Scientific Philosophy and the Dynamics of Reason

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Scientific philosophy—in the sense of an attempt to revolutionize philosophy as a whole and as a discipline under the guidance of such an idea—is a characteristically twentieth century phenomenon, but, like so many aspects of twentieth century culture, it has deep roots in the nineteenth century. In particular, the idea of a scientific philosophy first arose in the second half of the nineteenth century, as a self-conscious intellectual reaction to what was then perceived as the "speculative" and "metaphysical" excesses of post-Kantian German idealism. In this context, a number of late nineteenth century natural scientists and mathematicians, faced with radically new problems within the shifting intellectual foundations of their own disciplines, turned to philosophy for conceptual resources. Repelled by the speculative metaphysics of post-Kantian idealism, they looked in what they took to be healthier philosophical directions—partly towards a return to the more sober, more scientifically oriented philosophizing of Kant himself, and partly towards a parallel return to the earlier anti-metaphysical stance represented by British empiricism. But the late nineteenth century scientific thinkers in question—principally Hermann von Helmholtz, Ernst Mach, and Henri Poincaré—were also faced with the problem of adapting such earlier philosophical ideas to a revolutionary new situation within the mathematical and physical sciences. Both Kantianism and empiricism had now to be adjusted to the development of non-Euclidean geometries, closely related fundamental changes in logic and the foundations of mathematics, the emergence of physiology and psycho-physics as experimental scientific disciplines, and novel, non-Newtonian styles of physical theorizing in such new areas as thermodynamics, the statistical-molecular theory of matter, and electro-magnetism.

In the early years of the twentieth century a new philosophical movement arose against this fertile background. Coming of age in the heady days of the Weimar Republic, the movement we now know as logical empiricism was primarily centered in Vienna and Berlin. In Vienna Moritz Schlick—a student of Max Planck's and an early

apologist for and expositor of Albert Einstein's theory of relativity—took the Chair for the Philosophy of the Inductive Sciences originally held by Mach. A Philosophical Circle—now known as the Vienna Circle—quickly formed around Schlick, including such figures as Rudolf Carnap, Otto Neurath, Kurt Gödel, and Friedrich Waismann. At the University of Berlin a complementary philosophical circle, the Society for Empirical Philosophy, gathered around Hans Reichenbach (also an early apologist for and expositor of Einstein's theory), including such figures as Walter Dubislav, Kurt Grelling, and Carl Hempel. This new form of scientific philosophy—logical empiricism—took Einstein's theory of relativity as the culmination of the late nineteenth century developments in both the sciences and scientific philosophy exemplified in the thought of Helmholtz, Mach, and Poincaré; and its aim, accordingly, was to effect a similar revolutionary transformation of philosophy as a whole. Unlike Helmholtz, Mach, and Poincaré, however, Schlick, Carnap, and Reichenbach, although originally trained within the sciences, were themselves professional philosophers, and their attention had shifted from specific problems within the sciences to the problem of laying new foundations for the discipline of philosophy that would enable it, like the sciences, to achieve cumulative consensus and stable results entirely unencumbered by the sterile and endless controversies afflicting traditional metaphysics.

The logical positivist movement reached its apogee in Europe in the years 1928-34, but the Nazi seizure of power in 1933 marked the effective end of this phase. Thereafter, however, many of its most important representatives—including Reichenbach and Carnap—emigrated to the United States. Reichenbach, who had fled to Istanbul in 1933, moved in 1938 to UCLA. Carnap, who had taken a position in Prague in 1931, moved to the University of Chicago in 1935; and, after Reichenbach's death in 1953, Carnap took over his position at UCLA beginning in 1954 (where he remained until his death in 1970). Reichenbach's last book, published in 1951, was a popular work, *The Rise of Scientific Philosophy*, intended persuasively to encapsulate the results of the new philosophy for a general audience. Writing with characteristic clarity and verve, and with very few sacrifices of accuracy and rigor, Reichenbach fully succeeded in this aim—as can be inferred from the fact that the book was quickly translated into a large number of

languages throughout the world, including German, French, Spanish, Swedish, Italian, Japanese, Polish, Yugoslavian, and Korean (all between 1953 and 1960).

I will focus the argument I want to make today on this last book of Reichenbach's, because of the clarity and widespread influence of this particular presentation of the aims and methods of twentieth century scientific philosophy—authored, in addition, by one of the founders and most accomplished practitioners of this philosophy. And this contrast sharply, for example, with that other "best seller" of logical empiricism—A. J. Ayer's *Language Truth and Logic* (1936)—for Ayer himself was neither a founder nor a particularly accomplished practitioner of the new scientific philosophy he did so much to popularize. Moreover, I myself have a personal connection with twentieth century scientific philosophy and with Reichenbach: my dissertation advisor (at Princeton) was Clark Glymour; Glymour's dissertation advisor (at Indiana) was Wesley Salmon; and Salmon's dissertation advisor (at UCLA) was Hans Reichenbach. I thus bear what scientifically minded modern logicians call the ancestral of the graduate student relation to Reichenbach. Since completing my dissertation I have become increasingly involved with studying the history of scientific philosophy, and, on the basis of this study, I have recently arrived at a revisionist understanding of the subject which, as we shall see, would be anathema from the point of view of Reichenbach's book.[*]

Reichenbach vividly states the point of his book in the very first paragraph of the Preface:

> Philosophy is regarded by many as inseparable from speculation. They believe that the philosopher cannot use methods which establish knowledge, be it knowledge of facts or of logical relations; that he must speak a language which is not accessible to verification—in short, that philosophy is not a science. The present book is intended to establish the contrary thesis. It maintains that philosophic speculation is a passing stage, occurring when philosophic problems are raised at a time which does not possess the logical means to solve them. It claims that there is, and always has been, a scientific approach to philosophy. And it wishes to show that from this ground has sprung a scientific

philosophy which, in the science of our time, has found the tools to solve those problems that in earlier times have been the subject of guesswork only. To put it briefly: this book is written with the intention of showing that philosophy has proceeded from speculation to science. (p. vii)

And, at the end of the book, Reichenbach sums up what he has achieved in terms of now attained "results" of philosophical research comparable to the results of the sciences:

This is a collection of philosophic results which have been established by means of a philosophical method as precise and dependable as the method of science. The modern empiricist may quote these results when he is invited to supply evidence that scientific philosophy is superior to philosophical speculation. There is a body of philosophical knowledge. . . . Philosophy is scientific in its method; it gathers results accessible to demonstration and assented to by those who are sufficiently trained in logic and science. If it still includes unsolved problems subject to controversy, there is good hope that they will be solved by the same methods as those which, for other problems, have led to solutions commonly accepted today. (p. 308)

The overall structure of the book expresses this same sharp contrast between the "results" of scientific philosophy, on the one side, and the contrastingly unscientific "guesswork" of traditional speculative philosophy, on the other. In the first part, on "The Roots of Speculative Philosophy," Reichenbach discerns two main unscientific temptations that have been responsible for the confusions and mistakes of traditional philosophy: the search for generality and the search for certainty. The first temptation arises when the legitimate search for scientific explanations of particular empirical phenomena under increasingly general empirical laws reaches an impasse at some point, and we are then tempted to invent spurious analogical or pictorial "pseudo explanations" to make up for this defect. The Aristotelian metaphysics of form and matter, and the

Hegelian metaphysics of reason realizing itself in history, are both, according to Reichenbach especially good examples of the temptation in question, and Reichenbach has nothing but contempt, more generally, for this philosophical tendency. The second temptation, by contrast, arises from being excessively impressed with the very real achievements of mathematical science—beginning with the development of axiomatic geometry and mathematical astronomy by the ancient Greeks and continuing up to the great synthesis of these two traditions effected by Newton's *Mathematical Principles of Natural Philosophy* at the end of the seventeenth century. It is no wonder, then, that rationalist philosophers from Plato to Kant, whose philosophies were reflections of the best available mathematical science of their own times, were led to the conviction that we could attain absolutely certain knowledge of reality, as exemplified in pure mathematics, entirely independently of all empirical information supplied by the senses. Nevertheless, the further progress of the mathematical sciences themselves, especially in the nineteenth century, decisively undercut this rationalist vision. In particular, we now know that there are a multiplicity of mathematical geometries—not solely the traditional type of geometry originally axiomatized by Euclid—and that the choice between such geometries must, in the end, by empirical. And we know, in addition, that pure mathematics—as opposed to the applied mathematics coordinated to the physical and empirical world— owes its characteristic form of certainty entirely to its emptiness: it is essentially a purely tautologous or analytic branch of formal logic saying nothing whatsoever about reality.

Reichenbach makes it very clear that he greatly prefers the rationalist philosophies of Plato and Kant to what he takes to be the pseudo scientific, entirely misguided speculations of a Hegel or an Aristotle. He makes it very clear, in particular, that, of all the traditional "speculative" philosophers, Kant was by far the best. For, on the one hand, Kant grasped the problem posed by the existence of mathematical geometry much more clearly than did Plato: the problem is precisely to understand how an apparently pure and entirely a priori science can nevertheless apply to empirical reality. And, on the other hand, Kant appealed to the unprecedentedly successful application of mathematics to empirical nature articulated in Newton's *Principia* to give an exact formulation, for the first time, of both the problem in question and a possible solution. The problem, in Kant's now famous formulation, is how are synthetic a priori propositions—propositions

that are both true independently of experience and necessarily applicable to experience—possible?   And Kant's equally famous solution states that   synthetic a priori propositions—such as the laws of Euclidean geometry and the fundamental laws of motion governing Newtonian dynamics—are possible because they express a priori cognitive structures (a priori forms of intuition and concepts or categories of rational thought) internal to the human mind, on the basis of which alone the mind can order and process the a posteriori empirical data supplied by the senses.

Kant's intimate relation with the best available science of his own time—Newtonian mathematical physics—is responsible for both the strength and the weakness of his philosophical position:

> What makes Kant's position so strong is its scientific background. His search for certainty is not of the mystical type that appeals to an insight into a world of ideas, nor of the type that resorts to logical tricks which extract certainty from empty presuppositions, as a magician pulls a rabbit out of an empty hat. Kant mobilizes the science of his day for the proof that certainty is attainable; and he claims that the philosopher's dream of certainty is borne out by the results of science. From the appeal to the authority of the scientist Kant derives his strength.
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> But the ground on which Kant built was not so firm as he believed it to be. He regarded the physics of Newton as the ultimate stage of knowledge of nature and idealized it into a philosophical system. In deriving from pure reason the principles of Newtonian physics, he believed he had achieved a complete rationalization of knowledge, had attained a goal which his predecessors had been unable to reach. The title of his major work, *Critique of Pure Reason*, indicates his program of making reason the source of synthetic a-priori knowledge and thus to establish as a necessary truth, on a philosophical ground, the mathematics and physics of his day. (pp. 42-3)

The weakness, of course, is that Newtonian mathematical physics has now been overthrown:

> Had Kant lived to see the physics and mathematics of our day he might very well have abandoned the philosophy of the synthetic a priori. So let us regard his books as documents of their time, as the attempt to appease his hunger for certainty by his belief in the physics of Newton. In fact, Kant's philosophical system must be conceived as an ideological superstructure erected on the foundations of a physics modeled for an absolute space, an absolute time, and an absolute determinism of nature. This origin explains the system's success and its failure, explains why Kant has been regarded by so many as the greatest philosopher of all time, and why his philosophy has nothing to say to us who are witnesses of the physics of Einstein and Bohr. (p. 44)

In the end, therefore, we are ultimately left with a new form of scientific empiricism. All the results of modern mathematical physical science, no matter how sophisticated and refined, are nothing but empirical descriptions of our sensory experience, based, like all such descriptions, on inductive generalization from this experience. At the same time, however, we must nevertheless continue to recognize that pure mathematics itself—mathematics considered independently of its application to physical reality—is just as certain and a priori as traditional rationalism always held; it is just that such mathematics, by itself, is nothing but an empty formalism, a purely analytic branch of formal logic.—Hence logical empiricism.

The second part of Reichenbach's book, on "The Results of Scientific Philosophy," continues to develop these themes. The eighth chapter, for example, further articulates the solution offered by the new empiricism to the age-old problem of the nature of geometry—a problem which, as Reichenbach emphasizes, has always provided traditional rationalism with its strongest motivation. Echoing famous words of Einstein's from a celebrated paper entitled "Geometry and Experience" from 1921, the heart of

Reichenbach's solution is a sharp distinction between two essentially different types of geometry, pure or mathematical geometry and applied or physical geometry. The former is an uninterpreted formal calculus having no intrinsic relation with spatial intuition or any other type of experience. Geometry in this sense is not about space at all, but is merely an analytic system of logical implications of the form: *if* the axioms are true, *then* the theorems are as well. Applied or physical geometry, by contrast, results from a particular interpretation of such an axiomatic system set up by coordinating its uninterpreted formal symbols with real objects of experience—for example, the behavior of light rays or rigid rods—and Reichenbach calls such interpretations coordinative definitions. The applied or interpreted geometry that results is then true or false of physical reality, but the question of its truth is now straightforwardly empirical—only experience can tell us whether the resulting physical geometry is Euclidean or non-Euclidean. In no sense, therefore, is geometry synthetic a priori:

> This consideration shows that we have to distinguish between mathematical and physical geometry. Mathematically speaking, there exist many geometrical systems. Each of them is logically consistent, and that is all a mathematician can ask. He is interested not in the truth of the axioms, but in the implications between axioms and theorems: "if the axioms are true, then the theorems are true"—of this form are the geometrical statements made by the mathematician. But these implications are analytic; they are validated by deductive logic. The geometry of the mathematician is therefore of an analytic nature. Only when the implications are broken up, and axioms and theorems are asserted separately, does geometry lead to synthetic statements. The axioms then require an interpretation through coördinative definitions and thus become statements about physical objects; and geometry is thus made into a system which is descriptive of the physical world. In that meaning, however, it is not a priori but of an empirical nature. There is no synthetic a priori of geometry: either geometry is a priori, and then it is mathematical geometry and

analytic—or geometry is synthetic, and then it is physical geometry and empirical. The evolution of geometry culminates in the disintegration of the synthetic a priori. (pp. 139-40)

The famous words of Einstein's which Reichenbach here echoes are: "In so far as the propositions of mathematics refer to reality they are not certain; and in so far as they are certain they do not refer to reality." Einstein, like Reichenbach, here has geometry specifically in mind—and, indeed, the radically new non-Euclidean geometry of variable curvature Einstein has himself just used to describe the physical world in his general theory of relativity. And it is clear, in context, that Einstein, like Reichenbach, intends his statement precisely as a refutation of the Kantian synthetic a priori. I should note, before continuing, that this kind of answer to Kant's theory of geometry became "common knowledge" within those philosophical circles influenced by logical empiricism—but I am going to argue here that there is actually still a sense in which geometry is close to being synthetic a priori after all, and this is an essential first step in my revisionist understanding of scientific philosophy.

The thirteenth chapter of Reichenbach's book, on modern logic, then develops the solution to the problem of the nature of pure or unapplied mathematics offered by the new empiricist philosophy articulated by *logical* empiricism. Unlike the more traditional empiricist theory of pure mathematics found in John Stuart Mill, for example, this new form of empiricism does not hold that pure mathematics is itself empirical—where Mill held that the science of arithmetic, in particular, is just as empirical as any other, recording the most general empirical properties of groups of discrete physical objects, such as pebbles used in counting. On the contrary, as the work of such logicians as Gottlob Frege and Bertrand Russell has now shown, all of mathematics—including especially arithmetic—is, in the end, a branch of formal logic; so it, too, is analytic rather than synthetic a priori:

The construction of symbolic logic made it possible to investigate from a new angle the relations between logic and mathematics. Why do we have two abstract sciences dealing with the products of thought?

The question was taken up by Bertrand Russell and Alfred N. Whitehead, who arrived at the answer that mathematics and logic are ultimately identical, that mathematics is but a branch of logic developed with special reference to quantitative applications. This result was set forth in a lengthy book, written almost completely in the symbolic notation of logic. The decisive step in the proof was made by Russell's definition of number. Russell showed that the integers, the numbers 1, 2, 3, and so forth, can be defined in terms of the fundamental concepts of logic alone. . . . With his reduction of mathematics to logic, Russell completed the evolution which began with the development of geometry and which I described above as a disintegration of the synthetic a priori. Kant believed not only geometry but also arithmetic to be of a synthetic a-priori nature. With his proof that the fundamentals of arithmetic are derivable from pure logic, Russell has shown that mathematical necessity is of an analytic nature. There is no synthetic a priori in mathematics. (pp. 221-2)

However, as Reichenbach himself goes on to point out, the situation is actually considerably more complicated. For the original reduction of arithmetic to logic developed by Frege in the nineteenth century turned out to suffer from a fatal mathematical flaw: the system of logic Frege used turned out to be inconsistent! It was Russell, in a famous letter to Frege of 1902, who first exposed this inconsistency, and Russell's own system, developed in his *Principia Mathematica* written with Whitehead, avoided this particular problem by the so-called theory of types. However, in order then to be able to derive the truths of arithmetic from this essentially weaker logical system, Russell had to introduce controversial new axioms—such as the axiom of infinity—which were not clearly of a logical nature. Since one of the central mathematical features of arithmetic, that there are an infinity of integers, is now simply assumed at the beginning, one may very well wonder how much this "reduction" actually achieves. Moreover, since the axioms Russell has to add to his system are now mathematically quite strong, one may also still wonder whether his system, like Frege's original system,

may not, after all, be inconsistent. How do we know that a logical contradiction may not some day be found? And note that the very same problem arises with respect to geometry: How, even if we avoid the problem of the *truth* of the axioms, do we know that they in fact are *consistent*? Geometry, like arithmetic, assumes an *infinity* of objects, and so the problem is certainly non-trivial.

In the late 1920s the problems arising here shook the world of scientific philosophy to its very foundations. Three positions were developed in response to this "crisis in the foundations of mathematics." Logicism attempted to preserve the insights of Frege and Russell in the face of the new developments. Intuitionism, developed by the Dutch mathematician L. E. J. Brouwer, attempted to address the problem of infinity by weakening the laws of classical logic so that the law of excluded middle is no longer universally valid. Finally, the so-called formalism developed by the great German mathematician David Hilbert attempted to preserve classical logic (and thus classical mathematics) in the face of Brouwer's challenge by looking for a consistency proof of the logical system first articulated by Russell—where this system is now viewed as an entirely uninterpreted formal calculus consisting of strings of formal symbols representing the sentences and logical derivations constructible in this system. To show that the system is consistent is therefore to show that no strings of formal symbols representing derivations in the system can terminate in both a sentence and the negation of this sentence. Unfortunately, however, it was soon proved by Kurt Gödel, in his celebrated incompleteness theorems of 1931, that no such proof of consistency can be given—unless the logical system in which the proof is to be carried out is at least as strong as the very system whose consistency is at issue. And, although this is all familiar ground to those trained within the tradition of scientific philosophy; it is less well known what the logical empiricist *response* to this situation actually was. My revisionist understanding depends on starting with this response and then connecting it with a descendant of Kant's original synthetic a priori.

So how did logical empiricism respond to this situation? Reichenbach himself did not participate; and it fell to Carnap, in his *Logical Syntax of Language* of 1934, to develop an appropriate response. What Carnap came up with, moreover, was astonishingly radical. There is no such thing as the "correct" logical system at all.

Instead, the three classical positions in the foundations of mathematics (logicism, formalism, and intuitionism) are to be reconceived as *proposals* to formulate the total language of science in one or another way—using one or another set of formal rules as providing the underlying logic of this language. Intuitionism is the proposal to use only the weaker rules of the intuitionistic logical calculus, so as thereby to reduce the chances of finding a contradiction at some point. Formalism is the proposal to use the stronger rules of classical logic, but only if an appropriate consistency proof is possible. Logicism, finally, is the proposal to use both classical logic and mathematics, in a formulation that makes it clear that logical and mathematical rules are of the same kind— that they are both, in an appropriate sense, analytic. Since Gödel's results have shown that the consistency proof envisioned by formalism is very unlikely, Carnap himself prefers the logicist proposal. We formulate both classical logic and mathematics within a single system of total science—leaving aside, at this stage, the question of consistency— because this provides us with the simplest and most convenient version of the mathematics needed for empirical science. Our reasons for using classical logic and mathematics are therefore, in the end, purely pragmatic. Nevertheless, even through we can no longer aim to reduce classical mathematics to logic à la Frege and Russell, we still hope to preserve the insight of classical logicism that logical and mathematical sentences, unlike empirical and physical sentences, are analytic—entirely dependent on the meanings of their logical (as opposed to non-logical or descriptive) terms.

I will not pursue these developments further, except to note that the prospects for taking mathematics to be analytic, even in Carnap's greatly attenuated sense, are currently very much in doubt. In particular, Carnap's student W. V. Quine has appealed to further difficulties arising from Carnap's proposal in arguing—in a celebrated paper entitled "Two Dogmas of Empiricism" published in 1951—that there is in fact no distinction between analytic and synthetic sentences after all. So it is clear, in any case, that Reichenbach's presentation of the logical empiricist solution to the problem of the nature of pure mathematics is subject to severe limitations. What I would now like to argue, against this background, is that Reichenbach's presentation of the logical empiricist solution to the nature of applied mathematics—such as applied or physical

geometry—is subject, as well, to parallel limitations; and here we will see how a descendent of Kant's synthetic a priori arises in *both* cases, pure as well as applied.

To see this, it is first necessary to observe that Carnap's solution to the classical debate in the foundations of mathematics developed in *Logical Syntax* can be seen as the natural generalization of a relativized and dynamical version of the original Kantian conception of the a priori developed by Reichenbach himself in his very first published book. In this book, *The Theory of Relativity and A Priori Knowledge*, published in 1920, Reichenbach by no means concludes, as he does in 1951, that "[Kant's] philosophy has nothing to say to us who are witnesses of the physics of Einstein and Bohr." On the contrary, Reichenbach here argues (in 1920) that at least one essential element of the Kantian a priori can still be maintained. In particular, in order to effect the necessary coordination of abstract mathematical structure to concrete empirical reality we need a special class of mathematical-physical principles—coordinating principles or axioms of coordination—whose role is precisely to insure that the coordination we are attempting to set up is uniquely defined. And such principles, Reichenbach argues, are therefore to be sharply distinguished from mere empirical laws—which Reichenbach calls axioms of connection. In Newtonian physics, for example, the coordinating principles are the Newtonian laws of motion; the mathematically expressed empirical law made possible by this coordination (which picks out, for example, the center of mass of the solar system as the proper frame of reference for defining the true motions within this system) is the law of universal gravitation. And so far, then, Kant's original conception appears to be correct. But, and this is now Reichenbach's key innovation (in 1920), it is necessary to *change* our coordinating principles as mathematical physics develops. When we move to special relativity we replace the Newtonian laws of motion with Einstein's revised version thereof (which define, as we would now put, the new structure of Minkowski space-time replacing the original spatio-temporal framework described by Newton); and when we move to general relativity, finally, we use Einstein's principle of equivalence to effect an entirely new type of coordination relating the abstract four-dimensional geometry of variable curvature defined by Einstein's field equations of gravitation to the empirical behavior of freely falling bodies subject only to the influence of gravity.

Reichenbach distinguishes, on this basis, between two meanings of the a priori originally combined in Kant: necessary and unrevisable, fixed for all time, on the one hand, and "constitutive of the concept of the object of knowledge," on the other. Coordinating principles cannot be a priori in the first sense, of course, because we have just seen that they change from theory to theory as our scientific knowledge grows and develops. Nevertheless, they are still a priori in the second sense, for unless they are antecedently in place our mathematical theories have no empirical content—no coordination with physical reality—at all. Without the principle of equivalence, for example, the abstract four-dimensional space-time geometry defined by Einstein's equations would belong wholly to the realm of pure mathematics: it would not yet make an assertion about physical and empirical phenomena such as gravitation. Therefore, according to Reichenbach (in 1920), we still need a priori principles in one important meaning of Kant's original term—we still need *constitutively a priori* principles—but such principles, as Kant did not and could not see, change and develop as mathematical-physical theorizing progresses. Carnap's *Logical Syntax* conception of the principles of logic and mathematics is a generalization of this view, in so far as the principles of logic and mathematics themselves—the very principles that are similarly constitutive, in particular, of our most general inferential practices—are now seen as subject to a parallel relativization: there is no longer a uniquely correct set of a priori (analytic) rules of logic, but rather a multiplicity of such rules (classical, intuitionistic, and so on) definitive of a multiplicity of what Carnap now calls formal languages or linguistic frameworks.

I myself believe that Reichenbach's original, 1920, conception is much closer to the truth than the more starkly empiricist position he articulates in 1951, and I believe that this conception is further confirmed by Carnap's extension of it to the nature of logic and mathematics, more generally, in his *Logical Syntax* of 1934. For me, the central contribution of logical empiricism in this regard does not lie in a logically sophisticated revival of more traditional inductivist empiricism, but rather in a new version of the original Kantian insight that a proper interpretation of modern mathematical science requires a carefully balanced synthesis of both rationalism and empiricism. In particular, we still need to acknowledge the fundamental importance of a priori constitutive principles—both logical and mathematical principles, on the one side, and physical

coordinating principles, on the other—and there is no longer any clear sense, moreover, in which a priori principles of either kind are empty, tautologous, or analytic. What makes them a priori is rather their characteristically Kantian constitutive function of first making possible the properly empirical knowledge of nature (Carnap's synthetic sentences or Reichenbach's axioms of connection) thereby structured and framed by such principles. But these constitutive principles, as Kant did not see, are also relativized and dynamical: they change and develop as mathematical natural science develops, especially in deep conceptual revolutions such as the transition from Newtonian physics to Einsteinian relativity theory. So it is precisely here, I believe, that a clear philosophical descendent of Kant's original synthetic a priori remains—but, unlike Kant's original conception, it is relativized, historicized, and dynamical.

This revisionist understanding of the central contribution of logical empiricism leads to an at first sight rather surprising coincidence between at least one strand of twentieth century scientific philosophy and Thomas Kuhn's theory of the nature and character of scientific revolutions. Indeed, one of Kuhn's central examples of revolutionary scientific change, just as it was for the logical empiricists, is precisely Einstein's theory of relativity. Moreover, Kuhn's central distinction between change of paradigm or revolutionary science, on the one side, and normal science, on the other, closely parallels the Carnapian distinction between change of language or linguistic framework and change of empirical or synthetic sentences formulated within such a framework (or, as Reichenbach puts it, between change of coordinating principles or axioms of coordination and change of mere empirical laws or axioms of connection). Just as, for Carnap, the logical rules of a linguistic framework are constitutive of the notion of "correctness" or "validity" relative to this framework, so a particular paradigm governing a given episode of normal science, for Kuhn, yields generally-agreed-upon (although perhaps only 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, questions concerning which linguistic framework to adopt are not similarly governed by logical rules, but rather require a much less definite appeal to purely pragmatic considerations, so 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.

On second sight, however, this coincidence is not so surprising at all, when we remind ourselves that Kuhn's *The Structure of Scientific Revolutions* was originally published in 1962 in the Encyclopedia of Unified Science, which served as the logical empiricists's official monograph series in the new world. Indeed, Carnap himself acted as editor of Kuhn's volume for this series, and, in correspondence with Kuhn, expressed his warm appreciation for Kuhn's achievement. And it is also worth noting, finally, that, towards the end of his career, Kuhn expressed regret that he had originally taken Carnap's statements of approval as "mere politeness," and Kuhn acknowledged the point, accordingly, that his own view—which he then often characterized as "Kantianism with movable categories"—was very similar in fact to the relativized and dynamical conception of constitutively a priori principles earlier developed within the logical empiricist tradition. Had Kuhn known of this coincidence in 1962, *The Structure of Scientific Revolutions* would certainly have been very different from a philosophical point of view.

Now Kuhn's theory of scientific revolutions, as is well known, has more recently led to skeptical and relativistic conclusions regarding the ultimate rationality of mathematical scientific knowledge. For, if scientific change can no longer simply be understood, in accordance with traditional inductivist empiricism, as the continuous accumulation of more and more observable facts, and, in periods of deep conceptual revolution or paradigm-shift, it must rather be likened to a conversion experience or Gestalt-switch, then it would appear that the development of science as a whole can no longer be conceived as an essentially rational enterprise. Since deep conceptual revolutions or paradigm-shifts, by hypothesis, do not proceed against the background of common generally-taken-for-granted rules, as does normal science (they do not proceed, in Carnapian terminology, against the background of a single system of logical rules or linguistic framework), then (so the argument goes) it would appear that there is no sense left in which these scientific transitions (by far the most interesting ones) can still be conceived as rational—as driven by good reasons. Modern mathematical science, in the

end, is just as ultimately subjective and historically relative as any other aspect of human culture: all knowledge is local.

The twentieth century tradition of scientific philosophy I have been examining did not, of course, explicitly address this issue. But it is still worth asking ourselves, at precisely this point, whether it has the resources to resolve it. I believe that careful attention to the history of scientific philosophy points the way towards a proper resolution—by means of a further elaboration of what I want to call the dynamics of reason. To see this, however, we need to look at the history of scientific philosophy I sketched at the beginning from a slightly different point of view, and we need to ask, in particular, how Einstein's creation of the theory of relativity in the early years of the twentieth century was intimately entangled with this history. When we do this, I shall argue, we will see that the rationality of the radical conceptual revolution effected by Einstein was in fact essentially mediated precisely by developments in scientific philosophy. Kuhn himself, I note, left out this parallel history of scientific philosophy, and this is precisely why, from my point of view, he himself had no adequate solution to the problem of conceptual relativism raised by his own historiography of science.

Einstein's finished theory of relativity—including the special theory of relativity formulated in 1905 and the general theory of relativity formulated in 1915—constituted a deep conceptual revolution relative to the pre-existing conceptual framework of classical Newtonian physics. The special theory abandoned the notion of absolute time or absolute simultaneity lying at the basis of Newtonian kinematics and gravitation theory (where, according to the theory of universal gravitation, gravitating bodies attract one another immediately—instantaneously—across arbitrarily large spatial distances), and it replaced this classical notion of simultaneity with a new, relativized notion defined in terms of the invariance of light signals (more generally, electro-magnetic processes) in different inertial frames. Einstein was then faced with the problem of reconceiving the theory of gravitation, so that it, too, avoided instantaneous action at a distance and employed, in its stead, a truly dynamical field propagating at the speed of light. Einstein attacked this problem by means of his principle of equivalence, which appealed to the already well-established equality of gravitational and inertial mass to conclude that gravity and inertia are the very same physical phenomenon. Einstein exploited this insight by investigating

the accelerative forces arising in non-inertial frames of reference (such as centrifugal and Coriolis forces arising in rotating frames of reference), within the new inertial structure of what we now call Minkowski space-time, and he developed, on this basis, relativistically acceptable models of the gravitational field. The finished result, the general theory of relativity, uses a variably curved version of the four-dimensional space-time geometry arising in special relativity (the geometry of Minkowski space-time), where the curvature of space-time now represents the gravitational field, and freely falling bodies affected only by gravitation follow geodesics or straightest possible paths of this new, non-Euclidean space-time geometry.

This finished theory of relativity uses radically new conceptual resources that were simply unavailable to classical physics. Indeed, the mathematics required for formulating a non-Euclidean geometry of variable curvature was not itself available until the second half of the nineteenth century, and so Newton himself, for example, could not even have formulated the *idea* of Einstein's theory. Moreover, even after the pure mathematics here deployed by Einstein (the general theory of n-dimensional manifolds) had been introduced by Bernhard Riemann in 1854 (although it was not actually published until 1867), one still had no notion at all how to apply such a geometry to the physical world until Einstein himself explored the principle of equivalence in the years 1907-12. In Kuhn's terminology, therefore, there is an important sense in which the new theory is incommensurable or non-intertranslatable with the old, in that, as I myself would put it, the new theory involves a genuine expansion of our space of intellectual possibilities—not simply the discovery of a new fact (or actuality) within an already existing space of possibilities. In my view, once the new space of possibilities is accepted (once it is accepted, for example, that gravitation *may* be represented by a variably curved four-dimensional version of Minkowski geometry) empirical facts can then be invoked to settle the question of which possibility is actually realized (as the anomaly in the advance of the perihelion of Mercury, for example, then favors Einstein's field equations of gravitation over Newton's). The crucial question, however, is how does a new space of intellectual possibilities—a new constitutive framework defining such new possibilities—itself become accepted in the first place? How did Einstein, in particular, somehow contrive to expand the constitutive framework of classical physics?

Einstein appealed to conceptual resources that were already present and available in pre-relativistic scientific thought—what else could he appeal to?—but these were not so much resources already present in classical Newtonian *physics*, but rather those available in pre-relativistic scientific *philosophy*. For, in the first place, problems arising from the concepts of absolute space, time, and motion were already intensively discussed considerably before Einstein's work—indeed, these problems had been already intensively discussed since the time of Newton's original creation of his theory in the seventeenth century. Moreover, at the philosophical or meta-scientific level at which this discussion proceeded there was very little consensus on the proper answers to the questions at issue—relativistic views of space, time, and motion perpetually opposed absolutist views with no clear resolution in sight—but it would nevertheless be wrong to assert that no progress of any kind was made. On the contrary, by the end of the nineteenth century there had been considerable clarification of the role of the problematic concepts within Newtonian physics—by, among others, Ernst Mach—and, in particular, the crucial concept of inertial frame had been articulated by a number of late nineteenth century classical physicists. It is no wonder, then, that when Einstein was faced with the radically new situation vis-à-vis the relativity of motion created by the surprising empirical discovery of the invariance of the velocity of light, he appealed to both the concept of inertial frame and the critical analysis of Newtonian absolute motion due to Mach in developing first the special theory of relativity and then the general theory.

Further, and in the second place, in creating the general theory of relativity, in particular, Einstein explicitly appealed to a preceding tradition of reflection on the nature and character of geometry within nineteenth century scientific philosophy. This was the famous debate between Helmholtz and Poincaré, in which empiricist and conventionalist interpretations of the new non-Euclidean geometries opposed one another against the ever present backdrop of Kant's original theory. Both Helmholtz and Poincaré rejected Kant's theory in its original form, according to which Euclidean geometry itself expresses the necessary structure or form of our spatial intuition. Nevertheless, they agreed that there is a generalization of Kant's theory (to spaces of constant curvature) in which a more general principle—the principle of free mobility permitting arbitrary continuous motions of rigid bodies—replaces the particular axioms of Euclid. They

disagreed, however, on how the more specific geometry of physical space (of positive, negative, or zero curvature) was then to be determined. For Helmholtz it was to be determined empirically, by actually carrying out measurements with rigid bodies; for Poincaré, by contrast, it could only be determined by a convention or stipulation—such that, for example, Euclidean geometry is laid down by stipulation on the basis purely of its greater mathematical simplicity.

I do not have time to go into this adequately here, but it emerges from Einstein's celebrated paper on "Geometry and Experience," cited earlier, that his own application of non-Euclidean geometry to a relativistic theory of gravitation grew naturally out of precisely this late nineteenth century debate—as Einstein reinterprets this debate in the context of the new non-Newtonian mechanics of special relativity. The key transition to a non-Euclidean geometry of variable curvature, in particular, results from applying the Lorentz contraction arising in special relativity to the geometry of a rotating disk (and thus to a particular example of a non-inertial frame of reference), as Einstein simultaneously delicately positions himself within the debate on the foundations of geometry between Helmholtz and Poincaré. Thus, whereas Einstein had earlier made crucial use of Poincaré's idea of convention in motivating the transition, on the basis of mathematical simplicity, from Newtonian space-time to what we currently call Minkowski space-time (for, following Poincaré, Einstein took the critical relation of simultaneity to be determined by neither reason nor experience, but rather by a convention or *definition* of our own), now, in the case of the rotating disk, Einstein rather follows Helmholtz in taking the behavior of rigid measuring rods to furnish us with a straightforwardly empirical determination of the underlying geometry—in this case, a non-Euclidean geometry. This, in fact, is how non-Euclidean geometry was actually applied to physics in the first place, and so without Einstein's delicate engagement with the preceding philosophical debate between Helmholtz and Poincaré, it is indeed hard to imagine how the idea of such an application could have ever been envisioned as a real possibility—as a genuinely live alternative.

What we see here, I finally want to suggest, is that there is a fundamental ambiguity in the notion of a scientific philosophy—an ambiguity that is clearly present in Reichenbach's 1951 book. On the one hand, it can mean a philosophy that is intimately

engaged with the very deepest results of the best available science of its time—in this sense, Kant, on Reichenbach's telling, was himself a scientific philosopher, as were Helmholtz, Mach, Poincaré, and the logical empiricists (including Reichenbach himself, of course). On the other hand, and this is Reichenbach's preferred sense, it can mean a philosophy that emulates the sciences, in so far as it aims for cumulative consensus and stable "results" comparable to the results of the sciences themselves. The notion of a scientific philosophy, in this second sense, is, I believe, an illusion. Indeed, since the role of scientific philosophy, in the first sense, is to reflect, at the meta-level, on the fundamental conceptual frameworks constitutive of the best available science of the time, and since precisely such frameworks, in periods of deep conceptual revolution, then undergo fundamental revision, it is clear that scientific philosophy, in this sense, neither can nor should aim at definitive "results"—those characteristic, at the scientific level, of normal science. Thus Kant, in articulating the most fundamental constitutive principles of Newtonian mathematical physics, thought that he had achieved stable and definitive results in philosophy comparable to Newton's achievements in physics: he thought, in particular, that he had finally set philosophy or metaphysics on what he called "the secure path of a science." We now know, however, that Kant's hope was in vain and that his true historical mission was rather, precisely by delving so deeply into the conceptual foundations of specifically Newtonian mathematical physics, to prepare the ground for later revisions of the Newtonian conceptual structure when the situation at the scientific level demanded it. The further fertilization of this soil was then carried out by nineteenth century scientific thinkers such as Helmholtz, Mach, and Poincaré; and, although no stable consensus, at the philosophical or meta-scientific level, was actually achieved by these thinkers either, they nonetheless essentially advanced the process of conceptual clarification and expansion as their epistemological reflections interacted with nineteenth century scientific results such as the development of non-Euclidean geometries, the emergence of physiology and psycho-physics, and so on. This nineteenth century process of philosophical or meta-scientific fertilization ultimately bore spectacular fruit in the early years of the twentieth century, when Einstein, against this essential background, created a radically new constitutive framework at the scientific level—on the basis of

which, at least for a time, we could then achieve stable and definitive scientific results (that is, normal science).

The scientific philosophy of logical empiricism, which took its distinctive task to be precisely the fuller articulation and clarification of the radically new scientific conceptual framework created by Einstein, took itself, understandably, to be in a similar position with respect to Einsteinian physics that Kant was in vis-à-vis Newtonian physics. And, like Kant, the logical empiricists thought that they had finally achieved the ideal of a scientific philosophy in our second sense—philosophy, once again, was to be in a position to achieve (at least temporarily) stable and definitive results. Philosophy, once again, was finally to be set on "the secure path of a science." But, as we have seen, this hope, too, was in vain. In particular, no stable and definitive general theory of the character of pure mathematical knowledge, and its application to nature in modern mathematical physical science, has in fact been achieved. Yet there is no denying, at the same time, that considerable progress has nevertheless been make, in that we have a much better understanding of the deep mathematical, physical, and conceptual problems involved in attempting to articulate such a general understanding than ever before—better than was achieved in either Kant's original theory, for example, or in the starkly empiricist position Reichenbach himself represents in *The Rise of Scientific Philosophy*. I myself believe, as I have explained, that a descendent of Kant's original synthetic a priori—a relativized, historicized, and dynamical descendent—is what emerges most clearly in our present conceptual situation. I freely acknowledge, however, that we have no stable and definitive philosophical theory of the a priori—no *scientific* theory in the second sense of "scientific philosophy"—to underwrite this view. What we have instead is a chapter of philosophical and scientific history, embracing the parallel evolution of both scientific philosophy from Kant through Reichenbach and Carnap, and the mathematical exact sciences from Newton through Einstein and Gödel, on the basis of which it finally becomes clear, as I have argued, that a relativized and dynamical descendent of the Kantian synthetic a priori is what remains standing, as it were, at the end of the historical dialectic. In this sense, what I am calling the dynamics of reason can itself only end—perhaps not too surprisingly—on a frankly Hegelian note.

Note

* My revisionist understanding of logical empiricism is developed in *Reconsidering Logical Positivism* (Cambridge: Cambridge University Press, 1999). I begin to articulate the resulting reconceptualization of the nature and goals of "scientific philosophy" in *Dynamics of Reason* (Stanford: CSLI, 2001). The reader may consult these two works for further details and references. I here explain the basic ideas of what I call the dynamics of reason against the background of Reichenbach's *The Rise of Scientific Philosophy* (Berkeley and Los Angeles: University of California Press, 1951). All parenthetical page references are to this volume.