreed upon rules, as in normal science, but rather require something more akin to a conversion experience.[7]

It is especially noteworthy, then, that Kuhn, toward the end of his career, explicitly acknowledges these parallels. Kuhn expresses embarrassment, to begin with, that "[w]hen I received the kind letter in which Carnap told me of his pleasure in the manuscript [one of the letters concerning the initial publication of *Structure* cited by Reisch], I interpreted it as mere politeness, not as an indication that we might usefully talk."[8] But Kuhn then goes on to explain the "correspondingly deep difference" between Carnap and himself that he thinks survives the acknowledged parallels. This does not consist, as one might first expect, in the circumstance that Carnap's linguistic rules must always be explicitly formulated, whereas Kuhn's "rules and standards for scientific practice" are largely tacit and are thus enforced by implicit convention rather than explicit formal legislation. Kuhn rather emphasizes that he, unlike Carnap, is concerned from the start with historical *development*, so that, in particular, "[l]anguage change is *cognitively* significant for me as it was not for Carnap" (1993, p. 314). The point, I take it, is that change of language involves an external question for Carnap and is therefore merely pragmatic, and not cognitive or epistemic in the only sense of "epistemology" Carnap recognizes. For, although Carnap, as Reisch emphasizes, does connect his notion of change of language with scientific revolutions, he never discusses such revolutions in any serious way. Such a historical investigation could never be a part of what Carnap himself preserves of epistemology, namely, *Wissenschaftslogik* [the logic of science] – the formulation and examination of a variety of possible linguistic frameworks within which the results of the special sciences may be represented. What is crucial, for Carnap, is that the only remaining properly philosophical problems are purely formal – belonging to the application of logic to the language of the special sciences. Although many interesting *empirical* questions may arise in analyzing the historical transitions from one theory to another during a scientific revolution (and Carnap expresses keen interest in such questions in his letters to Kuhn cited by Reisch), the only properly *philosophical* questions here concern the (purely timeless) articulation of the logical structures of the two different languages under consideration.[9] For Kuhn, by contrast, as the very first chapter of *Structure* makes clear, the point is precisely that historical examination of scientific change can, above all, be genuinely philosophical.

We can deepen our appreciation of the parallels between Carnap and Kuhn – and also their important differences – by looking a bit more closely into the development of both views. I consider first the development of logical empiricism.

Conventional wisdom portrays logical empiricism as directly descended from the classical empiricism of Locke, Berkeley, and Hume, with a more recent boost from the scientific positivism articulated by Ernst Mach at the end of the nineteenth century.[10] And it is true, of course, that the influence of Mach – and, more generally, of broadly empiricist currents of thought – is certainly important. (Indeed, as is well known, the logical positivism of the Vienna Circle was first formulated as an official movement under the rubric of the *Verein Ernst Mach*.) Nevertheless, there are equally important influences on the development of logical empiricism that lie quite outside the classical empiricist tradition. Two such influences are especially important in the present context: developments in non-Euclidean geometry and its philosophy that formed the indispensable background to Albert Einstein's formulation of the theory of relativity and developments in early-twentieth-century neo-Kantian epistemology – especially within the tradition of the Marburg School represented by Hermann Cohen, Paul Natorp, and Ernst Cassirer.[11]

The crucial figures in the development of non-Euclidean geometry, for the logical empiricists, were Hermann von Helmholtz and Henri Poincaré. And neither of these two thinkers defends a straightforwardly empiricist conception – such as was then standardly identified with John Stuart Mill – of either geometry in particular or scientific knowledge more generally. Whereas it is true, for example, that Helmholtz views the choice between Euclidean and non-Euclidean geometries as empirical, he also suggests that the more general structure of space common to both Euclidean and non-Euclidean systems (that of constant curvature or what Helmholtz called "free mobility") is a necessary presupposition of all spatial measurement and thus a "transcendental" form of our spatial intuition in the sense of Kant. Helmholtz's own approach to scientific epistemology is therefore Kantian insofar as space indeed has a "necessary form" expressed in the condition of free mobility; his approach is empiricist, however, insofar as which of the geometries of constant curvature actually holds is then determined by experience. So what we find here, in the end, is an attempt to *combine* Kantian and empiricist ideas so as to be as faithful as possible to the new scientific (and philosophical) situation.[12]

We find an analogous attempt to adapt both Kantian and empiricist ideas to the new scientific situation in the thought of Henri Poincaré, although here there is even less emphasis on traditional empiricism. According to Poincaré, whereas no particular geometry – neither Euclidean nor non-Euclidean – is an a priori condition of our spatial intuition, it does not follow that the choice between them, as Helmholtz thought, is empirical. For there remains an irreducible gulf between our crude and approximate sensory experience and our precise mathematical descriptions of nature. Establishing one or another system of geometry, Poincaré argues, therefore requires a free choice, a *convention* of our own – based, in the end, on the greater mathematical simplicity of the Euclidean system. And this notion of convention (which, as we shall see, is central to the development of logical empiricism) is explicitly adopted as a substitute for Kant's original, necessarily fixed notion of the a priori (represented by the laws of specifically Euclidean geometry), intended to respect Kant's insight into the "experience-constituting" role of geometry while simultaneously accommodating the new scientific developments showing that Euclidean geometry, in particular, is in no way uniquely forced upon us.[13]

As I suggested, these mathematical and philosophical developments formed the indispensable background to Einstein's formulation of the theory of relativity, and they were taken as such by Einstein himself and by the logical empiricists.[14] Indeed, the earliest philosophizing of those thinkers later to be identified most closely with logical empiricism arose directly from an attempt to assimilate both Einstein's new theory and the epistemological reflections of Helmholtz and Poincaré. Moritz Schlick, the founder and guiding spirit of the Vienna Circle, began this process in his *Space and Time in Contemporary Physics* (Schlick 1917), which went through four editions between 1917 and 1922. (Indeed, it was on the basis of this work, enthusiastically endorsed by Einstein, that Schlick gained the Chair for the Philosophy of the Inductive Sciences previously occupied by Mach and Ludwig Boltzmann at the University of Vienna in 1922.) Here Schlick argues that the lesson of the theory of relativity is not, as one might expect, that Euclidean geometry is a false description of physical space. It is rather, following Poincaré, that there is no fact of the matter about the geometry of physical space: choosing one or another physical geometry is not forced upon us by any observable facts but rather depends on a prior convention or stipulation without which the question of physical geometry is simply undefined. In particular, we can, if we wish, retain Euclidean geometry in the context of Einstein's theory, but this choice would result in formidable complications in our total system of geometry plus physics that make it pragmatically inexpedient (but not false).[15]

Carnap, in his doctoral dissertation (1922), explicitly follows Schlick in this Poincaré-inspired interpretation of the status of physical geometry in Einstein's theory. But here, in contrast to Schlick, there is a more positive estimation of the Kantian theory of space. Indeed, Carnap began his doctoral work under the guidance of the neo-Kantian philosopher Bruno Bauch at Jena, and, after taking a year-long seminar on the *Critique of Pure Reason* with Bauch, Carnap "was strongly impressed by Kant's conception that the geometrical structure of space is determined by the form of our intuition" (Carnap 1963, p. 4). Of course, one cannot now maintain Kant's original conception of the fixed synthetic a priori status of specifically Euclidean geometry; so Carnap rather defends a generalization of Kant's conception of spatial intuition according to which only the *infinitesimally* Euclidean character of physical space is a priori determined by the form of our intuition. Only this merely "topological form," for Carnap, is necessary, whereas the choice of specifically "metrical form" (whether Euclidean or non-Euclidean) is "optional [*wahlfrei*]" – and is in fact determined by convention (on the basis of the overall simplicity of our total system of geometry plus physics) in precisely the sense defended by Schlick.[16]

But the most fully developed attempt to reconcile the Kantian conception of scientific knowledge and Einstein's theory of relativity within the logical empiricist tradition was undertaken by Hans Reichenbach in his first book, *The Theory of Relativity and A Priori Knowledge* (1920). Reichenbach there draws a distinction between two meanings of the Kantian a priori: necessary and unrevisable, fixed for all time, on the one hand, and "constitutive of the concept of the object of [scientific] knowledge," on the other.[17] Reichenbach argues, on this basis, that the lesson of the theory of relativity is that the former meaning must be dropped and the latter must be retained. Relativity theory involves a priori constitutive principles (which Reichenbach calls "axioms of coordination") as necessary presuppositions of its properly empirical claims ("axioms of connection"), just as much as did Newtonian physics, but these principles have changed in the transition from the latter theory to the former: whereas Euclidean geometry is indeed constitutively a priori in the context of Newtonian physics, for example, only infinitesimally Euclidean geometry is constitutively a priori in the context of general relativity. What Reichenbach ends up with is thus a *relativized* conception of a priori mathematical-physical principles (axioms of coordination), which change and develop along with the development of the mathematical and physical sciences but which nevertheless retain the characteristically Kantian constitutive function of making the empirical natural knowledge (axioms of connection) thereby structured and framed by such principles first possible. Thus, as Reichenbach points out in a prepublication footnote added in proof, his ideas have much in common with contemporaneous attempts by neo-Kantian philosophers to develop an analogous reconciliation between the theory of relativity and Kantian philosophy.[18]

That logical empiricism was significantly influenced by recent developments within neo-Kantian epistemology – and especially by the Marburg School of neo-Kantianism represented by Cohen, Natorp, and Cassirer – is therefore evident (see notes 16 and 18). This influence is seen most clearly, however, in the first work on epistemology produced within the Vienna Circle, Carnap's *Der logische Aufbau der Welt* (1928). Although conventional wisdom has portrayed Carnap's *Aufbau* as the epitome of the logical positivists' supposed empiricist foundationalism,[19] more recent historical research has shown that this picture, too, is seriously oversimplified and that the influence of Marburg neo-Kantianism, in particular, is perhaps even more significant.[20]

For Carnap, the neo-Kantianism of the Marburg School had been given its most satisfactory and significant formulation in Cassirer's *Substance and Function* (1910). The burden of this work is to argue that modern developments in logic, the foundations of mathematics, and mathematical physics show that the traditional theory of the concept, based on Aristotelian syllogistic logic, is entirely inadequate – and, as a result, that the traditional epistemological conceptions of both rationalism and empiricism are entirely inadequate as well. On the one hand, Aristotelian subject-predicate logic mistakenly privileges the relation between substance and accident, and it is the attempt to develop an a priori ontology based on this privileged relation that is characteristic of traditional rationalism. On the other hand, however, traditional empiricism is equally dependent on Aristotelian logic in mistakenly privileging the procedure of concept formation by abstraction, whereby we inductively ascend from sensory particulars to ever higher superordinate concepts (genera and species) predicated of these particulars. Modern logic has shown the poverty of both views, according to Cassirer, by developing a new theory of the concept based on the mathematical notion of function or relation – a theory of what we would now call "abstract relational structures" (the series of natural numbers, for example, or the abstract structure exemplified by Euclidean space).[21] In developing an alternative theory of knowledge and reality, Cassirer then rejects empiricist and inductivist accounts of scientific knowledge in favor of the so-called genetic conception of knowledge characteristic of the Marburg School. Empirical science proceeds by progressively embedding natural phenomena in an ordered sequence of relational structures as we successively articulate and refine mathematical representations of these phenomena in the historical development of our theories. This procedure results in an infinite, never-ending sequence of relational structures, but one that is nonetheless converging on a limit structure or limit theory representing the ideal completion of scientific progress. The object of scientific knowledge is thus never completely given: it is only successively approximated in the limit as the ideal X toward which our mathematical representations of nature are converging.[22]

Carnap, in the *Aufbau*, shares the ambition of replacing all forms of traditional epistemology – theories of knowledge and its relation to reality – with a new approach based on the modern logical theory of relations. Indeed, Carnap (1928, §3) initially characterizes the method to be followed as "the analysis of reality with the help of the theory of relations." Moreover, when Carnap first introduces the question of the basic or fundamental relations on which his "constitutional system of reality" is to be erected, he cites Cassirer (1910) as showing the necessity of formally defined relational concepts for ordering the "undigested experiential given" favored by "positivism."[23] Carnap thus hopes to achieve a synthesis of empiricism and Kantianism – a synthesis that emphasizes, as does the Marburg School, the absolute indispensability of logico-mathematical formal structures for underwriting the clarity, precision, and intersubjective communicability of empirical scientific knowledge.[24]

Carnap also follows the Marburg School in representing empirical knowledge by a serial or stepwise sequence of formal logical structures, depicting, in an idealized fashion, how our scientific methods for acquiring knowledge actually play out in practice. This sequence does not represent the historical progression of mathematical-physical successor theories, however, but rather the epistemological progress of a single individual or cognitive subject as its knowledge extends from the initial subjective sensory data belonging to the *autopsychological* realm, through the world of public external objects constituting the *physical* realm, and finally to the intersubjective and cultural realities belonging to the *heteropsychological* realm. Carnap's methodological series is thus a "rational reconstruction" of the actual present state of scientific knowledge intended formally to represent the "actual process of cognition."[25] For Carnap, moreover, this is not a series of successor theories in the historical progress of mathematical natural science, but rather *a sequence of levels or ranks in the hierarchy of logical types* of Whitehead's and Russell's *Principia Mathematica* (1910–13),[26] a sequence of levels *ordered by type-theoretic definitions*. Objects on any level (other than the first) are thus formally defined as classes of objects (or relations between objects) from the preceding level.

The "logicization" of empirical scientific knowledge undertaken by the Marburg School is thereby implemented in an even more radical fashion. For the historically oriented epistemology of the Marburg tradition – which proceeds largely by the methods of intellectual history – is here transformed into a purely formal exercise: the project of formally presenting the logical definitions of all objects of (current) scientific knowledge subsisting at the various levels of Carnap's constitutional system. And, in the course of this formal exercise, Carnap is able, by means of the theory of types, to transcend the Marburg doctrine of the essentially incomplete character of the object of scientific knowledge – its character, that is, as a never to be completed X. For Carnap, all objects whatsoever are defined or "constituted" at *definite finite ranks* within the hierarchy of logical types, and it is only the further empirical specification of these objects that remains essentially incomplete. As a result, Carnap is also able to reject the Kantian conception of synthetic a priori principles, for objects are defined or constituted by *stipulation* and then further investigated by *experience*: "[a]ccording to the conception of constitutional theory there are no other components in cognition than these two – the conventional and the empirical – and thus no synthetic a priori [components]."[27]

In a direct engagement with neo-Kantian epistemology, Carnap thereby arrives at the same point that was reached in the context of the logical empiricists' earlier engagement with the foundations of geometry and relativity theory: Kant's original conception of fixed synthetic a priori principles governing our empirical scientific knowledge is to be replaced by Poincaré's notion of convention, so that, in particular, the principles in question are no longer necessarily fixed but become "optional," subject to choice, and relative or internal to a specific scientific context. Thus Carnap here stands on the brink of his mature philosophy of linguistic frameworks,[28] which, as we saw at the outset, has deep affinities with the Kuhnian theory of scientific revolutions. This philosophy, as we now see, can be viewed as a kind of generalization and logicization of the conception of relativized a priori principles developed by Reichenbach (1920),[29] resulting from Carnap's simultaneous engagement with both the details of neo-Kantian epistemology and the most recent developments in modern mathematical logic.[30]

It is noteworthy, once again, that Kuhn, toward the end of his career, explicitly acknowledges the Kantian and neo-Kantian background to the development of logical empiricism and the resulting parallels with his own views. In particular, commenting on Reichenbach's distinction between two meanings of the a priori (fixed and unrevisable versus constitutive relative to a theory), Kuhn remarks that "[b]oth meanings make the world in some sense mind-dependent, but the first disarms the apparent threat to objectivity by insisting on the absolute fixity of the categories, while the second relativizes the categories (and the experienced world with them) to time, place, and culture." And he continues in an important passage worth quoting in full:

> Though it is a more articulated source of constitutive categories, my structured lexicon [Kuhn's late version of "paradigm"] resembles Kant's a priori when the latter is taken in its second, relativized sense. Both are constitutive of *possible experience* of the world, but neither dictates what that experience must be. Rather, they are constitutive of the infinite range of possible experiences that might conceivably occur in the actual world to which they give access. Which of these conceivable experiences occurs in that actual world is something that must be learned, both from everyday experience and from the more systematic and refined experience that characterizes scientific practice. They are both stern teachers, firmly resisting the promulgation of beliefs unsuited to the form of life the lexicon permits. What results from respectful attention to them is knowledge of nature, and the criteria that serve to evaluate contributions to that knowledge are, correspondingly, epistemic. The fact that experience within another form of life – another time, place, or culture – might have constituted knowledge differently is irrelevant to its status as knowledge.[31]

Kuhn, like the logical empiricists, has thus adopted a relativized conception of Kantian a priori principles. However, since Kuhn's perspective, unlike that of the logical empiricists, is essentially historical (their a priori is relativized to a theory or linguistic framework, not to a "time, place, or culture"), he also raises (and here rather abruptly dismisses) the central historicist problem concerning the social and cultural relativity of scientific knowledge that dominates post-Kuhnian work in science studies.[32]

Let us now take a brief look at the background to Kuhn's theory of scientific revolutions. Although there has not yet been much study of the development of Kuhn's views, Kuhn has left some intriguing hints.

Thus, in the Preface to *Structure*, Kuhn portrays how he shifted his career plans from physics to the history of science, and, in explaining his initial intensive work in the subject, he states that he (1970, pp. v–vi) "continued to study the writings of Alexandre Koyré and first encountered those of Emile Meyerson, Hélène Metzger, and Anneliese Maier [; more] clearly than most other recent scholars, this group has shown what it was like to think scientifically in a period when the canons of scientific thought were very different from those current today." Then, in the introductory first chapter on "A Role for History," Kuhn explains the background to his rejection of the development-by-accumulation model:

> [H]istorians of science have begun to ask new sorts of questions and to trace different, and often less than cumulative, developmental lines for the sciences. Rather than seeking the permanent contributions of an older science to our present vantage, they attempt to display the historical integrity of that science in its own time. They ask, for example, not about the relation of Galileo's views to those of modern science, but rather about the relationship between his views and those of his group, i.e., his teachers, contemporaries, and immediate successors in the sciences. Furthermore, they insist upon studying the opinions of that group and other similar ones from the viewpoint – usually very different from that of modern science – that gives those opinions the maximum internal coherence and the closest possible fit to nature. Seen through the works that result, works perhaps best exemplified in the writings of Alexandre Koyré, science does not seem altogether the same enterprise as the one discussed by writers in the older historiographic tradition.[33]

Kuhn, not surprisingly, thus places himself squarely within the historiographical tradition initiated by Koyré in his works on Galileo first published in 1939 – a tradition that established the history of science as an independent discipline in the immediate postwar period.[34]

In a survey article on the development of the history of science, Kuhn (1968) again explains the initial break with the development-by-accumulation model, which began, according to Kuhn, with "the influence, beginning in the late nineteenth century, of the history of philosophy." We here learned an "attitude towards past thinkers," Kuhn explains, that

> came to the history of science from philosophy. Partly it was learned from men like Lange and Cassirer who dealt historically with people or ideas that were also important for scientific development. . . . And partly it was learned from a small group of neo-Kantian epistemologists, particularly Brunschvicg and Meyerson, whose search for quasi-absolute categories of thought in older scientific ideas produced brilliant genetic analyses of

concepts which the main tradition in the history of science had misunderstood or dismissed.[35]

Finally, in a "Historiographic/Philosophical Addendum," concluding his response to criticisms of his work on Planck and black-body theory, Kuhn (1984) makes some further intriguing remarks. Responding to questions about the relationship between his work on Planck and the theory of scientific revolutions presented in *Structure*, Kuhn (1987, p. 361) explains that "[t]he concept of historical reconstruction that underlies [the Planck book] has from the start been fundamental to both my historical and my philosophical work[; it] is by no means original: I owe it primarily to Alexandre Koyré; its ultimate sources lie in neo-Kantian philosophy."

What does Kuhn mean here by "neo-Kantian epistemology" and "neo-Kantian philosophy"? It is not entirely clear. Whereas, as we have seen, Cassirer is certainly a leading figure in early-twentieth-century neo-Kantianism, and it is also very plausible to locate Maier, in particular, in the context of Kantian and neo-Kantian thought,[36] the other figures on Kuhn's list can be referred to as "neo-Kantians" only by making more or less of a stretch. To be sure, they agree in rejecting naive empiricist accounts of the development of modern science (and thus the development-by-accumulation model) and emphasize instead the fundamental importance of mind sets, conceptual frameworks, or "mentalities" contributed by thought itself.[37] At the same time, however, several of these figures make a point of taking issue with Kantian and neo-Kantian ideas, both philosophically and with reference to the interpretation of the history of science.[38] But perhaps there is, nonetheless, something importantly right in Kuhn's assertion that the "ultimate sources [of his concept of historical reconstruction] lie in neo-Kantian philosophy." For all the figures on his list, in one way or another, are taking inspiration from, and reacting to, Cassirer's seminal work on the history of modern science and philosophy, *Das Erkenntnisproblem* [*The Problem of Knowledge*] (1906–7).[39]

Das Erkenntnisproblem is the first work of intellectual history to develop a detailed reading of the seventeenth-century scientific revolution in terms of the "Platonic" idea that the thoroughgoing application of mathematics to nature (the so-called mathematization of nature) is the central and overarching achievement of this revolution.[40] Cassirer simultaneously articulates an interpretation of the history of modern philosophy as the development and eventual triumph of what he calls "modern philosophical idealism." This tradition takes its inspiration from idealism in the Platonic sense, from an appreciation for the "ideal" formal structures paradigmatically studied in mathematics, and it is distinctively modern in recognizing the fundamental importance of the systematic application of such structures to empirically given nature in modern mathematical physics – a progressive and synthetic process wherein mathematical models of nature are successively refined and corrected without limit. For Cassirer, it is Galileo, above all, in opposition to both sterile Aristotelian-Scholastic formal logic and sterile Aristotelian-Scholastic empirical induction, who first grasped the essential structure of this synthetic process; and the development of "modern philosophical idealism" in the work of Descartes, Spinoza, Gassendi, Hobbes, Leibniz, and Kant then consists in its increasingly self-conscious philosophical articulation and elaboration. Cassirer therefore interprets the development of modern thought as a whole from the point of view of the philosophical perspective of Marburg neo-Kantianism. In particular, he here anticipates his own systematic work in *Substance and Function* by interpreting the characteristically modern conception of nature as the triumph of the mathematical-relational concept of *function* – as expressed in the universal laws of mathematical physics – over the traditional Aristotelian concept of *substance*.

Yet Meyerson, who is clearly the next most seminal figure on Kuhn's list of inspirational precursors,[41] takes a quite different view. He agrees with Kant and the neo-Kantians on the necessity for a priori requirements of the mind to give meaning and structure to the results of empirical science. But he is vehemently opposed to the attempt to assimilate scientific understanding to the formulation of universal laws governing phenomena. Indeed, the central thought of his *Identity and Reality* (1930, first published in 1908) is that genuine scientific knowledge and understanding can never be the result of mere lawfulness (*légalité*) but must instead answer to the mind's a priori logical demand for identity (*identité*). And the primary requirement resulting from this demand is precisely that some underlying *substance* be conserved as absolutely unchanging and self-identical in all sensible alterations of nature. Thus, the triumph of the scientific revolution, for Meyerson, is represented by the rise of mechanistic atomism, wherein elementary corpuscles preserve their sizes, shapes, and masses while merely changing their mutual positions in uniform and homogeneous space via motion; this same demand for transtemporal identity is also represented, in more recent times, by Lavoisier's use of the principle of the conservation of matter in his new chemistry and by the discovery of the conservation of energy. However, in the even more recent discovery of what we now know as the second law of thermodynamics ("Carnot's principle"), which governs the *temporally irreversible* process of "degradation" or "dissipation" of energy, we encounter nature's complementary and unavoidable resistance to our a priori logical demands. In the end, therefore, Meyerson views the development of natural science as progressing via a perpetual dialectical opposition between the mind's a priori demand for substantiality and thus absolute identity through time, on the one side, and nature's "irrational" a posteriori resistance to this demand, on the other.

In the work of Cassirer and Meyerson, then, we find two sharply diverging visions of the history of modern science. For Cassirer, this history is seen as a process of evolving rational purification of our view of nature, as we progress from naively realistic "substantialistic" conceptions, focusing on underlying substances, causes, and mechanisms subsisting "behind" the observable phenomena, to increasingly abstract, purely "functional" conceptions, in which we abandon the search for underlying ontology in favor of ever more precise mathematical representations of phenomena in terms of exactly formulated universal laws. For Meyerson, by contrast, this same history is seen as a necessarily dialectical progression (in something like the Hegelian sense), wherein reason perpetually seeks to enforce precisely the substantialistic impulse, and nature continually offers resistance via the ultimate irrationality of temporal succession. It is by no means surprising, therefore, that Meyerson, in the course of considering, and rejecting, "anti-substantialistic conceptions of science," explicitly takes issue with Cassirer's characteristic claim that "[m]athematical physics turns aside from the essence of things and their inner substantiality in order to turn towards their numerical order and connection, their functional and mathematical structure."[42] And it is also no wonder, similarly, that Cassirer, in the course of his own discussion of "identity and diversity, constancy and change," explicitly takes issue with Meyerson's views by asserting that "[t]he identity towards which thought progressively strives is not the identity of ultimate substantial things but the identity of functional orders and coordinations."[43]

It is especially striking, in view of this sharp divergence, that Koyré, in particular, emphatically places himself on the side of Meyerson. Indeed, his *Galileo Studies* is dedicated to Meyerson, and Koyré's allegiance to Meyerson's position in the dispute with Cassirer clearly emerges, if only implicitly, in Koyré's criticism of Cassirer's "excessively Kantian" reading of Galileo's "Platonism."[44] That this criticism does not merely concern the interpretation of Galileo, however, is explicitly expressed in an earlier paper explaining and defending Meyerson's philosophy to a German audience. Specifically, Koyré (1931) defends Meyerson's conception against the "anti-substantialistic" pretensions of neo-Kantianism, according to which "science has nothing to do with substantial causes, but is occupied only with constructing functional dependencies, functional interconnections of the phenomena and clothing them in mathematical formulas."[45] While science does aim at mathematical laws, of course, this is not the ultimate goal of the rational comprehension of phenomena required by thought. Here Meyerson, following the ancient tradition initiated by Parmenides and Plato, is perfectly correct: the demand for rational comprehension can be satisfied only by absolute unity and self-identity. Yet, as Plato – and, following him, Hegel – clearly saw, the reality with which thought is confronted is essentially irrational. In particular, temporal succession is ultimate and irreducible, and reality itself is a necessary mixture of (rational) "sameness" and (irrational) "otherness." In the end, therefore, Koyré, despite his well-known emphasis on rationalism and the mathematization of nature, is a Meyersonian. His "Platonism" – in explicit opposition to the more Kantian version articulated by Cassirer – is clearly and firmly based on a recognition of the *limits* of mathematical thought.[46]

The historiographical tradition Kuhn attempts to assimilate in his theory of scientific revolutions (see note 33) is thus by no means unitary and uncontentious. On the contrary, it is characterized by a deep philosophical opposition between a mathematical idealist tendency taking its inspiration from Kant and a more realistic, substantialistic tendency taking its inspiration – via the thought of Meyerson – from a mixture of Platonic, Cartesian, and Hegelian ideas. The former tendency, following Kant, renounces the ambition of describing an ontological realm of substantial things subsisting behind the empirical phenomena in favor of a rigorous mathematical description of the lawlike relations among the phenomena themselves. It differs from Kant, however, in recognizing that no particular mathematical structures (such as those of Euclidean geometry and Newtonian physics) are necessarily instantiated in the phenomena, and, accordingly, it portrays the rationality and universality of scientific progress as a historical evolution marked by a continuous unfolding and generalization of the powers of mathematical thought.[47] The latter tendency, by contrast, maintains precisely an ontology of substantial things, and, accordingly, it emphatically rejects the attempt to reduce the task of science to the formulation of precise mathematical laws. It thus ends up with a more pessimistic reading of the history of modern science in which our demand for fundamentally ontological rational intelligibility is met by an inevitable resistance to this demand arising from the irrational, essentially temporal character of nature itself.[48]

If I am not mistaken, this deep philosophical tension is echoed in Kuhn's theory of scientific revolutions, particularly where he considers the question of continuity over time at the *theoretical* level. Here Kuhn shows himself, in this respect, to be a follower of the Meyersonian tendency, for he consistently gives the question an ontological rather than a mathematical interpretation. Thus, for example, when Kuhn considers the relationship between relativistic and Newtonian mechanics, in explicit opposition to what he calls "early logical positivism," he rejects the notion of a fundamental continuity between the two theories on the grounds that the "physical reference" of their terms is essentially different;[49] and he nowhere considers the contrasting idea, characteristic of the Marburg School, that continuity of relevant mathematical structures might be sufficient. Moreover, Kuhn consistently gives an ontological rather than a mathematical interpretation to the question of theoretical convergence over time: the question is always whether our theories can be said to converge to an independently existing "truth" about reality, to a theory-independent external world.[50] By contrast, as we have seen, the Marburg School rejects this realistic reading of convergence at the outset: our theories do not (ontologically) converge to a mind-independent realm of substantial things; they (mathematically) converge *within* the historical progression of our theories as they continually approximate, but never reach, an ideally complete mathematical representation of the phenomena.

Our examination of the development of both logical empiricism and Kuhn's theory of scientific revolutions took its starting point from the affinities between Kuhn's theory and Carnap's philosophy of linguistic frameworks. We have now seen that these affinities are in no way accidental but rather reflect an early-twentieth-century intellectual situtation encompassing both the history and the philosophy of science. All the thinkers we have considered agreed, on broadly Kantian grounds, in rejecting naive empiricist epistemology in favor of an emphasis on demands set by the mind itself, and virtually all (with the possible exception of Meyerson) departed from Kant in recognizing that the resulting mind sets, conceptual frameworks, or mentalities significantly evolve throughout the development of the sciences and are thus relative to or dependent on a given stage of theoretical progress. The logical empiricists, in particular, were closest, in this respect, to the Marburg neo-Kantianism articulated in the work of Cassirer, wherein the conceptual frameworks in question are exemplified in their purest form in the development of modern mathematics, mathematical physics, and mathematical logic. The logical empiricists went one step further than Cassirer, however, in their ambition to formulate philosophy, too, as a branch of exact mathematical science – that is, as *Wissenschaftslogik*. In this way, as we have seen, they removed the history of science from the purview of philosophy. And it was then Kuhn's great merit, against this common background, to have reinstated the history of science as perhaps the most important object considered in the philosophy of science. As we have also seen, however, this very "historicization" of the philosophy of science inevitably raised the problem of social and cultural relativism that dominates post-Kuhnian discussion today. The question arises, then, of whether it is possible to address this problem in a more satisfactory way by continuing to emphasize the importance of developments in modern mathematics, mathematical physics, and mathematical logic (as in both logical empiricism and the Marburg School) while simultaneously recognizing the importance of the factual historical evolution of the sciences (as in both the Marburg School and the historiographical tradition leading up to, and including, the work of Kuhn). But a further consideration of this question will have to wait for another occasion.[51]

Notes

1. Kuhn (1970) begins by rejecting this model in chapter 1, "A Role for History," although he does not there explicitly associate it with logical empiricism. In chapter 9, "The Nature and Necessity of Scientific Revolutions," however, he rejects the view, "closely associated with early logical positivism," that "would restrict the range and meaning of an accepted theory so that it could not possibly conflict with any later theory that made predictions about some of the same natural phenomena" (p. 98). Logical positivism is supposed to do this, of course, by holding that the meaning of a theory is exhausted by its logical implications within a class of theory-neutral observation sentences.
2. See, for example, Giere (1988, p. 32): "Kuhn's *Structure of Scientific Revolutions*... was a major contributor to the decline of logical empiricism beginning in the 1960s.... Of course, already in 1962 other works even more directly critical of logical empiricism had been published, including some appealing to the history of science. But Kuhn was the only theorist at the time to provide an alternative overall framework in which to investigate the nature of science." A similar viewpoint is found in the Introduction to Suppe (1977), where logical empiricism is characterized as "the Received View" to which more recent views (including Kuhn's) are opposed. See also Rorty (1979, pp. 59, 332–3).
3. For both of these quotations see Reisch (1991, pp. 266–7).
4. This philosophy of linguistic frameworks, including the sharp distinction between internal and external questions, is formulated most explicitly in Carnap (1950). The basic ideas go back to Carnap (1934).
5. For discussion and references see Reisch (1991, pp. 270–4).

6. For Carnap, the standard procedure of testing a scientific theory by the deduction of observational predictions fails, in the end, to be entirely governed by "established rules." For, as Carnap (1934, §82) explains, when faced with a conflict between theoretical predictions and observational results, we have three options open to us: reject the theoretical sentences from which the unsuccessful predictions are derived, reject the observational reports that conflict with the theory in question, or alter the logical rules of the language so that there is no longer an inconsistency between observation and theory. In the same section, Carnap explicitly links his viewpoint with the epistemological holism he associates with Duhem and Poincaré. This makes it especially clear, in particular, how far Carnap's philosophy is from traditional empiricist foundationalism.
7. These affinities between Carnap and Kuhn are discussed by several authors in addition to Reisch. See Friedman (1992a), Earman (1993) – which follows Reisch – and Friedman (1993).
8. Kuhn (1993, p. 313). Kuhn is here responding to the second two papers cited in note 7.
9. Carnap (1936) explicitly proposes *Wissenschaftslogik* as a replacement for all forms of traditional epistemology. See also Carnap (1934, §72): "The alleged peculiarly philosophical point of view, from which the objects of science are supposed to be considered, is abolished, just as the alleged peculiarly philosophical stratum of objects was already previously eliminated. Aside from the questions of the individual special sciences, the only questions that remain as genuinely scientific questions are those of the logical analysis of science – its sentences, concepts, theories, etc. We will call this complex of questions *Wissenschaftslogik*. . . . *Taking the place of the inextricable tangle of problems that is known as philosophy is Wissenschaftslogik*."
10. Again, one finds this picture in the three representatives of the conventional wisdom cited in note 2. Ayer (1936) is largely responsible for its initial formulation and promulgation. In general, this view of the background to logical empiricism is most frequently articulated, by both defenders and critics, within the Anglo-American philosophical tradition – including, for example, the sympathetic commentaries and criticisms of Nelson Goodman and W. V. Quine (see note 19).
11. Suppe (1977, pp. 6–15) notes the importance of the Marburg School within the scientific epistemology of the time, but he then goes on to associate the origins of logical empiricism exclusively with Machian positivism and other more empiricist tendencies of thought.
12. A good selection of Helmholtz's papers on geometry and scientific epistemology can be found in Cohen and Elkana (1977). For further discussion see Friedman (1997, 2000a).
13. Poincaré (1902, 1905, 1908) is the classical source of his scientific epistemology. For further discussion see Friedman (1996, 2000a).
14. For further discussion see Friedman (2002).
15. For further discussion see Friedman (1983, 2002).

16. Carnap retrospectively sums up his position as follows (1963, p. 12): "Knowledge of intuitive space I regarded at that time, under the influence of Kant and the neo-Kantians, especially Natorp and Cassirer, as based on 'pure intuition' and independent of contingent experience. But, in contrast to Kant, I limited the features of intuitive space grasped by pure intuition to certain topological properties; the metrical structure (in Kant's view, the Euclidean structure)... I regarded not as purely intuitive, but rather as empirical. Knowledge of physical space I already considered as entirely empirical, in agreement with empiricists like Helmholtz and Schlick." As we have seen, the "entirely empirical" in the last sentence is actually quite misleading – and signifies only that Carnap here follows "empiricism" in rejecting Kant's original conception of the fixed synthetic a priori character of the metric of physical space. For further discussion see Friedman (1995; 2000b, chapter 5).
17. See Reichenbach (1920, chapter 5).
18. See Reichenbach (1920, note 20), which refers to Cassirer (1921). (Similarly, Cassirer acknowledges Reichenbach's work in a prepublication note added in proof to his book.) The relationships among Schlick's, Carnap's, and Reichenbach's developing conceptions of the foundations of geometry are quite complex. In particular, following Schlick's insistence, both Carnap and Reichenbach came to drop all references to Kant and to characterize their views under the simple rubric of "empiricism" (compare note 16). For further discussion see Friedman (1994).
19. See, e.g., Goodman (1951, 1963) and Quine (1951, 1969).
20. For growing awareness of the more Kantian roots of the *Aufbau* see Haack (1977), Moulines (1985), Sauer (1985, 1989) – Sauer gives particular emphasis to the influence of the Marburg School – Friedman (1987, 1992b), and Richardson (1992, 1998). See also Friedman (2000b, chapter 5).
21. Cassirer refers, in this context, to the work of Richard Dedekind, Gottlob Frege, David Hilbert, and especially Bertrand Russell (1903).
22. For further discussion of Cassirer and the Marburg School see Friedman (2000b, chapters 3 and 6).
23. Carnap (1928, §75). For further discussion of this and other passages expressing Carnap's agreement with Cassirer and the Marburg School see the works of Sauer, Richardson, and myself cited in note 20.
24. Compare the description of his work as a synthesis of traditional empiricism and rationalism in the Preface to the second edition of Carnap (1928).
25. See Carnap (1928, §§100, 143).
26. This work, for Carnap and the logical empiricists, represented the definitive formulation of modern mathematical logic. Its theory of logical types went far beyond the theory of relations presented in Russell (1903), which alone was known to Cassirer (see note 21).
27. Carnap (1928, §179). Here Carnap also explains the corresponding divergence from the Marburg School: "According to the conception of the *Marburg School* [Carnap refers here to Natorp (1910)] the object is the eternal X, its determination is an incompleteable task. In opposition to this it is to be noted that finitely

many determinations suffice for the constitution of the object – and thus for its univocal description among the objects in general. Once such a description is set up the object is no longer an X, but rather something univocally determined – whose complete description then certainly still remains an incompleteable task."

28. As I indicated in note 9, Carnap's mature standpoint adopts *Wissenschaftslogik* as the substitute for all forms of epistemology, including the epistemology of the *Aufbau* – which, from Carnap's new standpoint, is still inappropriately committed to a basis in "private experience." For further discussion see Richardson (1996) and Friedman (1992b, §IV).
29. For further discussion of the relationship between Carnap's philosophy of linguistic frameworks and Reichenbach (1920) see Friedman (1994).
30. In particular, whereas the *Aufbau* deals only with the theory of types, *Logical Syntax* is responding to the so-called foundations crisis of the late 1920s involving logical systems differing essentially from the "logicist" system of Whitehead and Russell, such as the intuitionistic logic of Brouwer and the "formalism" of Hilbert. Carnap's conclusion is that, just as the earlier crisis in the foundations of geometry had been resolved through the insight that no particular geometry – whether Euclidean or non-Euclidean – is the true one, there is now similarly no question of a single true logic. For further discussion see Friedman (1999, chapter 9). Here especially see also Coffa (1991), which is a posthumous publication of Coffa's pioneering work on the history of logical positivism specifically oriented around the parallel between conventionalism in the foundations of geometry and Carnap's philosophy of linguistic frameworks.
31. Kuhn (1993, p. 331). (Kuhn is here responding to the last article cited in note 7.) There is a special irony in the circumstance that it is precisely the logical positivists' engagement with Einstein's theory of relativity that led, as we have seen, to the affinities between their conception and Kuhn's; for Kuhn (1970, chapter 9) appeals to Einstein's theory, in particular, to combat the supposedly naive empiricism of "early logical positivism" (compare note 1).
32. For discussion of this problem in its historical and philosophical context see Friedman (1998).
33. Kuhn (1970, p. 3). The passage concludes: "By implication, at least, these historical studies suggest the possibility of a new image of science. This essay aims to delineate that image by making explicit some of the new historiography's implications."
34. See Koyré (1978). Kuhn (1970, p. vi) also cites Meyerson (1930, first published in 1908), Metzger (1923, 1930), and Maier (1949).
35. Kuhn (1977, pp. 107–8). In the same pages Kuhn cites the work of E. A. Burtt and A. Lovejoy and refers to "the modern historiography of science" founded by "E. J. Dijksterhuis, Anneliese Maier, and especially Alexandre Koyré." (I am grateful to Alan Richardson for calling my attention to this passage.)
36. Maier's father was an influential Kantian philosopher of the time, and her first published work, her dissertation (1930), is a historical examination of a Kantian