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*Conventionalism and Modern Physics: A Re-Assessment**

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Conventionalism implied that physical geometry must be fixed by an arbitrary choice among equivalent alternatives. In the last half-century, this view has retreated before arguments that allegedly equivalent geometries are not at all equivalent on decisive empirical and methodological grounds.[1] Yet such arguments were familiar to, and even proposed by, the conventionalists themselves. Poincaré, Schlick, and Reichenbach—to take just three prominent examples—aimed not to deny that one could rationally choose among physically possible alternative geometries, but to articulate an epistemological theory of the origins of geometrical postulates. According to this theory, the empirical application of geometry depends on principles that are not themselves empirical, principles which were characterized as stipulations. But this view certainly allowed that some stipulations were better than others for the analysis of natural phenomena. Thus Reichenbach, Schlick, and Carnap could maintain that Einstein's general theory of relativity had revealed the arbitrary element in physical geometry, while at the same time demonstrating the superiority of non-Euclidean geometry.

A more recent[2] challenge to conventionalism is that the very idea of a geometrical stipulation does not even make sense in the context of general relativity, which relates geometrical structure to the distribution of matter. On Friedman's view, conventionalism presupposes the nineteenth-century view of geometry as a fixed and uniform background against which the laws of physics are framed. But according to general relativity, physical geometry varies with material circumstances, and so cannot be settled in advance by convention. Thus geometry can no longer be interpreted as part of an a priori background for physics, settled by an initial choice of a theoretical language. Friedman's assessment brings out two conflicting aims behind the conventionalism of the logical positivists: on the one hand, to understand the a priori foundations of science to be not synthetic, but analytic—their analyticity arising from their dependence on the definitions of fundamental concepts; on the other hand, to understand general relativity as an insight into the empirical nature of spacetime geometry.

However, Friedman's analysis overlooks a theme of conventionalism that is not incompatible with general relativity, but on the contrary, is indispensable to our understanding of how spacetime geometry has become an empirical science. In their reaction against the Kantian and empiricist accounts of the foundations of geometry, the conventionalist tradition of Poincaré and the positivists saw correctly that the postulates of physical geometry are neither uniquely determined by the form of outer intuition, nor inferred by induction from experience. The postulates of physical geometry do not express factual claims at all, but have an inescapably *interpretive* character: they connect abstract geometry to physical space by assigning physical meanings to geometrical concepts.

This is the insight that the positivists attempted to capture through the notions of "coordinative definition," "correspondence rule," and the like. It seemed clear to them that this insight requires us to view alternative theories of physical geometry as no more than equivalent linguistic frameworks, and to view the definitions that connect the frameworks with experience as arbitrary choices, justified by pragmatic a posteriori considerations. The assimilation of the choice of geometry to the choice of linguistic framework invited the now-familiar challenges from holism and empiricism: if those fundamental principles are contingent upon, and revisable in the face of, empirical knowledge, in what interesting sense could they possibly be analytic truths? If the choice of physical geometry is clear on empirical grounds, in what interesting sense could it be conventional? For Poincaré and the logical positivists, failure to acknowledge the constitutive role of definitions in physical geometry was the source of naive empiricism about the subject; that the "definitional" character of the postulates implies conventionalism seemed obvious to them. The implication has also seemed obvious to their critics, who, in rejecting conventionalism, have seen no need to address the conventionalists' concern with definitions or to offer an alternative account of their origin and status.

I will argue that the conventionalists' focus on the role of definitions in the foundations of geometry contains an important insight. However, my purpose is not to defend conventionalism, but to understand that insight from a completely different perspective. That the constitutive principles of physical geometry are somehow constitutive of meaning seems to distinguish them from ordinary empirical claims or physical hypotheses, but it need not mean that they are fixed by convention. I will argue that they are, instead, discovered by a process of conceptual analysis. The main task of this paper is to characterize this process of conceptual analysis, and to exhibit the role that it has played in the development of theories of space and time.

Of course, the positivists saw a crucial role for the *destructive* analysis of ill-defined concepts—as in Einstein's exemplary critiques of the Newtonian concepts of space and time. What they failed to perceive was the positive role of conceptual analysis in the construction of concepts, believing that this was adequately captured by their notion of a stipulation. As we will see, however, new foundations for physical geometry have emerged, not from arbitrary new coordinations between geometry and physics, but from novel analyses of what is implicit in established physical principles. By seeing how such analyses have worked in the most decisive historical cases, we can understand something that eluded both conventionalism and traditional empiricism: how principles that are in some sense definitional could nonetheless arise directly from empirical arguments, and how theories of space and time that arise from those definitions are genuinely empirical theories. We will also gain a clearer perspective on the relationship between spacetime theories and the philosophical concerns of logical positivism.

1. Poincaré and the Logical Positivists on Space, Spacetime, and General Relativity.

According to Friedman (1999c), the positivists' difficulties with conventionalism arose from taking Poincaré's doctrine out of its original scientific and philosophical context: Poincaré's geometric conventionalism was plausible in the context of classical physics, and on the basis of his theory of the synthetic a priori. It is well known that Poincaré, while rejecting Kant's views of space and geometry, held to the view that arithmetic has an irreducible basis in temporal intuition; the principle of mathematical induction has no justification save an appeal to our intuition of the iterability of temporal processes. What is noteworthy in Friedman's account is an analysis of the role that this view of intuition plays in Poincaré's account of space, and its importance to his conventionalism about spatial geometry. The very notion of space, Poincaré pointed out, is an idealization derived from our sense of the free mobility of our own bodies. Therefore the "group of rigid motions," identified by Helmholtz and Lie as the foundation of geometries of constant curvature, is an idealization of the primitive experience that acquaints us with the properties of space in the first place. As such, this group is the necessary and sufficient foundation of geometry as an empirical science. But the idealization involved in passing from our own local motions to the group of motions, which is the necessary foundation for our conception of space as globally homogeneous, makes crucial use of the form of temporal intuition: our conception of the large-scale structure of space derives from the presupposition that our local displacements are infinitely iterable, a presupposition whose sole basis is our temporal intuition. And the science of synthetic geometry is just an abstraction from these displacements, as classical geometrical constructions are based on the rigid motions of an idealized compass and straightedge.

Since the principles underlying geometrical construction are just these procedures derived from the "ancestral experience" of local free mobility, and idealized as indefinitely iterable, they are necessarily bound to the geometries of uniform curvature, in which the principle of free mobility holds. It follows that only the geometries of constant curvature can be properly regarded as synthetic, for only in those geometries is constructive proof possible. The much larger class of Riemannian geometries of variable curvature must be regarded as analytic, since no "classical" constructive procedure can yield their propositions. Poincaré's account of the intuitive foundations of geometry thus differs from Kant's in two crucial respects: on the one hand, Poincaré has recognized the existence, and mathematical legitimacy, of infinitely many geometries that are not constrained by the form of intuition, while granting that intuition constrains which of these geometries may claim to constitute synthetic knowledge; on the other hand, Poincaré has found that the constructive procedures licensed by intuition license, in turn, a more general class of geometries than Kant would have admitted, i.e., all the geometries of constant curvature.

According to Friedman, the difference between geometries of constant curvature and general Riemannian geometries is essential for assessing conventionalism. On the empiricist view of Helmholtz, which was Poincaré's starting-point, the kind of experience that convinces us that space is Euclidean—the outcomes of measurement based on the displacements of rigid bodies and the paths of light rays—could equally convince us that space has a non-Euclidean geometry of constant curvature, just in case rigid bodies and light rays behave in ways compatible with such a geometry. But Poincaré placed the exact sciences in a hierarchy of "conditions of possibility." The physics of rigid bodies could not lead one to give up Euclidean geometry, because that physics is possible only within a framework provided by some geometry, which therefore must be presupposed before any physical laws can be developed. Similarly, no geometry can be known unless a general theory of magnitude is assumed, and no theory of magnitude can be known unless the fundamental principles of arithmetic are assumed. Thus no result in physics can be a compelling reason to revise geometry. And since the legitimate candidates for physical geometry—the geometries of constant curvature, which alone are properly synthetic—are equivalent from a mathematical point of view, we can choose any one of these, provided that we adapt our physics to the choice. Therefore the choice of a geometry can only be a matter of convention, and Euclidean geometry is the simplest possible choice.

This interpretation of Poincaré's position explains why the logical positivists' attempts to apply it to the interpretation of general relativity would end in confusion. As we have seen, the conventions intended by Poincaré fix the entire (homogeneous) structure of space, and thereby provide an "a priori" framework for the formulation of physical laws. In general relativity, no such convention is possible, as the geometry of spacetime is everywhere dependent on the distribution of matter, and therefore can't be specified in advance of the laws of physics; in this respect general relativity realizes the vision of Riemann in direct contrast to that of Poincaré, and the positivists' efforts to maintain its compatibility with both views could not have succeeded.

It should be noted, however, that the appropriateness of the positivists' appeals to Poincaré depends on how general relativity is understood. From our standpoint, the positivists' understanding of it rested on some fundamental errors. In particular, general relativity is not, as the positivists thought, a theory of the "general relativity of motion," but a theory of the structure of spacetime; as such, it no more satisfies positivistic strictures against "unobservable entities," or "absolute" distinctions among states of motion, than did special relativity or even Newtonian mechanics. This understanding was encouraged, however, by Einstein himself, and the same may be said of the positivists' account of the arbitrariness of geometry. For Einstein had identified the objective empirical basis of geometry as the determination of "space-time coincidences," or "verifications of…meetings of the material points of our measuring instruments with other material points." (Einstein 1916, p. 117). And this implied that all geometrical structures that agree on these coincidences are equivalent, and that a choice among them is an arbitrary stipulation (an implication that Schlick called "the geometrical relativity of space," cf. 1917, chapter 3). Such a view is not obviously untenable, regarded merely as a philosophical *explication* of the concept "spatiotemporal measurement"; if the legitimate meaning of that concept is exhausted by Einstein's analysis, then the theory of the geometrical structure of spacetime has to be regarded as imposed by a conventional choice, motivated by the search for the simplest possible laws of physics.

On the logical positivists' interpretation of general relativity as "relativizing" space, time and motion, the analogy between their version of conventionalism and Poincaré's is not so implausible. It fits neatly with their belief that hypotheses about the states of motion of bodies—e.g., the Copernican and Ptolemaic hypotheses—are on a par with geometrical conventions, and distinguished from one another only by their relative simplicity; for example, if the centrifugal force in a rotating system is equivalent to a particular gravitational field in a resting system, then we have a conventional choice about which bodies are rotating, and can choose the simplest hypothesis. More particularly, it fits with their conventionalistic account of spacetime curvature in general relativity: applying what they took to be the lesson of Einstein's equivalence principle, they regarded curved spacetime as equivalent to flat spacetime with a gravitational field, which renders the choice between the two hypotheses a matter of convention. These examples are not unreasonably regarded as parallel to those of Poincaré, where the choice is between, say, non-Euclidean spatial geometry and Euclidean geometry along with an additional force-field. The choice is in one case between a homogeneous non-Euclidean geometry and a homogeneous force-field, in the other case between a variable geometry and a variable force-field. In all such cases, the competing hypotheses were regarded as merely different languages for expressing, with varying degrees of convenience or simplicity, the same physical situation. The positivists' main criticism of Poincaré was only that he had a narrow conception of simplicity, which would always single out Euclidean geometry over all others; if we apply the criterion to the total system of geometry and physics, we may find that non-Euclidean geometry is simpler (cf. Carnap 1966, pp. 161–162), as we find with general relativity.

The foregoing leads to an important qualification of Friedman's account. We can see now that some version of Poincaré's view could be made compatible with general relativity, at least as the logical positivists understood the theory, in spite of the passage from homogeneous to inhomogeneous geometry. To do so is to recast conventionalism to reflect the passage from space to spacetime. Poincaré's conventionalism about geometry is essentially bound to the context of three-dimensional space, because the a priori constructive principles of geometry are just those connected with our spatial intuition, and, as we saw, these restrict us to geometries of constant curvature. But Poincaré was also a conventionalist about the laws of mechanics: that bodies free of force move uniformly in straight lines, and that force is proportional to mass times acceleration, were for him mere definitions rather than factual claims. From here to a conventionalist view of inhomogeneous geometry there are only two steps. The first is to recognize that the laws of motion, and in particular the principle of inertia, serve as constructive principles for spacetime geometry; the inertial motions identified by the laws are represented by the geodesics of the spacetime structure. This means that the synthetic (in Poincaré's sense) geometries of spacetime will include those whose geodesics correspond to some set of inertial trajectories identified by some possible physical theory. The geodesics of Newtonian spacetime, to use a familiar example, are identified with the trajectories of particles not subject to gravitational or other forces, while those of general-relativistic spacetimes are the trajectories of freely-falling particles. The Newtonian inertial trajectories have no relative accelerations, and so correspond to the geodesics of a flat spacetime; the latter do typically have relative accelerations that vary with the local distribution of matter, and so correspond to the geodesics of a spacetime of variable curvature.

But those same relative accelerations are interpreted, in the Newtonian theory, as caused by the gravitational field, and therefore as deviations from geodesic motion. Thus this seemingly innocuous reasoning leads immediately to the second step: to assert that whether we understand free-fall trajectories as accelerated by gravitational force, or as inertial trajectories, is simply a matter of convention. That is, some physical trajectories must be arbitrarily *stipulated* to be the inertial ones, and force correspondingly defined by deviation from such trajectories. The statement "Falling bodies travel on geodesics of spacetime" is therefore not an empirical claim, but has the stipulative character of a definition; adopting it permits us to make empirical claims about the curvature of spacetime that would otherwise be meaningless. One need not agree with this view to accept it as a reasonable analogue to Poincaré's: according to both we have an a priori framework in which we can formulate an equivalence class of physically possible geometries (in space, the geometries of constant curvature; in spacetime, the Riemannian geometries that agree on "spacetime coincidences"), and any further determination of geometry depends on a conventional choice about how the laws of physics are to be framed, and which physical trajectories are to represent the straight lines of space or spacetime. The logical positivists did not necessarily articulate this viewpoint clearly, especially since they themselves frequently appealed to examples concerning spatial curvature, or the effects of gravitational fields on spatial lengths, whereas the fundamental issues in general relativity are the curvature of spacetime, and the effects of gravity on spacetime geodesics. With a clear focus on these issues, their assimilation of general relativity to conventionalism turns out to be, at least, coherent.

It should be clear now, however, how much the coherence of this assimilation depends on the positivists' particular interpretation of general relativity. By the same token, the more modern interpretation of general relativity places the conventionalism of the positivists in a clearer light. If that version of conventionalism does make sense, it does so at some cost: on the standard modern interpretation, in which general relativity describes the "real" curvature of spacetime and its connections with matter and energy, the positivists' view of geometry would appear to make it difficult to describe the physical content of the theory, or to explain why the theory of spacetime curvature should have been preferred over its predecessors. For one could not say that Newton or Minkowski was wrong to attribute a certain spatiotemporal structure to "the absolute world"—since that structure is a matter of arbitrary choice—but only that they were wrong to think of that structure as "absolute" rather than as a useful convention. The theory that geometry is arbitrary is, on this view, objectively better than its predecessor; the theory that spacetime is curved is merely more convenient. Overcoming this philosophical limitation, if it is one, does not by itself motivate the theory that (e.g.) gravitation and inertia are aspects of the same physical field, that spacetime is therefore curved in the presence of matter, and that therefore certain phenomena enable us to measure the spacetime curvature. That any particular phenomena are taken to be indicative of curvature, or of any other geometrical property, is precisely what must be settled by convention. In effect, the distinctive physical content of the theory, as opposed to that of its predecessors, is precisely its conventional part. And this is a significant disanalogy with Poincaré's conventionalism. For Poincaré, the synthetic a priori principles that define the equivalence class of physical geometries are themselves physical principles which impose, at least, constant curvature on space. In the positivists' interpretation of general relativity, the analogous equivalence class is defined by the general theory of Riemannian manifolds, which then serves as a mathematical metatheory within which any physical theory of spacetime geometry might be formulated, but whose basic principles contain no physical claims. But if we understand general relativity as a theory, not of relativity and covariance, but of the relation between spacetime curvature and mass-distribution, it is difficult to square with conventionalism. In short, it was not unreasonable of the positivists to maintain Poincaré's conventionalism in the face of general relativity; it would be unreasonable only to think that Einstein's theory, as a theory of spacetime geometry, is true.

If the only justification for any particular choice of the constitutive principles of physical geometry is the role they play in the total system of physical laws, and the extent to which this total system accords with our experience, then the a priori status of those principles is either questionable, or of little interest. This outcome seems to have been unsatisfactory at least to Reichenbach (1957), who argued that coordinative definitions may be motivated by something more than a posteriori convenience. His requirement that a metrical coordinative definition must stipulate the absence of universal forces is, in effect, an argument that what Poincaré would call "empirically equivalent" alternative geometries may be inequivalent on empirically-motivated methodological grounds, and that non-Euclidean geometry is superior to a Euclidean geometry conjoined with the hypothesis of a universal force. Whatever the defects of Reichenbach's discussion,[3] it does attempt to portray the transition from Newton's physics to Einstein's as an empirically-motivated change in constitutive physical principles—as a change in the "relativized a priori" foundations of geometry.[4] But the account of those empirical motivations is obscured by the broadly conventionalistic setting in which they are presented, according to which objective empirical reasoning about geometry is only possible within a framework established by arbitrary definitions (1957, pp. 36–37). Moreover, Reichenbach's requirement offers a negative principle for rejecting proposed metrical coordinations, rather than a positive account of the origins of or physical motivations for any particular one. Thus, despite its admixture of anti-conventionalist elements, Reichenbach's view reinforces the conventionalist interpretation of general relativity: that the theory reveals the arbitrary element in physical geometry, and that only holistic considerations of simplicity can distinguish among geometrical conventions.

It is helpful to view this entire development in a broader historical perspective. Conventionalism aimed to correct the error of Kant's theory of the synthetic a priori, which lay in supposing that genuine propositions concerning the world of experience could have the apodeictic force of logic. If the postulates of physics and geometry have a certainty beyond that of ordinary empirical generalizations, it is because they aren't genuine propositions, but definitions. But Kant's theory also aimed to correct a traditional error, that of supposing that the postulates of geometry have some extralogical content, yet are purely "intellectual" truths. Whatever non-logical content is possessed by such postulates is prescribed by the forms of sensible intuition. Without defending Kant's intuitionism against later advances in logic and the rigorization of mathematics, we can see its merits relative to earlier views: on the one hand, it recognizes that, at least up to Kant's time, the use of intuition in mathematical reasoning was pervasive but not acknowledged; on the other hand, it recognizes that the concepts of mathematics were "productive" in a way that the concepts of traditional metaphysics had never been, precisely because the former are constructed in accord with the forms of spatial and temporal intuition, and not merely "invented" by the arbitrary assignment of meaning to words. Kant's theory thus provides an explanation—for whatever it's worth—of why the principles of geometry should seem to force themselves on us with something like the certainty of logic, while yet having some definite empirical content. By contrast, conventionalism understands this certainty as the certainty of analytic truth—the principles are just the rules that are definitive of a given linguistic framework—but can't address their empirical content except as something fixed by an arbitrary designation, or a coordinative definition in the most literal sense, that identifies some phenomenon as the referent of a concept. For Poincaré's conventionalism, this question of content doesn't arise. If the subject matter of geometry is the group of rigid spatial displacements, then we may have to make conventional choices about which homogeneous geometry to use, and precisely which bodies are rigid, but the empirical content of geometrical claims in general is fixed, as is the structure of space up to its measure of curvature. But if the subject matter of physical geometry is the "meetings of the material points of our measuring instruments with other material points," then practically all of its content is open to arbitrary decision: except for coincidences with my own worldline, all of my judgments about such coincidences will require some theoretical decisions to be made on the grounds of simplicity and convenience.

Thus, it is the need for stipulations about the very content of physical geometry that separates the logical positivists' conventionalism from Poincaré's. This outcome is ironic, not merely because of the positivists' identification with Poincaré, but, more important, because of the enormous emphasis they placed on Einstein's revolution as a radical conceptual change, and on new and better definitions of geometrical concepts as characteristic of that change. Carnap, for example, surely thought of these as examples of the sort of "change in the language" that "constitutes a radical alteration, sometimes a revolution," as distinct from "a mere change in or addition of, a truth value ascribed to an indeterminate statement" within a language (1963, p. 921). This distinction is generally assumed to have been discredited by Quine, but it has a prima facie claim of relevance to the history of 20th-century physics. On what is at least a plausible reading, special relativity was founded on defining the velocity of light as a fundamental invariant, and on defining simultaneity by light-signalling; general relativity was founded on defining the geodesics of spacetime as the paths of falling bodies; at least until we begin to suspect the notion of analyticity in general, it would seem as if the acceptance of such definitions is essential to the acceptance of the theories, so that denying or changing them would amount to creating an alternative conceptual framework. What is the sense of denying, then, that "The trajectories of freely-falling bodies are the geodesics of spacetime" is an analytic truth of the linguistic framework of general relativity? The serious answer to this question has to do with the difficulty about content. If such defining principles are arbitrary stipulations that can't be independently motivated—if their physical content can't be independently specified and judged—then such a presumptive analytic truth—by contrast with "Bodies free of all Newtonian forces follow spacetime geodesics"—amounts in practice to nothing more than the claim that general relativity is, on the whole, a more useful conceptual scheme than Newtonian mechanics.

Of course, the logical positivists did attempt to justify Einstein's conventions on philosophical grounds. Reichenbach's proscription of universal forces was one such an attempt; others took their lead from Einstein's own arguments, and typically pointed to the "epistemological" inadequacy of the Newtonian definitions, e.g., of simultaneity or absolute motion, and their failure to provide the sort of "verifications" described by Einstein's accounts of simultaneity and of "spacetime coincidences." As we have seen, however, such arguments are, at best, destructive critiques of the Newtonian framework rather than positive motivations; at worst, they are confused, since general relativity, properly understood, doesn't really satisfy such general epistemological strictures either. At the same time, Schlick and Reichenbach, at least, argued that the philosophical insights of Einstein's theory depended on specific theoretical developments in physics.[5] This kind of justification would seem to be incompatible with the first kind: surely Einstein's theory could not have evolved out of a purely epistemological critique of earlier theories, *and* be contingent on the fate of particular scientific hypotheses such as "Mach's principle." Neither sort of justification can be reconciled with the view that physical geometry is founded on arbitrary stipulations.

2. Conventions, Definitions, and Conceptual Analysis.

Obviously the logical positivists left the understanding of physical geometry, and the nature and function of its a priori principles, in a very unsatisfactory state. They had good reasons to believe that Einstein's revolutionary conceptions of space and time were developed with the help of a philosophical analysis of some kind or another; that the theories were, at the same time, founded on purely empirical principles from electrodynamics and gravitation; and that in both theories, as in physical geometry generally, definitions of fundamental concepts played crucial constitutive roles. Against Quine, they might have argued that all three beliefs reflected Einstein's characterization of his own scientific practice—an argument that would deserve at least the attention of a professed epistemological naturalist. But their account of the definitions as arbitrary stipulations, and their attendant failure to analyse the origins or motivations of those definitions, makes the philosophical connections among the three ideas difficult to see. By the same token, the intimate relations among them would appear obvious if the origins of the fundamental definitions could be rationally explained. The most effective refutation of conventionalism would show that we can understand those definitions through their philosophical and physical motivations, instead of treating them as conventions.

That understanding begins with recalling that concepts may come to be defined, not only by stipulations about meaning, but by conceptual analysis. This distinction may not appear immediately to be promising, if we think of conceptual analysis just as analysis of "what is contained in" a given concept. That is precisely the sort of analysis that, according to Kant, could never provide the foundation for an empirical science; from a more modern perspective, of course, analytic judgments in Kant's sense seem to be empirical claims about the typical uses of words. A more promising notion of definition by conceptual analysis is based not on the question, "what do we typically mean by X?", but rather on the question, "what conception of X is implicit in our established empirical judgments and practices?" And even this may be difficult to see in a constructive role. In the philosophy of space and time, such analyses have typically been seen as destructive, reducing space, time, and motion to "nothing but" their supposed phenomenal basis, as in the "relativism" of (e.g.) Berkeley, Leibniz and Mach. To Einstein and the logical positivists, Einstein's discussions of simultaneity, rotation, and spatio-temporal measurement were analyses of just this sort, motivated by some form of verificationism: motion is "nothing but" relative motion; measurements are "nothing but" verifications of "spacetime coincidences". To the extent that it is construed in this reductionist manner, such an analysis typically is taken to establish, not an objective foundation for geometrical measurement, but the lack of any such foundation—or, in Reichenbach's words, "the need for a coordinative definition." This is why, on the view of the history of theories of space and time that has been common since Einstein, the progress from Newton to special to general relativity consists in a gradual "relativization" of what had been seen as objective or absolute, ending with a theory in which space and time have lost "the last remnant of physical objectivity" (cf. Einstein 1916, p. 117).

As we have seen, however, that sort of analysis yields an empiricist meta-perspective on spacetime theories rather than a physical motivation for any particular theory. Even if it is the right perspective, it can't be said to capture or to reconstruct the motivations for the theory of spacetime curvature, whose foundation must in that case be a convention. A constructive conceptual analysis would show that an established set of empirical judgments implicitly contains a constructive principle for physical geometry, one that only needs to be raised to an appropriate level of precision and generality. A clear example was provided by Poincaré himself, in his explication of "the notion of space." The essential idea originated with Helmholtz's analysis of what we mean by "spatial relations": he considered how, among all the changes that we can observe, certain changes can be identified as spatial displacements or changes of spatial relation. This is in some sense a demand for a definition, but it is not to be answered by an arbitrary stipulation, or by a verificationistic reduction of spatial determinations to some more elementary empirical basis. Rather, the analysis recognizes a distinguished type of phenomenon that we judge to be characteristically spatial, and demands a precise formulation of the principle that is latent in those judgments. The answer is that spatial displacements are those that can be effected and cancelled by the motion of the observer; in other words, they are defined by the manner in which they can be done, undone, and combined. And it is these actions, sufficiently idealized, that invite interpretation as the characteristic operations of a group. It was by this analysis that Poincaré arrived at his group-theoretic conception of space.

The empiricist motivation of this analysis, and of the resulting definition, is evident; it is of a piece with another famous analysis by Helmholtz, concerning the question whether we can "imagine" a non-Euclidean space:

> By the much misused expression "to imagine," or "to be able to think of how something happens," I understand that one could depict the series of sense-impressions which one would have if such a thing happened in an individual case. I do not see how one could understand anything else by it without abandoning the whole sense of the expression. (1884, p. 8)

Both are classic examples of the analysis of "the empirical content" of a notion previously muddied by intuitive or metaphysical associations, and both were recalled by the logical positivists as landmarks in the development of an empiricist view of geometry. Yet they have to be distinguished from ordinary empirical arguments. It is in some sense an empirical fact that the group of displacements can be distinguished; but that the rigid displacements have the structure of a group is not an empirical claim in the ordinary sense. Rather, we would not recognize as spatial displacements any changes that did not conform to that structure. At the same time, this principle is not an arbitrary designation of the phenomenal referent of an abstract mathematical concept. It is, rather, an argument that the mathematical concept captures precisely and formally what is contained, vaguely and informally, in our pre-systematic notion of a spatial change and our pre-systematic judgments of the spatial relations of things. In other words, it is not an empirical claim because it is an interpretation of our empirical judgments; it is not conventional because the interpretation arises, not from a stipulation, but from a conceptual analysis.

From the foregoing we can see why such analyses need not be empirically empty, but can have the most far-reaching implications for empirical science. Helmholtz's definition of "to imagine" does not merely propose an interpretation of the word on which we might meaningfully claim to imagine a curved space; rather, it uncovers the interpretation that is implicit in our claim that we can imagine Euclidean space, and reveals that claim to be of much wider application than we had previously appreciated. The Helmholtz-Poincaré definition of spatial displacement, similarly, does not merely invent a general conception that can be applied to any space of constant curvature. Rather, it reveals that the conception underlying our knowledge of Euclidean space is too general to single out Euclidean space from the other spaces of constant curvature. Where Kant had held that our notion of space is bound up with Euclid's axioms, since these state the constructive procedures on which our entire conception of space is based, Helmholtz and Poincaré showed that those constructive procedures are more general than that, and that "our notion of space"—at least, the notion implicit in our empirical judgments about space—is correspondingly general. And thus both analyses helped to make the difference between the discovery of non-Euclidean geometry, and the discovery that "space" might be non-Euclidean. Moreover, conceptual analyses of just this sort, as we will see, have been essential to the most revolutionary developments in the theory of physical geometry.

At the same time we can see, from a different perspective, why a conceptual analysis like Poincaré's would lead to a form of conventionalism, and we can see the limits of that form of conventionalism. The concept of space that emerges from Poincaré's analysis is not defined by the arbitrary association of certain observed displacements with geometrical concepts, in the manner of a coordinative definition, and so it is clearly not a matter of convention—although Poincaré acknowledges the possibility of exchanging it for some more convenient concept; the concepts arrived at by analysis are not assumed to be permanent. But it follows from Poincaré's analysis that the geometry of space, so defined, is indeterminate: the geometries compatible with the definition, those of constant curvature, form an equivalence class of mutually intertranslatable structures. Therefore the means of distinguishing among them must come from outside of geometry, i.e., from considerations that are not implicit in the notion of space, but that involve physical hypotheses that take the form of coordinative definitions: that light travels in a straight line, for example, can be an empirical claim only if light rays can be compared with straight lines. For Poincaré, this is the beginning of a regress that can only end in a stipulation. Unlike the definition of a spatial displacement, such a coordinative definition requires a choice among several equivalent alternatives, some of which may turn out to be more convenient than others, but none of which has a special claim to represent "what is implicit in our notion of straight line."

From this we could conclude that conventionalism is, in an important sense, *incidental* to Poincaré's analysis of geometry. The analysis reveals the foundation of geometry in a "disguised" or implicit definition. But having this foundation does not make geometry an uninterpreted structure. For the definition itself constitutes an interpretation of a specific type of phenomenon as instantiating a specific mathematical structure. For the positivists, the structure determined by a set of implicit definitions requires a convention to fix its empirical content, but in Poincaré's analysis of geometry, that content is already expressed in the definition of spatial displacement; convention plays a role only because that content, as it turns out, admits a class of equivalent geometrical realizations.

It is instructive to compare this analysis with that of Riemann, in particular with Riemann's emphasis on the approximate character of the principle of free mobility. According to Riemann, if the physical principle underlying homogeneous geometry is only an approximation, that geometry itself is only an approximation to an actual geometry that may well be inhomogeneous at very large or small scales. On small scales, especially, the inexact principle of the rigid body must yield to a more exact, and more fundamental, principle of the interactions that constitute rigid bodies in the first place. It follows that, if a degree of global spatial structure is implicit in the principle of the rigid body, implicit in the deeper principle may be a local structure that varies over space; it also means that the degree of arbitrariness inherent in the former—and therefore the occasion for conventionalism—may not exist in the latter. For both Riemann and Poincaré, then, analysis of the assumptions involved in measurement leads to a *definition* of spatial relations that associates them with physical processes, and in neither case is the definition therefore arbitrary. The arbitrariness arises from Poincaré's assumption that the concept of space is explicated *exhaustively* by the group of rigid motions, and that the latter provides the *only* basis for a truly synthetic geometry; on these assumptions, no investigation of the sort proposed by Riemann, into measure-relations "in the small," could possibly yield a legitimate constructive procedure for geometry. From Riemann's point of view, however, this must appear naive: rather than standing before physics as (in this restricted sense) an a priori framework, Poincaré's conception of space ties geometry to a particularly simplistic physical principle, just because of the latter's privileged role in the genesis of our geometrical ideas, instead of recognizing that subtler physical principles might yield subtler principles of measurement and correspondingly more complex geometries. And, from the same point of view, even Helmholtz's attitude would seem comparatively sophisticated. Helmholtz also believed that free mobility was both the original and the only possible basis for geometry, but forestalled conventionalism by insisting that mechanics could decide among the geometries of constant curvature; he thus recognized at least one part of Riemann's view, that the notion of rigid body is not privileged over the rest of physics. For Poincaré, its privileged status is what defines our notion of space.

This last remark is especially important. The only plausible defence of Poincaré's narrow conception is that the physical principles invoked by Riemann and Helmholtz are not proper to the theory of space, but involve extrinsic factors; in particular, as *dynamical* principles, they essentially involve *time*; therefore the principle of free mobility is privileged over them, as far as geometry is concerned, precisely insofar as it is a purely spatial principle. But if this argument excuses Poincaré, it also reveals the deeper insight behind the empiricism of Riemann and Helmholtz: that the geometry of space is not independent of the principles that connect space with time—in modern language, that *spacetime is more fundamental than space*. Indeed, for an understanding of Poincaré's conventionalism in historical perspective, the separation of space from spacetime is perhaps more important than the distinction between homogeneous and inhomogeneous geometry. In particular, that an analysis of geometrical postulates as "disguised definitions" should automatically lead to conventionalism is an aspect of space as opposed to spacetime: Poincaré's hierarchy of sciences is possible just to the extent that geometry is defined to be spatial geometry, and that the concept of space is completely explicated independently of any dynamical principles. In that case the latter are open to conventional choice, and certainly cannot play any constitutive role for the spatial geometry against which they are framed. For the geometry of spacetime, however, those principles are precisely the constitutive principles. The truth obscured behind an aforementioned remark of Poincaré's, that inhomogeneous Riemannian geometries are not properly synthetic, is that a purely spatial principle such as that of free mobility is unlikely to provide a constructive basis for an inhomogeneous geometry; the appropriate principle would likely be a dynamical, i.e., a spatio-temporal, principle, like the microphysical principles of causal connection envisaged by Riemann, or general relativity's identification of gravitational free-fall with inertial motion. The familiar historical example of inhomogeneous *spatial* geometry is Einstein's prediction of spatial curvature near the sun, as corroborated by the bending of starlight; but the prediction follows from the theory of non-uniform *spacetime* geometry, in which light-propagation plays a crucial constitutive role.

3. Conceptual Analysis and the Foundations of Spacetime Theories.

Poincaré's view of geometry, in sum, starts from a conceptual analysis that leads to a constitutive principle, a principle that is a kind of definition—insofar as it is a principle of interpretation—without therefore being an arbitrary stipulation; it ends with arbitrary stipulations, however, because the analysis is restricted to the constitutive principles of spatial geometry. In the development of constitutive principles for spacetime geometry, we see the fundamental role played by such conceptual analysis, and the comparative irrelevance of arbitrary stipulations. One clear illustration is the emergence of the Newtonian conception of inertia, beginning with the work of Galileo. As we saw, that force is proportional to acceleration, rather than to velocity or some other quantity, and inertia is thus resistance to acceleration rather than to change of position, were for Poincaré obvious examples of mere definitions. Galileo did not attempt to argue that these are factual claims arrived at by induction, but recognized them as interpretations of facts already known to the Aristotelians. So the question arises, what kind of non-circular argument could Galileo possibly provide for a definition, other than that it leads to a generally simpler system of physics? This question is especially pressing in view of the problem of "incommensurability," for, in the absence of a plausible extension of his physical principles to all the phenomena embraced by Aristotle's physics—an extension that was not available before Newton—this pragmatic argument from global simplicity is not one that Galileo was in a position to make.

The answer lies in the dialectical process described in the *Dialogue Concerning the Two Chief World-Systems* (1632), by which Galileo's spokesman, Salviati, elicits assent to his conception from the Aristotelian Simplicio. Of course the conversion of Simplicio, and the dialogue as a whole, are highly contrived. But the general principle behind Galileo's argument is more compelling: that his conception of inertia is *already in use*, implicitly, in familiar and well-established empirical judgments. The empirical facts are, apparently, that motion does not persist and that bodies come to rest on the earth when forces cease to move them. But it is equally apparent that in familiar cases of relative motion, we implicitly assume that motion does persist, and implicitly associate force with change of motion—for example, in the case of a horse-rider who throws an object directly to instead of in front of another rider, or of a shooter who follows a moving target with the gun-barrel instead of "leading" the target. And while Aristotle's conception of motion may serve as an interpretation of the first set of facts, and may not directly conflict with the second, Galileo's conception is implicit in the assumption—tacitly but successfully employed by anyone conducting experiments on a moving ship—that both sets are phenomena of essentially the same kind. To the challenge of incommensurability, then, Galileo could answer that just this conceptual analysis measures his conception against Aristotle's and shows Galileo's to be superior.[6]

Galileo's analysis of motion falls short of establishing a constructive principle for spacetime geometry, because of the well-known fact that it remains ambiguous about the natural state of motion for bodies: either uniform motion in a straight line, or uniform circular motion (e.g., parallel to the surface of the earth) may be indistinguishable from rest. And this is in a sense appropriate, since the analysis attempts only to draw out the concepts implicit in the dynamics of motion near the earth's surface; the precise Newtonian concept of inertia arises from the extension of Galilean dynamics to the entire planetary system, as first envisaged by Descartes and his followers. It is also well known that the spacetime structure proposed by Newton, "absolute space," is not the structure implicit in his conception of inertia, but something stronger. What Newton's laws enable us to construct is not absolute space, but an equivalence class of inertial frames; absolute space, however, makes just that distinction between uniform motion and rest that the equivalence of inertial frames denies. But two points about absolute space require emphasis. First, it is, in fact, a spacetime structure; as Newton defines it, at least, it implies the connection of space *through time* in such a way that states of motion are defined, albeit more states of motion than the dynamics can distinguish. Therefore

Poincaré was wrong to think that what he calls "the relativity of space" implies the impossibility of absolute space (1913, pp. 84–85). For the former follows from his understanding of space through the group of rigid motions, which, again, is independent of any dynamical principle; it is perfectly compatible with a theory that connects homogeneous space *through time* in the manner of absolute space, as well as with the correct theory of Newtonian space-time.[7] Second, a constructive principle for absolute space is easy to imagine, and was in fact imagined by Poincaré himself when he noted the possibility of defining force by velocity instead of acceleration; in that case rest and motion in absolute space would be as well defined as acceleration is in the Newtonian case, and one could attach a dynamical meaning to the claim that the world has the structure of absolute space, which for Poincaré was only a convention (1913, pp. 109–111). We have here another example of something that is a matter of convention from Poincaré's view, only because the constructive basis of geometry is seen in exclusively spatial terms.

This understanding of Newtonian spacetime goes against the familiar view of the positivists, on which not only absolute space, but also absolute time and absolute rotation, are outstanding examples of empirically ill-defined notions. Newton's arguments in support of these notions, especially the "water-bucket" argument for absolute rotation, seemed to them to be illegitimate inferences from observation to metaphysical conclusions. It is now obvious, however, that Newton was not trying to infer the *existence* of "absolute rotation" from observations, but was *defining* absolute rotation as a theoretical quantity by exhibiting the phenomena that enable us to measure it.[8] To this extent Newton's proposal has the essential characteristics of a coordinative definition precisely in Reichenbach's sense. To be fair, then, the positivists ought to have conceded Newton's freedom to define rotation as he saw fit, provided that he could state—as he undoubtedly did—empirical criteria for the application of the concept. But Newton's own defence of the definition is not merely that it has an empirical application as part of a useful conceptual framework. Newton provides, in addition, a conceptual analysis similar to Galileo's, with a similarly dialectical emphasis: he argues that this conception of rotation is *implicit* in the dynamical reasoning of his contemporaries, whatever their official pronouncements about the relativity of motion; in particular, it is *already in use* in their dynamical theory of celestial vortices.[9]

Indeed, the dynamical distinctions that Newton defines among states of motion—that is, the distinctions of absolute rotation and acceleration from uniform motion—are implicit in the 17th-century understanding of causal interaction: a body acts on another by changing its state of motion; non-uniform motion thus requires a causal explanation that uniform motion does not. On such grounds even Leibniz held that kinematically equivalent motions could be dynamically distinct. But if this implicit causal distinction is taken seriously, the relativist approach to the "system of the world" is untenable, and the issue between Copernicus and Ptolemy cannot be a matter of hypothesis or convention. For the concepts of inertia and force provide, for any system of bodies, procedures for constructing a frame of reference—an inertial frame—relative to which their states of motion correspond to their causal influences on one another, and these are the motions that Newton felt justified in calling the "true" motions. This process changes the fundamental question of cosmology: the question "which body is at rest?" is no longer appropriate, and is replaced by empirical questions about the relative masses of the bodies and the location of their common centre of mass. Since the sun contains most of the mass of our system, Newton shows, it will never be far from the centre of mass, and so the heliocentric theory is a better approximation than the geocentric. If the mass were more evenly distributed, however, the difference between the two would be correspondingly less interesting; it could even be said to be a matter of convention for a system of two nearly equal masses.

These aspects of Newton's theory are difficult to appreciate from the logical positivists' perspective. Reichenbach, for example, regarded Newton's choice of the Copernican system as a coordinative definition of a rest-frame, motivated by the need to accommodate his theory of gravity in the simplest possible way. This made it difficult to recognize the constitutive principles of the Newtonian spatio-temporal framework, and their origin in a conceptual analysis of dynamics. On the contrary, the positivists regarded the framework as an unnecessary metaphysical addition to the dynamical theory. At the same time, they regarded Einstein's theories as products of philosophical analysis, but, again, they understood this as "epistemological analysis" of the most reductive sort. Therefore they obscured the essential philosophical continuity between Newton's and Einstein's work, and the essential similarity of the conceptual analyses involved.

As we have already seen, misunderstandings of this sort were encouraged by Einstein's own remarks, particularly about general relativity as a reduction of geometry to coincidences. Even in the case of special relativity, there is some apparent encouragement for the positivists' view of a formal structure connected to experience by stipulation. We know that Einstein (1905) derived the Lorentz contraction from the "relativity principle" in conjunction with the constancy of the velocity of light, and we know that the apparent contradiction between these two premises stems from the hidden assumption of absolute simultaneity; we can resolve the contradiction, then, by granting the relativity of simultaneity. From here we see that the Lorentz contraction, instead of being an "ad hoc" adjustment of Maxwell's theory to the failure to detect the earth's motion relative to the ether, follows logically and naturally from Einstein's premises. But this reasoning only reveals the existence of two equivalent interpretations of the same facts: either the Lorentz contraction is genuine and explains the apparent invariance of the velocity of light, or the invariance of the velocity of light is genuine and explains the apparent Lorentz contraction. What is needed, in addition to the formal reasoning, is some justification for Einstein's starting-point, which his discussion of simultaneity is obviously meant to supply. If the difference between the two interpretations hangs on the definition of simultaneity, however, then just to that extent it would appear to be a matter of convention.

Einstein himself begins by asserting that a "common time" for different observers "cannot be defined at all unless we establish by definition that the 'time' required by light to travel from A to B equals the 'time' it requires to travel from B to A" (1905, p. 40). Later discussions seem to portray this assumption of the isotropy of light-propagation as an arbitrary stipulation. In his popular exposition of his work (1917), he writes, "Only *one* requirement is to be set for the definition of simultaneity: that in every real case it provide an empirical decision about whether the concept to be defined applies or not"; that light takes the same amount of time to travel in both directions "is neither a supposition nor a hypothesis, but a stipulation that I can make according to my own free discretion, in order to achieve a definition of simultaneity" (1917, p. 15.) In his Princeton lectures (1922), he says that "It is immaterial what kind of processes one chooses for such a definition of time," except that it is "advantageous…to choose only those processes concerning which we know something certain" (1922, pp. 28–29). Remarks like these suggest that special relativity, as a theoretical framework, is connected with reality only by the *choice* of light-propagation as the standard of simultaneity. The best that one could say of the framework is that it is based on a "practical" procedure for determining which events are simultaneous; in the Newtonian framework, instantaneous causal propagation provides absolute simultaneity with a theoretical basis, but not a practical procedure.

On a closer look at Einstein's stipulation, however, we can discern the process of conceptual analysis that provides its physical and philosophical motivation. One aspect of the analysis has already been mentioned, that is, the analysis of the contradiction between the relativity principle and the light postulate, and the resulting recognition that their incompatibility depends on the assumption of absolute simultaneity; observers in relative motion can agree on the velocity of light only if they disagree on which events are simultaneous. A similar analysis shows that the concept of simultaneity is bound up with our measurements of spatial and temporal intervals, so that observers who disagree on which events are simultaneous have no common measure of length and time. These are familiar aspects of special relativity. But while they illuminate the distinction between Einstein's framework and Lorentz's and the assumptions on which each is founded, they don't by themselves argue for either. The second, in particular, had already been articulated within the Newtonian framework, with no intention of questioning the framework, but merely in order to acknowledge its fundamental assumptions.[10]

The decisive analysis is the one that exhibits Einstein's definition of simultaneity, not merely as a free stipulation that is logically unexceptionable, but as an account of the physical content of our empirical judgments of simultaneity. Thus it is not freely chosen, because it has a number of requirements to satisfy. Nor is the definition an operationalistic reduction of those judgments to practical procedures, because it makes essential use of theoretical principles. Einstein begins by proposing two practical procedures, each of which supplies an "operational definition" of simultaneity: to define "time" by "the position of the small hand of my watch," and to coordinate the time of every event with a watch at a fixed location, by the time at which a light-signal from each event reaches the watch. The first obviously fails to meet the requirement of defining simultaneity for distant events; the second meets that requirement, but "has the disadvantage that it is not independent of the standpoint of the observer" (1905, p. 39). From the rejection of these possibilities we see that Einstein is not only trying to coordinate the concept with a physical process of propagation. He is also trying to capture what is contained in the abstract notion of "absolute simultaneity"—not necessarily absolute simultaneity in Newton's sense, but, at least, a criterion of simultaneity that does not depend on the standpoint of the observer, and that makes simultaneity a symmetric and transitive relation. Therefore the required coordination is not an arbitrary choice, for it has two independent motivations. On the one hand, it is "the most natural definition of simultaneity" (1917, p. 18); it is in fact the definition that human beings ordinarily use, insofar as we consider events to be simultaneous when we see them at the same time, without stopping to wonder whether this criterion would give the same results for observers in relative motion. On the other hand, once we raise the problem of relative motion, we need a criterion derived from an *invariant* process of propagation, and the invariance of the velocity of light uniquely meets this need. It is this unexpected accord between the "natural" definition of simultaneity and the empirically established invariance of the laws of electrodynamics, rather than the need for a stipulation or an operational definition, that Einstein's conceptual analysis reveals.

The need for an abstract notion of "absolute simultaneity" is an instructive point of comparison between Einstein and Newton. Our intuitive sense of simultaneity, based on seeing events at the same time, neglects both the motion of the earth and the time of propagation for light, which for most practical purposes is immeasurably small. From Newton and Einstein, respectively, we have two ways of abstracting from this criterion of simultaneity to arrive at one that has a theoretical basis in the laws of physics, and that is independent of the reference frame of the earth. Newton's approach neglects as a matter of principle the time of propagation: the abstraction consists precisely in leaving out altogether the method of signalling, and assuming that which events are simultaneous is a matter of fact that does not depend on the standpoint of any observer. And this is no more than what is implicit in the principle that force, mass, and acceleration are independent of the motion of the observer, and the principle that gravitational attraction depends only on the masses and distances of the interacting bodies without reference to time. The latter provides (in principle) an instantiation of absolute simultaneity, rather than a practical means of determining it, but the phenomena addressed by Newton present no reason to suspect that this difference might be important. In any practical situation, it would seem intuitively obvious that the light-signalling method would provide, at least, a retrospective account of which past events were absolutely simultaneous—assuming that, in cases of relative motion, the Newtonian addition of velocities would apply to light signals as well.

Einstein's abstraction, by contrast, consists in giving precedence to the intuitive method of determining simultaneity, and asserting its independence of the motion of the observer—an entirely contingent assertion that was warranted by the state of electrodynamic theory and experiment in 1905, but that would have made little sense before then. It turns out, however, that this "absolute" criterion of simultaneity does not give the same results for observers in relative motion, but results that vary from frame to frame strictly according to the degree of relative motion. We can see from this that Einstein's analysis of simultaneity was not, any more than Newton's was, an epistemological reduction of the concept to purely phenomenal means of verification. Rather, each was an abstraction from the familiar concept, made possible by the contemporary state of development of theoretical physics. In other words, neither conceptual analysis is merely an analysis of "what we mean by simultaneity"; both are analyses of the relationship between what we ordinarily mean by simultaneity, and the meaning that is implicit in established theoretical principles.

The foregoing highlights the difference between the positivists' philosophical reconstructions of Einstein's analysis of simultaneity, and its true philosophical content. Einstein's analysis enabled Minkowski (1908) to formulate the spacetime geometry that is implicit in special relativity, and, in particular, to see that the Lorentz transformations, rather than the Galilean, constitute the symmetry-group of spacetime. As Helmholtz and Poincaré had understood the notion of space through the possibility of certain spatial displacements, Minkowski recognized that the laws of Newtonian mechanics and special relativity enable us to understand a spatiotemporal structure through the possibility of certain "spatiotemporal displacements": the coordinate transformations that preserve the dynamical invariants of each theory. The symmetry group "$\mathbf{G}_\infty$" of Newtonian mechanics defines one sort of spatiotemporal structure, while the symmetry group "$\mathbf{G}_\mathbf{c}$" of electrodynamics, as Einstein had shown, defines a different structure, and $\mathbf{G}_\infty$ arises from $\mathbf{G}_\mathbf{c}$ in the limit as the parameter $\mathbf{c}$ (the invariant velocity) goes to infinity. From the conventionalist point of view, to accept either of these as the structure of spacetime is to make an arbitrary stipulation. But this situation is not quite analogous to the one confronted by Poincaré. As we have seen, explicating the concept of space through the group of rigid motions identifies the physical principle that is constitutive of space, but leaves the precise geometry indeterminate. Minkowski's conceptual analysis of spacetime, however, identifies spatiotemporal displacements as symmetries of the laws of physics. Therefore when we identify the constitutive principles of spacetime, or the fundamental physical laws, we are already determining the geometry of spacetime, or at least placing it beyond the reach of conventional choice.[11] Minkowski makes it clear that his picture of spacetime is not founded on a stipulation, nor is it advanced as a hypothetical explanation of electrodynamic phenomena. Rather, it simply is the structure implicit in our understanding of the laws of electrodynamics: "Now the impulse and true motive for assuming the group $\mathbf{G_c}$ came from the fact that the differential equation for the propagation of light in empty space possesses that group $\mathbf{G_c}$" (1908, p. 81). That the electrodynamics of moving bodies possesses this structure is not decisive, of course, since, as Minkowski points out, the same structure characterizes Lorentz's theory. But the decisive arguments had already been given by Einstein, who had recognized that "the time of the [moving] electron is just as good as that of the [electron at rest]" (1908, p. 82). In other words, that this structure expresses the symmetries of electrodynamics is a mathematical fact (one already noticed by Poincaré, who nonetheless held to the Lorentzian framework), but that this structure also expresses the fundamental symmetries of spacetime emerges from Einstein's analysis of time.

The cases of Newtonian mechanics and special relativity reveal, in sum, the manner in which the laws of physics serve as the constitutive principles of spacetime geometry, and the kind of conceptual analysis from which those principles have emerged. For general relativity, however, Einstein explicitly offered the kind of reductive epistemological analysis that we have already discussed, in order to eliminate not only the privileged status that the previous theories granted to inertial frames, but the physical objectivity of space and time in general. In all of this, a crucial role was played by the reduction of spatiotemporal measurement to the determination of coincidences. As we have seen, this conceptual analysis yields a completely general mathematical framework for spacetime geometry that appears to have no physical content. But it is easy to see why it would not have appeared so to Einstein and the logical positivists: in addition to the principle of general covariance, which in itself functions as a kind of meta-principle, they assumed the (generally covariant) Einstein field equation, which does impose more definite constraints on spacetime than merely capturing "the objective spacetime coincidences," and which constitutes a physical relation that is unchanged by the arbitrary change of coordinates. Thus the accounts of general covariance and of point-coincidences suggest a radical "geometrical relativity of space," while the field equation saves the theory from physical vacuousness. In that case, however, the motivation for the field equation becomes a serious philosophical question. Einstein's explicit philosophical starting-points—Mach's principle, and the identification of general covariance with the "general relativity" of motion and the equivalence of all frames of reference—motivate, at most, a framework in which any Riemannian geometry is physically possible, and the homogeneous geometries of Newton and Minkowski appear to be relatively naive physical idealizations. This framework may perhaps raise the expectation of spacetime curvature, and it unquestionably played an important psychological and heuristic role in Einstein's thought. It is a great leap, however, from such a general expectation to the theory of spacetime curvature as a physical quantity that depends on physical conditions. As Friedman (2002) points out, a crucial link was Einstein's analysis of geometrical measurement on a rotating disc (cf. Einstein 1916, pp. 115–117), which provided his first glimpse of non-Euclidean geometry as a way of modelling a physical field, and the first step toward a theory of non-Euclidean spacetime. But more than this is required to provide the basis for a constructive theory of spacetime geometry, in which curvature plays the role of the gravitational field.

To the positivists, what was required was a stipulation—again, a stipulation that the spacetime geodesics are the paths of falling bodies. And we have already seen why this would have seemed plausible: not only because of the reduction of the objective content of geometry to coincidences, but because the identification of free-fall trajectories as geodesics seems to be a clear case of a coordinative definition. But we are now in a position to understand the origins of this definition in a conceptual analysis. The analysis starts from the empirical equivalence of inertial and gravitational mass, and the consequent indistinguishability of inertial motion from free fall.[12] In Einstein's well-known example, a frame of reference at rest in a homogeneous gravitational field, with gravitational acceleration **g**, is observationally indistinguishable from a frame with uniform acceleration **-g**; for the same reason, a freely-falling frame is indistinguishable from an inertial frame. It is also well known that in Einstein's initial analysis, this indistinguishability was taken to indicate the physical equivalence not only of freely-falling and inertial frames, but of any frames whatsoever, and of all states of motion. To derive from this apparently destructive analysis a constructive basis for spacetime geometry, we have to see that it defines, in spite of the apparent arbitrariness, an objective physical quantity. From there we would see why the interpretation of gravitational free-fall as a privileged state of motion, and the trajectories of falling bodies as the constructive basis for spacetime geometry, is not a convention, but the outcome of an analysis of what is implicit in our knowledge of gravitational fields. For it follows from the equivalence principle that no *actual* measurement of gravitational acceleration is ever a measurement of deviation from a flat-spacetime geodesic—that is, the measured quantity is never absolute acceleration in Newton's sense, but the relative acceleration of free-fall trajectories. If, after the example of Minkowski's analysis of special relativity, we now ask what structure is exhibited by these trajectories, we naturally arrive at a spacetime whose curvature varies with the distribution of mass.

This last inference may sound like a drastic oversimplification, but it is in fact a paraphrase of Einstein's actual procedure in moving from "the general theory of relativity" as articulated in the first three sections of Einstein (1916)—in which spacetime is assumed to be locally Minkowskian, but otherwise open to arbitrary choices of reference-frame—to the theory of spacetime curvature as the objective expression of gravitational phenomena. For the first link between the generally covariant formalism and the physics of gravitation is the "equation of the geodetic line" (pp. 131–32): mathematically, it is independent of the choice of coordinates, and its physical correlate is the privileged state of motion of a particle, namely gravitational free-fall. From this perspective, the Newtonian account of free fall as "forced" deviation from geodesic motion turns out to depend precisely on the arbitrary choice of a coordinate system. For when we measure the acceleration of a falling body relative to a supposed inertial frame, such as the centre of mass frame of some system, we have no way to determine whether that frame itself is in free fall or inertial motion, since, by the equivalence principle, the system will behave in the same way in either case. What we actually measure, again, is merely the acceleration of the free-fall trajectories within the frame relative to the free-fall trajectory of the frame itself. Therefore to interpret the former as measures of "the gravitational field" is to make an arbitrary stipulation that the centre of mass follows a geodesic—a stipulation that manifestly amounts to a mere choice of coordinates; if the geodesic motions are to be objectively identified, and not merely stipulated, the free-fall trajectories are the only possible ones. But these trajectories have relative accelerations, and the relative acceleration of geodesics is a defining characteristic of curved spacetime.

One could make the same point by arguing from the Newtonian field equation (the Poisson equation), which relates gravitational acceleration to the distribution of mass. In principle we could learn the absolute magnitude of the gravitational potential by applying this equation; in fact, however, what we actually measure is only the relative acceleration of free-fall trajectories, or the gravitational tidal field, which is independent of the free-fall motion of the entire system—a fact already exploited by Newton's analysis of the gravitating system of Jupiter and its moons, whose interactions are (practically) independent of the system's free-fall toward the Sun. It follows that the gravitational potential itself depends on the arbitrary designation of some freely-falling frame as an inertial frame. By analogy to the argument about geodesic motion, if we seek to replace the coordinate-dependent "absolute" acceleration, in the field equation, with an objectively measurable quantity, we require a structure that simply represents the tidal accelerations themselves, without the arbitrary assumption of an inertial frame relative to which their true magnitudes are known. The identification of free fall as geodesic motion enables us to identify the required structure as the curvature of spacetime.

The foregoing helps to clarify Einstein's use of the principle of general covariance: hidden behind its destructive use to eliminate all objective spatiotemporal distinctions, we find a constructive conceptual analysis, showing that in our empirical knowledge of the gravitational field, there is an implicit distinction between objective physical quantities and coordinate-dependent quantities. But this distinction is not reflected in the spatiotemporal framework in which we ordinarily understand the field. In particular, the Newtonian definition of geodesic motion is embodied in Newton's laws, but is not really in use in the analysis of any gravitating system; any such analysis implicitly uses the definition of privileged trajectory, and of privileged frame of reference, that Einstein derives from the equivalence principle. In some sense this was acknowledged by Newton himself in Corollary VI to the laws of motion, which states that a uniformly accelerated system of bodies will be indistinguishable from one in uniform non-rotational motion; this enabled him not only to treat Jupiter and its moons as an isolated system, but also to treat the solar system itself as isolated, and to neglect any parallel acceleration of the whole by some other unknown forces, since by such forces "no change would happen in the situation of the planets to one another, nor any sensible effect follow" (1729, p. 558). In hindsight, this reasoning leads to a geometrical reformulation of Newtonian gravity, since it shows that the reasons to identify the gravitational field with the (curved) affine structure of spacetime are independent of the transition from Galilean to Lorentz invariance.

To compare Newton's and Einstein's points of view, we should note that, analogously to their definitions of simultaneity, their respective definitions of geodesic arise from two ways of abstracting from the empirical conception of gravitational force. The empirical problem is to decompose the acceleration of any body into components, one for each of the action-reaction pairs of which the body is a member; this is Newton's precise and general form of Galileo's analysis of inertial and projectile motion (which was too closely bound to the reference frame of the earth). In actual cases, however, this decomposition fails to identify the inertial component of the body's motion; for example, it identifies not the inertial component of a planet's motion, but the component that is uniform with respect to centre of mass of the solar system, which may itself be freely falling. Of course, by Newton's third law, if the solar system is falling, then it belongs to some larger interacting system; if that system is falling, it must belong to some still larger system; and so on. Thus Newton's conception of an inertial frame abstracts from this infinite regress, conceptually separating the process of decomposition from all finite physical systems in which it might conceivably be carried out. This amounts to supposing that, in principle, all the contents of the universe might be included in one interacting system, and an acceleration of the centre of mass of the system would then be excluded by the laws of motion. Einstein's abstraction, instead, identifies the *actual* process of decomposition as the definitive one: analogously to his method of determining simultaneity, it is this procedure that is the same for every actual physical system. And by this means he identifies the "local" inertial component as definitively inertial—that is, the free-fall trajectories as the genuinely inertial trajectories.

It would seem, then, that Einstein's conceptual analysis of gravity and inertia is one that Newton might already have undertaken, given the right mathematical tools. Yet on a closer consideration, we can see that the analysis is essentially contingent on the subsequent evolution of physics, and in particular on the state of electrodynamics in Einstein's time. From Newton's perspective, the indistinguishability of inertial motion and free fall does not necessarily undermine the global determination of spacetime geometry, because his theory of gravitation has no particular implications for the behaviour of light: to the extent that the mass of light is unknown, it is unclear whether optical phenomena ought to be subject to Corollary VI. This is why for Einstein's theory, it is crucially important to have shown that *no* phenomena, mechanical or electrodynamical, behave differently in a freely-falling frame and in a Lorentz frame, and therefore there are no phenomena that provide a measure of the relative acceleration of the one frame and the other.[13] And this is the deeper significance of the celebrated light-deflection observations, which supported Einstein's inclusion of light among the phenomena governed by the equivalence principle. Newton might hold out the possibility that some optical, or other non-gravitational, effect could provide a constructive basis for flat spacetime geometry, distinct from the implicit geometry of free-fall trajectories (equivalently, for an inertial frame as distinct from a freely-falling frame); in this context, one might even be able to make sense of the conventionalist claim that there is a free choice between two adequate constructive bases for spacetime geometry. Given Einstein's extended principle of equivalence, however, any physical procedure for identifying an inertial trajectory (or a Lorentz frame) must fail to distinguish it from a free-fall trajectory (or a freely-falling frame). Moreover, we can see from this argument why the status of Einstein's analysis remains contingent on the future development of physics: it leaves open the possibility that, at higher levels of precision or in novel experimental contexts (for example, those connected with quantum effects in gravitational fields), violations of the Einstein equivalence principle could weaken the grounds for identifying gravity with spacetime geometry.

4. Conclusion.

In all of these historical cases, we find that the constitutive principles of spacetime geometry are definitions of a sort; more precisely, they are interpretive claims rather than empirical claims, for they propose that certain characteristic physical phenomena be interpreted through certain geometrical structures. Yet these definitions are in no sense mere conventions. Instead, each arises from a conceptual analysis of procedures of spatiotemporal measurement; in each case the definition is not chosen from among equivalent alternatives, but discovered to be implicit in current empirical principles at a critical moment in the history of physics. The logical positivists, as we have seen, had difficulty reconciling the interpretive aspect of Einstein's principles with their constructive physical content. In Einstein's conceptual analyses, these aspects are not only compatible, but inseparable. Moreover, the analyses justify the positivists' sense that relativity represented a philosophical advance in our understanding of space and time, and not merely a set of new physical hypotheses.

But they do so without appealing to narrow and unrealistic epistemological restrictions. Probably no general methodological rule can be given for deciding when enlarged empirical knowledge should lead to a re-evaluation of fundamental concepts; the disappointed expectation of such a rule is perhaps a major psychological motivation for the view that conceptual change is rationally inexplicable. But the kind of change introduced by Galileo, Newton, and Einstein arises from a critical conceptual engagement with an existing framework, an engagement that is philosophically comprehensible, but not as an instance of any methodological maxim.

The great conceptual changes in spacetime theory thus vindicate, in a narrow sense, a central philosophical theme of logical positivism: that physical geometry constitutes a framework that makes ordinary empirical arguments and measurements possible, and therefore that arguments for the framework itself must be of a fundamentally different kind. To this narrow extent, moreover, the history of spacetime theory vindicates the positivists' neo-Kantian association of space and time with the general problem of a priori knowledge. Their account of the a priori aspect of physical geometry improved upon Kant's, at least, by recognizing the connection between geometrical postulates and physical hypotheses, and the interpretive character of the postulates. The positivists had learned, in other words, that the empirical content of the postulates derives from the physics of motion, while in form the postulates are more like analytic or meaning-constitutive principles than synthetic principles in Kant's sense. And this reconciled their brand of apriorism with the historical contingency and mutability of geometry in a way that was impossible for Kant's. Einstein's theories, in particular, seemed to exemplify the idea that empirical geometry has the status of an a priori framework: first, because they seemed to arise from analysis rather than from ordinary empirical inference, and second, because they present spacetime structure as a background against which the forces of nature are defined and investigated. (Even in the case of general relativity, in which spacetime is no longer prior to gravity, a form of Poincaré's hierarchy survives insofar as all *non-gravitational* interactions are defined with respect to the local Minkowski metric.)

The positivists were not completely misguided, therefore, in thinking that they had captured the Kantian idea of space and time as "conditions of the possibility" of ordinary empirical reasoning, while incorporating the insights of Helmholtz and Poincaré into the empirical origins of geometry. Like Kant in the 18th-century context, they were in a position to transcend metaphysical disputes about space and time—such as whether they are "substantival" or "relational"—by showing how spatiotemporal structures are *presupposed* by our usual reasoning about substances and their relations. By embracing conventionalism, however, the positivists went beyond acknowledging the distinction between a spatiotemporal framework and the kind of empirical reasoning that is possible within it; they claimed to remove the defining principles of the framework from theoretical reasoning altogether. This result would not arise for Kant, because he identified one particular framework as the sufficient *and necessary* condition of scientific reasoning. But by end of the 19th century, the multiplicity of possible frameworks was an obvious fact. For the positivists, this fact had obvious implications for the Kantian idea of objective knowledge, as judgment conforming to the conditions of the possibility of experience. For it implied not only the "relativized a priori"—i.e., the contingency and mutability of the framework-constituting principles—but also the relativity of objective knowledge itself, as something defined only with respect to a set of arbitrary stipulations. Carnap's distinction between internal and external questions merely expressed this relativity in its starkest form: for the comparison among frameworks one had, at best, a neutral descriptive language rather than a theoretical or critical standpoint, so that the adoption of any framework was necessarily a matter of pragmatic decision. In the dispute about abstract entities in the foundations of mathematics (cf. Carnap 1956), Carnap's distinction may appear to play a modest clarificatory role; in the dispute between competing theories of spacetime geometry, it seems to deny that there is any serious issue concerning the structure of the physical world.

By appreciating the role of conceptual analysis in conceptual change, we arrive at a subtler distinction than Carnap's: between questions that take a particular framework for granted, and questions about the conceptual structure of the framework itself—in particular, questions about the relation between the explicit principles of the framework, on the one hand, and the concepts that are implicit in our empirical knowledge, on the other. Questions of the first kind may be internal, but questions of the second kind are not really external in Carnap's sense. On Carnap's view, for example, the question whether falling bodies follow spacetime geodesics is either an internal question about how geodesics are defined within a given spacetime theory, in which case it is answered by internal logical analysis, or a question about the expediency of adopting a framework that defines geodesics as the paths of falling bodies. But neither question could have motivated Einstein's analysis of gravitation. Both the internal and the external questions take the competing frameworks as given, whereas the framework of curved spacetime is precisely the *product* of Einstein's analysis; the given material for the analysis is only the known behaviour of falling bodies as understood *within* the flat-spacetime framework. Carnap's distinction, in other words, does not comprehend the possibility of a conceptual analysis that discovers, within a given framework, the principle on which a radically new framework can be constructed.

The failure to comprehend this possibility epitomizes the failure of conventionalism as a critique of the synthetic a priori. The conventionalists supposed that objective reasoning is either logical analysis of the structure of a framework—including the identification of its arbitrary stipulations—or empirical reasoning within the constraints of a framework. Conventionalism thus did not go beyond or even reject Kant's notion of the synthetic a priori, but merely denied that any of our knowledge answers to that notion, and classified framework-constitutive principles as analytic. But the essential problem for Kant's notion, in light of the developments in physical geometry over the last two centuries, was not the discovery that alternative frameworks could be arbitrarily defined and freely adopted. Rather, it was the realization—implicit already in Galileo and Newton, and clearly articulated by Helmholtz, Riemann, and Einstein—that the constitutive principles of physical geometry are not quite synthetic in Kant's sense, and yet they are founded in our empirical knowledge of physics; the revolutionary changes in these principles were in some sense changes of definition, and yet they express a deepening understanding of physical space and time. These aspects of the framework-constitutive principles help to explain Quine's objections to calling them "true by convention." Comprehending all of these aspects requires a subtler view of conceptual analysis than conventionalism allows, and a role for analysis beyond the logical reconstruction of existing theories. Above all, it requires an appreciation of the continuing interaction between conceptual analysis and the growth of empirical knowledge, and the decisive part that such interaction has played in the evolution of physics.

Notes

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[1] See, for example, Putnam 1974, Glymour 1977, and Norton 1995.

[2] See Friedman, 1999c. For the sake of convenience, I follow Friedman in writing "the positivists" to refer, primarily, to Schlick, Reichenbach, and Carnap.

[3] Reichenbach's discussion of universal forces was endorsed by Carnap (cf. 1966, p. 171), but subsequently has been widely criticized; see, e.g., Torretti (1983) for an especially useful analysis. Still it should be recognized that, as an effort to judge possible coordinative definitions on methodological grounds—by proposing to eliminate hypothetical and undetectable "forces" that might be introduced in order to save a particular geometry—Reichenbach's discussion anticipates some celebrated later attacks on conventionalism, such as Glymour (1977).

[4] Cf. Friedman (1999b). According to Friedman, Reichenbach's earlier position on the a priori constitutive principles of physics, in his 1920 work *The Theory of Relativity and A Priori Knowledge* (1965), was an insightful one that was largely obscured by his attempt to assimilate it to the conventionalism of Schlick in the 1927 work, *The Philosophy of Space and Time* (1957). Admitting the justice of this criticism, I suggest that Reichenbach recast his constitutive principles as conventions because he realized that such principles have a definitional aspect, even if they also appear to be suggested by the facts: "It is again a matter of fact that our world admits of a simple definition of congruence because of the factual relations holding for the behaviour of rigid rods; but this fact does not deprive the simple definition of its definitional character" (1957, p. 17). But he did not arrive at a characterization of those principles that would do justice both to their definitional character and to his earlier philosophical concerns. On the one hand, he came to share Schick's view that these definitions are arbitrary; on the other hand, he continued to criticize Poincaré for "overlook[ing] the possibility of making objective statements about real space" (1957, p. 36 n.3).

[5] See, for example, Schlick 1917, section VI, and Reichenbach 1957, section 36.

[6] This illustrates Torretti's conclusion that incommensurability is not a serious difficulty in a case where the new conceptual framework arises from conceptual criticism of the old, as does Galileo's in relation to Aristotle (1989, section 2.5). It may also illustrate what Stein means by the "dialectic" of science, as a criticism of Carnap's view that conceptual frameworks can be compared only on general pragmatic grounds (1992, pp. 291–292).

[7] It is not generally acknowledged, in the literature on the "absolute-relational" controversy, that the familiar "indiscernibility" arguments against absolute space (as first presented by Leibniz) are, like Poincaré's, arguments from the homogeneity and isotropy of *space*; therefore, they are as irrelevant as Poincaré's arguments to the *spatiotemporal* structure identified by Newton as "absolute space." The confusion arises from the failure to separate the question whether the structure of space allows for a distinguished position, and the question whether the structure of space-time allows for a distinguished velocity.

[8] This is documented in detail by Stein (1967).

[9] Newton refers directly to the Cartesians' use of the centrifugal forces in vortices in the causal explanation of planetary motion, and points out its implicit accord with his definition: "Thus, even in the system of those who hold that our heavens revolve below the heavens of the fixed stars and carry the planets around with them, the individual parts of the heavens, and the planets that are relatively at rest in the heavens to which they belong, are truly in motion. For they change their positions relative to one another (which is not the case with things that are truly at rest), and as they are carried around together with the heavens, they participate in the motions of the heavens and, being parts of revolving wholes, endeavour to recede from the axes of those wholes" (1726, p. 413). In his unpublished manuscript, "*De gravitatione et aequipondio fluidorum*," he explicitly notes the discrepancy between Descartes's relativistic definition of "motion in the philosophical sense," and his use of "motion in the vulgar" sense for actual philosophical (i.e. physical) reasoning: "And since the whirling of the comet around the Sun in his philosophical sense does not cause a tendency to recede from the center, which a gyration in the vulgar sense can do, surely motion in the vulgar sense should be acknowledged, rather than the philosophical" (Hall and Hall, p. 125). This interpretation of Newton is documented at greater length in DiSalle (2002).

[10] James Thomson articulated the connection between the assumption of absolute simultaneity and the measurement of length and time, in introducing the notion of (what we now call) an inertial frame in Newtonian mechanics (1884). See also Torretti (1983, pp. 52–53).

[11] The qualification is required for the case of general relativity, in which the laws fix not the geometry itself, but the correspondence between the geometry and the distribution of matter.

[12] For useful studies of Einstein's use of the equivalence principle, see Torretti (1983, chapter 5.2), and Norton (1985).

[13] Cf. Einstein: "But this view of ours [i.e. of the equivalence of a system **K** at rest in a homogeneous gravitational field, and a system **K′** that is uniformly accelerating] will not have any deeper significance unless the systems **K** and **K′** are equivalent with respect to all physical processes, that is, unless the laws of nature with respect to **K** are in entire agreement with those with respect to **K′**" (1911, p. 101).

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