

# “Let me briefly indicate why I do not find this standpoint natural.” Einstein, General Relativity, and the Contingent A Priori

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## *Introduction*

In his recent *Dynamics of Reason* (Friedman 2001) and other writings (see, especially, Friedman 2002a, 2002b), Michael Friedman urges the reintroduction of a robust notion of the a priori within the philosophy of science as a bulwark against the relativism that many see as the most worrisome consequence of Thomas Kuhn’s critique of neo-positivist fantasies about a theory-choice algorithm (Kuhn 1962), a bulwark not afforded by either the linguistic conventionalism of Rudolf Carnap (Carnap 1934, 1950) or the semantic and theory holism of W. V. O. Quine (1951, 1960). Taking inspiration from early work by Hans Reichenbach (1920) and Ernst Cassirer (1921), Friedman argues that the history of mechanics and the theory of gravitation from Newton to Einstein reveals an enduring structure wherein selected elements of theory, such as Newton’s laws in mechanics and the principle of equivalence in general relativity, play a contingent a priori role relative to the specific theories within which they live. They are a priori in the sense that their assumption is necessary in order to invest other elements of the theory in question with empirical content. In the Newtonian case, it is the law of universal gravitation that becomes, thereby, an empirical law; in Einstein’s case, it is the field equations of general relativity. These a priori elements are contingent in the sense that, with the progress of theory, they can be revised or jettisoned, but this is held always to happen in such a way that both the successes and the failures of a predecessor theory can be explained from the perspective of the successor theory, as when

Newtonian dynamics is viewed as a low-velocity limit of special relativity, even if, from the point of view of the predecessor theory, the successor theory appears incommensurable with the predecessor. Friedman dubs this diachronic relationship one of “prospective irrationality” and “retrospective rationality” (Friedman 2001, 93-103).

That a theory such as general relativity can be so parsed is indisputable, and Friedman’s perspective offers helpful insight into the systematic role that certain principles can play in the larger body of physical theory. Open to dispute, however, is the question whether this is the correct view of the history of mechanics and gravitation theory or the most helpful view of the manner in which theories acquire empirical content. It is, surely, a noteworthy historical fact that the author of the general theory of relativity, Albert Einstein, pondered precisely this view in its earlier forms associated with Reichenbach and Cassirer, and found it wanting. Einstein recognized that any theory can be reconstructed in such a way as to have some elements of the theory serve as that via which other elements acquire their empirical content, and he argued that some such parsing is always necessary if one is to exhibit clearly the manner of a theory’s attachment to the empirical. What he disputed was the necessity of any one such parsing of theory, arguing that other parsings are always possible, and that it is the arbitrariness in our choice among parsings that is the main reason for not viewing the elements privileged in any one such parsing as a priori.

Who is right? Friedman or Einstein? That is one question that will concern us in what follows. A more fundamental question is this: Why, in his reconstruction of the history of mechanics and gravitation theory, does Friedman not tell the story of Einstein’s rejection of more or less exactly the same view of the contingent a priori that Friedman defends? This is a question about method in arguments for the contingent a priori. We are given no transcendental argument—in the strict Kantian

sense—for the contingent a priori, nor should we expect one.<sup>1</sup> The primary evidence for the contingent a priori is historical evidence. That Friedman does offer in the form of detailed reconstructions of Newtonian dynamics and Einsteinian gravitation theory. But why are these reconstructions alone evidential? Why do the self-understandings of the historical actors not also count as evidence for and against claims on behalf of the contingent a priori? Why do they not have standing as evidence for and against the historical reconstructions otherwise taken as evidential? Of course it could well be that the historical actors are just wrong in their understandings of the way the theories that they created work. But if the question is whether some element of gravitation theory, such as the principle of equivalence, *must* be regarded as a priori, on pain of depriving of empirical content other elements of the theory, such as the field equations, then is it not relevant that some of those most intimately and prominently involved in the genesis of the theory and in early debates about its empirical credentials did not so regard the equivalence principle? Is it not all the more relevant when, as in the case of Einstein, their alternative picture of the theory's relation to the empirical was the product of long, careful, philosophically sophisticated reflection on all that is at stake in the choice? If Einstein was wrong, then we need an argument showing how and why he was wrong. My own view is that Einstein's argument against the contingent a priori is a compelling one.

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<sup>1</sup> The analysis of Weyl's transcendental idealism in Ryckman 2005 perhaps affords a kind of transcendental argument for an a priori element in space-time theory in the form of the demand that only such structures be admitted as fundamental in physical theory as are epistemically accessible in the space of intuition of the ideal observer. But that some specific structure is thus epistemically accessible is, presumably, a universal and eternal, if previously unknown, fact. That circumstance seriously mitigates the degree of contingency ascribable to such elements of theory. A general dilemma suggests itself: transcendental arguments do not yield contingent conclusions. So historical evidence could well be the only possible evidence for the contingent a priori.

*Inertial Motion, the Equivalence Principle, and the Contingent A Priori*

Friedman distinguishes three (or four) strata of scientific knowledge that are held to be discernible at least in the history of mechanics and gravitation theory from Newton to Einstein:

1. *Empirical laws of nature*, such as Newton's law of universal gravitation or Einstein's field equations for general relativity.
2. *Constitutively a priori principles*. These are contingent and revisable, but not "empirical in the same sense" as the empirical laws of nature, because they are presuppositions for the possibility of the empirical testing of empirical laws. They function in an a priori manner only relative to a specific theory. They include:
  - 2a. *Mathematical and geometrical principles*, such as the calculus for Newton and differential geometry for Einstein. They are a priori in the sense that they function as the language in which basic mechanical principles and empirical laws of nature are formulated.
  - 2b. *Basic principles of mechanics*, such as Newton's laws of motion and Einstein's principle of equivalence. These are a priori in the sense that they are necessary conditions for the possibility of the empirical laws' possessing empirical content.
3. *Philosophical meta-paradigms or meta-frameworks*, such as Scholastic natural philosophy or the mechanical world view.

A similar structure is expected to be found in theories generally, but a detailed exposition of this structure is provided only for the case of mechanics and gravitation theory from the seventeenth through the twentieth centuries.

Our concern here is with the relationship between basic principles and empirical laws. The general pattern is evinced already in Newtonian gravitation theory, where the laws of motion "are presuppositions of the properly empirical laws," their function being "precisely to mediate between abstract mathematical representations and the concrete empirical phenomena these abstract mathematical representations are intended to describe" (Friedman 2001, 77). They do this via their work in picking out the center-of-mass frame of, first, the planetary system and, then, the galaxy as

a whole (the universe as Newton knew it), thereby approximating absolute space as the frame of reference with respect to which unforced motion is properly viewed as inertial and, thus, the frame of reference with respect to which a force law such as the law of universal gravitation claims empirical validity.<sup>2</sup>

An analogous relationship is held to obtain between Einstein's field equations for general relativity and the principle of equivalence. Of the space-time structure determined by the field equations, Friedman writes:

Such a variably curved space-time structure would have no empirical meaning or application, however, if we had not first singled out some empirically given phenomena as counterparts of its fundamental geometrical notions—here the notion of geodesic or straightest possible path. The principle of equivalence does precisely this, however, and without this principle the intricate space-time geometry described by Einstein's field equations would not even be empirically false, but rather an empty mathematical formalism with no empirical application at all. (Friedman 2001, 38)

There are, of course, differences between the Newtonian and Einsteinian cases. Inertial motion in general relativity corresponds to free fall in a gravitational field, whereas inertial motion in Newtonian mechanics is completely unforced, gravity not there playing a role in structuring space-time itself. Still, the analogy runs deep:

The principle of equivalence depicts the space-time trajectories of bodies affected only by gravitation as geodesics or straightest possible paths in a variably curved space-time geometry, just as the Newtonian laws of motion, when viewed from the space-time perspective, depict the trajectories of bodies affected by no forces at all as geodesics or straightest possible paths in a flat or Euclidean space-time geometry. (Friedman 2001, 38)

Though functioning in the indicated a priori fashion with respect to basic empirical laws, elements of theory such as the principle of equivalence cannot—as *contingent* principles—be wholly

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<sup>2</sup> For a detailed reconstruction of this way of understanding the relationship between the laws of motion and the law of universal gravitation, see Friedman 1992, 141-177.

devoid of connections to the world of experience. Friedman acknowledges that they must at least have empirical “counterparts” and empirical motivations (Friedman 2001, 86-92). But they cannot possess empirical content in the same way as empirical laws, for it is such principles that ground the possibility of the laws’ having empirical content. Contingent a priori principles thus play a role similar to that of coordinating principles in the orthodox logical empiricist story about empirical content, but with the difference that they are not mere conventions.<sup>3</sup> For Friedman, they are distinguished from empirical laws not as that which may be freely varied, as with stipulative, coordinating definitions of rods and clocks, but structurally and functionally as that without which the rest of a theory would lack content.

That there must be some such functional distinction between empirical laws and a priori principles is a central premise behind Friedman’s larger philosophical ambitions for the contingent a priori. He argues that it is only by recognizing the existence of such asymmetries between the roles played by different components of theory, and by viewing the principles that effect the coordination with experience as more than mere conventions, that we can block the slide into epistemological relativism that is, on Friedman’s analysis, the fate of Carnap, Kuhn, and Quine. Thanks to their investigating the crucial framing work performed by languages and paradigms, Carnap and Kuhn were at least working in the right direction, according to Friedman, even if they lacked the gumption fully to embrace the a priori status of those frames. Quine’s holism, on the other hand, fails

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<sup>3</sup> Reichenbach 1928 is the locus classicus for this notion of coordinating definitions, though the idea first makes its appearance several years earlier in Reichenbach’s work (see Reichenbach 1924) as the now merely conventional descendent of the robust notion of the constitutive a priori introduced in Reichenbach 1920. It was Moritz Schlick who urged upon Reichenbach the reconceptualization of the constitutively a priori principles of coordination as merely conventional coordinating definitions. For details, see Howard 1994 and Ryckman 2005.

utterly as an antidote to relativism thanks to its turning any principled, functional difference in kind among components of theory into at best a difference in degree of centrality to the web of belief and, consequently, in degree of resistance to revision. It is, moreover, simply historically false as a story about how principles like Newton's laws and the principle of equivalence actually function (Friedman 2001, 33-46). Much is at stake, therefore, in assessing Friedman's argument for the contingent a priori way of regarding the status of such principles.

*Duhem's Critique of Poincaré and Le Roy*

Einstein's doubts about the contingent a priori are, as announced above, the main issue to be explored in assessing Friedman's way of regarding the principle of equivalence. Let us begin, however, with Pierre Duhem. That Duhem was Quine's patron saint is one excuse for so beginning. More pertinent is the fact that Duhem's theory holism was—so I have long argued (see Howard 1990)—among the most important influences shaping Einstein's philosophy of science. Most pertinent, however, is the following ironic fact, namely, that one of Duhem's own most important deployments of his theory holism against those who would regard some physical principles as insulated against easy empirical refutation thanks to their role, qua definitions, in securing the empirical status of laws concerned precisely the example of the law of free fall.

The immediate target of Duhem's criticism was the conventionalism of Henri Poincaré (1901) and Edouard Le Roy (1901). I quote Duhem at length so as to make clear the almost uncanny relevance of his discussion of the issue:

Foremost among the assertions in which we shall aim at eliminating the appearance of paradox, we shall place one that has recently been formulated and discussed. . . . It has been developed at length and forcefully by H. Poincaré with regard to principles of mechanics. Edouard Le Roy has also formulated it with great clarity.

That assertion is as follows: Certain fundamental hypotheses of physical theory cannot be contradicted by any experiment, because they constitute in reality *definitions*, and because certain expressions in the physicist's usage take their meaning only through them.

Let us take one of the examples cited by Le Roy:

When a heavy body falls freely, the acceleration of its fall is constant. Can such a law be contradicted by experiment? No, for it constitutes the very definition of what is meant by "falling freely." If while studying the fall of a heavy body we found that this body does not fall with uniform acceleration, we should conclude not that the stated law is false, but that the body does not fall freely, that some cause obstructs its motion, and that the deviations of the observed facts from the law as stated would serve to discover this cause and to analyze its effects.

Thus, M. Le Roy concludes: "laws . . . constitute the very criterion by which we judge appearances as well as the methods that it would be necessary to utilize in order to submit them to an inquiry whose precision is capable of exceeding any assignable limits." (Duhem 1906, 208-209)

What Le Roy terms "laws" are what are presently under discussion under the heading of putatively contingent a priori principles or coordinating principles, namely, those elements of a theory whose role is to secure the empirical import of the other elements of a theory. Is Le Roy correct? What if experiment seemed to contradict our expectation that a body would fall freely? It follows from Duhem's holism that we are not constrained to postulate heretofore unsuspected forces influencing the body's fall. No, as always, we have a choice. We could postulate new forces or we could change the "law," that is, change the definition of "free fall." Duhem remarks:

We have here nothing more than a particular application of the principle set down above in Section 2 of this chapter. A disagreement between the concrete facts constituting an experiment and the symbolic representation which theory substitutes for this experiment proves that some part of this symbol is to be rejected. But which part? This the experiment does not tell us; it leaves to our sagacity the burden of guessing. (Duhem 1906, 211)

A second major example adduced by Duhem is the principle of inertia, about which Duhem's antagonists—this time Poincaré, himself—had made claims more or less identical to that made on behalf of the law of free fall. Can we regard the principle of inertia as being susceptible to empirical test? Duhem writes:

One thing is indeed certain, namely, that whatever the motion of a material point is, when seen from a first frame of reference, we can always and in infinite ways choose a second frame of reference such that seen from the latter our material point appears to move in a straight line with uniform motion. We cannot, therefore, attempt an experimental verification of the principle of inertia; false when we refer the motions to one frame of reference, it will become true when selection is made of another term of comparison, and we shall always be free to choose the latter. . . .

Therefore, it would be absurd to wish to subject certain principles of mechanics to *direct* experimental test . . . .

Does it follow that these hypotheses placed beyond the reach of direct experimental refutation have nothing more to fear from experiment? That they are guaranteed to remain immutable no matter what discoveries observation has in store for us? To pretend so would be a serious error.

Taken in isolation these different hypotheses have no experimental meaning; there can be no question of either confirming or contradicting them by experiment. But these hypotheses enter as essential foundations into the construction of certain theories of rational mechanics. . . .

In truth, hypotheses which by themselves have no physical meaning undergo experimental testing in exactly the same manner as other hypotheses. Whatever the nature of the hypothesis is, we have seen at the beginning of this chapter that it is never in isolation contradicted by experiment; experimental contradiction always bears as a whole on the entire group constituting a theory without any possibility of designating which proposition in this group should be rejected. (Duhem 1906, 213-215)

Duhem discusses the law of free fall and the principle of inertia in chapter six, “Physical Theory and Experiment,” of *La Théorie physique, son objet et sa structure* (Duhem 1906), the chapter presenting the core argument for theory holism. The evidence strongly suggests that Einstein read *La Théorie physique* in 1909 or 1910 (see Howard 1990, 1994). He had clearly assimilated the lesson of Duhem’s holism by the winter semester of 1910-1911, when, in lectures on electricity and magnetism at the University of Zurich he explained to his students how one can introduce the concept of charge in the interior of solid charged body, even though one cannot employ here the fiction of a test charge:

We have seen how experience led to the introduction of the concept of electrical charge. It was defined with the help of forces that electrified bodies exert on each other. But now we extend the application of the concept to cases in which the definition finds no direct application as soon as we conceive electrical forces as forces that are exerted not on material

particles but *on electricity*. We establish a conceptual system whose individual parts do not correspond immediately to experiential facts. Only a certain totality of theoretical materials corresponds again to a certain totality of experimental facts.

We find that such an el[ectrical] continuum is always applicable only for representing relations inside ponderable bodies. Here again we define the vector o[f] el[ectrical] field strength as the vector of the mech[anical] force that is exerted on a unit of pos[itive] electr[ical] charge inside a ponderable body. But the force thus defined is no longer immediately accessible to exp[eriment]. It is a part of a theoretical construction that is true or false, i.e., corresponding or not corresponding to experience, only *as a whole*. (Einstein 1910-1911, 325)

A few years later, Einstein's sympathy for theory holism was reinforced when he encountered in the writings of Schlick (1915, 1917, 1918) a version of theory holism owing more to David Hilbert than to Duhem,<sup>4</sup> and both semantic and theory holism became a cornerstone of Einstein's mature philosophy of science (see Howard 1984, 1994, 2004, 2006). How did this holism shape Einstein's reaction to Kant and to the notion of the contingent a priori known to him mainly through Reichenbach's work?

### *Einstein's Critique of the A Priori*

Like many of his generation, Einstein had read and studied Kant from an early age. He apparently read all three *Critiques* for the first time in his teens. He elected to enroll in August Stadler's lectures on Kant while a student at the ETH in the 1890s. On many later occasions he returned to Kant, as when in 1918 he read the *Prolegomena*. He had personal connections to both the Marburg critical idealist community centered around Hermann Cohen and Paul Natrop and the Berlin critical realist community centered around Alois Riehl. And he carried on sustained and

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<sup>4</sup> Note that this is the pre-Vienna Circle Schlick, whose epistemology in the 1910s differed markedly from the more orthodox logical empiricism that he famously defended in the 1920s and 1930s. Foremost among the differences are the theory holism and the blunt defense of realism—in opposition to Mach—in the 1910s. For more on the development of Schlick's thinking see Howard 1994.

sophisticated discussions with thinkers like Reichenbach and Ernst Cassirer who were, in the 1920s, renegotiating the relationship between the Kantian tradition and the new empiricisms flowering in Berlin and Vienna.<sup>5</sup>

The general tendency of Einstein's mature reaction to Kant is clear at least from July of 1918, when, while on vacation, he wrote to Max Born:

I am reading Kant's *Prolegomena* here, among other things, and am beginning to comprehend the enormous suggestive power that emanated from the fellow and still does. Once you concede to him merely the existence of synthetic a priori judgments, you are trapped. I have to water down the "a priori" to "conventional," so as not to have to contradict him, but even then the details do not fit. Anyway it is very nice to read, even if it is not as good as his predecessor Hume's work. Hume also had a far sounder instinct. (Born 1969, 25-26)

On more than one later occasion he made essentially the same point about preferring to designate as conventional those elements of theory whose characterization as a priori is the mark of one's allegiance to the Kantian tradition. Why did he prefer to regard as conventional what the followers of Kant deemed a priori? One reason was Einstein's skepticism about the Kantian doctrine of intuition, a skepticism amusingly expressed in a letter of 15 September 1919 to Ilse Schneider:

I have received the mentioned dissertation by S. [Ewald Sellien] (Epistemology and Relativity Theory). . . . Kant's celebrated view on time reminds me of Andersen's tale about the emperor's new clothes, except that instead of the emperor's clothes, it concerns the form of intuition. (Rosenthal-Schneider 1980, 83-84; see Sellien 1919)

But the more fundamental reason for Einstein's doubts about the a priori was rooted in his commitment to theory holism of the Duhem and (early) Schlick varieties.

In the early 1920s, especially, Einstein's correspondence and writings are dense with discussions of precisely why and how he does not find the notion of the a priori a helpful one. His

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<sup>5</sup> For more on Einstein's early exposure to Kant, see Beller 2000 and Howard 1994 and 2006.

reasons are not those of a crude “tabula rasa” empiricist who ignores the knower’s active contribution to cognition. On that point he agrees with the Kantian. What then is the reason? Here is how he expressed his reservations to Cassirer in a letter of 5 June 1920 after reading in manuscript Cassirer’s *Zur Einsteinschen Relativitätstheorie* (Cassirer 1921):

I can understand your idealistic way of thinking about space and time, and I even believe that one can thus achieve a consistent point of view. To me, as a non-philosopher, philosophical contrarities appear more contrarities of emphasis than contrarities of a principled kind. What Mach calls *connections*, are for you ideal names, which experience first makes possible. But you emphasize this side of knowledge, whereas Mach wants to make them appear as insignificant as possible. I acknowledge that one must approach the experiences with some sort of conceptual functions, in order for science to be possible; but I do not believe that we are placed under any constraint in the choice of these functions *by virtue of the nature of our intellect*. Conceptual systems appear empty to me, if the manner in which they are to be referred to experience is not established. This appears most essential to me, even if, to our advantage, we often isolate in thought the purely conceptual relations, in order to permit the *logically* secure connections to emerge more purely.

What is objectionable in the doctrine of the a priori is the idea that any one feature of our representation of nature is fixed. Einstein elaborates on this point in a letter to Eberhard Zschimmer of 27 September 1922:

That which you work out in your essay seems correct to me, at least from the physical side, which is all that I can judge with certainty. In my opinion, though, the important question for the opposition of relativity theory and Kantian philosophy does not emerge sharply enough: are the spatio-temporal, etc. forms, which also ground “a priori” the relativity theory, only convenient tools of description—to be appraised as conventions—or are they givens, necessitated simply by the character of human thought, and inalterable in detail? I, myself, occupy the former standpoint, represented also, e.g., by Helmholtz and Poincaré, whereas it appears to me that Kant’s standpoint was more the latter. (See Zschimmer 1923)

The focus of Einstein’s concern in remarks such as these is more traditional arguments for the a priori based on the nature of the intellect per se, as with Kant’s argument in the “Transcendental Aesthetic” for space and time as a priori forms of inner and outer intuition. But, of course, that is not the kind of argument that Reichenbach proposed (or Friedman proposes) for the contingent a priori.

Might there be facts local to the structure and empirical interpretation of specific scientific theories that earn for selected elements of those theories the honor functioning in an a priori manner? Even if no a priori is globally and forever fixed by the nature of cognition, itself, might there, nevertheless, be contingent a priori components of specific theories, an a priori relative to a specific theoretical or phenomenal domain?

Cassirer (1921) had suggested one such perspective on the a priori in arguing that an a priori spatio-temporal form—one mathematically weaker than the Euclidean spatial form that Kant regarded as a priori—was picked out by various invariant topological, conformal, and other mathematical facts about space-time in general relativity. Cassirer here effected a link between invariance and the a priori that was also at the center of Carnap's analysis of the space problem in his doctoral dissertation (Carnap 1921). Contingency is not highlighted as a property of an a priori distinguished by its invariance properties, the signature of the metric, for example, bidding fair to be a henceforth abiding if newly discovered fact about space-time. A more explicitly contingent a priori was championed by Reichenbach (1920), his a priori principles of coordination being singled out not as the invariant but as that which secures our space-time theory's linkage with the empirical realm. Reichenbach argued that the received notion of the a priori involved both apodicticity and a constitutive function, whereas the lesson of relativity theory was that the two aspects of the a priori should be disentangled. No necessity attaches to what we take as a priori. That changes with the forward march of science. But at any stage in the development of theory some elements must play the fundamental constitutive role of providing empirical content for theoretical primitives via a priori principles of coordination.

There is no evidence that Einstein knew of Carnap's doctoral dissertation. He knew well, however, the projects of Cassirer and Reichenbach. Yet his own thinking about an a priori grounded in something other than claims about the nature of the intellect seems to have been stimulated more by a seemingly very different project, namely, Hermann Weyl's proposal of a unified theory of electricity and gravitation (Weyl 1918a, 1918b, 1918c). In a Weyl space-time, length is defined only locally and transforms according to a linear differential form whose components become the components of the electromagnetic four-potential. Weyl's philosophical motivation for restricting the fundamental geometrical structures of a space-time theory to those definable locally, a kind of structure he termed a "purely infinitesimal geometry," came from the transcendental phenomenology of Edmund Husserl, the idea being that one has epistemic access only to such structure as is definable in the space of intuition of an ideal observer.<sup>6</sup> What focused Einstein's thinking about elements of theory that might be regarded as a priori for reasons other than those having to do with the nature of the human intellect was not so much Weyl's transcendental phenomenological argument for the purely local perspective as it was Einstein's own reaction to the Weyl theory.

A notorious consequence of the restriction to the purely local is that length is path dependent. Even before Weyl's first publication appeared, Einstein had objected to this feature of Weyl's theory, arguing that it was refuted by such facts as the stability of spectral lines—shouldn't atoms with different histories radiate at different frequencies?—and that it made impossible the empirical interpretation of the theory because it implied that there could be no structure in nature corresponding to an ideal measuring rod (or an ideal clock), a freely transportable, practically rigid

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<sup>6</sup> For a fuller discussion of Weyl's program as a species of transcendental idealism, see Ryckman 2005 and the critical discussion of it in Howard 2005b.

instrument for checking metrical relations as predicted by the theory. A measuring rod that gives different results depending on its history is no good measuring rod at all (see Einstein 1918).

There is irony here in the circumstance that Einstein, the epistemological holist, assumes in his critique of Weyl a view antithetical to that holism in arguing that selected theoretical primitives such as the infinitesimal metrical interval, have to be given direct empirical interpretations independently of the theory in which those primitives are embedded. In effect, Einstein is arguing for a privileged role—he would not, himself, use the term “a priori”—for coordinating definitions of basic metrical notions. Einstein recognized the tension with his elsewhere asserted holistic form of conventionalism, whose correctness in principle he never denied, but he persisted in the demand for a direct empirical interpretation of basic metrical notions. Why?

Einstein’s best known discussion of why one must demand direct empirical interpretations of basic metrical notions is contained in his widely-read and influential 1921 lecture, “Geometrie und Erfahrung.” The focal question is whether one must assume a coordination between practically rigid bodies in the world of experience and fundamental geometrical primitives:

Why is the equivalence of the practically-rigid body and the body of geometry—which suggests itself so readily—rejected by Poincaré and other investigators? Simply because under closer inspection the real solid bodies in nature are not rigid, because their geometrical behavior, that is, their possibilities of relative disposition, depend upon temperature, external forces, etc. Thus, the original, immediate relation between geometry and physical reality appears destroyed, and we feel impelled toward the following more general view, which characterizes Poincaré’s standpoint. Geometry (G) predicates nothing about the behavior of real things, but only geometry together with the totality (P) of physical laws can do so. Using symbols, we may say that only the sum of (G) + (P) is subject to experimental verification. Thus (G) may be chosen arbitrarily, and also parts of (P); all these laws are conventions. All that is necessary to avoid contradictions is to choose the remainder of (P) so that (G) and the whole of (P) are together in accord with experience. Envisaged in this way, axiomatic geometry and the part of natural law which has been given a conventional status appear as epistemologically equivalent.

*Sub specie aeterni* Poincaré, in my opinion, is right. The idea of the measuring rod and the idea of the clock coordinated with it in the theory of relativity do not find their exact

correspondence in the real world. It is also clear that the solid body and the clock do not in the conceptual edifice of physics play the part of irreducible elements, but that of composite structures, which must not play any independent part in theoretical physics. But it is my conviction that in the present stage of development of theoretical physics these concepts must still be employed as independent concepts; for we are still far from possessing such certain knowledge of the theoretical principles of atomic structure as to be able to construct solid bodies and clocks theoretically from elementary concepts. (Einstein 1921, 7-8)

Holism is, in principle, correct. It is only the immaturity of current physical theory that forces us demand direct empirical interpretations of geometrical primitives. But what, if anything, are the implications for whether we are to regard as a priori the coordinations that provide those interpretations? That is not immediately obvious.

Einstein returned to this cluster of questions when, in 1924, he reviewed a couple of books on relativity theory by minor the neo-Kantians Josef Winternitz (192x) and Alfred Elsbach (1924). In the first of these reviews, Einstein again asserts his basic view according to which what others regard as the a priori is more helpfully seen as the conventional:

Thus Winternitz asserts with Kant that science is a mental construction on the basis of a priori principles. That the edifice of our science rests and must rest on principles that are not themselves derived from experience, will be acknowledged without doubt. For me, doubt only arises if one asks about the dignity of those principles, that is, about their irreplaceability. Are those principles at least in part so constituted that their modification would be incompatible with science, or are they collectively mere conventions, like the ordering principle of the words in a lexicon? W. inclines toward the former view, I to the latter. (Einstein 1924a, 21-22)

But in the second review, Einstein provides his most careful and lucid statement both about what he finds uncongenial in the notion of the a priori and about why one should still privilege in some fashion (as conventions) coordinating definitions of geometrical primitives. First about the a priori in general:

This does not, at first, preclude one's holding at least to the Kantian *problematic*, as, e.g., Cassirer has done. I am even of the opinion that this standpoint can be rigorously refuted by no development of natural science. For one will always be able to say that critical

philosophers have until now erred in the establishment of the a priori elements, and one will always be able to establish a system of a priori elements that does not contradict a given physical system. Let me briefly indicate why I do not find this standpoint natural. A physical theory consists of the parts (elements) A, B, C, D, that together constitute a logical whole that correctly connects the pertinent experiments (sense experiences). Then it tends to be the case that the aggregate of fewer than all four elements, e.g., A, B, D, *without* C, no longer says anything about these experiences, and just as well A, B, C without D. One is then free to regard the aggregate of three of these elements, e.g., A, B, C as a priori, and only D as empirically conditioned. But what remains unsatisfactory in this is always the *arbitrariness in the choice* of those elements that one designates as a priori, entirely apart from the fact that the theory could one day be replaced by another that replaces certain of these elements (or all four) by others. (Einstein 1924b, 1688-89).

Connecting theory to the empirical realm requires our fixing some elements of theory. What is not “natural” is the privileging of any one such fixing, the choice of what to hold fixed being, in principle, entirely arbitrary.

A few pages later, Einstein relates this general perspective on the a priori to the question of choosing between the holist’s view from eternity and the here-and-now demand for direct empirical interpretations of geometrical primitives. The locus is a critique of Elsbach’s defense of Paul Natorp’s claims that apparent deviations from Euclidean spatial structure can be accommodated by changes in physical law and that the spatial metric is not empirically determinable because is ideal, not real (Natorp 1910):

The position that one takes on these claims depends on whether one grants reality to the practically-rigid body. If yes, then the concept of the interval corresponds to something experiential. Geometry then contains assertions about possible experiments; it is a physical science that is directly underpinned by experimental testing (standpoint A). If the practically-rigid measuring body is accorded no reality, then geometry alone contains no assertions about experiences (experiments), but instead only geometry with physical sciences taken together (standpoint B). Until now physics has always availed itself of the simpler standpoint A and, for the most part, is indebted to it for its fruitfulness; physics employs it in all of its measurements. Viewed from this standpoint, all of Natorp’s assertions are incorrect. . . . But if one adopts standpoint B, which seems overly cautious at the present stage of the development of physics, then geometry alone is not experimentally testable. There are then no geometrical measurements whatsoever. But one must not, for that reason, speak of the “ideality of space.” “Ideality” pertains to all concepts, those referring to space

and time no less and no more than all others. Only a complete scientific conceptual system comes to be univocally coordinated with sensory experience. On my view, Kant has influenced the development of our thinking in an unfavorable way, in that he has ascribed a special status to spatio-temporal concepts and their relations in contrast to other concepts.

Viewed from standpoint B, the choice of geometrical concepts and relations is, indeed, determined only on the grounds of simplicity and instrumental utility. . . . Concerning the metrical determination of space, nothing can then be made out empirically, but not “because is not real,” but because, on this choice of a standpoint, geometry is not a *complete* physical conceptual system, but only a part of one such. (Einstein 1924, 1690-1691)

Though standpoint A is the one we have standardly employed, for good reason, and though adopting standpoint B now would be “overly cautious,” standpoint B—the holist standpoint—is, nevertheless, the right one from a first-principles point of view and is the right perspective from which to assess claims for an a priori component in scientific theory.

Einstein’s basic attitude toward the a priori remained unchanged to the end of his life. The knower contributes something to scientific cognition, but nothing about the nature of the intellect fixes that contribution. More specifically, our putting empirical content into theories requires our fixing some coordinations between theory and world, but how we fix those coordinations is, in principle, arbitrary. He said this in a 1928 review of Émile Meyerson’s *La Déduction Relativiste*:

There is no feature, no characteristic, of the system we are seeking, about which we can know a priori that it must necessarily belong to this system due to the nature of our thought. This also holds for the forms of logic and causality. We can only ask how the system of science (in its states of development thus far) is composed, but not how it *must* be composed. The logical foundations of the system as well as its structure are thus (from a logical point of view) conventional; their only justification lies in the performance of the system vis-à-vis the facts, in its unified character, and in the small number of its premises. (Einstein 1928, 253)

He said it again in his 1936 essay, “Physics and Reality”:

Concerning the manner in which we are to build and connect concepts, and the manner in which we are to coordinate them with sense experiences, not the least can be said a priori, in my view. Only success in regard to establishing an order among sense experiences is decisive. The rules for connecting concepts must be stipulated only in general, for otherwise knowledge of the kind for which we strive would be impossible. One has compared these

rules with the rules of a game, rules that are in themselves arbitrary but whose determinateness first makes the game possible. However, this stipulation can never be a final one, rather they can only claim validity for an intended domain of application (i.e., there are no ultimate categories in Kant's sense). (Einstein 1936, 315)

And he said it once more in the late 1940s in his replies to the essays collected in the Library of Living Philosophers volume dedicated to his work, *Albert Einstein: Philosopher-Scientist*:

The theoretical attitude here advocated is distinct from that of Kant only by the fact that we do not conceive of the "categories" as unalterable (conditioned by the nature of the understanding) but as (in the logical sense) free conventions. They appear to be *a priori* only insofar as thinking without the positing of categories and of concepts in general would be as impossible as breathing in a vacuum. (Einstein 1949, 674)

*Regulative Principle, Empirical Law, or Contingent A Priori?*

Einstein's general attitude toward the *a priori* is one thing. His specific attitude toward the methodological role of the principle of equivalence in general relativity is another. The heuristic work done by the principle of equivalence in guiding Einstein to general relativity was something upon which he remarked, and he was usually careful to explain his precise understanding of the content of the principle as asserting only the strictly local equivalence of inertia and a homogenous gravitational field (see Norton 1989). How did he view the principle's systematic, methodological role in general relativity?

Perhaps Einstein's most original contribution to our understanding of scientific method is his distinction between "principle theories" and "constructive theories." Though this distinction informed Einstein's way of doing physics from as early as 1908,<sup>7</sup> it found its first longish public

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<sup>7</sup> See Howard 2005a for an argument that the distinction is already implicit in Einstein's 1905 photon hypothesis paper as well as a more general discussion of the way the distinction works and possible historical sources and antecedents.

discussion in a 1919 article in the *Times* of London. The “principles” that make up a principle theory like special relativity or macroscopic thermodynamics are empirically well grounded generalizations that serve a heuristic role by constraining the search for the deeper constructive theories that provide ultimate explanations of phenomena in terms of models constructed from an ontology of systems, states, and interactions. Special relativity comprises two such principles: the principle of special relativity, itself, and the light principle. The first and second laws of thermodynamics are the principles in macroscopic thermodynamics. In general relativity we have again two principles: the principle of general relativity and the equivalence principle. Noteworthy in Einstein’s discussion of the latter in 1919 is his introducing it, after the fashion of all such “principles,” by stressing its empirical origins and credentials, specifically the “long known experience” of the equivalence of gravitational and inertial mass (Einstein 1919, 13). Far from the principle’s being set in a category different in kind from ordinary empirical laws, the principle of equivalence is accorded its special role precisely because of its being a generalization from experience. Nor does it work by way of effecting coordinations between theory and world (Friedman 2001, 90). It works, instead, by constraining our musings about constructive foundations to respect the wealth of experience encapsulated in it. Privileged it is, but as an empirical generalization, not an a priori principle.

Viewed thus, the principle of equivalence has only as much security as any battle-tested empirical generalization, which means that it could well turn out to be wrong when we extend our experience into new domains. That’s what happened to the second law of thermodynamics when we pressed into regimes where microscopic fluctuations became manifest. And the same has been the fate of the equivalence principle—if it is taken to imply that, in the absence of other forces, massive bodies must follow geodesic trajectories—for particles with spin do not necessarily follow geodesic

trajectories. How are we helped by one's characterizing as constitutively, if contingently and relatively a priori a principle that is just plain false?

If we restrict our ken to systems without spin, must we still view the principle of equivalence as an empirical law? Einstein did so regard it, but then he was not at home with modern axiomatizations of general relativity à la Ehlers, Pirani, and Schild (1972), which take as primitive the notions of freely-falling test bodies and photon trajectories. In the EPS framework it is natural to regard the principle of equivalence as making possible the empirical interpretation of the primitive notion of free fall by picking out the inertial motions. But the naturalness of the perspective does not compel us to respond to possible deviations from free fall by positing new forces, any more than Duhem felt himself so compelled, and there is, by no means, a modern consensus on regarding the principle of equivalence as anything other than a testable empirical proposition (see Ciufolini and Wheeler 1995 for a view contrary to EPS and to Friedman).

Einstein was more familiar with axiomatizations of relativity theory of the kind to be found in Reichenbach's 1924 *Axiomatik der relativistischen Raum-Zeit-Lehre*, wherein the rods and clocks that provide direct empirical interpretations of basic metrical notions are taken as primitive (Reichenbach's "matter axioms") and the extension of the framework to general relativity explicitly renders the principle of equivalence empirically testable. Had he been familiar with the EPS framework, would he have found it more helpful? My guess is that, for the kinds of reasons asserted in "Geometrie und Erfahrung" (Einstein 1921) and the review of Elsbach (Einstein 1924b), he would still have preferred a rods-and-clocks axiomatization. But one would expect him also to point to the very possibility of two such very different ways of axiomatizing general relativity as illustrating the

more general point that he made in the review of Elsbach, the point about the inherent arbitrariness of any one way of parsing a theory into empirical laws and coordinating principles.

*Conclusion: So What?*

Who cares what Einstein thought? Is the foregoing anything more than a long-winded argument from authority? As such fallacies go, and given the subject matter, arguing from the authority of Einstein is more respectable than arguing from the authority of many others would be. Einstein made his mistakes, but as recent experience with problems such as that of the “hole” and “point-coincidence” arguments in general relativity has shown, much can still be learned from our seeking carefully and in detail to excavate Einstein’s views on fundamental conceptual, interpretive, and methodological problems in the physics with which he grappled. Still, advancing an argument from authority is not the point.

The important point is the one mentioned at the outset. Given that the primary evidence for claims about the contingent a priori will have to be historical evidence, why privilege contemporary rational reconstructions over the self-understandings of the historical actors themselves? To be sure, the historical actors cannot be counted upon to understand themselves, their actions, and their creations better than we do today, and historical distance often brings deeper insight thanks partly to the very detachment that such distance brings. But historical distance can distort as well as clarify. Ears grow deaf to the vocabularies of the past. Period problematic settings are obscured by our tendencies to whigishness and presentism.

How then weigh Einstein’s views on the a priori in physical theory against the reconstruction proffered by Friedman? The main reason for taking Einstein’s view of the matter seriously is that

he was at least as good a physicist and philosopher of science as are those involved in the current debate over the contingent a priori, he thought long and hard about the very issues under dispute, and he defended and refined his views through decades of close critical debate with the best minds of his own day. Does it not make sense to invite such a thinker to be part of our contemporary conversation? Does it not make sense to weigh what is, in effect, his own reconstruction of his scientific achievements against later reconstructions? Does he not deserve a place at the table?

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