Reconsidering Kant, Friedman, Logical Positivism, and the Exact Sciences*

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This essay considers the nature of conceptual frameworks in science, and suggests a reconsideration of the role played by philosophy in radical conceptual change. On Kuhn's view of conceptual conflict, the scientist's appeal to philosophical principles is an obvious symptom of incommensurability; philosophical preferences are merely "subjective factors" that play a part in the "necessarily circular" arguments that scientists offer for their own conceptual commitments. Recent work by Friedman has persuasively challenged this view, revealing the roles that philosophical concerns have played in preparing the way for conceptual change, creating an enlarged conceptual space in which alternatives to the prevailing framework become intelligible and can be rationally discussed. If we shift our focus from philosophical themes or preferences to the process of philosophical *analysis,* however, we can see philosophy in a different and much more significant historic role: not merely as an external source of general heuristic principles and new conceptual possibilities, but, at least in the most important revolutionary developments, as an objective tool of scientific inquiry. I suggest that this approach offers some insight into the philosophical significance of Newton's and Einstein's revolutionary work in physics, and of the interpretation of their work by (respectively) Kant and the logical positivists. It also offers insight into the connections between modern philosophy of science and some traditional philosophical concerns about the nature of a priori knowledge.

Michael Friedman has remarked that "Kantian thought stands as a model of fruitful philosophical engagement with the sciences" (1992, xii). Examining this claim, exploring the senses in which, and the extent to which, it might be true, is not only an illuminating way to consider the aims and methods of the philosophy of science. It also promises some insight into the roles that philosophy has played in science, particularly in

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the great conceptual transformations that physics has undergone in the last three centuries. Friedman himself has, evidently, done much to fulfill this promise, yielding a deeper appreciation not only of Kant's philosophy of science, but also of the neo-Kantian aspects of logical positivism; he has rediscovered some insights of Kant and the logical positivists that more recent accounts of conceptual change have misunderstood, forgotten, or ignored. In doing so he has presented a compelling picture of the rationality of conceptual change, and of philosophy as a source of rational motivation for change.

I will argue, however, that the most significant common theme of Kant and the positivists does not concern the problem of scientific rationality, as we now understand it. Their special concern was the kind of conceptual change that constitutes, more than merely a rational choice, an advance to a deeper level of insight; they sought to show that a novel conception can represent, not just a more rational way of serving the predictive and other aims of science, but, more important, a clearer understanding of something that the existing theory grasps only dimly, or of concepts that the existing theory has not clearly defined. At a time when even the empirical progress of science is in question, this idea of conceptual progress may seem difficult to defend. Defending it requires us to reconsider the role of philosophy in the evolution of science—not merely as an external source of general heuristic principles and new conceptual possibilities, but, at least in the most important revolutionary developments, as an objective tool of scientific inquiry. Evidently Kant and the positivists, in differing ways, saw such a role for philosophy in science. But the limitations inherent in their respective viewpoints prevented them from seeing it in the clearest light, and perhaps discouraged its further exploration. To overcome their limitations, we need a better understanding of how revolutionary concepts can emerge from a critical philosophical engagement with established beliefs, and in what sense their emergence can be seen as genuine epistemic progress. In such a case, the resulting philosophical insight is not merely a motivation for the new theory; it is, in essence, the theory itself. And the kind of scientific progression that results is not linear or cumulative, in the sense long discredited by Kuhn, but dialectical.

Kuhn's notion of paradigm shift has invited many interpretations, more than one by Kuhn himself. Unquestionably this notion implies that a community of scientists collectively changes the framework that defines their metaphysics, their methods, and the nature of their collective social practice; what seems unclear is to what extent such a change can be regarded as rational. In any case, Kuhn stated quite clearly how he saw the role of philosophy in this process: when the scientific methods sanctioned by the existing paradigm fail to decide the great questions, scientists turn to "critical discourse," that is, to the exchange of "claims, counterclaims, and

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disagreements about fundamentals" that is characteristic of philosophy—and of the sciences in their immature, pre-paradigm phase (Kuhn 1970, 6). Thus for Kuhn, critical discourse about fundamental concepts is not an objective investigation, but a negotiation among competing philosophical preferences; philosophical views thus belong among the "subjective factors" that may influence scientists' judgments when no strictly rational decision is possible.

Friedman has helped us to see some of the serious weaknesses of this view. Properly understood, philosophy does not merely intervene in a paradigm clash; it plays a decisive role in making paradigm clashes possible and comprehensible. For, historically, philosophical ideas and traditions have been responsible for "enlarging the space of possibilities" for scientific theories: "Science, if it is to continue to progress through revolutions . . . needs a source of new ideas, alternative programs, and expanded possibilities that is not itself scientific in the same sense." (1999, 19). This is an important recognition of something that Kuhn's view is too narrow to acknowledge. Suppose that a scientific paradigm does enforce its own standards of scientific procedure, and that science itself offers no "extra-paradigm" viewpoint from which to compare and evaluate competing paradigms; it does not follow that there is no rational extra-paradigm perspective to which science can appeal, or that competing paradigms must be mutually incomprehensible. While a scientific theory naturally takes certain fundamental concepts for granted, and in itself offers no means of criticizing them, the larger philosophical setting in which scientific thinking takes place can provide novel concepts, and novel criticisms of the existing concepts, making an alternative conception, at least, a rational possibility. For Kuhn, if such criticism occurs, it is confined to times of crisis. But the history of science reveals the significant part it has played in times of "normal science." Friedman illustrates this point with telling examples. Einstein, for instance, created a new paradigm for electromagnetism, not out of nothing, but out of reflections on the existing paradigm that were informed by his philosophical study—especially of the ideas of Mach and Poincaré, who, without questioning the "paradigmatic" status of Newtonian mechanics, nonetheless subjected the fundamental concepts of the Newtonian framework to a searching critical analysis, seeking in particular to separate their factual from their theoretical, conventional, and even arbitrary elements. What distinguished Einstein from his contemporaries, then, was that he could bring to bear, on the empirical and theoretical principles that they all shared, a philosophical viewpoint from which the empirical significance of simultaneity was open to question, and a relativized conception of simultaneity was a serious possibility.

On Kuhn's account, the creative work of the revolutionary scientist is not rationally explicable. Of course, to explain this was, evidently, never

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part of Kuhn's aim. But his account of paradigm shift as conversion, of "lexical structures" as something like Kantian categories, represents scientists with opposing frameworks as incapable of mutual understanding. And this representation places the creative scientist in this same position with respect to her earlier self. Kuhn does not appear to have found this bizarre; apparently he thought that such scientists just "find themselves" living within the new framework with no clear sense of when or how they arrived there. But Einstein's creation of special relativity is impossible to reconcile with this Kuhnian picture. On the one hand, it is perhaps the clearest example of a revolution that does not merely make radical claims within an existing conceptual framework, but redefines fundamental concepts in just the way that Kuhn's view seems to require—and does so, moreover, relatively suddenly.[1] On the other hand, it is a case for which we have the most compelling rational account of the creator's process of creation, one that is not merely a rational reconstruction, but, rather, shows the remarkable consistency of philosophical perspective that motivated the process from beginning to end. Einstein's postulate that light has the same velocity in all inertial frames, and his designation of light-signaling as the new "coordinating principle" for the definition of simultaneity, decisively separated his thinking from nineteenth-century physics, but their sources in nineteenth-century philosophy cannot be doubted.

Friedman's account of the role of philosophy in conceptual change, then, apart from urging a philosophical point against Kuhn, also fills in a part of the historical picture that Kuhn had ignored. At least in the cases of greatest philosophical interest, the redefinition of a fundamental concept is not, as Kuhn would have it, a mere side-effect of paradigm shift. Instead, it is a self-conscious and rational act, with a combination of scientific and philosophical motivations, that is itself constitutive of a new conceptual framework. Such an act involves what Friedman calls, echoing Einstein (1905), "elevating an empirical law to the status of constitutive principle." Friedman's reconsideration of this process is, from my point of view, the most important part of his account of the rationality of science. But it is also precisely the point at which some further reflection is necessary. To begin with, we should qualify the notion of "empirical law." As Friedman points out, the empirical fact at Einstein's disposal was that differences of velocity relative to the ether, according to classical electrodynamics, ought to have observable effects, but experiments fail to detect

1. Galileo's revolution in mechanics, for a contrasting example, had a few centuries of preparation, most directly in the work of sixteenth-century Italian mathematicians such as Cardano, Tartaglia, and Benedetti. Unlike Einstein's theory—and contrary to the Kuhnian model—Galileo's displaced, not an accepted orthodoxy that had come to a state of crisis, but a collection of ideas that had been subjects of critical discussion, serious objections, and fundamental revision for many hundreds of years.

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any such differences. That the speed of light is invariant, or the same in all inertial frames, is an interpretation rather than a logical consequence of those experimental facts. Therefore, even on Friedman's view, it is inappropriate to say that an empirical principle has been elevated to the status of a constitutive principle, because the constitutive principle is so distinct, at least in its theoretical implications, from the original empirical one. It would be more appropriate to say that an interpretive extension of the empirical law becomes a constitutive principle. This way of describing it does not make the transition from the one to the other more transparent; on the contrary, it draws attention to the puzzling character of this transformation in status. It is understandable that Friedman, following the logical positivists, would identify this as the point at which "an element of decision or convention"—though, for Friedman, a rationally and philosophically intelligible decision—must be implicated. If the facts don't immediately present themselves in a form upon which a conceptual framework can be constructed, evidently they can only take on such a form through an act of our own. This account emphasizes that constitutive principles have the function of fixing meaning: in addition to the empirical facts themselves, we require the coordinative *definition* that fixes their relationship to a given mathematical structure, i.e., that fixes the intended interpretation of an abstract structure.

Friedman is thus reviving an insight associated especially with Hans Reichenbach, correcting some of its weaknesses instead of overlooking it as the post-positivist tradition has tended to do.[2] To Reichenbach, among Einstein's fundamental achievements was to have "demonstrated the necessity for metrical coordinative definitions in several places where empirical relations had previously been assumed" (1957, 15)—thus suggesting the empirical ill-foundedness of Newtonian physics generally, as if its empirical or "operational" meaning had never been established. As Friedman has emphasized, the Newtonian picture of space and time was tied to experience by clear and relatively successful coordinating principles. What Einstein called into question was the relation between those principles and

2. In more recent literature on the philosophy of space and time (e.g., Earman 1989), the positivists (especially Reichenbach) are roundly dismissed for their technical errors concerning the absolute-relational controversy: they did not understand the function of absolute motion in Newton's theory; they did not understand the true geometrical content of general relativity; they did not understand the theoretical continuity of general relativity with earlier theories of space and time; they did not understand where to draw the line between the factual and the conventional in spacetime theories. But these very just criticisms do not address the general philosophical question whether convention plays some role in our knowledge of space and time, and what bearing this might have on the traditional ontological issues—a question which is, accordingly, largely unexamined in the important literature on the philosophy of space and time of the last few decades. (Cf. DiSalle 2002b.)

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some novel and surprising experimental facts, namely, that electrodynamical phenomena appear to exhibit a surprising set of (Lorentzian) symmetries. Only in this theoretical and experimental context does it make sense to question the Newtonian account of simultaneity, and to raise the objection that the physical processes to which the concept of simultaneity is directly coordinated—especially the instantaneous propagation of gravitational force—are entirely *hypothetical.* One could have expected the gravitational definition of simultaneity and the light-signaling method to be in perfect accord, in the retrospective determination of simultaneity, as long as the travel-time of light-signals could be taken into account. But with the discovery that light-signals appear to violate the Galilean rule for addition of velocities, the gravitational definition now stands on its own, unsupported by any direct experiment. Einstein saw that in this context, a new coordinative definition was required—and that in the framework constituted by this new definition, the Lorentz invariance of electrodynamics no longer appears to be contradictory, and no longer requires a hypothetical explanation.

I would also urge, however, a more general philosophical point against Friedman's formulation, a point that emerges from Einstein's discussion of electrodynamics, but was first raised in Poincaré's philosophical reflections on geometry. Poincaré was, evidently, a founder of the notion of coordinative definition to which Friedman appeals, but his great insight was not that we *designate* certain empirical principles as having "coordinative" status. It was, rather, that we are simply mistaken in regarding certain principles as empirical in the first place. The principle of free mobility, for example, is a kind of "definition in disguise," not because we have elevated it to this status, but because it simply fails to make an empirical claim, or any kind of synthetic claim in the ordinary sense. For the concepts to which the principle refers are simply not well defined independently of the principle itself. As Poincaré argued in his celebrated exchange with Russell on the foundations of geometry, it cannot be an empirical claim that "shape" is preserved in rigid motions, because what we mean by "shape" is only that which is preserved in certain motions. Russell's (1897) view, that such principles presuppose our acquaintance with the concepts that occur in them, simply does not survive Poincaré's conceptual analysis.

Ironically, Einstein's argument for special relativity directs just this kind of philosophical criticism against Poincaré's own view of electrodynamics. What Einstein recognized around 1905 was not that the principle of the constancy of the velocity of light *could be* elevated from an empirical principle into a constitutive principle. He recognized, rather, that the principle was not an empirical principle in the first place, and could not be one. That the speed of light is the same in all inertial frames is a consti-

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tutive principle, not because we grant it that status, but because, so to speak, it has that status inherently: it is part of the definition of an inertial frame—a concept which, as Einstein shows, is not clearly defined independently of this principle. This is an aspect of Einstein's introduction of the "light-principle" that deserves to be made clearer. The principle appears to be a straightforward empirical claim, at least if the notion of inertial frame is taken for granted, as indeed it appears to be in Einstein's proposal that "the same laws of electrodynamics and optics will be valid for all frames of reference for which the equations of mechanics hold good" ([1905] 1952, 37). To assume this, however, is to assume that we can begin with an inertial frame and determine, as a matter of empirical fact, the velocity of light in that frame. Clearly the Michelson-Morley experiments assumed this much, as did the entire Maxwell-Lorentz theory. The remarkable difference between the Lorentzian perspective and Einstein's, then, is not Einstein's reinterpretations of the results of such measurements, but his recognition that such a measurement is, in the contemporary situation, impossible. The velocity of light cannot be measured relative to an inertial frame, because we can no longer assume that we have an independent way of constructing an inertial frame in advance.

Again, this thought doesn't appear to emerge in the introductory section of Einstein's 1905 paper, which already seems to appeal to the notion of inertial frame. But it emerges directly in the argument of Section I. The title of this section is already a kind of Socratic irony with respect to the Lorentzian view: The discussion of procedures for fixing a reference frame is called the "Kinematical Part," as distinct from the "Electrodynamical Part," but we see immediately that electrodynamics is implicated from the start. For the laying out of a spatial coordinate system, to say nothing of fixing a standard of time, already requires some means of determining simultaneous events, and light-signaling proves to be indispensable. From the Lorentzian perspective, the measurement of the speed of light takes place against an already well-defined spatial framework. Einstein's analysis reverses this order: the construction of the spatial frame of reference requires the assumption of the constancy of the velocity of light. But if the spatiotemporal framework is not given in advance, then the light principle is, ipso facto, not an empirical statement about the behavior of light in inertial frames; it is by nature a constitutive principle. Einstein arrives at this principle, then, not by making a decision to grant constitutive status to a known fact. Rather, it is the result of what I have elsewhere characterized as a conceptual analysis, an analysis that discovers, or uncovers, the constitutive principle that is implicit in a given body of theory and practice (cf. DiSalle 2002b).

What Einstein's analysis shows is that his definition of simultaneity, far from being an arbitrary new convention, is in fact the definition that we

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use—not only casually in our informal judgments of simultaneous events (i.e., the events that we see at the same time), but in the setting up of frames of reference for any kinematical analysis. Until now (1905), however, we have done so without regarding this definition as playing any fundamentally constitutive role. This is because, until now, knowing that light propagates at a finite speed, we supposed that it provides merely a local criterion of simultaneity, standing in, as it were, for the universal and invariant criterion that an infinitely fast signal would provide. And we supposed, as was already noted, that the local criterion could be brought into correspondence with the invariant one by way of the addition of velocities, theoretically if not practically. But experiment shows that the local criterion behaves as if it were an invariant one, failing to distinguish frames of reference as required by the law of addition. Light-signaling, then, apart from being the only practical means of determining simultaneity, turns out to be the only "absolute" means, i.e., the only one that does not depend on the perspective of the observer. As Einstein shows in Section 2 ([1905] 1952, 41–43), the relativity of simultaneity is the necessary consequence of adopting this observer-independent criterion.

Calling Einstein's reasoning a conceptual analysis might seem simplistic, recalling the oversimplified view of the logical positivists: that Einstein had merely applied some empiricist and antimetaphysical standards to poorly defined conceptions of space and time. But now we can see the reasoning as a more subtle process, involving not only destructive analysis, but the construction of something new through a dialectical engagement with the old; in this sense it can be seen as a genuinely creative process, not indeed because it invents something unheard of, but because it discerns something new in the existing theory and fact. That a philosophical perspective provided motivation and credibility to special relativity, and thereby made possible its replacement of the Lorentz theory, therefore seems an inadequate description of what actually occurred. It is more accurate to say that special relativity *is* a philosophical perspective on the Lorentz theory—a clearer perspective, resulting from analysis of what the Lorentz theory had assumed unreflectingly, namely, "the relationships between rigid bodies (coordinate systems), clocks, and electromagnetic processes" (Einstein [1905] 1952, 38).[3]

3. The present discussion is in sympathy with, and indebted to, that of Torretti, who also recognizes "conceptual criticism as a catalyzer of scientific change" (1989, section 2.5). "For there can be no question of choosing between two modes of thought if the very existence of the one issues from a recognition of the conceptual failings of the other" (1989, 44). His account differs from mine, and, I think, more nearly resembles that of the logical positivists, in emphasizing the destructive analyses of poorly-defined concepts, rather than the emergence of new concepts through conceptual analysis. Torretti's point is by itself an important one against Kuhn, who seemed to assume that a

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It is neither an exaggeration nor a mere metaphor to call Einstein's critical conceptual analysis dialectical, and in two classical senses of the term. First, the analysis is a kind of dialogue with an existing point of view, starting from the latter's unexamined assumptions in order to reveal the weaknesses of its fundamental concepts. In Einstein's "Kinematical Part," the premise of the Lorentzian point of view is provisionally accepted: that we can begin a physical inquiry by laying down a frame of reference—in Poincaré's terms, that we can treat the spatio-temporal background (classical kinematics) as the constitutive framework presupposed by the study of any specific field such as electrodynamics. The analysis quickly reveals, however, that the classical notion of a frame of reference cannot stand on its own, and that electrodynamics itself must play a constitutive role. Second, the analysis can be viewed in a straightforward way as resolving a contradiction by creating a more comprehensive point of view. It is not that classical mechanics is falsified by phenomena such as the Michelson-Morely experiment. But that experiment is supposed to apply criteria for being at rest in the ether; the contradiction is that these criteria are satisfied by systems that are in motion relative to one another. Einstein shows, however, that our understanding of these criteria is at fault, resting as it does on an assumption about simultaneity that no longer has independent grounds. Properly understood, experiments like the Michelson-Morely experiment can test for uniform velocity relative to the ether, but—in the absence of an independent criterion for simultaneity—not for motion or rest. For the experiment uses simultaneity as a criterion for detecting differences in the speed of light arising from motion through the ether. Under the circumstances, however, this is a piece of circular reasoning; in truth, the speed of light is providing the criterion for simultaneity.

Einstein's argument thus has the same dialectical structure as a famous one of Galileo's: If the traditional Aristotelian tests for the motionlessness of the earth are passed by systems in motion relative to the earth—e.g., by moving ships—then we are faced by a contradiction; we arrive at a coherent larger perspective, however, by realizing that the tests were not tests of motionlessness after all, but tests of uniform motion. A homelier example is the discovery of the shape of the earth from the directions of the stars at different latitudes. It is apparently contradictory that two lines that are both vertical should not be parallel, but it is natural and inevitable

given framework is inherently complete and coherent on its own terms, and that it could only appear inadequate to an adherent of some other framework—as if its own adherents were incapable of critical reflection on it (a view that is admirably charitable to older conceptual frameworks, but, arguably, uncharitable, even condescending, to older generations of scientists).

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if the earth is a sphere. Of course this requires us to rethink our conception of "vertical," and to recognize it as a relative term; that the earth is spherical is a conception within which every "local" perspective on the directions of up and down—from each of which the others are necessarily wrong—fits coherently and consistently. In just the same manner, by the arguments of Galileo and of Einstein, the seemingly incompatible perspectives of different inertial frames are comprehended in a single larger perspective. Perhaps, after such a change of perspective, the meaning of "up" (or "at rest" or "simultaneous") could never be quite the same. But to call the new, enlarged perspective "incommensurable" with the old is to miss precisely this dialectical relation between them, and therefore the objective sense in which a deeper understanding has been achieved.

It is perhaps true in some sense that, when we translate the theory that the earth is a sphere into a "limiting case" theory on which the earth is flat, we obtain something that would be unrecognizable to sincere believers in a flat earth. It should nonetheless be obvious that, since the new theory constitutes, in a perfectly straightforward sense, a dialectical resolution of the contradictions arising from the old theory, the new theory "contains" the old theory—contains, that is, every possible local point of view from which the earth seems flat—within a coherent whole, in which the new conception is perfectly compatible with our grounds for believing the old. Similarly, Einstein's theory "contains" Newtonian mechanics, not merely because it makes the same predictions in the limit of small velocities—although that is also crucially important—but because it so clearly and coherently exhibits one's former understanding of simultaneity as an incomplete perspective on a larger picture. It is at this conceptual level, rather than at the level of prediction or any other quantitative standard, that the new theory has the clearest right to be called more inclusive or comprehensive, and the resulting development cumulative.

In Einstein's revolution, then, philosophical reflection plays some of the roles that were attributed to it by the logical positivists, and that Friedman has placed in a much clearer light: it provides a critical meta-scientific perspective on the foundations of the Newtonian paradigm, from which the inadequacy of some fundamental Newtonian concepts, and the need for a new coordinative definition, could be clearly seen. But philosophy also plays a more internal role, as conceptual analysis proves to be an essential part of the construction of the new definition, and therefore of the very creation of a new conceptual framework. And it is in this role that philosophical analysis most directly confounds the Kuhnian view. More important, seeing it in this role suggests a broader historical perspective on the place of a priori principles in physics, one that reveals an unexpected kinship between Einstein's philosophical work and that of Newton.

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Historically, Newton has not received much credit for philosophical reflection—certainly not from hostile critics such as Leibniz, Berkeley, or the logical positivists, but neither from as sympathetic a reader as Kant. To Kant, Newton seemed content to treat the laws of physics as mere postulates without concerning himself with "their a priori sources" ([1786] 1911, 472). It is important to understand precisely what Kant means by this. He did not mean, for example, that philosophy was in possession of a priori metaphysical principles from which physical laws could be derived; that was among the chief errors of dogmatic metaphysics as undertaken by Descartes, Leibniz, and their followers. Rather, he meant, in the spirit of the Critical philosophy, that Newton's laws did not need to be postulated *or* derived: they could be shown to be conditions of the possibility of comprehending nature as a system, that is, of subsuming nature as a whole under the concepts of the understanding. Hence Kant's complaint against Newton's reluctance to see gravity as an immediate action at a distance, and professed eagerness to discover the underlying causal mechanism—a profession which "set Newton at variance with himself" ([1786] 1911, 515), since on Kant's analysis, an "original attraction" is a condition of the possibility of our experience of matter.[4]

Moreover, it might seem implausible that Newton's revolutionary achievements are the outcome of any sort of conceptual analysis. Universal gravitation was seen by seventeenth-century mechanical philosophers—not unjustly—as a radical discontinuity with established scientific and philosophical thinking. Of the laws of motion, Newton could claim that they were "accepted by mathematicians," meaning that, though no one had explicitly stated them quite as Newton had, they were assumed in the work of Galileo, Huygens, Wallis, and Wren on projectile motion and elastic collisions—indeed, this is precisely what he means when he says that the laws are "confirmed by experiments of many kinds," i.e., that they are presupposed in the solutions of all the mechanical problems that have actually been solved. To this extent the laws can be said to result from a conceptual analysis of what is implicit in the work of his predecessors. But to the extent that the laws are said to hold for attractions or action at a distance as well, it seems that we have to acknowledge a genuine novelty in Newton's conception, something not countenanced by the mechanical philosophy.[5] And it is just this sort of novelty that a mere conceptual analysis would not be expected to produce.

We know the history of Newton's attitude toward his theory of uni-

4. For details of Kant's argument see Friedman 1992, chapter 3.

5. The application of the third law of motion to the attractions between the planets was, for Huygens, one of the most questionable steps in Newton's argument for universal gravitation. See Stein 1967, 179–180.

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versal gravitation, and its relation to the mechanical philosophy. He began by defending its compatibility with mechanism, saying that gravity was a genuine force which he had "deduced from the phenomena," and that it undoubtedly must have a mechanical cause, though the cause has yet to be discovered; he continued for some time to deny that immediate action at a distance is possible, or even reasonable to anyone "who has in philosophical matters any competent faculty of thinking" (Newton [1692] 1995). I think that there is some room for doubt concerning how seriously we should take these protestations on Newton's part. In the Queries to the *Opticks,* we see speculations about possible mechanical causes of gravity alongside a forthright proposal that there is no deeper cause: Newton suggests that the force of gravity, like the forces by which bodies "approach and recede from one another, and cohere in regular figures," is simply one of the "active principles" laid down by the Creator at the beginning, principles characterizing the "active powers" that disturb the particles of matter from their purely inertial states. These remarks indicate that Newton's attitude towards immediate action at a distance was closer to Kant's than Kant had allowed. Not that Newton was prepared to countenance immediate action at a distance in the sense demanded by Kant, or, rather, that Kant thought was demanded by the nature of universal gravitation, but, rather, that Newton took seriously the possibility that gravity had no mechanical explanation, and was instead a kind of "power" or "principle" in its own right—in Kant's words, a kind of "original attraction" after all.

I don't insist on this interpretation of Newton, but in any case his interest in the mechanical cause of gravity (or his acceptance of action at a distance) is not the issue. The important issue is Newton's understanding of the relation between the laws of motion and the forces of nature, and the precise character of the transformation he effected in the concept of forces of nature by allowing for the possibility of "active principles" and even action at a distance. Perhaps the key difference between Newton and the mechanists is the following: for the mechanists, inertia was a property of matter out of which its other properties have to be constructed or composed, at least in the sense that all of their interactions are by the direct "communication of motion," i.e., by impact. If this is a kind of constitutive principle, it is different from the sense in which the laws of motion were constitutive for Newton. For Newton, the laws of motion are constitutive of the kinds of questions that may be asked about matter and its properties—in particular, questions about the ways in which the particles of matter can interact. Whether particles can act on one another directly at a distance is a question that cannot be settled on general philosophical grounds, but may be meaningfully posed within the framework constituted by the laws of motion. In other words, inertia is not the property of matter

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to which (along with extension) its other properties had to be reducible; it is the property that defines how its other properties may be investigated and understood. It is not an exhaustive account of the physical nature of bodies, but the defining principle of a program for discovering the physical nature of bodies, and the forces that animate them. This program is encapsulated in Newton's distinction between the "passive principles" defined by the laws of motion and the "active principles" that cause deviation from an inertial state—between the laws that define inertia and force, and the forces that those definitions enable us to discover. Rather than admit immediate action at a distance, Newton preferred to think of such forces as "spirits" or powers "diffused through space." But to whatever extent he rejected action at a distance, he was not to that extent "set at variance with himself," as Kant claimed; he would be at variance with himself only if he insisted that such "active principles" are reducible to the "passive principle" of inertia, in the manner demanded by the mechanical philosophy.[6]

In spite of its radical novelty, however, Newton's program can still be seen to arise from a conceptual analysis of mechanics as practiced by his mechanistic contemporaries. What enabled him to countenance powers or forces of nature that seemed unintelligible, from the mechanistic perspective, was precisely his analysis of the intelligibility of mechanism itself—that is, his critique of prevailing notions of what made mechanical interactions so preeminently intelligible in the first place. From the mechanical view, as defended by Boyle, Huygens, and Leibniz, among others, the laws of mechanical impact had an intelligible foundation in "the nature of body"—a foundation that was lacking by definition in "attractive forces" as conceived by Newton. Newton realized, however, that we have no clearer idea of what really happens in impact—of what it is in "the nature of body" that explains the phenomena of collisions—than we do of the influence of the moon on the tides. All that we understand of impact is precisely what is contained in the laws of impact; their intelligibility, in other words, consists precisely in their conformity to the laws of motion. It followed that the mechanical philosophy rested on a philosophical misunderstanding: an intelligible interaction is not one that can be reduced to impact, but one that satisfies the conditions under which impact itself is intelligible.[7] If Newton's theory of gravitational force is in obvious ways

6. As Friedman correctly points out, however, Kant's remark is based on a more specific and telling criticism: that Newton's repudiation of immediate action at a distance undermines his entire project for determining the true motions in the solar system. For only as immediate interactions do the mutual influences of the planets fall under the law of action and reaction, and only under this law do they enable Newton to determine the system's center of mass. Cf. Friedman 1992, pp. 172–174.

7. Newton asserted on several occasions, with obvious reference to the mechanical

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a radical challenge to the prevailing conception of physical interaction, it nonetheless rests on a philosophical analysis that recognizes that conception for what it truly is, i.e., an analysis that places its objective empirical content, and its function in physical reasoning, in a clearer light. Given this deeper understanding of force and interaction, the question "what are the forces of nature?"—including whether they can act otherwise than by contact—is a straightforward empirical question that can be answered by definite procedures.[8]

This dialectical confrontation with prevailing conceptions, revealing their objective content, their shortcomings, and their implications for physics, is, I suggest, typical of Newton's philosophical interaction with his contemporaries. Another obvious example is his solution of the "frame of the system of the world," which begins with "Hypothesis I: That the center of the system of the world is at rest." The hypothesis is explicitly meant as a dialectical opening, because it is the common assumption of both sides in the controversy between the geocentric and heliocentric views; in the analysis of "the world" as a dynamical system, this hypothesis leads to the conclusion that both sides are wrong, as only the center of gravity can be at rest ([1726] 1999, 816). Evidently this argument rests on Newton's recognition that dynamics, as understood by himself and his contemporaries, explicates the notion of a "center of a system of bodies" in a novel way, and provides a criterion for identifying such a center that had been entirely lacking before. A less obvious example is Newton's argument for his theory of absolute motion. As I have argued elsewhere (DiSalle 2002a, 2002b), Newton's "water bucket" argument, and other

philosophy, that we have no deeper understanding of impenetrability or extension than we have of gravity—i.e., that what he had discovered about gravity in the course of the *Principia* is at least as securely known, and at least as well understood, as the properties of bodies that were countenanced by the mechanists. As he remarked in the discussion of the third Rule of Philosophizing, we know that bodies are impenetrable "not by reason but by our senses" ([1726] 1995, 795). Moreover, "the argument from phenomena will be even stronger for universal gravity than for the impenetrability of bodies, for which, of course, we have not a single experiment, and not even an observation, in the case of the heavenly bodies" ([1726] 1995, 796).

8. This discussion is not meant to gloss over the ambiguities, even the tensions, in Newton's conceptions of force and inertia, as treated in detail by McMullin (1978, especially pp. 33–47). It is meant to suggest, however, that Newton's own decision to "play down" such ontological difficulties, in the presentation of the *Principia,* reflects more than a reluctance to invite controversy on philosophical issues that are secondary to the formal argument of the book (cf. McMullin 1978, 52). It also reflects the direct engagement of a philosophical task that is absolutely central to the aims of the book: to make explicit the conceptual framework implicitly shared by Newton and the mechanical philosophers, in order to show that Newton's most controversial ideas—especially concerning space, time, motion, and force—make sense within that framework. (See below and DiSalle, 2002a.)

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similar arguments, are not meant to demonstrate the existence of absolute motion (or space and time), against the relativistic theory of the Cartesians. Rather, they show that his conception of rotation is implicit in his opponents' own thinking; it is the very conception that they themselves employ in their treatment of dynamical problems, above all in their vortex theory of planetary motion. Newton's argument therefore has the character, not of an inductive argument for his hypothesis, but of a conceptual analysis, revealing the contradictions between the ideology of the relativists, and the assumptions that implicitly guide their scientific reasoning. Most strikingly, the analysis reveals that the most basic question posed by Cartesian science—what is the causal explanation for the celestial motions?—makes sense only in a framework in which Newton's distinctions between rotation and nonrotation, uniform motion and acceleration, are taken for granted.

Contrary to what the Kuhnian view would lead us to expect, Newton's philosophical criticisms are focused on a set of widely shared beliefs and practices, rather than on a conflict between incommensurable points of view; Newton's era was, after all, one of ascendancy rather than crisis for the basic framework of classical mechanics. Yet Newton's philosophical analysis of the framework was essential to its achieving maturity as a science. This circumstance sheds some further light on the philosophical interest that Newtonian physics had for Kant, as well as on the larger concerns of the present paper. For the Newtonian revolution, to Kant, manifested a philosophical revolution:

> Reason . . . approaches nature in order to be taught by it: but not in the character of a pupil who submits to everything that the teacher tells him, but as an appointed judge who compels the witness to answer the question which he poses. And thus even physics owes the beneficial revolution in its way of thinking entirely to the happy thought that, in accordance with what reason itself has originally placed in nature, we ought to seek in nature (and not invent for it) whatever reason has to learn from nature and could not know by itself. In this way the study of nature first came upon the secure path of a science, after having for many centuries done nothing but grope in the dark. (Kant [1787] 1956, Bxiii–xiv).

It is evidently no accident that such remarks reflect a proto-Kuhnian distinction between "pre-paradigm" science and "mature" science, in which the paradigm defines a research program and a set of determinate "puzzles"—in short, a framework for "normal science." For Kuhn himself, as Friedman reminds us, characterized his own general view as "Kantianism with movable categories." But for Kant this distinction is connected with a larger philosophical problem: to explain the *conceptual prerequisites* for

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normal science, i.e., the conceptual foundation that makes a theory *capable* of supporting a tradition of normal science—i.e., capable of generating puzzles to solve, capable of generating questions that may be answered by some definite procedure. And this problem was connected with a question about the comparative failure of philosophy: how is it that Newtonian science has achieved the kind of universal assent that has always eluded philosophy, and most of the sciences as well? Like Kuhn, Kant did see traditional philosophy as mired in hopeless clashes of perspective—inconclusive "debates over fundamentals" of the sort that scientists engage in when they are "behaving like philosophers"—but hoped that philosophy could escape this predicament by understanding how Newtonian science had done so.

From the Kuhnian perspective, the progression from pre-paradigm science to mature science essentially involves a social decision by the relevant group, to accept a particular "form of life" without further serious question, at least until sufficiently disturbing anomalies begin to accumulate. It is understandable that this view would recommend itself over that of, say, Popper, who seemed to suggest that the typical practice of scientists was to put their theories to severe tests. To the extent that "philosophy of science" aims to characterize the typical practice of scientists, and in particular the relation between their empirical research and the prevailing theory, philosophers of science could not be blamed for sharing Kuhn's assessment of Popper, that "Sir Karl has characterized the entire scientific enterprise in terms that apply only to its occasional revolutionary parts" (Kuhn 1970, 6). And Kuhn even suggested, echoing Kant, that the ability to generate puzzles provided the "demarcation criterion" between science and pseudo-science that Popper had mistakenly sought elsewhere. But to consider how a theory might arrive at this state—what must characterize the fundamental concepts of a theory, so that it might provide a foundation for normal science—lay quite beyond the scope of Kuhn's concerns.

For Kant, by contrast, this was perhaps the central problem of philosophy. If philosophy was to achieve the certainty of the sciences, it would not suffice to imitate their deductive structure; as the history of philosophy clearly showed, metaphysical systems could be just as "rational" as the sciences without thereby establishing any certain results, or settling in any way the traditional metaphysical disputes. Philosophy could be entirely rational, in other words, without ceasing to be entirely subjective. The difficulty lay in the essential arbitrariness of the fundamental concepts, which tended to be incompletely understood, like those of God and the soul, or merely invented, like that of the monad. In the exact sciences, by contrast, deductive arguments could yield genuine knowledge because the fundamental concepts were arrived at by *construction* in accord with the forms of spatial and temporal intuition—in other words, by objective pro-

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cedures that left no room for doubt or disagreement about the meaning of a concept, or what might fall under it. The achievement of Newtonian mechanics, for Kant, was more than the scientific one of accounting for the phenomena with unprecedented simplicity and accuracy; it was also the philosophical achievement of avoiding the (Kuhnian) predicament in which philosophy stood, by placing natural philosophy on the foundation of objective concepts of space, time, motion, and force. Mechanics, like geometry, had attained "paradigmatic" status not by representing the most rational choice among competing possibilities, much less through a mere social-political decision by the relevant scientific groups. Rather, it was paradigmatic because it expressed the first and only clear conceptions of space and time, motion and force—most remarkably, the first and only conception of causal interaction under which the entire universe could be comprehended in a system of causal interdependency. Precisely this philosophical achievement, not some inexplicable change of perspective, transformed cosmology from a mere objectification of the subjective geocentric perspective, to an exact science of the spatio-temporal and causal relations among the bodies of the universe.

This account of Kant applies, mutatis mutandis, to the logical positivists in relation to Einstein's theories. To Kuhn, philosophers of science before him seemed to find such revolutionary work "better reading" than the everyday practice that is truly definitive of science (1970, 6). But the great concern of the positivists was not philosophy of science, but philosophy. And the work of Einstein concerned them, not as typical of the practice of scientists in general, nor even as an ideal for scientific practice, but as exemplifying the philosophical analysis of fundamental concepts, and the critical examination of the connections between our conceptual schemes and our experience. In other words, Einstein's revolutionary work was uniquely informative, not for philosophy of science as understood by Popper and Kuhn, but for philosophy proper. Einstein's particular paradigm shift had to happen, as the positivists saw it, precisely because the evolution of theoretical physics had brought it into confrontation with these deeper philosophical concerns, and because the outstanding theoretical problems demanded a philosophical analysis. For the positivists, as for Kant, reflection on fundamental concepts was indeed part of "scientific practice"—not only because it was scientists such as Einstein and Newton who had done it, but because the practice of the uncritical "normal scientist" utterly depends on its having been done.

This brings me to my main point about Friedman, Kant, and logical positivism, which concerns the relation between philosophy of science and science itself. Philosophy of science generally assumes the task of assimilating the best of contemporary science, analyzing its methods, and digesting its philosophical implications. What was most distinctive about

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the views both of Kant and of the positivists, in contrast, was their sense that the best science of their time was of philosophical interest not merely as such, but because it had achieved something definitive *as philosophy;* the point of the philosophical analysis of science, as distinct from "philosophy of science" in our sense, was to explain how and why this was the case—not to justify the methods or the results of science, but to show that in the progress of science, a certain philosophical ground had been definitively and irrevocably won.

Friedman, meanwhile, is content to illuminate the crucial role that philosophy has played in ensuring the rationality of scientific revolutions, by providing an intellectual context in which the prevailing paradigm can be rationally criticized, and unorthodox ideas developed and rationally discussed. This is not simply because he is a turn-of-the-century "philosopher of science" addressing the late-twentieth-century doubts about the rationality of science; no one has shown more sympathy and concern than Friedman for the broader philosophical aims of Kant and the positivists. Rather, it is because just those philosophical aims, and just their sense of the unique philosophical significance of their own contemporary science, seem to embody the weaknesses of their respective philosophical outlooks. That Newton's laws are, more than exemplary science, synthetic a priori principles, necessary conditions of our understanding of nature, appears to be both the most distinctively Kantian and the least credible claim in Kant's treatment of physics. The positivists' treatment of relativity, on the other hand, and of its special philosophical achievements, appealed to epistemological notions that now seem quite naive and out of touch with the nature and the practice of physics. Their belief that general relativity had eliminated all metaphysical entities in favor of immediately observable relations, and that this marked a decisive break with all previous theories of space and time, involved confusions about general relativity, the previous theories of space and time, and epistemology in general. It is with respect to the positivists' understanding of relativity, in particular, that Kant's understanding of Newtonian physics seems exemplary.

These shortcomings of Kant and the positivists, we can now see, reveal the shortcomings in their respective views of conceptual analysis. According to the positivists, Kant had correctly identified the non-empirical and formal character of the fundamental concepts of natural science—had recognized, that is, that they are a priori and cannot originate in experience, because of the constitutive role that they play in our grasp of experience. But he mistakenly viewed those concepts as founded on the a priori categories of the understanding, and the laws of nature as synthetic a priori truths about possible experience. He thus missed the point that they are essentially meaning-constitutive or analytic, and therefore can only be fixed by convention. But this assessment does little justice to the subtlety

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of Kant's view. In regarding the constitutive principles as synthetic a priori, Kant was not implying that they are immediately given or uncritically assumed; he had too clear a sense of their historical development, and the efforts that had brought about their definitive expression, to take such a view. (Kant's view was clearly *not* "Kuhnianism with a fixed paradigm.") On the contrary, he was recognizing that the principles had been, and could only have been, discovered by some kind of conceptual analysis or "transcendental deduction" that exposes their constitutive role—as exemplified by Newton's defense of the laws of motion as the assumptions implicit in the solution of mechanical problems. And the empirical principles that Kant had taken to be valid a priori were overturned precisely when a novel conceptual analysis, in novel historical and epistemic circumstances, brought some new constitutive principles to light: in the case of Euclidean geometry, the analyses of Helmholtz and Poincaré that revealed the constitutive role of the principle of free mobility; in the case of Newtonian mechanics, Einstein's analysis of the constitutive role of light-propagation in the fixing of frames of reference. The problem with Kant's view, then, was not his failure to regard such principles as conventional stipulations. The problem was, instead, his failure to appreciate the empirical and historical contingency of the supposed "conditions of the possibility of experience"; he understood how such principles can be objectively regarded as necessary, but missed the point that their necessity is *relative*—not indeed relative to a set of stipulations, like the necessity imposed by the conventional rules of a game, but relative to a given body of empirical principles and practices, of whose possibility they are the conceptual conditions.

The positivists, meanwhile, appreciated the contingency and the mutability of constitutive principles, and gathered from Einstein's example that fundamental concepts must be, as our empirical knowledge develops, re-evaluated and even replaced. But they had an overly restrictive view of the function of conceptual analysis in this process; they saw it as the destructive application of empiricist and "antimetaphysical" epistemological criteria—criteria that have come to seem, both for scientific methodology and for general philosophy, quite naive—and so had no account of the construction of novel conceptual frameworks except by arbitrary stipulation. The Kantian notion of the *discovery* of a constitutive principle simply made no sense from their point of view.

This essay has been an effort to make sense of that notion, to free it of the difficulties with which Kant had burdened it, and to show that it illuminates some of the most sweeping conceptual transformations in the history of science—transformations of which Kuhn's notion of paradigm shift provides only the most obscure picture. It is not surprising that Kuhn should have doubted that "scientific method," in any familiar sense, could

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justify such a change of constitutive principles; since the basic laws of physics cannot be understood as empirical laws, the argument for them cannot be empirical either, but must be a kind of transcendental argument. But such arguments give an intelligibility to conceptual transformations that Kuhn's view denies them. Moreover, they illustrate a central aspect of Kant's philosophy of science that the downfall of Euclidean geometry and Newtonian physics has not overturned: as we have seen, the dialectical arguments used by people such as Helmholtz, Poincaré, and Einstein have exactly this transcendental character. What is captured neither by Kant's view, nor by the conventionalism of the logical positivists, is a proper sense of the empirical context of such transcendental arguments, a sense of the ways in which physical conditions, and the empirical principles that we develop in our efforts to comprehend them, can determine our conceptual resources, and limit or enlarge our conceptual horizons.

REFERENCES

DiSalle, Robert (2002a), "Newton's Philosophical Analysis of Space and Time", in *The Cambridge Companion to Newton.* Cambridge: Cambridge University Press.

——— (2002b), "Conventionalism and Modern Physics: A Re-Assessment", *Noûs* 36: 169–200.

Earman, John (1989), *World Enough and Spacetime.* Cambridge, MA: M.I.T. Press.

Einstein, Albert (1905), "On the Electrodynamics of Moving Bodies", in Einstein et al. 1952, 35–65.

——— (1911), "On the Influence of Gravitation on the Propagation of Light", in Einstein et al. 1952, 97–108.

——— (1916), "The Foundation of the General Theory of Relativity", in Einstein et al. 1952, 109–164.

——— (1917), *Über die spezielle und die allgemeine Relativitätstheorie (Gemeinverständlich),* 2nd ed. Braunschweig: Vieweg und Sohn.

——— (1922), *The Meaning of Relativity.* Princeton: Princeton University Press.

Einstein, Albert, H. A. Lorentz, Hermann Minkowski, and Hermann Weyl (1952), *The Principle of Relativity.* Translated by W. Perrett and G. B. Jeffery. New York: Dover Books.

Friedman, Michael (1992), *Kant and the Exact Sciences.* Cambridge, MA: Harvard University Press.

——— (1999), *Reconsidering Logical Positivism.* Cambridge: Cambridge University Press.

——— (2002), *Dynamics of Reason: The 1999 Kant Lectures at Stanford University.* Chicago: University of Chicago Press.

Galilei, Galileo ([1632] 1967), *Dialogue Concerning the Two Chief World Systems–Ptolemaic and Copernican.* Translated by Stillman Drake. Berkeley and Los Angeles: University of California Press.

Hall, A. Rupert and Marie Boas Hall (eds.) (1962), *Unpublished Scientific Papers of Isaac Newton.* Cambridge: Cambridge University Press.

Kant, Immanuel ([1786] 1911), *Metaphysische Anfangsgründe der Naturwissenschaft.* Volume 4 of *Gesammelte Schriften,* Akademie Ausgabe. Berlin: Reimer.

——— ([1787] 1956), *Kritik der reinen Vernunft,* 2nd ed. Hamburg: Felix Meiner Verlag.

Kuhn, Thomas (1962), *The Structure of Scientific Revolutions.* Chicago: University of Chicago Press.

——— (1970), "Logic of Discovery or Psychology of Research?", in Imre Lakatos and Alan Musgrave (eds.), *Criticism and the Growth of Knowledge.* Cambridge: Cambridge University Press.

https://doi.org/10.1086/341049 Published online by Cambridge University Press

McMullin, Ernan (1978), *Newton on Matter and Activity.* Notre Dame: University of Notre Dame Press.
Minkowski, Hermann (1908), "Space and Time", in Einstein et al. 1952, 75–91.
Newton, Isaac ([1692] 1995), Fourth Letter to Richard Bentley, in I. Bernard Cohen and Richard S. Westfall (eds.), *Newton: Texts, Backgrounds, Commentaries.* New York: W. W. Norton, 336–339.
——— ([1726] 1999). *The Principia: Mathematical Principles of Natural Philosophy,* tr. I. Bernard Cohen and Anne Whitman. Berkeley and Los Angeles: University of California Press.
——— ([1729] 1962), *The System of the World,* in Florian Cajori (ed.), *Sir Isaac Newton's Mathematical Principles of Natural Philosophy and His System of the World,* 2 vols. Translated by Andrew Motte. Berkeley: University of California Press.
Poincaré, Henri (1899), "Des fondements de la géométrie: A propos d'un livre de M. Russell." *Revue de Metaphysique et de Morale* 7:251–279.
——— (1913), *The Foundations of Science: Science and Hypothesis; The Value of Science; Science and Method.* Translated by George Bruce Halsted. Lancaster, PA: The Science Press.
Reichenbach, Hans ([1927] 1957), *ThePhilosophy of Space and Time.* Translated by Maria Reichenbach. New York: Dover Publications. Originally published as *Philosophie der Raum-Zeit-Lehre* (Berlin).
——— ([1920] 1965), *The Theory of Relativity and a Priori Knowledge.* Translated by Maria Reichenbach. Berkeley and Los Angeles: University of California Press. Originally published as *Relativitätstheorie und Erkenntnis Apriori* (Berlin).
Russell, Bertrand (1897), *An Essay on the Foundations of Geometry.* Cambridge: Cambridge University Press.
Stein, Howard (1967), "Newtonian Space-Time", *Texas Quarterly* 10: 174–200.
Torretti, Roberto (1983), *Relativity and Geometry.* Oxford: Pergamon Press.
——— (1989), *Creative Understanding.* Chicago: University of Chicago Press.

https://doi.org/10.1086/341049 Published online by Cambridge University Press