Essay Review: *Physical Relativity* and *Understanding Space-Time**

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The two books discussed here make important contributions to our understanding of the role of spacetime concepts in physical theories and how that understanding has changed during the evolution of physics. Both emphasize what can be called a 'dynamical' account, according to which geometric structures should be understood in terms of their roles in the laws governing matter and force. I explore how the books contribute to such a project; while generally sympathetic, I offer criticisms of some historical claims concerning Newton, and argue that the dynamical account does not undercut ontological issues as the books claim.

Harvey R. Brown, ***Physical Relativity: Space-Time Structure from a Dynamical Perspective***. Oxford: Oxford University Press (2005), 240 pp., $75.00 (cloth).

Robert DiSalle, ***Understanding Space-Time: The Philosophical Developments of Physics from Newton to Einstein***. Cambridge: Cambridge University Press (2006), 188 pp., $90.00 (cloth).

1. The Dynamical Interpretation of Spacetime. In broad terms, both of these provocative, informative, and significant books are philosophical histories of relativity—particularly of the special theory (STR). *Physical Relativity* (*PR*), corecipient of the 2006 Lakatos Award, traces this history from nineteenth-century attempts to measure motion through the ether, and the efforts of Heaviside, Poincaré, and Lorentz to understand their null results. *Understanding Space-Time* (*US*) covers a longer period, start-

*Received January 2009; revised March 2009.

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Philosophy of Science, 76 (July 2009) pp. 404–422. 0031-8248/2009/7603-0009$10.00
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https://doi.org/10.1086/649814 Published online by Cambridge University Press

ing with Newton's views. While these histories are put to rather different philosophical ends, there are nevertheless considerable similarities in the views of space and time contained in the books.

PR is a defense of an account of spacetime that, inspired by its subtitle, we can call a 'dynamical interpretation'. Brown not only argues for such an interpretation, but also that similar views were held by important figures in the development of our understanding of relativity—notably, Lorentz, Einstein, Weyl, and Bell. We'll explore this kind of view in more detail as we go along, but the basic principle is that *features of spacetime are understood to be fundamentally features of the dynamical laws*. As we shall see, such a principle can be cashed out in a variety of (possibly conflicting) ways, but for instance: one might argue that spatial coordinates are not to be understood in terms of strictly rigid bodies, but should be referred to rods bound by atomic forces; or that inertia and the relativity of simultaneity are not to be explained in terms of the geometry of spacetime, but reflect features of mechanical laws. Clearly a range of issues can fall under this approach, and it is important for understanding *PR* that the reader appreciates that they are unified by this rather general principle; otherwise, the diverse topics tackled are liable to seem disconnected. One aim of this review is to contribute to such appreciation.

The goal of *US*, on the other hand, is not to offer a philosophy of spacetime. Instead it aims to show how physics and philosophy can have a substantive, 'dialectical engagement': how the philosophical analysis of key concepts plays a crucial role in rational, scientific progress during times of fundamental upheaval in science—in particular, the mechanical revolution, the discovery of non-Euclidean geometry, and the development of relativity. DiSalle argues that this analysis is needed to define new spatiotemporal concepts, and that such definitions are in terms of the dynamics of emerging theories. But that is broadly how Brown argues we should understand spatiotemporal concepts, and so the interpretations of spacetime that DiSalle finds in his history have important affinities with Brown's dynamical interpretation. (While *US* is not intended to provide any interpretation of spacetime, the figures it investigates do, and it is apparent in his other writings that DiSalle is sympathetic to their views as he sees them: a dynamical interpretation.)

One way that Brown and DiSalle also express affinity is in their rejections of canonical philosophical approaches to spacetime. On the one side there is the positivistic view of theories as uninterpreted formal systems in need of coordinative definitions of their spatiotemporal concepts. In such an approach the role of the laws in understanding the concepts is missed, contrary to the dynamical interpretation; while the ahistorical way in which the problem is posed leaves no room for 'dialectical engagement' between physics and philosophy. On the other side, there is the

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postpostivistic view in which interpretation is a matter of more-or-less direct realism about our best scientific theories. This approach led to the substantival account of spacetime geometry, to which the dynamical interpretation is opposed; for DiSalle, the problem is that the approach means that physics is treated simply as the arbiter of preexisting philosophical disputes, rather than engaging with it.

The scheme of the rest of this essay is to consider how Brown and DiSalle address some common issues, to make some criticisms of their conclusions, and in particular to argue that familiar issues in postpositivistic philosophy of spacetime cannot be swept aside quite as neatly as these works hope.

2. True Motion.

2.1. Inertia and Affine Geometry. Consider the issues surrounding the nature of motion in mechanics: the fact that mechanics (Newtonian or relativistic) distinguishes different states of motion, inertial and not, and what that might show about spacetime.

Brown rejects the idea that the affine geometry of spacetime—as opposed to mechanical law—plays an explanatory role in mechanics in any literal sense. Specifically, he rejects Nerlich's claim that bodies move the way that they do *because* of the geodesics of a substantival spacetime (*PR*, 24, 141–142). According to Brown, prior to general relativity (GTR) the correlations between the motions of free bodies are a 'miracle', but in GTR it follows from the Einstein-Hilbert field equations that bodies in freefall follow geodesics (e.g., the particles of a cloud of noninteracting dust). That is, the trajectories of inertially moving bodies have an explanation in terms of the dynamical laws of GTR. However, calling the First Law a 'miracle' seems unfortunate, since any explanation would be better, even Nerlich's. Instead, why not say that explanations end somewhere, and that place is the laws—including the First Law—subject to advances in physics? Thus both before and after relativity it is the laws not spacetime that explain inertial motion.

And the same points hold for inertial effects. An 'abductive' argument for substantivalism moves from the premise that spacetime geometry is a component of the best explanation of inertial effects, such as Newton's bucket, to the conclusion that the geometry is real (e.g., Sklar 1977, 226–269, though Sklar ultimately rejects this line of thought). Instead, surely it is the forces between bodies, and the laws that govern them that explain what we see in the bucket—with standard idealizations to take account of the fact that classical mechanics cannot accommodate the existence of fluids at the molecular level—and so on (such a response is argued for at

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greater length in DiSalle 1994 and in Huggett 1999). Again, it is the dynamics to which we appeal, not the geometry of a substantial spacetime.

But explanation is only one issue: even if the abductive arguments fail, there is still the question of whether a substantial geometry is required to make well-defined the motive quantities of acceleration and rotation involved in the laws. For instance, I would read Newton's *Scholium* (to the *Definitions*) in this way (following Rynasiewicz 1995). Descartes' definitions of motion in *Principles of Philosophy* 2:24–25 are incompatible both with intuitive properties of motion and with the causes and effects of motion according to the mechanics emerging in the work of Galileo, Descartes, Huygens and Newton—incompatible, that is, with 'true motion'. However, motion with respect to absolute space—that is, 'absolute motion'—is compatible with all the properties, causes and effects of true motion; hence, by disjunctive syllogism, true motion is absolute motion, and space is absolute. More generally, something must distinguish states of different acceleration, and what else if not the geometry of spacetime itself?

The alternative was first clearly formulated at the end of the nineteenth century by James Thomson (brother of Lord Kelvin) and others (*US*, 100): 'true' motion is motion in spatiotemporal coordinates in which the laws of motion hold—the inertial frames.[1] Since Thomson's proposal is that the frames are picked out by the dynamical laws—specifically that inertial frames are those in which Newton's Second Law holds (Thomson 1884, 576)—it is in our sense a dynamical account of the inertial structure of spacetime.

Again, it is not DiSalle's aim here to defend an interpretation of spacetime, though elsewhere (e.g., DiSalle 1994) he endorses a dynamical approach (and in *US*, too, his sympathies feel pretty obvious). DiSalle views this account as a position between substantivalism and relationism. First, there is no literal spacetime geometry; that only represents the symmetries of the laws. Second, there is more to motion than relative motion, for the laws themselves pick out preferred measures: specifically, acceleration and rotation in inertial frames.

However, such a view collapses into relationism—understood as the view that the only fundamental spatiotemporal quantities are the relations between bodies—if the laws themselves are reducible to relations. In Huggett 2006, I endorse a Thomson-like proposal, but argue further that the laws of mechanics are reducible to relations, in the sense that the laws

1. The absence of Thomson—and similiar work by Lange—from the philosophical literature is an embarrassment (with notable exceptions, e.g., Barbour 1989 and DiSalle 1991). Thomson's idea effectively first appears in the recent philosophical literature in van Fraassen 1970, 116.

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are the simplest-strongest axiomatization—a 'best system'—of the regularities in relative motions. DiSalle can only maintain a position between substantivalism and relationism if, for instance, he holds that the laws are unanalyzable natural necessities or Armstrong-Dretske relations between universals. I would say that DiSalle would thereby pay too high a metaphysical price, for the usual reasons one would want to endorse a regularity account of laws. Those arguments are well-rehersed in the literature, so I won't dwell on them. The point that I wish to urge here is that DiSalle has not found a way to avoid metaphysical issues, or to place the debate in squarely 'scientific', empirical terms—for the stability of his account relies on a particular metaphysical view of laws. (Since Brown too wishes to make the laws the locus of philosophical debate, similar remarks apply to him.) Of course this result is to be expected: if the laws are to carry the weight of interpretation, it's going to make a difference what view of laws you hold.

That said, an important contribution of these books is to emphasize that the question of the role of geometry in mechanics can be separated from more familiar questions about spacetime ontology. The two issues are connected—a more fundamental, irreducible role for geometry fosters substantivalism—but the former is significant in its own right. Moreover, *US* in particular shows that it is that question, not the ontological one, which has been most important in the development of physics.

2.2. Newton. I want to take issue with DiSalle's treatment of Newton on the nature of motion, which reads him as going a long way in the direction of Thomson. According to *US*, Section 2.5, Newton's arguments in the *Scholium* amount to a *definition* by conceptual analysis of absolute motion. The kind of definition DiSalle has in mind here is more like that of a linguist than that of a logician introducing a new term into a formal language: Newton explicates how natural philosophers have come to use 'motion' in their applications of mechanics, rather than giving an independent meaning in terms of which laws can then be formulated.

Now, in his arguments Newton appeals to the 'properties, causes and effects' of motion. The properties are pretheoretical and, according to DiSalle, thus fail to give satisfactory definitions: for instance, 'bodies truly at rest are at relative rest'. However, he views Newton's definitions in terms of causes and effects (i.e., forces, and inertial effects in rotational motion) as more successful. Specifically, *US* claims that the result of Newton's analysis of how the concepts of motion are used in the mechanics of impelled and rotating bodies is that

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i) "Newton defines true motion as that which cannot change without the action of a force, and which must change when a force is applied" (*US*, 31).
ii) Similarly, "true rotations are by definition those that give rise to centrifugal effects" (*US*, 35).

In other words, motion is again to be defined dynamically.

At stake here are both Newton's metaphysical views regarding absolute space in the *Scholium* and the overall logic of the text. In canonical readings, Newton postulates absolute space as a *subsistent* background frame relative to which 'true' motion is taken. Interpretations differ on the role of Newton's following discussion: does Newton fallaciously claim that the convex surface of the water in his bucket can only be correlated with motion relative to absolute space (as positivistic critics allege)? Or, is absolute space required by the best explanation of the surface (as substantivalist proponents urge)?

According to DiSalle, when Newton 'distinguishes' absolute motion from relative he is not defending some prior understanding of motion at all; certainly not that of motion with respect to subsistent space. Instead, Newton explicates the meaning of 'motion' in mechanics: in the 'true, philosophical and mathematical' sense. The proposed logic of the *Scholium* is completely different from the other accounts—and in particular Newton eschews metaphysical claims about space altogether.

However, I do not think that DiSalle's reading can be sustained. In the first place, according to DiSalle, Newton's discussion of the properties of true motion turns out to be a flawed attempt at a definition. While this suggestion is an improvement over accounts that simply ignore those passages, charity would prefer a reading that sees them as more satisfactory. Second, there is the fact that i and ii aren't equivalent to Thomson's definition, for they do not *quantify* acceleration: to say forces act is not to say what the acceleration is. Of course, one could give related definitions that do quantify motion (e.g., 'the acceleration is proportional to the force and inversely to the mass'), but the fact is that in the *Scholium* Newton only quantifies rotation (by centrifugal effects): Newton does not give a quantitative version of i. Since mechanics requires a quantitative conception of motion, we should prefer a reading that doesn't have Newton giving an obviously inadequate definition. Finally, we have to square our reading with Newton's claim that "absolute motion is the change of position of a body from one [part of absolute space] to another" ([1687] 1999, 409). But as Newton knew full well, i and ii are not an adequate explication of such absolute motion because the causes and effects of motion do not determine the rate at which a body passes through absolute

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space. Indeed, under DiSalle's interpretation Newton's invocation of absolute space in the *Scholium* is quite mysterious.

DiSalle is right to reject the positivistic and substantival readings of Newton's argument—they also sit poorly with the text. Rynasiewicz's (1995) reading addresses all the concerns raised above.[2] The arguments from properties, causes, and effects are of a piece, showing the incompatibility of Descartes' conception of motion with a variety of widely agreed principles, and the compatibility of motion with respect to absolute space. (DiSalle does recognize that the arguments address Descartes: I object to taking them also as attempts at definition.) I would emphasize that 'absolute motion' *is* defined in the *Scholium,* as motion with respect to absolute space (quantified by the metrical properties of space and time). While Newton does not dwell on the metaphysical aspects of absolute space in the *Scholium* (compared to *De Gravitatione*, for instance), it surely is his intention that absolute motion is defined to be motion with respect to it. And while Newton discusses—at length—the conception of motion implicit in dynamics, he does not offer a dynamical *definition*, but instead argues that such motion should be *identified* with absolute motion, as he defined that concept.

3. Conventionalism. In the not so recent past, the issue of geometric conventionalism might have been sunk and forgotten, but in the recent literature, and especially in these two books, the issue has resurfaced. Though they both discuss conventionalism at length, Brown and DiSalle have quite different purposes. The former urges conventionalism against the priority of geometry over dynamics—unsuccessfully, I will argue. The latter places conventionalism in the history of attempts to understand scientific definitions.

3.1. Conventionalism versus Geometric Literalism. Brown engages first-order issues, arguing for certain conventions, especially that of simultaneity: the convention of instantaneous force propagation in Newtonian mechanics (*PR*, Section 2.3) and of the isotropic speed of light in STR (*PR*, Section 6.3). Now of course we are logically free to coordinatize as we please, and so we can assign, in principle, the same 'time' coordinate to any pair of points we wish. Indeed, in the sense that coordinates are just labels for points, we could attach absolutely any numbers to any points we liked. At certain points (e.g., *PR*, 20, 97) Brown seems to mean nothing more by 'convention', but surely this sense has little philosophical import.

2. DiSalle's failure to discuss Rynasiewicz is the one real slip in an otherwise well-sourced work.

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A more weighty issue that motivates conventionalism is that of the status of spacetime geometry. The realist-minded about geometry will evaluate different choices of coordinates according to how well they express the geometric properties of the spacetime manifold. Of course, even if the manifold were a substance, with intrinsic geometric structure, then we could still assign coordinates as we chose without affronting logic; but if there are intrinsic facts of the matter about the geometry of spacetime then some coordinates are 'better' than others. For example, in flat spacetime, coordinates in which geodesics satisfy linear algebraic equations are better than those in which they don't, in the sense that the affine structure is represented by an identifiable (and particularly 'simple') mathematical class. Similarly, coordinates in which the metric takes a simple form, invariant under isometries, are better than those in which it does not—better that is, in the sense of expressing more information about the geometry, namely its symmetries. The point is not that there is a single dimension on which choices of coordinates must be compared; rather, we can meaningfully distinguish cases in which coordinates 'latch on' to the geometry.

I suspect that a denial of law-independent geometric structure is what motivates Brown's conventionalism: if you hold that there are no subsistent geometrical features then no coordinates can be distinguished by latching onto them. But conventionalism would be a misleading way of expressing the ontological priority of laws over geometry because coordinates can latch on to features of the laws just as well as geometry. In good frames in Newtonian physics, points receive the same time coordinate if forces can propagate from one to the other; or, if they lie on a limiting curve of possible worldlines in a given direction. But the laws govern the propagation of forces and possible trajectories (which are of course quite different if the laws are Lorentz invariant, say), so the standard coordinates reflect real features of the laws. The same holds in STR. The laws govern the possible motions of photons, but as Malament (1977) points out, that suffices to define standard relative simultaneity;[3] hence, assigning the same time coordinate to simultaneous points in the standard way reflects a genuine feature of the laws.

It should be no surprise that a dynamical interpretation of spacetime makes no profound difference to questions of conventionalism. Conventionalism is best understood to concern the existence of objective geometric features. The dynamical interpretation doesn't deny their existence and objectivity, just their nature; are they subsistent, or arising from the laws? As a result, Brown's conventionalism seems disconnected from the

3. As do possible motions of massive particles (i.e., the spacelike curves), so the same point holds if the laws do not include electromagnetism.

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other themes of the book and, if taken as a substantial thesis, incompatible with his other views.

Brown does question the uniqueness of Malament's definition (*PR*, 98–102), noting that it utilizes a single worldline to construct a notion of simultaneity. Similarly, Norton (1992, 223–226)[4] points out that additional worldlines will permit the definition of other simultaneity relations—as will the temporal orientation required by the weak interaction (Spirtes 1981, Chapter 6). But objecting on these grounds misses the point; namely that STR—taken either geometrically or dynamically—permits the unique definition of a relation given the stated features, and hence by virtue of that fact alone singles out a class of coordinates. It is of course interesting to know how additional features undermine uniqueness, and such considerations may have bearing on whether the defined relation matches naive or prerelativistic conceptions of simultaneity, but again, none of that is to the point—that standard coordinates do express real features of the physical laws.

3.2. How to Define Theoretical Terms. Conventionalism is also a central topic of *US*, but DiSalle has rather different concerns. His philosophical puzzle is how certain concepts can have an a priori status in physical theories, as givens in terms of which empirical theories can be developed. For instance, Euclidean space and absolute time played such a role in physics prior to relativity, providing the framework in which dynamical theories could be developed. Kant offered the first account of their a priori status, but as a theory of their function in science it was undermined by the discovery of nonEuclidean geometry. Early conventionalists concluded instead that such concepts must be irrevocable stipulations of some kind. Poincaré, for instance, grounded the stipulation in prescientific notions of space: the apparently rigid Euclidean character of the motions of many everyday bodies, for instance. But relativity demonstrated that even though spacetime concepts function as givens within a theory, they are subject to revision in the face of observation and experiment, undermining this conventionalist position. The positivists saw relativity as demonstrating that scientific terms must get their meanings through another kind of convention, the empirical definition of observational terms: Reichenbach's coordinative definitions, for example. But that program is now widely rejected, too; on the one hand the idea of drawing a observable-theoretical distinction in language seems untenable; on the other it makes the meaning of theoretical terms theory-dependent in a way that threatens

4. Note that Norton's statement of Malament's definition is incorrect.

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incommensurability. So how do spatiotemporal (and similar) concepts get stipulated?

US is a history of philosophical attempts to answer this question, in which various forms of conventionalism played a prominent role. But *US* also offers DiSalle's own answer, and in support contains a history of how spacetime concepts, as stipulations, have changed over time. In short, he proposes that spacetime concepts get defined in terms of physical laws: the geometrical properties assigned to space and time are those that can be defined in terms of the dynamical quantities of the theory. For instance, the forces that appear in Newtonian physics allow a definition of absolute simultaneity, while the causal properties of relativistic theories allow only relative simultaneity to be defined—in the ways discussed earlier. And, of course, Thomson showed how to define absolute motion in terms of Newton's laws. Thus DiSalle sees physics as using, in a broad sense, a dynamical interpretation of spacetime quantities.[5]

He is especially concerned to argue that his account allows for theory change without Kuhnian incommensurability. In the positivist picture, theories are uninterpreted calculuses, whose terms get their meaning from empirical definitions and their logical relations. But different theories will have different accounts of what counts as empirical bedrock (the so-called theory ladenness of observation) and of the logical relations of terms; hence the theories will be formulated in languages that cannot be intertranslated (e.g., Kuhn 1962). There are of course postpositivistic responses to Kuhn: for one, the causal theory of reference (oddly neglected in recent philosophy of science) is supposed to ground common referents across theory change. DiSalle offers a different (perhaps complementary) reply: the fundamental terms of a theory acquire new meaning through their *evolving* use as a dynamical theory is developed.

It is anachronistic to think of theory change as a competition between old and new formal systems, rather a new theory develops in steps as new observations and theoretical advances are made. Of course, such advances require new concepts, and revisions to existing ones, and these changes are part and parcel of the step-by-step development of a theory. For DiSalle the development of a new theory can be a rational process because the development of its concepts involves "dialectical engagement with existing ideas, analysis of their presuppositions, and confronting them

5. The title of this section is of course a nod to Lewis 1970. It should be clear that the various theoretical terms here are being defined to refer to that thing which satisfies some Ramsified sentence (involving the laws of mechanics), as Lewis proposed. As he explains, such definitions fail to determine referents if nothing satisfies the Ramsey sentence; hence, they can be 'refuted' if evidence shows that the corresponding theories are false, as DiSalle's view requires.

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with new observation and theory" (*US*, 12): Einstein's analysis of spatio-temporal concepts in post-Maxwell physics is a paradigm example. Far from the image of scientists arguing in incompatible languages, we have them implicitly and explicitly figuring out the inadequacies of existing terms, and introducing new ones as needed, as new theories are developed in response to experiment. DiSalle sees this kind of conceptual analysis as both essential to the rationality of scientific progress, and inherently philosophical—thus he draws the exciting conclusion that philosophical analysis is essential to the progress of science.

In this project DiSalle is closely in step with Friedman's (2001) neo-Kantian program, which also seeks to show how changes in the most fundamental principles of mathematical physics can made by a process of rational deliberation–indeed exactly the process described by DiSalle. (The connection is no coincidence; both authors cite the other's influence.) As such, I find DiSalle's account (Newton aside) to be convincing and, since it is more focused on the history than the general philosophical issues, very useful complement to Friedman's. The two are also in agreement that abstract mathematics and principles of empirical meaning form a relative and revisable, but constitutive a priori. However, Friedman develops this neo-Kantian view further, in particular explicitly opposing it to Quine's holism: the relative a priori is not just a more entrenched set of beliefs, which could be given up to preserve others, but is the framework that makes the others possible. *US* does not address that issue.

4. Special Relativity. Brown's dynamical interpretation of STR opposes several further, interlocked views; consider two. (i) Brown objects to a 'kinematical' argument for relativity, seemingly found in *On the Electrodynamics of Moving Bodies* (Einstein 1905). Einstein invokes strictly rigid bodies and perfect clocks that somehow directly reflect the metrical properties of spacetime, without any consideration of their physical (i.e., dynamical) constitution—they appear outside the laws, and that sense are *miraculous* objects. What sort of role could such things play in a scientific argument (*PR*, Sections 5.1–5.2, 5)? (ii) Brown objects to the idea that any material systems respond directly to spacetime geometry, that the geometry of spacetime *explains* contraction, dilation and the like. He views the issue as a close cousin of his objection to geometric explanations of inertial effects, which we discussed earlier (*PR*, Chapter 8).

Let's take it that the claim of STR is that physics must be formulated in Minkowski spacetime: the laws should be invariant under Lorentz transformations, and the Minkowski metric (and its connection) is the only geometric object that can be used to formulate the laws. Then, according to *PR*, in addressing i we should read Einstein as arguing: 'While we don't know the laws governing the constitution of matter, we do believe

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that they permit bodies and clocks that are approximately rigid and periodic in inertial frames. Moreover, bodies satisfying those laws satisfy the 'Galilean' principle of relativity—identical systems in constant linear relative motion will behave identically—*even if they also include light signals*. Couple those claims with the postulate of the source-independence of the speed of light, and you have the basics of Einstein's argument, leading to STR.

Now it is clear what role the rods and clocks play: stand-ins for unknown physical laws, showing from what is already known by experiment that their laws must satisfy an important constraint. They are dynamical objects after all. Understood this way, STR is what Einstein himself described as a 'principle' theory, not itself truly explanatory, but capturing the basic phenomenological import of a deeper, 'constructive' theory: compare, as Einstein and Brown do, with thermodynamics and statistical mechanics (*PR*, Section 5.2). Brown emphasizes that Einstein understood this character of his argument (though *PR*, Section 7.1 is critical of Einstein for not recognizing later progress in quantum mechanics towards a constructive theory), but claims that his followers often do not.[6]

They fail to appreciate the status of the rods and clocks, but more importantly (ii) they treat STR as a constructive theory, taking Minkowski geometry itself to be explanatory of the characteristic phenomena of the theory, when—as for inertial effects—it is the dynamical laws governing the interactions of matter that do. For example, following Minkowski, one often 'explains' why bodies contract by projecting the world tube of a moving body into different inertial coordinates (*PR*, Section 8.1.1). But that line of thought fails to include the crucial fact that the forces between moving bodies are different from those between similar stationary bodies. (Obviously this story is relative to a given inertial frame: as before, a frame in which the laws hold.) A significant strand of *PR* deals with the history of attempts to understand the effects of motion on forces since the ether experiments. Here I will just mention Brown's discussion (*PR*, Section 7.5) of Bell's (1976) calculation of the classical, Lorentzian orbit

6. In pursuing the current line of discussion, I am neglecting other important threads of *PR*. Brown argues (*PR*, Sections 5.8, 8.4.1) that while the light postulate (and 'Galilean' relativity) play the foundational role in Einstein's original argument (showing Lorentz invariance), in later reflections he denied the significance of light in particular, and took Lorentz invariance itself to be the fundamental principle. The difference is not the possibly trivial one of just what counts as axiom and what as theorem, but in recognizing the surprising phenomenological behavior of light as a specific realization of the Lorentz symmetry of the laws. Since the light postulate, but not Lorentz invariance, is a more-or-less directly phenomenological principle, the later view makes STR not a pure principle theory. However, it is still not a constructive theory in the sense to be criticized here but, according to Brown, lies on the continuum between principle and constructive theories.

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of an electron around a moving charge; of course it undergoes Lorentz contraction.[7] That is not to demonstrate fully that bodies will Lorentz contract, because the model of the relevant forces is so simple, and because it is nonquantum mechanical, but it is to take a step towards an explanation in terms of the laws responsible for the stability of matter.

So, according to *PR* the constructive theory is the account of matter provided by relativistic quantum mechanics. And in that context STR has a rather different import from Einstein's derivation: Lorentz invariance is not a principle constraining as yet unknown laws, but a significant fact about the known laws. STR—like thermodynamics—loses priority to the constructive laws; the reasons for believing it is no longer the direct empirical support, but the evidence for the microlaws.

According to this view, every relativistic phenomenon has an explanation with no direct appeal to geometry: dynamics suffice. Why do the twins age differently? Not because they literally measure the interval along their respective worldlines, but because the laws governing the relevant biological processes in each dictate that on meeting they will be asynchronous. (That result is a consequence of the relativistic laws applied in an inertial frame; even neglecting inertial effects, the asymmetry of course relies on the fact that only one twin move constantly in such a frame.) The situation is parallel to that of inertial effects in Newtonian mechanics discussed earlier; the motion of the water in Newton's bucket can be fully explained by the forces involved and Newton's laws.[8]

At the cost of explanatory overdetermination, if one wishes to preserve a role for geometry in scientific explanation one must either deny that all relativistic phenomena have dynamical explanations, or argue that geometry plays an indirect role—grounding dynamical quantities for example. Alternatively, a thoroughgoing dynamical interpretation of STR will want to deny that geometry has any status whatsoever beyond the laws. By now, the obvious thing for the dynamical interpretation to say (though Brown does not explicitly) is that relativistic laws should be formulated to say 'there are frames in which the laws (e.g., Maxwell's) hold in their canonical form'. There is surely a great deal more that could be said to flesh out the consequences of such an interpretation, but here I will simply make a few remarks.

7. Solving the problem in the rest frame of the charge and then Lorentz transforming to a moving frame makes the problem simpler: Bell undertakes the much harder task of calculating the solution directly.

8. Note that this approach cannot treat the topology of spacetime in a similar fashion. For instance, consider the twins in a locally Minkowski spacetime with one rolled up space dimension. Now they may meet even if each moves inertially, so there is no local asymmetry for the laws to latch onto to explain their eventual asynchrony.

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First, these frames are related by the Lorentz transformations, which, since they correspond to its isometries, makes Minkowski spacetime the natural way of representing relativistic laws. According to a thoroughgoing dynamical interpretation, that is the primary status of geometry. Second, we should be careful to distinguish a pair of claims here: that geometric explanations are unnecessary or even nonexplanatory; and the ontological claim that geometry is only representational. Brown's main claim is for the former, but while less emphatic about the latter he does endorse it in places (as Norton 2008, 822, points out).

Finally, the ontological view faces the question of what the laws are ultimately about; especially what are the fundamental spatiotemporal quantities if not the metric of spacetime? When we say that the laws hold in some frame, what is it that they hold of, and *in virtue of what do we even have a manifold structure to which coordinates can be assigned*? There are two obvious answers, familiar from the literature: spacetime intervals between material points and material point coincidences. It would take considerable work to develop these possibilities, but it is worth making some important comments, focusing on field theories (i.e., on our actual relativistic theories).

The first proposal attempts to update 'Leibnizian' relationism into relativity: instead of spatial and temporal relations, just one relation, the spacetime interval, is taken as basic. However, no dynamical account of the ontology of spacetime geometry is possible in this approach. The problem of course is that the points of the field have exactly all and only the metrical relations of the points of spacetime, and so already possess the full geometry of Minkowski spacetime, leaving the laws with no additional work to do.[9] To see the force of this point, note that the same is not true for Newtonian fields. In that case the timelike affine connection grounds *spatiotemporal* relations between points (as opposed to purely spatial or temporal relations), but that is exactly what is underdetermined by relations—and which of course is the geometric correlate of absolute acceleration. So in that case the laws have a job to do that is not done by the relations, namely singling out what counts as the straight timelike curves; those satisfying linear equations in inertial frames.

What about the second proposal for a spacetime ontology, point coincidences? My observation here is that it is not clear that they suffice. Suppose that the laws state local relations between fields in terms of differential equations. Point coincidences between the fields relate field values at the same place, but we also need the topological structure of

9. What about regions in which the field vanishes? I'm inclined to say that the field is ubiquitous and that zero is just another value. Otherwise, as we shall see below, there is a role for the laws.

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the points of the field to make sense of locality, that is, a sense of which points are local to which. Put another way, we can't have differential equations for the fields unless they have a topology (we reached a similar conclusion, for different reasons, in note 8). One possibility is that unlike geometry, topology is fundamental; a more ambitious approach would understand the topology in terms of point coincidences alone, perhaps as part of a 'best system' for point coincidences (Huggett [2006] understands topology as part of a best system for Leibnizian relations). Whatever the answer, if any, the topology of the fields plus point coincidences do not determine the metrical properties, so there is additional work to be done by the laws, along the lines sketched above.

Whichever view of ontology one takes, Norton (2008, Section 6) objects that the account fails in regions devoid of matter: if the geometry of spacetime merely recapitulates facts about the dynamics of matter, then what if there is no matter? One could respond by restricting the view to physics involving ubiquitous fields, so that there are no empty regions, only regions where the field takes on a zero value. But Norton's objection also seems to neglect the modal force of the dynamical laws: even if a region is empty, there are facts about would matter would do in that region. Why shouldn't that suffice to fix the geometry? Whatever could happen, it would fall under Lorentz invariant laws

Clearly these ontological issues closely parallel those that we discussed earlier, in Newtonian mechanics. For now we have reached the point where we have to ask about the status of the laws: if, for example, we understand them as a Humean 'best system', then the dynamical interpretation of special relativity becomes a form of relationism. The point I wish to make again is that even if Brown (and DiSalle) are correct that the status and role of geometry are important questions in their own right, they still lead to familiar issues of ontology.

5. General Relativity. GTR is of course a theory in which geometry is related to matter by the dynamical laws, drastically changing its role. Unless one adopts a narrow empiricism and holds that only matter (and force) fields are observable, it seems hard to deny that the metric (or '*g*-') field plays as much of an explanatory role as any other; it may be singled out as the only field that appears in every model of the theory, but it plays as much a dynamical role as any. But how should we think of the *g*-field? As intrinstically geometrical, or somehow still getting its status as geometry from the laws?

Brown argues for the latter view (*PR*, Section 9.5). What gives the *g*-field its geometrical character is the *strong equivalence principle* (SEP),

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which entails that all matter fields couple to g as to a metric.[10] Brown argues that the SEP is just a feature of the laws, not something that follows from the g-field's intrinsically geometrical character (i.e., from its literally 'being the metric' in some nondynamical sense). It is worth emphasizing that SEP is a constraint on the dynamical laws of *all* interactions, not something that follows immediately from the field equations; thus one certainly can interpret it as a principle of *dynamics*. The situation is parallel to Brown's dynamical interpretation of STR, which we saw to hold that Lorentz invariance is just a feature of the laws, not a result of their having to conform to a previously given Minkowski spacetime. Thus to say the g-field is the metric is not to explain the SEP but to restate it; though now the SEP appears as much a 'miracle' as the principle of inertia once did; perhaps something to be explained by a deeper theory. To back up this view, Brown refers to interesting variants of GTR in which a second metric tensor, defined in terms of the standard one also appears; it is this less fundamental one that satisfies SEP (Bekenstein 2004). Following Bekenstein, Brown draws the lesson that it is only the way that the g-field couples to matter fields, through the dynamical laws, that gives it geometrical significance.

Briefly, this view does not seem to avoid the same ontological questions that I have pressed all the way through: what basic spatiotemporal facts are the laws about? Point coincidences again seem promising, though now including the g-field.

US declines to offer an interpretation of GTR (though see DiSalle 1995), instead explaining the views of Weyl and Eddington on the significance of the metric (*US*, Section 4.6). Like Brown they emphasize how the geometrical significance of the g-field arises from the behavior of material systems. The sections on GTR deserve careful reading: while they are very accessible and lucid, they are also very deep and repay a second read.

Briefly (and not doing full justice to the text), the key conceptual innovation of the theory is the recognition of the equivalence of free fall and inertial motion *as motion along spacetime geodesics.* Of course the *indistinguishability* of free fall and inertial motion was stated in Corollary 6 of Newton's *Principia,* so recognizing their geometric significance is crucial. From this innovation, one comes to understand variable gravitational 'forces' as variations in curvature, producing diverging and converging geodesics. Of course, GTR incorporates local Minkowski structure, but in the discussion of *US* that fact is secondary; as DiSalle discusses, the lesson of equivalence can equally well be applied to nonrelativistic mechanics, to produce the Newton-Cartan formulation of

10. SEP states that all laws are Lorentz invariant in local inertial frames: it entails that the only connection that appears in such laws is that compatible with g.

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Newtonian gravity. From this point of view, the preceding analysis of the role of geometry in the laws of Newtonian mechanics are conceptual steps to GTR; indeed GTR completes (or perhaps just advances) the integration of spacetime geometry into mechanics. It was another step in the rational development of mathematical physics.

6. Conclusion. This essay has reviewed a number of the issues that come under the general conception of a dynamical interpretation of spacetime. Agree or disagree with Brown's specific conclusions (and mine where they extend or conflict with his), it is almost indisputable that his approach frames the foundational debates in a fresh and fruitful way (though with precursors, notably DiSalle). The question of the role of geometry in physical theories has been subsumed within an ontological debate about spacetime points for twenty years; it's easy to see why, if one assumes there is no geometry without a manifold. *PR* makes a convincing case, by example, that there are really interesting questions about the role of geometry itself (though I have argued that they do not close off ontological questions). I would suggest that they get at more fruitful and significant issues than attempts to capture the debate in terms of the notorious 'hole argument'. (I further believe that what is physically interesting about diffeomorphism invariance can be captured in the dynamical approach to GTR sketched above.) Moreover, among its other achievements, *US* makes a strong case that understanding of the role of geometry in the laws of mechanics is the central issue in the history of spacetime theories, and that important conceptual developments have come from dynamical interpretations.

Perhaps it is interesting to note that such a change in emphasis is in some ways a return to earlier concerns about the nature of spacetime geometry: positivistic philosophy was also less concerned with its ontology, and more with its 'significance'. But in making that point, it is important to be aware that the issue is driven not by a narrowly empiricist conception of meaning, but by the idea that it is material properties and processes that are fundamental: by metaphysics, not epistemology.

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