“No Crude Surfeit”: A Critical Appreciation of *The Reign of Relativity*

Don Howard
Department of Philosophy and
Program in History and Philosophy of Science
University of Notre Dame

> *Eld. Bro.*
>
> Such are those thick & gloomie shadows dampe
> Oft seene in charnel vaults, & sepulchers,
> Lingering, & sitting by a new made grave,
> As loath to leave the bodie that it lov'd,
> & link’t it selfe by carnall sensualtie
> To a degenerate, & degraded state.
>
> *Sec. Bro.*
>
> How charming is divine philosophy!
> Not harsh, & crabbed, as dull fooles suppose,
> But musicall as is Apollo's lute,
> And a perpetuall feast of nectar'd sweets
> Where no crude surfeit reigns.
>
> John Milton. *Comus* (1634).

Introduction

Tom Rykcman’s *The Reign of Relativity* (2005) is an important book. Together with several of Mike Friedman’s recent works, including *Reconsidering Logical Positivism* (1999), *A Parting of the Ways* (2000), and *Dynamics of Reason* (2003), it will be remembered for having taught us that the history of philosophical engagements with the physics of the early twentieth century is a history not just of Viennese logical empiricism and its Berlin allies, that other thinkers explored the philosophical lessons of the theory of relativity with comparable technical and philosophical sophistication, and that in these other philosophical engagements with relativity–as even in the work of Rudolf Carnap and Hans Reichenbach–Kantian themes are more vigorous and prominent than triumphalist histories of logical empiricism and scientific philosophy would have led one to believe. The history of philosophical responses to relativity includes not only Moritz Schlick, Philipp Frank, Carnap, and Reichenbach, but also Ernst Cassirer, Hermann Weyl, and Arthur Eddington, along with many other more minor figures.[1]

Ryckman's more specific contribution to this long-overdue revisionist history is to argue that Cassirer, Weyl, and Eddington can and should be read as having promoted a self-consciously Kantian alternative to logical empiricism and scientific philosophy, their views all being variations on a perspective that Ryckman terms "transcendental idealism." The question at the focal point of that perspective, the question that makes this a distinctively Kantian alternative, is the question of the "transcendental constitution of objectivity," which means, in this context, the question of the a priori principles whereby a physical theory like general relativity constitutes its fundamental ontology. In different ways, Cassirer, Weyl, and Eddington all argue that the requirement of general covariance, or–better said–the requirement of the kind of invariance that one associates with structures such as the infinitesimal metric interval or space-time events individuated as points of intersection of world-lines, is foremost among those a priori principles. That such principles are, in a sense, contingent, that different theories can and do constitute their fundamental ontologies in accordance with different principles, does not detract from the status of such principles as a priori, for they still function, on this view, as necessary preliminaries to the ordinary explanatory and predictive work of the theories they ground.

In a moment I will offer a dissent from some claims in Ryckman’s story. I have a question about the allegedly transcendental argument for Weyl’s “purely local” perspective in space-time physics. That question will lead us to a larger question about the meaning of the notions of the “transcendental” and the “a priori” in all three of the projects that are the focus of Ryckman’s attention. Finally, I will argue that a complete history of philosophical reactions to relativity must include Albert Einstein, himself, who was critical of both logical empiricist and neo-Kantian interpretations of the philosophical significance of relativity, and this, surprisingly, for much the same reason.

But before I dissent, I want to make clear my wholehearted agreement with the basic point that standard histories of twentieth century philosophy of science are rightly to be faulted for their ignoring the fact that Cassirer, Weyl, and Eddington represent an important alternative to logical empiricist orthodoxy about the philosophical significance of the relativity theory. For years I have preached to my students–yes, “preached” is the right word–that Weyl’s *Philosophie der Mathematik und Naturwissenschaft* (1927), along with Cassirer’s *Substanzbegriff und Funktionsbegriff* (1910), *Die Einsteinschen Relativitätstheorie* (1921), and *Determinismus und Indeterminismus in der modernen Physik* (1936) constitute the great unread canon of early-twentieth century philosophy of science. More than anyone else, Ryckman is restoring these works to their rightful place.

In celebrating, with Ryckman, the importance of this alternative philosophical legacy, I want also to record the debt that both he and I owe for first having had our attention drawn to these texts. As Ryckman mentions in his Preface (2005, viii), it was Howard Stein who pointed him to Weyl. In my case, it was Stein, too, but at one remove, through his student and my teacher, Judson Webb.

Like all of Stein's students at Case-Western, Webb was asked to read Weyl's *Philosophie der Mathematik und Naturwissenschaft*, from which, early in my first semester of graduate school at Boston University, Webb read to me these words whose importance had been stressed by Stein:

> A science can only determine its domain of investigation up to an isomorphic mapping. In particular it remains quite indifferent as to the "essence" of its objects. That which distinguishes the real points in space from number triads or other interpretations of geometry one can only *know (kennen)* by immediate intuitive perception. But intuition is not blissful repose never to be broken, it is driven on toward the dialectic and adventure of cognition (Erkenntnis). It would be folly to expect cognition to reveal to intuition some secret essence of things hidden behind what is manifestly given by intuition. The idea of isomorphism demarcates the self-evident insurmountable boundary of cognition. (Weyl 1927, 25-26)

Webb said that, if, someday, he could understand the full significance of this one remark of Weyl's, then his would be a worthwhile philosophical life. A not insignificant portion of my philosophical life has been devoted to precisely this task.[2]

Weyl's Transcendental Phenomenology and the "Purely Local" Perspective

I turn, now, to my friendly dissent, and I begin with an issue that one might at first think peripheral to the larger historical story that Ryckman seeks to tell. That issue is Weyl's transcendental phenomenological argument for the purely local perspective in space-time physics. First the physics, then the philosophy.

Weyl's early attempt at a unified theory of gravitation and electromagnetism assumes a space-time structure that differs from the space-time of Einstein's general relativity in taking as basic only such structure as is well defined locally (Weyl 1918a, 1918b, 1918c). Most famously, length becomes path dependent, transforming under parallel displacement according to

$$dl = l d\varphi,$$

where

$$d\varphi = \varphi_i dx^i$$

is a linear differential form whose components, φ_i, become the components of the electromagnetic four-potential. Weyl's thus allowing objective comparisons of length only at a point, and not over some finite distance, is precisely what makes possible the theory's deriving electromagnetism as well as gravitation from the underlying space-time geometry.

Explanatory success might satisfy many as a rationale for the path-dependence of length in Weyl's unified field theory. But Weyl offers a general philosophical rationale for purely local geometry. Associated with every space-time point is a tangent space, essentially an infinitesimal, flat, i.e., Minkowski space-time. In a purely local Weyl theory, all fundamental field quantities are defined only in these individual tangent spaces. Weyl's idea–reflecting his debt to Edmund Husserl transcendental phenomenology–was that the tangent space at a point, the space in which all fundamental field quantities are defined, is epistemically privileged as the space of intuition of an ideal observer. Only local structure is permitted as fundamental, therefore, because it is only such local structure to which the ideal observer has the requisite epistemic access. This has the feel of a 'conditions for the possibility of X' argument. The idea seems to be that a necessary condition for the theory's having a solid empirical grounding is its taking as basic to its ontology only such structure as is accessible in the space of intuition of the ideal observer.

I've never understood this argument. Structure definable in the tangent space at a space-time point is, at first glance, a not unreasonable physico-mathematical reconstruction of the space of intuition of an ideal observer. The question is why the space of intuition of an ideal observer should play any such privileging role in determining a geometrical framework for fundamental physics. Yes, all observations are, in a sense local, but why should this epistemic fact have implications for a theory's fundamental ontology?

Consider this question from a couple of different perspectives. First, theories must be tested. Granted, then, that a physical theory must evince the kind of closure involved in its telling a coherent story about the physical circumstances in which it is tested. Why, however, must the theory's fundamental ontology therefore consist only of such structures as are directly accessible in the ideal observer's space of intuition? The observer's physical situation must be adequately described by the theory. But why must the observer's physical situation, thus described, be a starting point in theory construction, as opposed to its being the end point of a derivation from the theory, as when, in the case of special relativity, we consider the limit case of physical processes with velocities small in comparison with the velocity of light? Posit what fundamental structure one will, as long as, in the end, a coherent story about observation emerges.

Second, is the tangent space really the proper or most helpful reconstruction of the ideal observer's space of intuition? I'm not so sure. Real observers are finite beings, and they observe directly only finite objects, but the structures definable in the tangent space are, strictly speaking, structures defined only at the tangent space's associated point, hence not finite, but infinitesimal structures, such as the infinitesimal line segment. Such structures are, at best, only approximations to the real observer's finite measuring rods and clocks. Hence, even if special epistemic warrant attaches to that which is accessible in the observer's space of intuition, it is not clear that what is thus accessible is the infinitesimal geometrical structure fundamental to a Weyl space-time.[3]

Note that this is not a criticism of purely infinitesimal geometry or the purely local perspective. It is, rather, a criticism of Weyl's epistemological argument for that perspective. It is important because here–in Weyl's argument for the purely local perspective–is to be found a principal locus of what passes for the transcendental moment in Weyl's view of the structure and interpretation of space-time theories. When one finds that argument wanting, larger questions are suggested about what, in general, could pass muster as a transcendental argument for an a priori element in scientific cognition.

One such question is raised by Weyl's abandonment of the whole project of a space-time physics based on purely infinitesimal geometry when, after the mid-1920s, he contemplated the rise of the new quantum mechanics. The quantum theory's implication of a fixed, fundamental length scale–the Planck length–was one reason cited by Weyl for repudiating a theory featuring the path dependence of length. But quantum theory differs from a Weyl-type, space-time theory in other respects as well. For one thing, since the quantum theory fails to individuate systems on the basis of non-null spatial (or spatio-temporal) separation alone, this thanks to entanglement, its notion of an "object" differs fundamentally from that of any space-time theory–both Weyl's and Einstein's included–that, in effect, treats each point of the space-time manifold as a separate and separable physical system.[4] The very notions of "object" and "objectivity" being, thus, transformed, the manner of its constitution, transcendental or otherwise, will surely not follow the pattern of Weyl's unified field theory. Moreover, the quantum theory leads one, as clearly it led Weyl,[5] to reconceive the notion of univocally characterized structure undergirding a theory's representation of nature as being fixed univocally not, in the first instance, via considerations of invariance under a set of space-time transformations, but instead via the unitary equivalence of representations of an algebra.[6] Whether there is any place in this algebraic framework for talk of the transcendental constitution of objectivity is not clear, certainly not if such constitution is to be grounded in something like the space of intuition of an ideal observer, for one does not know where to look in, say, algebraic quantum field theory, for any structure corresponding to an ideal observer's perspective. But I digress.

Whither the Transcendental?

That there is something elusive about the relevant notion of the transcendental in Weyl's work is hinted at by the fact that, in the defense of the purely local perspective, Weyl disagrees, I think, with his kindred transcendental idealist, Eddington. Weyl privileges local structure on the grounds that only such is accessible in the space of intuition of the ideal observer. Eddington, by contrast, seeks an "absolute physics," one indifferent to the peculiarities of the individual observer. I like to think of Weyl as privileging a "view from anywhere" and Eddington as privileging a "view from nowhere." These two views might lead (by accident, I'm tempted to say) to somewhat similar conclusions about the properly invariant structures that must be basic to a fundamental physics, but they are very different philosophical stances, very different perspectives on the transcendental constitution of objectivity, and the differences are enough to lead one to wonder whether even so much as a family resemblance unites these two versions of transcendental idealism.

The tension between these two perspectives on the transcendental moment in scientific cognition might be seen as reflecting a still deeper tension in the broader neo-Kantian movement of the late-nineteenth and early-twentieth centuries and a tension in Ryckman's own reading of that tradition. As Ryckman quite lucidly explains, a famous problem bequeathed by Kant to his philosophical posterity concerns the relationship between the faculties of sensibility and understanding, or, from another perspective, the relationship between cognition via concepts and cognition via intuitions (Ryckman 2005, 24-26). Concepts are inherently general; intuitions are inherently particular, which is to say that, for Kant, it is intuitions that give us cognitive contact with the world in its particularity via univocal and, thus, particular representations. How concepts are supposed to apply to that which is represented via intuition is the problem that Kant tried to solve with the famously obscure doctrine of the transcendental schematism of the categories. The schemata, or rules for application of the pure concepts of the understanding, are supposed to be a third kind of representation, with both an intellectual and sensible aspect, that mediates the relationship between the pure concepts of the understanding and the pure forms of sensibility, the pure intuitions of space and time. Thus schematized by the pure forms of inner and outer intuition, concepts can find application to that which is given to cognition via empirical intuition.

Old doubts about whether Kant's doctrine of the transcendental schematism elucidated or further obscured the relationship between concepts and intuitions took on greater urgency in the late-nineteenth century as challenges to the Kantian doctrine of intuition accumulated, the most important such being the challenge to Kant's claim that Euclidean geometry is the necessary a priori structure of cognition in outer intuition. Most pertinent to the present discussion is the reaction to both worries on the part of the Marburg neo-Kantians, Hermann Cohen, Paul Natorp, and their student, Cassirer. Ryckman argues that Cassirer follows Cohen in trying to solve (or avoid) the problem of the schematism by asserting a common origin for understanding and sensibilty, asserting that “in intuition itself the function of the concept is already effectively demonstrated.” (Cassirer 1907, as quoted by Ryckman 2005, 27). On my reading, the Marburg reaction is more radical, involving the attempt simply to eliminate intuition from its analysis of scientific cognition.

Intuition, according to Kant, is what gives us knowledge of the world in its particularity. How can one dispense with that? How can concepts, alone, give us univocal knowledge of the world? The Marburg answer, which finds its mature and technically most sophisticated expression in Cassirer’s *Substanzbegriff und Funktionsbegriff* (1910), is that, within a given scientific domain, one can, ideally, accumulate sufficiently many purely conceptual, purely formal determinations so as to constrain the resulting scientific picture up to the point of uniqueness.[7] Of course this project fails. As Cassirer, himself, recognizes, one gets at best a set of isomorphic conceptual representations, not a unique cognition, and even the goal of isomorphism seems unattainable in the wake of Gödel’s incompleteness theorem, a corollary to which is the inevitable non-categoricity of any first-order formal theory as powerful as or more powerful than Peano arithmetic. But that the Marburg project was doomed to failure was not known in 1910 or 1925 (at least not with full generality).

If one aims to replace intuition in one’s story about scientific cognition by a purely conceptual account of empirical knowledge, then two consequences follow for our locating a transcendental moment in cognition. Firstly, and obviously, one would then not seek the transcendental in an analysis of the space of intuition of an ideal observer. One would look, instead, for a transcendental grounding of the purely conceptual or purely formal principles that constrain the object domain. Secondly, and more fundamentally, one comes to regard scientific cognition as having little or nothing to do with the manner of any one individual's cognitive contact with the world. On the Marburg picture, scientific knowledge is knowledge that lives in a kind of trans-personal domain, not in the head of even an ideal observer. The Marburg picture is, thus, a view from nowhere, not a view from somewhere or even anywhere.

Kant's story about empirical knowledge, or any story that makes sensibility an independent source of cognition, is, by definition, a view from somewhere or view from anywhere story. In any such story, a transcendental grounding of at least some a priori aspects of cognition, those living in the arena of sensibility, hence the pure forms of space and time, will have to involve an analysis of the ideal subject's or ideal observer's specific cognitive situation and the structures of cognition implicit in that situation. The geometry that is the pure form of outer intuition is a form of *appearing to* a subject. On the Marburg story, however, the structure of the world's *appearing to* a subject, an observer, or an object's *givenness to* a subject is not the issue, or at least not the basic issue. On the Marburg story, the issue is the transcendental warrant for the purely conceptual constraints on a theory's object domain. In this setting, invariance bids fair to be the right road, if there is any, to a transcendental grounding, as it is, explicitly, for Cassirer and Eddington.[8] But whatever the manner of the transcendental grounding, it is the general principles of science that are what must, thereby, be grounded.

As noted, the Weyl of 1918, the Weyl of *Raum•Zeit•Materie* tells a view from anywhere story. But I think that the Weyl of 1927 and 1928, the Weyl of *Philosophie der Mathematik und*

Naturwissenschaft and *Gruppentheorie und Quantenmechanik* has adopted, instead, the view from nowhere. The Weyl of 1927 wrote:

> Intuitive space and intuitive time are thus hardly the adequate medium in which physics is to construct the external world. No less than the sense qualities must the intuitions of space and time be relinquished as its building material; they must be replaced by a four-dimensional continuum in the abstract arithmetical sense. . . . What remains is ultimately a *symbolic construction* of exactly the same kind as that which Hilbert carries through in mathematics. (Weyl 1927, 113)

(Recall, too, the above-quoted remark that "intuition is not blissful repose never to be broken, it is driven on toward the dialectic and adventure of cognition.") The Weyl of 1927 also tends to regard the a priori in our space-time theories as the invariant:

> In the case of physical space it is possible to counterdistinguish aprioristic and aposterioristic features in a certain objective sense without, like Kant, referring to their cognitive source or their cognitive character. . . . Among the aprioristic features of the world, beside and above the one nature of the metrical field [invariance and signature of the infinitesimal metric interval–"the Euclidean-Pythagorean *nature* of the metric"], there is the topological connectivity, which is fixed once and for all, especially the dimension number 4. (Weyl 1927, 134-135)

Whither the transcendental? After the Marburg turn, this becomes an acute question. With intuition put in its proper, now subordinate place, what remains in need of a transcendental grounding, if any such is to be had, are the fundamental general principles via which a theory constitutes its object domain. But it is not clear that such principles are even to be found in theories like the quantum theory, which (a) do not individuate systems against a space-time background, what Schopenhauer and Weyl, following Schopenhauer, call the *principium individuationis* (Weyl 1927, 131; see also Howard 1997), and (b) beg for an algebraic formulation in which there is no obvious role for the notion of an "object"–a spatially (or spatio-temporally) situated system to which properties attach through associated states. And even in a space-time theory, in which there are objects individuated on the basis of their spatial situation, it is no longer clear that some subset of the propositions constituting the theory can be singled out, on a principled, objective basis as alone the object-constituting principles that would be candidates for a transcendental grounding. The problem here is that, qua principles, these principles seem to be on an epistemic footing comparable to that of all other principles, not obviously purely a priori, not obviously purely a posteriori. Weyl, himself, says of the just-cited, putatively a priori elements of our theory of space (or space-time), the nature of the metrical field and the topological connection:

> To be exact, this juxtaposition, or separation [of a priori and a posteriori elements], must be understood as meaning–as always in cases of this kind–that the aprioristic factor can be isolated from the whole without thereby exhausting the latter; there is no residue of purely *aposteriori* character, however, that would be left after the first part has been 'subtracted' from the whole. (Weyl 1927, 135)

Why is there no "purely aposteriori" residue after a priori principles have been subtracted? It's because, according to Weyl, theories are tested only as wholes, not one proposition at a time:

> It may be pointed out that the individual laws of physics no more than those of geometry admit of an experiential check if each is considered by itself, but that a constructive theory can only be put to the test as a whole. (Weyl 1927, 134)

Weyl learned to think of theories holistically not from Pierre Duhem but from David Hilbert, as did a number of his contemporaries.[9] But whatever the source, the conclusion is the same: There is no principled, systematic basis upon which to parse the propositions constituting a theory into a priori and a posteriori subsets. But now with talk of Duhem and holism, we come to Einstein.

A Pox on Both of Your Houses: Einstein's Critique of Logical Empiricism and neo-Kantianism

Ryckman rightly corrects the standard narrative of philosophical responses to relativity by reminding us that logical empiricism was not the only movement defining itself by its way of making philosophical sense of relativity. There was, in addition, a technically adept, philosophically profound response in the form of what Ryckman dubs "transcendental idealism," comprehending at least Cassirer, Weyl, and Eddington. So far, so good. But a truly complete story of philosophical responses to relativity would have to give equal billing to the philosophical response of the theory's own author, Einstein. For Einstein develops and defends a sophisticated point of view, a variant of Duhemian holism, that is critical of both logical empiricism and neo-Kantianism, and, surprisingly, critical of them for more or less the same reason. Moreover, Einstein developed his views through intense and sustained direct debate with the leading champions of both the logical empiricist and neo-Kantian responses to relativity. On the one side, Einstein was friend, colleague, and interlocutor to both Schlick and Reichenbach. One the other side, while not quite a friend to Cassirer, Weyl, and Eddington, he read their work carefully, especially that of Cassirer and Weyl, and he offered thoughtful, critical commentary on their projects. And both contingents listened to Einstein, took his philosophical interventions seriously, and refined their own views in the light of Einstein's criticisms.

Elsewhere I have recounted, in detail, the history of Einstein's complex relationship to emergent logical empiricism in the 1910s and 1920s (Howard 1994). The story, in brief, is this. In the early 1910s, Einstein and Schlick develop independently of one another philosophies of science that emphasize holism, underdeterminationism, conventionalism, and realism, Einstein's source being mainly Duhem (Howard 1990), Schlick's source being mainly Hilbert. After they first make contact with one another in 1915, they collaborate for several years in the deploying those philosophies of science for the purpose of extracting the chief philosophical implications from the general theory of relativity, Schick's influential monograph, *Raum und Zeit in der gegenwärtigen Physik* (1917) representing, in a sense, the main fruit of that collaboration.

As a negative, neo-Kantian reaction to general relativity emerges in the late 1910s and early 1920s, Einstein and Schlick turn their attention to formulating what Schlick terms a new kind of empiricism (Schlick 1921) for the purpose of defending the empirical integrity of general relativity against neo-Kantian challenges to theory's implications concerning physical geometry. But then a tension begins to emerge. Schlick is trying to reconcile his view of truth as just a univocal coordination between theory and world with Reichenbach's early defense of the contingent a priori, wherein constitutive principles of coordination are identified as the locus of the a priori in scientific cognition (Reichenbach 1920). Schlick persuades Reichenbach that his principles of coordination are more helpfully viewed not as a priori constitutive principles, but as merely conventional coordinating definitions. Together, they elaborate what becomes the classic, mature, logical empiricist view on the empirical interpretation of theories, according to which the propositions making up a theory can be segregated into a set of purely analytic and conventional coordinating definitions and a set of purely synthetic, empirical propositions. Confine the moment of convention to the choice of coordinating definitions. Fix those coordinating definitions by convention, and one thereby fixes a univocal empirical content for each individual empirical proposition, such that the truth or falsity of that proposition is, in principle, univocally determined by the corresponding experience. Two theories that differ only in a choice of coordinating definitions are to be seen as mere linguistic variants of one another, since they share the same empirical content and, on a verificationist theory of meaning, empirical content is the only content. This is picture of the empirical interpretation of theories famously articulated by Reichenbach in both of his major works on relativity in the mid-1920s (Reichenbach 1924 and 1928).

The charm of the Schlick-Reichenbach point of view is that, if it works, it provides a very strong reply to neo-Kantian critics of general relativity, for it made possible the unblushing claim that general relativity’s assertion of the non-Euclidean character of physical geometry is a straightforwardly empirical claim. The big problem with this view is that it’s not clear that one can make a principled distinction between analytic, coordinating definitions and synthetic, empirical propositions. In a striking anticipation of Quine’s more famous, later critique of the analytic-synthetic distinction (Quine 1951), Einstein pointed out early on this very vulnerability of the Schlick-Reichenbach point of view. In much the same way as Duhem had still earlier argued, against Henri Poincaré, that no scientific proposition is immune to revision thanks to its allegedly definitional character (Duhem 1906), so Einstein argued that while one could, of course, parse a theory into definitions and empirical claims, and perhaps must do so in order clearly to describe specific empirical tests of a theory, nothing other than convenience privileged any one such parsing. Slice and dice as you will, says Einstein, in effect, for how one slices is largely a matter of taste.

But if the central premise of the Schlick-Reichenbach point of view, the analytic-synthetic or definition-empirical proposition distinction, is thus undermined, do we any longer have an effective empiricist reply to the neo-Kantian? Einstein’s answer is, yes. And, ironically, the very same holism that undermines the Schlick-Reichenbach point of view is the basis for Einstein's preferred empiricist reply to Kant. Here is the clearest statement of this argument, from a too-little-known 1924 review by Einstein of one of the many books of that day promoting Kantian orthodoxy. After asserting that general relativity contradicts the Kantian doctrine of the a priori character of space and time as forms of intuition, Einstein writes:

> 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 which 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 1924, 1688-1689)

So much for Cassirer. Are there implications equally destructive of the Weyl version of transcendental idealism? Here the story is a bit more complicated (see Howard 2005). Einstein advanced a famous criticism of Weyl's 1918 unified field theory at the moment of its first appearance (Einstein 1918). Einstein argued that the path-dependence of length in the Weyl theory was objectionable not only because it seemed to be directly disconfirmed by phenomena such as the stability of spectral lines, but also on methodological grounds, because it made it impossible to regard idealized practically rigid rods and practically regular clocks as providing a direct empirical interpretation for the physico-geometrical primitive notion of the line segment. In other words, Einstein argued that a theory such as Weyl’s could have no coherent empirical interpretation.

Right away, there’s a puzzle here, for Einstein the principled holist about the empirical interpretation of theories seems to be invoking more or less exactly the view later to be associated with Schlick and Reichenbach, the view explicitly repudiated by Einstein, this by way of what seems to be a demand for a coordinating definition linking the theoretical concept of the line segment with physical, empirical structures, the idealized practically rigid rods and regular clocks. Is it any wonder that Weyl, another theory holist, played the holist card in response? Weyl argued that the path dependence of length entailed no methodological problem at all, since the demand for direct empirical interpretation of individual theoretical primitives was not warranted, it being only whole theories that possessed empirical content. Has Einstein contradicted himself?

No, Einstein is not guilty of a thoughtless inconsistency. Instead, he has an interestingly subtle amendment to propose to his first principles theory holism: The demand for a stipulative coordinating definition for the notion of the line segment is perfectly reasonable as a kind of stop-gap, given the still immature state of our fundamental physics. We expect a future fundamental physics to yield structures like rods and clocks as solutions of our theories basic equations. A holist picture of empirical interpretation would be right, under those circumstances. But we aren’t there yet. At present we have to put in such structures by hand, as it were, from outside of the theory, and precisely because our theories don’t yet constrain our account of structures like rods and clocks, those structures have to be coordinated with theoretical primitives such as the line segment by conventional stipulation. In principle, a mature, future, fundamental physics would effect the coordination of the line segment with some physical structure from within the theory itself. In practice, such structures must be posited from without. This argument is found in a condensed form in Einstein's widely read and influential 1921 lecture, "Geometrie und Erfahrung":

> 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, 8)

But a more helpful, more expansive version of the same argument is found in the same little-known 1924 review from which I quoted a moment ago. The author of the book under review, Alfred Elsbach, had endorsed several claims about physics and geometry made by the Marburg neo-Kantian, Paul Natorp in his *Die logischen Grundlagen der exakten Wissenschaften* (1910), including the claims that apparent deviations from Euclidean geometry could be accounted for by changes in physical law and that the metrical structure of space cannot be determined empirically because space is something ideal, not real. Einstein then comments:

> 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 it 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)

We see, then, that Einstein exploits the same principled holism about the empirical interpretation of theories to oppose both logical empiricism and neo-Kantianism. Both views fail, on Einstein's view, because they seek to make impossible distinctions among the propositions constituting a scientific theory. Schlick and Reichenbach pretend to cull out analytic coordinating definitions the choice of which is the sole conventional moment in theory construction. Neo-Kantians pretend to cull out a priori constitutive principles whose privileged role in scientific cognition is secured by transcendental arguments of one kind or another. But on Einstein's view, no scientific propositions are privileged as either definitions or constitutive principles, at least not as a matter of principle. Some such singling out might be required for practical purposes, but that is all.

Conclusion

Ryckman's book concludes with an appendix, "Michael Friedman and the 'Relativized *A Priori*.'" In it, he takes sides with Friedman against Quine, Friedman having argued that Quine's holism is incapable of accounting for asymmetries within theories of the kind helpfully characterized by the notion of the contingent a priori (Friedman 2003, 33-35). Ryckman concludes that appendix with these words:

> But then why should we give any credence *at all* to an epistemological holism that has said nothing (or nothing of interest) specifically about relativity theory? Until Quinean naturalists deign to provide *epistemological analyses* (however this is understood) *of particular physical theories* in something like the same degree of detail as Reichenbach, or Friedman, or any of those listed above, I see no reason *not* to regard its naturalistic injunctions against the "relativized *a priori*" as the armchair bluster of a rather naive "first philosophy." (Ryckmann 2005, 249)

One cannot help but love the style. Still, I submit that precisely such a detailed epistemological analysis of general relativity was long ago provided by one of the most important epistemological holists of the twentieth century, Albert Einstein. And I want to hear an argument for why his central claim–the inevitable arbitrariness of any culling of distinctively a priori or distinctively conventional components of theory–is wrong.

ENDNOTES

1. For a comprehensive survey of philosophical reactions to relativity, see Hentchel 1990.

2. Two attempts to explore the larger philosophical problematic standing behind Weyl's views on isomorphism and scientific cognition are Howard 1992 and 1996.

3. A related cluster of questions about whether it is finite or infinitesimal structure that is empirically accessible in a space-time theory such as general relativity and how the theory's fundamental ontology is or is not, therefore, constrained is explored in Howard 1999.

4. See, for example, Howard 1989 and 1997 for further development of the point about how quantum mechanics and field theories like general relativity differently individuate the basic elements of their ontologies.

5. See Weyl 1928 and Wigner 1931.

6. On the shift from invariance to unitary equivalence in Weyl's thinking, see Hawkins 2000, 420-512. My thanks to Tom Ryckman for pointing me to Hawkins, part four of which constitutes a masterful, two-hundred page study of Weyl's seminal work on representations of Lie groups.

7. For a further development of this reading of Cassirer, see Howard 1992 and 1996. For helpful background on the early history of the Marburg program, see Patton 2004. For insight into the way the later development of Marburg transcendental idealism was viewed from with the movement, see Cassirer 1912 and Natorp 1912 (my thanks to Lydia Patton for drawing my attention to the collection of essays on Cohen in *Kant-Studien* in which these two papers are contained).

8. And the young Carnap; see Carnap 1921 and Howard 1996.

9. Schlick is another example of a philosopher whose theory holism derives mainly from Hilbert, not Duhem; see Howard 1994.

References

Carnap, Rudolf (1921). *Der Raum. Ein Beitrag zur Wissenschaftslehre*. Inaugural-Dissertation zur Erlangung der Doktorwürde der hohen philosophischen Fakultät der Universität Jena. Göttingen: Dieterich'schen Univ.-Buchdruckerei, W. Fr. Kaestner. Reprinted as "Kant-Studien" Ergänzungshefte, no. 56. Berlin: Reuther & Reichard, 1922.

Cassirer, Ernst (1907). *Das Erkenntnisproblem in der Philosophie und Wissenschaft der neueren Zeit*, vol. 2. Berlin: Bruno Cassirer.

——— (1910). *Substanzbegriff und Funktionsbegriff. Untersuchungen über die Grundfragen der Erkenntniskritik*. Berlin: Bruno Cassirer.

——— (1912). "Hermann Cohen und die Erneuerung der Kantischen Philosophie." *Kant-Studien* 17, 252-273.

——— (1921). *Zur Einsteinschen Relativitätstheorie. Erkenntnistheoretische Betrachtungen*. Berlin: Bruno Cassirer.

——— (1936). *Determinismus und Indeterminismus in der modernen Physik. Historische und systematische Studien zum Kausalproblem.* Vol. 42, part 3, *Göteborgs Högskolas Ärsskrift.* Separatum: Göteborg: Elanders Boktryckeri Aktiebolag, 1937.

Duhem, Pierre (1906). *La Théorie physique, son objet et sa structure*. Paris: Chevalier & Rivière.

Einstein, Albert (1918). “Nachtrag.” *Königlich Preußische Akademie der Wissenschaften* (Berlin). *Sitzungsberichte. Physikalisch-mathematische Klasse*. 478..

——— (1921). *Geometrie und Erfahrung. Erweiterte Fassung des Festvortrages gehalten an der Preussischen Akademie der Wissenschaften zu Berlin am 27. Januar 1921.* Berlin: Julius Springer.

——— (1924). Review of Elsbach 1924. *Deutsche Literaturzeitung* 45: 1685-1692.

Elsbach, Alfred (1924). *Kant und Einstein. Untersuchungen über das Verhältnis der modernen Erkenntnistheorie zur Relativitätstheorie*. Berlin and Leipzig: Walter de Gruyter.

Friedman, Michael (1999). *Reconsidering Logical Positivism*. New York: Cambridge University Press.

——— (2000). *A Parting of the Ways: Carnap, Cassirer, and Heidegger*. Chicago, IL: Open Court.

——— (2003). *Dynamics of Reason*. Stanford, CA: CSLI Publications.

Hawkins, Thomas (2000). *Emergence of the Theory of Lie Groups: An Essay in the History of Mathematics 1869-1926*. New York: Springer.

Hentschel, Klaus (1990). *Interpretationen und Fehlinterpretationen der speziellen und der allgemeinen Relativitätstheorie durch Zeitgenossen Albert Einsteins*. Boston: Birkhäuser.

Howard, Don (1989). “Holism, Separability, and the Metaphysical Implications of the Bell Experiments.” In *Philosophical Consequences of Quantum Theory: Reflections on Bell’s Theorem*. James T. Cushing and Ernan McMullin, eds. Notre Dame, Indiana: University of Notre Dame Press, 224-253.

——— (1990). “Einstein and Duhem.” *Synthese* 83: 363–384.

——— (1992). “Einstein and *Eindeutigkeit*: A Neglected Theme in the Philosophical Background to General Relativity.” In Jean Eisenstaedt and A. J. Kox, eds. *Studies in the History of General Relativity.* Einstein Studies, vol. 3. Boston: Birkhäuser, 154–243.

——— (1994). “Einstein, Kant, and the Origins of Logical Empiricism.” In *Language, Logic, and the Structure of Scientific Theories*. Wesley Salmon and Gereon Wolters, eds. Pittsburgh: University of Pittsburgh Press; Konstanz: Universitätsverlag, 45-105.

——— (1996). "Relativity, *Eindeutigkeit,* and Monomorphism: Rudolf Carnap and the Development of the Categoricity Concept in Formal Semantics." In *Origins of Logical Empiricism*. Ronald N. Giere and Alan Richardson, eds. Minnesota Studies in the Philosophy of Science, vol. 16. Minneapolis and London: University of Minnesota Press, 115–164.

——— (1997). "A Peek behind the Veil of Maya: Einstein, Schopenhauer, and the Historical Background of the Conception of Space as a Ground for the Individuation of Physical Systems." In *The Cosmos of Science: Essays of Exploration*. John Earman and John D. Norton, eds. Pittsburgh: University of Pittsburgh Press; Konstanz: Universitätsverlag, 87-150.

——— (1999). "Point Coincidences and Pointer Coincidences: Einstein on Invariant Structure in Spacetime Theories." In *History of General Relativity IV: The Expanding Worlds of General Relativity*. Hubert Goenner, Jürgen Renn, Jim Ritter, and Tilman Sauer, eds. Einstein Studies, vol. 7. Boston: Birkhäuser, 463–500.

——— (2005). "Einstein and the Development of Twentieth-Century Philosophy of Science." In *The Cambridge Companion to Einstein*. Michel Janssen and Christoph Lehner, eds. New York: Cambridge University Press (forthcoming).

Natorp, Paul (1910). *Die logischen Grundlagen der exakten Wissenschaften*. Leipzig and Berlin: B. G. Teubner.

——— (1912). "Kant und die Marburger Schule." *Kant-Studien* 17, 193-221.

Patton, Lydia (2004). "Hermann Cohen's History and Philosophy of Science." Ph.D. Dissertation. McGill University.

Quine, Willard Van Orman (1951). "Two Dogmas of Empiricism." *Philosophical Review* 60, 29-43.

Reichenbach, Hans (1920). *Relativitätstheorie und Erkenntnis Apriori*. Berlin: Julius Springer.

——— (1924). *Axiomatik der relativistischen Raum-Zeit-Lehre*. Braunschweig: Friedrich Vieweg und Sohn.

——— (1928). *Philosophie der Raum-Zeit-Lehre*. Berlin and Leipzig: Walter de Gruyter.

Ryckman, Thomas (2005). *The Reign of Relativity: Philosophy in Physics 1915-1925*. New York: Oxford University Press.

Schlick, Moritz (1917). *Raum und Zeit in der gegenwärtigen Physik. Zur Einführung in das Verständnis der allgemeinen Relativitätstheorie*. Berlin: Julius Springer.

——— (1921). "Kritizistische oder empiristische Deutung der neuen Physik." *Kant-Studien* 26, 96-111.

Weyl, Hermann (1918a). "Gravitation und Elektrizität." *Königlich Preußische Akademie der Wissenschaften* (Berlin). *Sitzungsberichte. Physikalisch-mathematische Klasse*. 465-480.

——— (1918b). *Raum•Zeit•Materie. Vorlesungen über allgemeinen Relativitätstheorie*. Berlin: Julius Springer.

——— (1918c). "Reine Infinitesimalgeometrie." *Mathematische Zeitschrift* 2, 384-411.

——— (1927). *Philosophie der Mathematik und Naturwissenschaft.* Munich and Berlin: R. Oldenbourg. Page numbers and translations from the English translation: *Philosophy of Mathematics and Natural Science.* Olaf Helmer, trans. Princeton, New Jersey: Princeton University Press, 1949; rpt. New York: Athenaeum, 1963.

——— (1928). *Gruppentheorie und Quantenmechanik*. Leipzig: S. Hirzel.

Wigner, Eugene P. (1931). *Gruppentheorie und ihre Anwendungen auf die Quantenmechanik der Atromspektren*. Braunschweig: Friedrich Vieweg & Sohn.