Matter or geometry as fundamental in relativity theory

How not to teach special relativity

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May 14, 2013

Abstract

In this paper, I review a number of interpretational frameworks for relativistic phenomena like length contraction and relativity of simultaneity. Of central focus is the book *Physical Relativity* by Harvey Brown, where Brown advocates a view in which matter takes ontological priority over geometry. I discuss Brown's claims and examine some of the criticisms they have received. I discuss the nature of simultaneity in particular, sketching the historical context and commenting on its relation to some of Brown's broader arguments. Finally, I examine the consequences that Brown's thesis has for what constitutes good pedagogy when teaching special relativity.

This essay was submitted on 10 May, 2013, for examination in Part III of the Mathematical Tripos at the University of Cambridge. It has been modified since its submission.

Contents

1 Introduction

In a 1976 paper ambitiously titled "How to teach special relativity", Northern Irish physicist John Bell advocated a "bottom-up" pedagogical approach, which he contrasted with the top-down approach used by Einstein to develop the theory:

> The difference of style is that instead of inferring the experience of moving observers from known and conjectured laws of physics, Einstein starts from the hypothesis that the laws will look the same to all observers in uniform motion. This permits a very concise and elegant formulation of the theory, as often happens when one big assumption can be made to cover several less big ones. There is no intention here to make any reservation whatever about the power and precision of Einstein's approach. But in my opinion there is also something to be said for taking students along the road made by Fitzgerald, Larmor, Lorentz, and Poincaré. The longer road sometimes gives more familiarity with the country.[1, p. 77]

For Bell, phenomena like length contraction and time dilation can be discussed in terms of the material composition of physical rods and clocks in motion. While Bell approached this is as essentially a didactic issue, the approach he advocates, if taken seriously, has implications for the status of space-time in relativity theory. Consideration of these implications has been taken up by philosophers of physics in recent years.

The debate over what is what meant by words like "space" and "time" is by no means a product of post-Einsteinian scientific and philosophical inquiry. For ancient civilizations in the Near East, space was a solid dome vaulted over a flat earth—the "firmament" in which celestial structures were embedded like gems on a canvas.[1] As cosmologies developed and accepted the idea of a round earth, the firmament remained in various forms, from the philosophy of Plato in the fourth century BCE to the heliocentric model of Copernicus in the sixteenth century CE. As natural philosophers pondered the structure and substance of the heavens (what one might call "space-out-there"), debates raged over the nature of motion and whether there is some fixed notion of space and time to which motion can be unambiguously related (what one might call "space-down-here").

[1]See, for example, early Jewish cosmology: "And God said, Let there be a firmament in the midst of the waters, and let it divide the waters from the waters. And God made the firmament, and divided the waters which were under the firmament from the waters which were above the firmament: and it was so." (Gen. 1:6–7, KJV).

For Newton, the idea of absolute space and time was a component of his analyses of motion and gravitation (though he did not envisage it as a literal substance but an entity of a different kind [10]). Among Newton's contemporaries, Leibniz in particular was a strong opponent of this picture and gave an account of motion in which space was demoted to a mental construct [*Id.*]. It is with the benefit of the perspective of twentieth century physics and its discoveries of the special and general theories of relativity, along with quantum theory, that we understand how unified these various questions are. Understanding the nature of space-out-there, space-down-here, time, motion, and how all these things relate to what we know as matter, all belong under a single overarching framework: how should one interpret the mathematical formalism of relativity theory? For in relativity theory, space and time are unified into a single mathematical entity, a space-time manifold[2], while motion is specified via coordinate systems imposed on the manifold. The *answers* to these questions are disputed no less ferociously than they were before Einstein; however, the fact that these various questions are not, after all, independent of one another has transformed the way in which they are approached.

In the following pages, I examine recent work on two related questions within this framework. The first, in Section 2.1, is whether space-time as an independent entity is necessary for explaining the behaviour of matter, or if space-time should be understood as supervening on matter, acting merely as a convenient encoding of properties that belong to the matter itself. To put it another way, should one of the two—matter or geometry—be considered more fundamental than the other: and, if so, which one?

Central to this question is Harvey Brown's 2005 book *Physical Relativity* [3] and related papers that Brown co-wrote with Oliver Pooley [4, 5], along with responses to Brown's book by various authors. Brown is an advocate of the idea that Minkowski space-time is nothing more than a mathematical encoding of the dynamics of interacting bodies/matter fields—that it is, following the title of his 2004 paper with Pooley [5], "a glorious non-entity". I review in detail Brown's position and arguments, along with some of the alternative views that have been offered in response. The emphasis throughout

[2]Formally understood as a smooth manifold, M, possessing a metric structure compatible with the theory's postulates. The metric structure is provided by a tensor field, g, defined on the manifold. Any suitable pair (M, g) may be called "a space-time" in the mathematical sense. Physicists may also simply refer to "space-time" (indefinite article omitted) with the implication that they mean the *particular* space-time of our own universe, not the various counterfactual alternatives that may be studied within relativity theory. These mathematical foundations will be rehearsed in more detail in the following section.

is on the *special* theory of relativity (SR), but insights provided from—and applied to—general relativity (GR) are included where appropriate.

Following this discussion, my attention turns in Section 3 to a second question: what does it mean in the framework of special relativity to say that two events are simultaneous? That simultaneity is relative is one of the first insights provided by the theory; hence, one cannot say that two events are simultaneous, full stop. Rather, the typical language of special relativity is to say, "Event A and event B are simultaneous in the reference frame S associated with some observer." However, even the unambiguousness of this statement can be questioned in the context of relativity's formalism. One interpretation posits that such statements about events amount to nothing more than establishing a mathematical convention: a convenient, but ultimately arbitrary, decision no different than defining a particular direction to be "up". Another interpretation argues that the notion of simultaneity in a particular frame is unambiguous and represents a genuine property of distant events. Even within the latter perspective, there are two views to consider: that the non-conventionality of simultaneity should be taken as a theorem of special relativity or as an empirical fact. While these questions are fundamentally independent from the discussion about matter or geometry's priority, the latter topic heavily influences my overview of the former.

It is in the context of these perspectives on relativity that I then briefly return in Section 3.2 to the issue that was on John Bell's mind in 1976: teaching relativity to the next generation of physicists. The pedagogy of special relativity requires having a sound approach to discussing the foundations of special relativity (those being the relativity principle, the light principle, and related subtleties) and the standard set of "paradoxes"—the domain of adventurous twins and inadequate barns—introduced to convince students of the principles' internal consistency. In this section, I will discuss the postulates and the twin paradox; specifically, how certain approaches to them are incompatible with some of the central ideas discussed in the rest of the paper.

2 Dynamical underpinnings of relativity theory

In Section 2.1, I will discuss Brown's proposal for viewing relativity dynamically. In Section 2.2 I will consider some of the responses his proposal has received. Section 2.3 is a short discussion, following Butterfield, on the core "moral" that can be distilled from Brown's arguments.

2.1 Brown's constructivist approach

2.1.1 Overview

The approach taken by Harvey Brown and Oliver Pooley [4, 5], and developed in greater depth in Brown's book [3], has its kernel in Bell's paper [1]. In particular, they follow the latter's approach to teaching special relativity (SR)—referred to by Bell as the "Lorentzian pedagogy"—and take it seriously as a model for understanding the conceptual foundations of the theory. Of central importance in Brown's understanding of SR is the distinction, as it was formulated by Einstein[3], between a "principle (or phenomenological) theory" and a "constructive theory". An analogy he reiterates a number of times is the comparison between the classical thermodynamics of Clausius and Kelvin and the statistical mechanics of Boltzmann and Maxwell:

> If for some reason one is lacking the means of mechanically modelling the internal structure of the gas in a single-piston heat engine, say, one can always fall back on the laws of thermodynamics to shed light on the performance of that engine—laws which stipulate nothing about the structure of the working substances, or rather hold whatever that structure might be. The laws or principles of thermodynamics are phenomenological, based on a large body of empirical data; the first two laws can be expressed in terms of the impossibility of certain types of perpetual-motion machines.[3, p. 72]

In comparison, Brown argues, one can understand the statistical mechanical approach as a *constructive* method for approaching the same phenomena: overarching principles like the non-decreasing entropy of isolated systems are seen to emerge from the collective dynamics of particles, rather than being postulated *a priori*.

Brown's (and, it appears, Pooley's) view is three-fold: that the best explanations of phenomena come from constructive theories; that, as usually

[3]Indeed, Brown's book goes to great lengths to argue that Einstein's understanding of the descriptive power of SR was largely in line with Brown's own. Some of Brown's critics, Janssen in particular [12], dispute this point. While a historical study of relativity's context and development is interesting in its own right, what Einstein may or may not have thought is of little importance to the ontological questions under consideration. I do not mean to suggest Brown is making an appeal to authority—his quotations from Einstein and his contemporaries generally serve an illustrative purpose, rather than a rhetorical one—but I do think that belabouring the point risks derailing the discussion. Consequently, in what follows I omit most of the discussion about the views of relativity's founders on its descriptive status.

formulated, SR is a principle theory; and that an interpretative approach to SR based on Bell's Lorentzian pedagogy provides a constructive view that should be preferred to the orthodox principle theory approach [5]. I review the arguments put forward for each of these claims, in turn. Before doing so, however, it will be helpful to clarify a few points about what Brown's program is *not* an attempt to do.

2.1.2 Formalism

To this end, I briefly review some of the formalism of relativity theory so as to put later comments in the appropriate mathematical context. It is assumed that the reader is familiar with these definitions; they are stated here primarily to establish notational conventions. Any introductory textbook on general relativity (GR) or differentiable geometry may be consulted for further details. For my definitions, I follow those of Wald [18].

Definition 1. *An* ***n-dimensional differentiable real manifold*** M *is a geometric structure satisfying the following properties [18, p. 11]:*

1. M *consists of a set of points, together with a collection of subsets* O_α *such that* $\cup_\alpha O_\alpha = M$.

2. $\forall \alpha \exists \psi_\alpha$ *such that* ψ_α *is a bijective map* $\psi_\alpha : O_\alpha \to U_\alpha$, *where* U_α *is an subset of* $\mathbb{R}^n$. *Each pair* (O_α, ψ_α) *is called a* chart *and the collection of all the charts is called an* atlas.

3. *For any charts whose overlap in the manifold is non-trivial,* $O_\alpha \cap O_\beta \neq \emptyset$, *we can consider the map* $\psi_\beta \circ \psi_\alpha^{-1}$ *which takes subsets of* U_α *to subsets of* U_β *in* $\mathbb{R}^n$. *This map is required to be differentiable and its domain and range are required to be open sets. If instead of merely differentiable, this* transition map *is smooth (infinitely differentiable), we say the manifold is smooth.*

It is a smooth manifold that provides the bare structure on which the elements of GR (tensors) are overlaid. Of fundamental importance is the *metric tensor* [18, p. 22]:

Definition 2. *A* ***metric tensor*** *(or just* ***metric****),* g, *on a manifold* M *is a symmetric, non-degenerate tensor field of type* $(0, 2)$. *That is, given two vector fields* v_1 *and* v_2 *defined on* M, g *is a linear map to* $\mathbb{R}$ *such that* $g(v_1, v_2) = g(v_2, v_1)$ *and such that* $g(v, v_1) = 0$ *for all* v *in the tangent space of* M *only if* $v_1 = 0$.

If the signature of the metric tensor is positive definite, the combination (M, g) is called a *Riemannian manifold*. If the signature is $(p, 1)$ ("mostly plus") or $(1, q)$ ("mostly minus"), the combination (M, g) is called a *Lorentzian manifold* or a *space-time*. In SR, the Riemann curvature tensor vanishes and the metric takes the special form, according to the two sign conventions, of either $diag(-1, +1, +1, +1)$ or $diag(+1, -1, -1, -1)$, respectively. This is the Minkowski metric and is usually written as η.

2.1.3 What Brown does not say

Having gotten these mathematical preliminaries out of the way, I am in position to clearly state two claims that Brown's philosophical position does *not* entail. First, Brown's constructive approach to relativity does not necessitate a complete denial of space-time realism.[4] Granted, this seems a bizarre assertion given the "glorious non-entity" description of space-time acknowledged earlier. The key point is to note how, in the above definitions (which, again, are standard), the definition of space-time is both a manifold, M, *and* a metric, g. It is g, not M, that encodes what one generally means by "geometry"; absent g, the bare manifold M is *topological*, not geometrical. While the usual notion of space-time substantivalism attributes an independent existence to space-time, (M, g), it is possible that one could follow Brown's arguments that geometry—that is, g—supervenes on matter, while maintaining a realist view of the space-time *points*—that is, of M. Nonetheless, as we shall see when I get to Brown's critics in Section 2.2, there is potential for some confusion on this issue.

Second, Brown's constructive approach does not require that one treat SR as little more than the zero curvature limit of GR (though, as I have said, vanishing Riemann curvature is indeed a *feature* of SR):

> The special theory of 1905, together with its refinements over the following years, is, in one important respect, *not* the same theory that is said to be the restriction of the general theory in the

[4]That is, the view the view that space-time—or at least a part of it—is a real entity that exists apart from the matter that inhabits it. Of course, other objections like the Hole Argument [15] may well do this anyway. To be fair, it is clear that Brown has a fully relationalist view in mind; the point is simply that much of what he argues for about the priority of matter over geometry can be considered entirely independently from the central point of contention between relationalists and substantivalists. Indeed, Brown's collaborator in earlier versions of his program, Oliver Pooley, has outlined a defence of what is described as "sophisticated substantivalism" [16], though it is not immediately clear whether such a program might be compatible with what Pooley has co-authored with Brown.

> limit of zero gravitation (i.e. zero tidal forces, or space-time curvature). The nature of this limiting theory, and its ambiguities, will be discussed later; for our present purposes we shall associate it with the local, tangent-space structure of GR, which to a good approximation describes goings-on in sufficiently small regions of space-time.[3, p. 15]

The distinction is an important one, because it, in a sense, legitimizes Brown's approach.[5]

The importance of this fact is underscored in a separate discussion—that of taking the Newtonian limit of SR—in what Brown, citing F. Rohrlich, identifies as "dimensionless" and "dimensional" methods of reducing one theory to another:

> The former generally takes suitable dimensionless quantities—the ratio of two physical quantities of the same dimensions—to be negligibly small. The latter involves taking limits of dimensional parameters such as the light speed c or Planck's constant $\hbar$. Rohrlich emphasized that the dimensionless process represents a case of 'factual' approximation and that the dimensional approximation is 'counterfactual', because for instance it is a fact that c is finite. What we are interested in here is the factual approach.[3, p. 110]

The analogy carries through in going from GR to SR. Here, the factual method is to treat SR as a free-falling, sufficiently small laboratory with sufficiently short measurements (with suitable caveats about how inertial frames are defined [3, p. 170]) limit of GR. On the other hand, treating SR as just the zero-curvature limit of GR is obviously counterfactual and is further complicated by the fact that Minkowski space-time isn't even the *unique* zero-curvature limit of GR (unless one also specifies $\mathbb{R}^4$ topology). Considered in this context, Brown's approach of devoting almost all his effort to the status of Minkowski space-time isn't problematic: there is no reason to think we need to start with a full-blown ontological interpretation of GR and then take away all the matter (except perhaps a few test particles) to gain insight into the nature of Minkowski space-time. This is not a contentious point for Brown's critics; however, it is worth taking the time to clarify it.

[5] The *form* of the approach, I should say. The legitimacy of the *content* of the approach is, of course, a separate question.

2.1.4 What Brown does say

Returning, then, to what Brown *is* arguing in favour of, I outline his first claim: that constructive theories of nature should be preferred over so-called principle theories. I have illustrated what Brown means by these terms with his frequent analogy of thermodynamics vs. kinetic gas theory as complementary ways of approaching the collective motion of large systems. To further emphasize this point, I refer to a quotation from a lecture given by Bell, which Brown and Pooley quote (approvingly) as well:

> If you are, for example, quite convinced of the second law of thermodynamics, of the increase of entropy, there are many things that you can get directly from the second law which are very difficult to get directly from a detailed study of the kinetic theory of gases, but you have no excuse for not looking at the kinetic theory of gases to see how the increase of entropy actually comes about. In the same way, although Einstein's theory of special relativity would lead you to expect the FitzGerald contraction, you are not excused from seeing how the detailed dynamics of the system also leads to the FitzGerald contraction.[2, 4]

As I have said, Brown further maintains that this was also Einstein's position,[6] but that is beside the point. Apart from historical analogies and quotations, Brown's essential position may be summarized as the idea that principle theories *constrain* and constructive theories *explain*. The former characterization is best illustrated by another distinction Brown uses in parallel (and occasionally interchangeably) with principle vs. constructive theories: kinematics and dynamics.

In this view, a kinematical picture is one that, to use my terminology in the introduction, gives a top-down description of the physics. Meanwhile, a dynamical picture is taken to give a bottom-up account of the physics. Brown and Pooley take the position that the key characteristic that is "definitive of [their] position is the idea that constructive explanation of 'kinematic phenomena involves investigation of the details of the dynamics of the complex bodies that exemplify the kinematics"[5, p. 11]. The proposal is that, in the absence of a truly dynamical—that is, phrased in terms in the properties of components of the system—understanding of a physical process, one can rely on a kinematical—that is, phrased in terms of general principles—model: both as a stop-gap until a dynamical understanding is found and as a means

[6] See Section 2 of Brown and Pooley's first paper [4] for a summary of this argument; contrast with Janssen [12, p. 45] for a counter-argument.

of constraining the form that such dynamical laws will inevitably take [4, pp. 5-6].

That such principle-based kinematical explanations fail to be explanatory is argued by Brown and Pooley specifically in connection with SR. Suppose one follows the usual mathematical arguments, beginning with Einstein's postulates, to derive the (kinematic) Lorentz transformations and then claims that these algebraic relations encode the behaviour of rods and clocks. Is such a claim defensible?

> What has been shown is that rods and clocks must behave in quite particular ways in order for the two postulates to be true together. But this hardly amounts to an explanation of such behaviour. Rather things go the other way around. It is because rods and clocks behave as they do, in a way that is consistent with the relativity principle, that light is measured to have the same speed in each inertial frame.[5, p. 7]

This is to say that it is the Lorentz covariance of the physical relations that govern the behaviour rods and clocks that justifies Einstein's postulates—not *vice versa*.

One must be cautious about identifying a particular phenomenon as an *explanandum* and another as its *explanans*. As Norton[7] notes, one can approach Brown's notion of "explanation" in two ways: explanation in a purely abstract (and perhaps, like Bell, pedagogical) sense, and explanation in a causal sense. The latter has genuine implications for space-time ontology, and is present throughout Brown's book (see, for example, [3, pp. 141-142,p.100]). For clarity's sake, it is best to focus on the causal, ontological implications of Brown's (and Pooley's) approach.[8] Thus, another way of contrasting the (top-down, kinematic) principle approach with the (bottom-up, dynamical) constructive approach is in terms of "global" and "local" processes.

To illustrate this, I remind the reader of the difference between the Newtonian and Lagrangian (or Hamiltonian) approaches to classical mechanics. In Newtonian mechanics, the language is that of forces: each particle in the system acts upon (and is acted upon by) its environment in an immediate sort of way. Even allowing for the non-local action-at-a-distance of pre-relativistic physics, one does not need to take into account past or future states of the system to determine what will happen in the next instant. In contrast, the computational success of Lagrangian mechanics, which is

[7] Whose objections are more completely discussed in Section 2.2.1.

[8] Of course, in the spirit of Bell's paper [1], these ontological conclusions *also* have pedagogical consequences—an observation I explore more fully in Section 3.2.

intimately connected with conservation laws, appears mysterious when it is first introduced: to determine the behaviour of the system, one needs to determine, via the Euler-Lagrange equations, the extremal trajectory in configuration space. Extremal action, however, is a *global* property. It seems that each component of the system must, some how, been tuned in to counterfactual states of the system in order to "know" what to do next.

This analogy is very useful for three reasons. First, the Lagrangian approach translates directly to the "space-time diagram" approach to SR due to Minkowski. Hence, the reader may already get a sense of what Brown's main objections will be. Second, this illustrates that the line between principle and constructive theories can be fuzzy. While in its original formulation Lagrangian mechanics was necessarily understood as a principle-theoretic reformulation of Newton's constructive theory, an understanding of Feynman's sum of histories interpretation of quantum mechanics gives a *constructive* reading of it. Brown agrees with the possibility of overlap between principle and constructive approaches [3, p. viii] and so there is no need for us to view them as rigid, exclusive classifications. Third, Mathias Frisch [9] develops this same analogy in his critique of Brown's work[9], which we consider in Section 2.2. Hence, it will be helpful to keep the analogy in mind as we go along.

Having argued that constructive theories are preferable to principle theories, the next component of Brown's thesis is that "orthodox" SR should be understood as a principle theory. His primary target is a particular alternative candidate for constructive SR: Minkowski's formulation in terms of space-time geometry. Such a view, Brown suggests, would see space-time as a substantial entity with intrinsic geometry; geometry that is read off via rods and clocks. Such a view is problematic:

> The *mechanism* of the old waywiser[10] is obvious; there is no mystery as to how friction with the road causes the wheel to revolve, and how the information about the number of such ticks is mechanically transmitted to the dial. But the true clock is more subtle. There is no friction with space-time, no analogous mechanism by which the clock reads off four-dimensional distance. How does it work? [emphasis Brown's][3, p. 8]

[9] Albeit to a somewhat different end than my own reasons.

[10] An old device used to measure of road distances using a rolling wheel and gear system. An image of a waywiser emblazons the cover of Brown's book and serves as a sort of caricature of the view he opposes, that space-time is the causal agent acting on rods and clocks.

Brown further emphasizes that, despite first appearances, GR does *not* come to the rescue by introducing a dynamical metric.

The issue, as in SR, is that the metric does not come equipped *a priori* with a chronogeometric interpretation; rather, that is an interpretation the metric *earns* on the basis that rods and clocks are made out of physical material which behaves in a dynamical way that reflects the metric. Thus, in addition to the field equations, we need the strong equivalence principle and the clock hypothesis to promote one particular tensor to the status of "metric". The strong equivalence principle allows us to use SR locally and hence reduces the question of rod and clock dynamics to various relativistic dynamical theories of matter—nowadays, quantum field theories. The clock hypothesis posits that the behaviour of clocks according to some reference frame depends only on their instantaneous velocity in that frame, not their acceleration. That this is a statement about the detailed dynamics of clocks is seen, Brown points out, just by considering how obtaining a chronometer that would remain accurate at sea was once a considerable problem for mariners [3, p. 94].

In their 2004 paper [5], Brown and Pooley develop a related criticism of treating space-time as a causal entity that determines the behaviour of matter. They criticize the notion that that, in the absence of forces, the affine structure of Minkowski (or curved) space-time can be said to explain the inertial motion of test particles. This is, they say, putting the cart before the horse:

> But to appeal to the action of a background space-time connection in which the particles are immersed—to what Weyl called the "guiding field"—is arguably to enhance the mystery, not to remove it. For the particles do not have space-time feelers either. In what sense is the postulation of the 4-connection doing more explanatory work than Molières famous dormative virtue in opium?[5, p. 4]

It is here that one can see the direct parallel with Lagrangian mechanics: in either case, the geometric explanation seems to imply that a test particle has some means of detecting which direction will advance it along the path of extremal action. Indeed, the analogy goes further: like the constructive insight that Feynman's path integration brings to Lagrangian mechanics, the geodesic principle in relativity can be understood dynamically once we extend to GR. It is, as Brown and Pooley make a point of emphasizing, derivable from GR's field equations—specifically, the fact that they imply the stress-energy tensor of matter has vanishing (covariant) divergence.[11] Indeed, this derivation forces one to accept modifications to the geodesic equation for spinning test bodies (of finite dimensions), implying that it is "not an essential property of localised bodies that they run along the ruts of space-time determined by the affine connection, when no other dynamical influences are at play." [5, p. 3]

Thus, for Brown, geometry cannot be separated out from matter. In GR, where the metric *is* a real entity—a field like any other that simply earns the designation "metric" because of the behaviour of matter—we should not get carried away by the active role space-time seems to play in the theory. Its effect on matter—along the lines of John Wheeler's famous aphorism that "Space-time tells matter how to move; matter tells space-time how to curve"—need not be accepted as an independent postulate. Furthermore, it is difficult to tell what good such a postulate would be if it *were* necessary, since it is not clear how the geometry of space-time would impose itself on the matter that inhabits it (due to a lack of "space-time feelers"). He observes, with reference to particular examples, that "if one postulates space-time structure as a self-standing, autonomous element in one's theory, it need have no constraining role on the form of the laws governing the rest of content of the theory's models" [3, p. 149], calling this the "mystery of mysteries" for the position that geometry is prior to matter.

Hence, the main lesson Brown draws from his discussion is that space-time geometry, be it flat or curved, does not amount to a constructive understanding of relativity. Given his relationalist views on space-time, it is reasonable to think he is implying a secondary (though optional) lesson: if an ontologically independent space-time is irrelevant to the logical consistency and explanatory ability of relativity, why not do away with the idea altogether?

The final ingredient, then, in Brown's approach to relativity theory is a means of constructively building up the various phenomena that are collectively labelled "space-time geometry". The general idea is already implicit in much of what has been said. Bell's pedagogical program—which forms the core of Brown's philosophical approach—proceeds as follows:

1. Begin with some toy model of matter. The only requirement is that the dynamical laws obeyed by the matter be Lorentz covariant. For reasons of historical continuity (not to mention a certain degree of scientific accuracy!), Maxwell's formulation of electromagnetism is a good choice. Consider, for example, something like the Bohr model of the atom with an electron orbiting a nucleus.

[11] The proof of this extraordinary result is well worth reading for its own sake and can be found in, e.g., Misner, Thorne, and Wheeler [13, pp. 471-480].

2. Examine the toy atom from a reference frame S in which the atom has some velocity. A standard calculation, usually done in the course of an undergraduate program in physics, shows that due to the nature of electromagnetic fields, the electron's orbit will be compressed by a velocity-dependent factor along its axis of motion. Meanwhile, the period of the orbit will increase by the same factor. These are the so-called "FitzGerald contraction" and "Larmor dilation" (and the velocity-dependent factor is, of course, the Lorentz factor γ).

3. Deduce that an object made out a lattice of these toy atoms in electrostatic equilibrium would contract by a factor of γ parallel to its velocity because the atoms themselves get closer together due to the flattening of their electron orbits. Meanwhile, an atomic clock based on electronic oscillations would slow down by the same factor because its period would increase. Similar arguments can be made for other kinds of clocks (like the admittedly contrived "light clock" that invariably shows up in most introductions to SR), demonstrating that time kept according to *any* clock constructed out of our toy matter will dilate.

4. Show that in a set of coordinates, S', related to S via the Lorentz transformations (and hence such that the atom is at rest in S'), the description of the toy atom is precisely the same as it was in S before it was set in motion. That is, in the S' coordinates, the toy atom once again becomes circular and the γ drops out of its period (according to the primed time variable). Deduce that according to the S' frame, rods and clocks in S should contract and dilate in precisely the same way that those of S' did in S.

This is what Bell calls the "Lorentzian pedagogy"[12] and Brown more restrictively calls the *full* Lorentzian pedagogy [3, p. 5]. It is important to stress that neither Bell nor Brown is proposing a preferred reference frame (as Lorentz did). The lesson is not that one *needs* to work in a single a reference frame to understand the electrodynamics of moving bodies; rather, the lesson is that it is *sufficient* to work in a single reference frame to understand such dynamics.

The point of this exercise is not to demonstrate precisely how to dynamically account for length contraction and time dilation—since, after all, this is merely a toy model in which it isn't even possible to have stable atoms—but to show that it *can* be done if one has an appropriate theory of matter. Here,

[12] Incorrectly, according to Brown, who believes it would have been more accurately called the "FitzGeraldian pedagogy" [3, p. 5].

"appropriate" is taken to mean "Lorentz covariant". This is precisely what Brown and Pooley call the "truncated Lorentzian pedagogy":

> In order to predict, on dynamical grounds, length contraction for moving rods and time dilation for moving clocks, Bell recognised that one need not know exactly how many distinct forces are at work, nor have access to the detailed dynamics of all of these interactions or the detailed micro-structure of individual rods and clocks. It is enough, said Bell, to assume Lorentz covariance of the complete dynamics—known or otherwise—involved in the cohesion of matter. We might call this the truncated Lorentzian pedagogy. [4, p. 7]

It is this truncated approach that forms the backbone of Brown's space-time ontology.

In this approach, one starts with the fact that the known laws of physics are Lorentz covariant and *deduces* behaviour like length contraction and time dilation for rods and clocks made out of matter that obeys these laws. As in the final step above, we can then go further and deduce the postulates of SR. Thus, the principle of relativity, for example, is to be understood as an derived property of the laws of physics. The explanation for *why* the various laws of physics are Lorentz covariant is left unexplained:

> In the dynamical approach to length contraction and time dilation that was outlined in the previous chapter, the Lorentz covariance of all the fundamental laws of physics is an unexplained brute fact. This, in and of itself, does not count against the approach: all explanation must stop somewhere. [3, p. 143]

While at first glance, this may appear somewhat anti-climactic, it is important to bear in mind Brown's arguments against space-time priority over geometry: according to these objections, outlined above, the Lorentz covariance of various laws of physics would *remain* an "unexplained brute fact" *even* if Minkowski space-time was a substantive entity. Furthermore, I would also observe that the apparent coincidence of all the laws of physics being Lorentz covariant becomes considerably less curious when we note the progress that has been made on unifying the fundamental forces. If grand unification (or, better yet, the "Theory of Everything" in which gravity is incorporated) were to succeed, the "coincidence" would vanish altogether.

A sketch for how one could treat a moving rod in a manner more amenable to the truncated approach (that is, not committing to any particular model of solid state physics) is provided by Jeremy Butterfield [6, p. 8]: we simply represent the rod of length L by the quantum state $|\psi\rangle$ and presume the quantum field theoretic laws that govern the matter it is composed of are Lorentz covariant. We slowly accelerate the rod—slowly enough that the rod's lattice vibrations can dissipate via heat loss to its environment—so that the rod is now in state $\hat{B}|\psi\rangle$, where $\hat{B}$ is the relevant Galilean boost operator. Since a Lorentz boost may be approximated with a series of small Galilean boosts, we can repeat this process until the rod is in the state $\hat{B}_L|\psi\rangle$, with $\hat{B}_L$ an arbitrary Lorentz boost. By the Lorentz covariance of the physical laws describing the material of which the rod is composed, we conclude that $\hat{B}_L|\psi\rangle$ has length L in the frame corresponding to the boost $\hat{B}_L$ according to the coordinates of that frame. Hence, the lengths measured by the rod in the boosted frame agree with lengths predicted by the Minkowski metric—which is precisely what something we are purporting to call a "metric" is supposed to do!

That is precisely the essence of Brown's view of space-time geometry, in which he argues that "the operational meaning of the metric is ultimately made possible by appeal to quantum theory" [3, p. 9]. Thus, the metric—the bearer of all things geometric—only earns its name because it is a convenient mathematical encoding of the dynamical behaviour of matter. Hence, matter is taken to be more fundamental than geometry and the principle of relativity is taken to be descriptive rather than prescriptive. As for space and time—the former of which was once thought of as solid enough to hang the stars upon—they are demoted to being nothing more than a "glorious non-entity".

2.2 Responses to Brown

Since the publication of Brown's book, a number of responses—in varying degrees of disagreement—have appeared. In this section, I examine those of John Norton [14] in Section 2.2.1 and Michel Janssen [12] in 2.2.2, while making reference throughout to a third response by Mathias Frisch [9]. Norton's and Janssen's papers both purport to criticize Brown's position, though they do so for considerably different reasons. I outline their objections and, where appropriate, respond to their arguments. Frisch's paper, which is broadly in agreement with Brown, argues that Janssen's and Brown's positions may be (at least partially) harmonized and that much of the apparent disagreement is due to a difference in conceptual frameworks.

2.2.1 Norton

Rather than tackle Brown's arguments point-by-point, Norton's response [14] is a more general argument against *any* form of constructive special relativity—which Norton contrasts with a "realist" view of Minkowski space-time. The essence of his thesis is that a constructive approach to relativity can be consistent only "if one tacitly assumes much or all of the realist conception" [14, p. 823]. The conclusion, then, is that a constructive approach like Brown's ultimately fails since it requires a space-time that is not significantly different from the realist view. Since much of Norton's argument hinges on his formulation of realist Minkowski space-time, I reproduce his sketch of the approach:[13]

> **(a)** There exists a four-dimensional space-time that can be coordinatized by a set of standard coordinates (x, y, z, t), related by the Lorentz transformation.
>
> **(b)** The spatiotemporal interval s between events (x, y, z, t) and (X, Y, Z, T) along a straight line connecting them is a property of the space-time, independent of the matter it contains, and is given by
>
> $$s^2 = (t - T)^2 - (x - X)^2 - (y - Y)^2 - (z - Z)^2. \qquad (1)$$
>
> When $s^2 > 0$, the interval s corresponds to times elapsed on an ideal clock; when $s^2 < 0$, the interval s corresponds to spatial distances measured by ideal rods (both employed in the standard way).
>
> **(c)** Material clocks and rods measure these times and distances because the laws of the matter theories that govern them are adapted to the independent geometry of this space-time.
>
> [14, p. 823]

This is the position that Norton takes a constructivist like Brown to oppose[14]: but the bulk of which, he argues, such a constructivist must tacitly assume.

[13]This is essentially an exact reproduction; however, I have made three changes from the exact text: I enumerate Norton's items as (a), (b), and (c)—in line with how he refers to it in the rest of the paper—rather than (1), (2), (3), I omit his footnotes, and I write "space-time" rather than "spacetime" for consistency with the rest of this paper.

[14]Though not problematic for Norton's later arguments, I should point out that there is some imprecision with item (b) of his framework. It is not quite correct to say that a positive space-time interval (in the "mostly minus" signature convention being used here) "corresponds to times elapsed on an ideal clock". Rather, *it is possible* for a positive interval to correspond to times elapsed on an ideal clock; however, this will only be the case for *inertial* clocks passing between the two events. The same caveat applies to ideal rods and negative intervals. I take the implications of Norton's chosen wording to be unintentional. If it genuinely represents an aspect of the realist space-time view that Norton defends, then it unnecessarily privileges inertial frames—and the objects that inhabit them—to far greater extent than special relativity requires.

To advance this claim, Norton outlines the steps that must be taken by a constructive approach, which he characterizes as one in which "It is possible to recover the geometry of Minkowski spacetime from Lorentz covariant matter theories devoid of spatiotemporal presumptions" [14, p. 825].

I note that Frisch considers even this starting point a straw man, i.e. an extreme version of the relationalist view of space-time that Brown's constructivism favours, objecting that "Norton's relationalist not only denies the existence of a four-dimensional substantival spacetime that exists independently of matter but also that matter has no basic spatio-temporal properties. A relationalist who merely denies the former claim can escape Norton's conclusion." [9, p.177]. Presumably for Frisch's relationalist, such intrinsic spatiotemporal properties of matter would not gain the interpretation as such until rods and clocks were constructed out of matter, in keeping with Brown's view that the metric only earns its chronogeometric interpretation as a result of the behaviour of such rods and clocks. Thus, such properties would stand as part of the "unexplained brute fact" that is the universal Lorentz covariance of matter which Brown is prepared to accept.

It should be noted that while Norton frames his argument as one opposed to Brown's constructivism, there are certainly constructive elements in his own approach. Using the geometry of space-time as a genuinely causal explanation for Lorentz covariance is, in a sense, constructing relativity from space-time. Nonetheless, Brown and Pooley argue that such an approach doesn't really count as constructive in the sense they are after: the sense in which global relativistic phenomena are deducible from the dynamical behaviour of matter. For them, geometry-based construction projects are little more than principle theories in disguise:

> The geometrical features of the objects that are assumed, and appealed to, in these explanations are similar in status to the postulates of principle theories. They do not, *directly*, concern the details of the bodies microphysical constitution. Rather they are about aspects of their (fairly) directly observable macroscopic behaviour. And this reflection prompts an obvious question: why do these objects obey the constraints of Minkowski geometry? It is precisely this question that calls out for a constructive explanation. [4, p. 9]

Recalling that Brown does not see space-time as capable of explaining universal Lorentz covariance, this dim view of geometric constructions should not be surprising.

In any case, even if one rejects Frisch's view that a relationalist can ascribe certain spatiotemporal properties to matter without the aid of space-time, it is curious that so much of Norton's efforts are spent arguing that a constructivist must accept his item (a) of the realist approach. As I noted in Section 2.1, the key component of manifold substantivalism (where SR is concerned) that Brown rejects is the independent existence of the Minkowski metric. While implying the supervenience of *geometry* on matter, Brown's approach is essentially agnostic about the existence of topology—that is, of the bare manifold—independent from matter fields. And yet, Norton apparently considers the acceptance of (a) to be a severe concession of the part of a constructivist. However strong Norton's arguments may be to this end—and I examine them below—the effort seems somewhat misplaced.

To argue that a constructivist must accept (a), Norton notes that in any given matter theory, such as Maxwell's electrodynamics or some quantum field theory of interactions, in constructive relativity the coordinates (x, y, z, t) are merely parameters whose interpretation comes about only as a consequence of their role in the matter theory [14, p. 825]. Given the variety of possible matter theories one can consider, these parameters should be differentiated from one another according to the theory in which they appear. Norton does so with sequential subscripts: (x_1, y_1, z_1, t_1) for the first matter theory, (x_2, y_2, z_2, t_2) for the second matter theory, and so on. A particular solution to the field equations in a particular matter theory imply a family of solutions, related to one another by active transformations from the Poincaré group.

The next step in Norton's construction is to note that certain structures made out of the matter under consideration may be used as rods and clocks. He very briefly objects to this step, before admitting it for the sake of argument, on the grounds that "Some matter theories do not straightforwardly admit clock-like or rod-like structures. An example is Maxwell's electrodynamics, for none of its localized structures is stable. It must be coupled with another theory to produce such structures. We might also wonder how the structures might arise in quantum field theory"[14, p. 826]. Of course, such a detailed understanding of a particular matter theory is unnecessary in Brown and Pooley's truncated Lorentzian pedagogy[15] and so Norton is right to not

[15] Furthermore, I take the example of Maxwell's electrodynamics to be an incorrect one. Another theory is needed precisely because Maxwell's theory is not properly a theory of matter at all. The form taken by elementary matter must be added as an additional postulate to Maxwell's equations, and nothing in the equations themselves compels the choice of the point-like electric charge carriers that, as Norton notes, can't be assembled into stable atoms. Since Norton's point, like Brown's, only requires the *possibility* of rods and clocks in some matter theory, we are free to posit any hypothetical matter consistent with the theory with which to construct them. In particular, nothing stops us from positing the existence, independent from atoms, of a conducting material out which one can construct mirrors for use in a light clock. Thus we find ourselves in possession of

dwell on the point.

Having allowed for the existence of rods and clocks, Norton claims a flaw in this constructive approach:

> To begin with, the construction presumes that the parameters of each matter theory (x_1, y_1, z_1, t_1), (x_2, y_2, z_2, t_2), (x_3, y_3, z_3, t_3), ... refer to the same events of spacetime. For example, we presume that clocks from different matter theories will return the same expression (1) for the spacetime interval. It presumes that (for suitable selections of parameter sets) the origin of the parameters $(x_1, y_1, z_1, t_1) = (0, 0, 0, 0)$ of matter theory 1 refers to the same event in spacetime as the origin of the parameters $(x_2, y_2, z_2, t_2) = (0, 0, 0, 0)$ of matter theory 2.[14, p. 828]

He argues that such an assumption amounts to an attribution of spatiotemporal properties to the matter fields, which he takes to be disallowed for the constructivist. In addition to denying this possibility to the constructivist (a denial that, as I have noted, Frisch rejects), Norton anticipates another constructivist response: that the coincidence of the parameters in different matter theories can be attributed to interactions between the different theories. He dismisses this response by analogy with spin states, noting that, "It is standard to write spin-spin coupling terms in Hamiltonians, where the coupling energy depends on the closeness of the spin parameters, without thereby assuming that sameness of the parameters betokens spatiotemporal coincidence.[14, p. 828]"

Putting aside Frisch's objections for argument's sake, there is—at least—one other defence available to the constructivist. As I observed when considering the apparent distastefulness of Brown's "brute fact" of Lorentz covariance, the tendency of modern quantum physics has been towards a unification of the various forces. If Grand Unification succeeds[16], Norton's constructivist has nothing to explain: the parameters of the various matter theories may be taken as coincident because each theory is merely a different limiting case of the same theory. This defence is already available for Norton's use of Maxwell's electrodynamics. If (x_1, y_1, z_1, t_1), the coordinates for the first matter theory, are taken to parametrize Maxwell's equations and (x_2, y_2, z_2, t_2), the coordinates for the second matter theory, are taken to parameterize quantum electrodynamics, why should the constructivist need to *presume* that these parameters refer to the same events? Maxwell's theory is

an imaginary, but stable, clock that is consistent with Maxwell and can be examined by Norton.

[16] A possibility which, while by no means certain, is not one whose failure it would be prudent for a space-time realist to rely upon.

explicitly a restriction of QED to a particular domain; that the former may inherit its parameters from the latter is hardly surprising. Hence, unification provides another possible escape from Norton's conclusion.

Norton goes on to argue that, in addition to the bare manifold, spatial distances and elapsed times are properties of space-time [14, p. 830], which is more on the mark against Brown's view of matter's priority over geometry. Much of this argument builds on Norton's comments about space-time events, as well as claiming common cause with Janssen (whose objections I review next). In spite of this, Norton allows that a constructivist can avoid his conclusion, but at a cost:

> To see this, imagine some part of spacetime that is either devoid of matter or hosts a static matter distribution. In this part of spacetime, we can select two noncoincident timelike-separated events A and B such that nothing changes as we pass along the straight segment of spacetime connecting them. In the ordinary realists conception, we would say that some time elapses between them. What can a constructivist say? There are no material clocks actually present measuring the time elapsed, for there is either no matter present or no change in the matter present as we pass from A to B. So the constructivist has no material basis for the recovery of a time change.[14, p. 831]

Norton allows one escape for the constructivist:

> The constructivist must, in effect, say that the entire scenario envisaged is impossible. The notion that there can be a part of spacetime without matter or a part of spacetime with static matter is simply a confusion on the part of realists. It makes no sense to talk of time in such scenarios.[14, p. 832]

Norton objects to this defence, calling it extreme operationalism.

Interestingly, Brown suggests he is entirely willing to follow the escape Norton suggests [3, pp. 100-101], however negatively the latter might regard it.[17] The context in which Brown discusses this is his comments on the conventionality of simultaneity in SR; hence, I defer a more thorough examination of this to Section 3.

2.2.2 Janssen

Moving on to another of Brown's critics, I review the paper by Janssen [12] alongside the commentary of Frisch [9]. Whereas Norton's focus is the

[17]And, as demonstrated by his footnote 13, Norton is fully aware of this.

implications that Brown's approach to SR has for the status of Minkowski space-time, Janssen targets Brown's related assertion that the constructivist approach provides a better *explanatory* framework for understanding relativity. In Section 2.1, I noted Norton's observation that this whole business of "explanation" may be taken either way: as an epistemic claim or as an ontological claim. Norton purposefully avoids the former reading, objecting that it just leads to "futile disputes over just what it means to explain"[14, p. 5]. Janssen, however, considers the epistemic claims worth discussing, on the grounds that "explanation is tied up with inference, which is absolutely central to the scientific enterprise"[12, p. 2]. He frames his objections to Brown's constructive relativity with reference to J. J. Thomson's characterization of a good theory in physics:

> In 1906, J. J. Thomson made an observation about the role of theories in physics that, I think, applies equally well to explanations. For a working physicist, Thomson wrote, a theory "is a policy rather than a creed." Physicists use explanations not to adorn the results of their investigations with the elusive quality of understanding, but to help them come up with ideas for what to investigate next. They seek answers to why-questions in part no doubt for the sake of those answers themselves, but mostly to find clues and pointers in them for further research.

Hence, for Janssen there is an element of utilitarianism in what he counts as a good explanation.

As a result of this focus, there is a sense in which Janssen's objections, whatever their merit, are irrelevant to the aspects of Brown's constructive relativity I have highlighted in this paper. Janssen makes a point of emphasizing that he has "disavowed the notion that Minkowski space-time be a substance with causal efficacy, so the sense in which Minkowski space-time explains Lorentz invariance is certainly not causal" [12, p. 68]. Hence, for the questions I underlined in Section 1—"the nature of space-out-there, space-down-here, time, motion, and how all these things relate to what we know as matter"—there is not a significant difference between the space-time ontologies of Brown and Janssen.

This is the view endorsed by Frisch, who argues that "their disagreement appears larger than it actually is due to the two frameworks used by Brown and Janssen to express their respective views" and that it largely amounts to a "disagreement about labels but not about substance" [9, p. 1]. Norton's caution against futile disputes tied up in linguistic knots appears to be worth heeding. Nonetheless, I briefly review Janssen's arguments—while emphasizing their independence from the ontological dispute between Brown and Norton that I have examined above.

A recurring theme in Janssen's paper is what he dubs the "common origin inference" (COI), a subset of the "inference to the best explanation" (IBE). [12, p. 3]. The COI is the principle that separate instances of the same phenomenon imply an explanation in which there is a common origin for each instance. Norton notes that such reasoning is a "line of argument extending back to Einstein" [14, p. 830], presumably referring to Einstein's discomfort in his 1905 paper with the apparent need to use two separate physical phenomena to explain the current induced when a conductor and a magnetic field are in relative motion:

> The observable phenomenon here depends only on the relative motion of the conductor and the magnet, whereas the customary view draws a sharp distinction between the two cases in which either the one or the other of these bodies is in motion. For if the magnet is in motion and the conductor at rest, there arises in the neighbourhood of the magnet an electric field with a certain definite energy, producing a current at the places where parts of the conductor are situated. But if the magnet is stationary and the conductor in motion, no electric field arises in the neighbourhood of the magnet. In the conductor, however, we find an electromotive force, to which in itself there is no corresponding energy, but which gives rise—assuming equality of relative motion in the two cases discussed—to electric currents of the same path and intensity as those produced by the electric forces in the former case.[8, p. 1]

Einstein's conclusion, by a method like the COI, is to do away with the notion of absolute rest and conclude that the relative motion between the two bodies is the common origin for the two apparently distinct phenomena—setting the stage for his deductions about the electrodynamics of moving bodies.

Janssen makes use of the COI to defend the explanatory power of spacetime (which, again, it must be emphasized he does not view as a substantival entity) by distinguishing kinematical and dynamical behaviour. He characterizes the distinction thus:

> It is a mistake to keep looking for further explanation of a phenomenon once that phenomenon has convincingly been shown to be kinematical. What it means for a phenomenon to be kinematical, in the sense in which I want to use this term, is that it is nothing but a specific instance of some generic feature of

> the world, in the case of the phenomena examined in this paper instances of default spatio-temporal behavior.[12, p. 4]

Conversely, a dynamical explanation is needed where something deviates from its natural kinematical behaviour. Much of his paper is occupied with examining a series of early relativity experiments that demonstrate relativistic phenomena to be fundamentally kinematical rather dynamical. It is in this context that Janssen then draws on the COI to argue how Minkowski space-time explains Brown's "brute fact" of universal Lorentz covariance:

> It explains them by showing they need no explanation. Or, to put it less paradoxically, the statement that space-time is Minkowskian explains all of them in one fell swoop. This then is where that statement goes beyond the statement that all laws are Lorentz invariant. It commits one to assigning all manifestations of Lorentz invariance to the class of kinematical phenomena.[12, p. 69]

Recall that Brown criticizes the classification of phenomena like inertial motion in GR as "kinematical" on the grounds that the the geodesic equation can be derived from the Einstein field equations. Thus, Brown's use of the kinematic vs. dynamic distinction is not equivalent to Janssen's usage, where Brown ties them together with principle vs. constructive theories, respectively. I have noted his and Pooley's claim that "constructive explanation of 'kinematic' phenomena involves investigation of the details of the dynamics of the complex bodies that exemplify the kinematics"[5, p. 11].

It is precisely this different usage of terminology that Frisch identifies as enabling a considerable degree of harmonization between Brown and Janssen [9, p. 181]. In Section 2.1, I illustrated the principle vs. constructive distinction with force-based and energy-based approaches to classical mechanics. Frisch develops the same analogy, but with the intention of highlighting the way in which certain principles constrain the possible constructive explanations:

> To explain a phenomenon, I want to submit, is to embed the phenomenon into a pattern of functional dependencies...and phenomenological principles can provide us with answers to such questions just as general principles or constructive theories can.[9, p. 179]

It is as this sort of general principle that Janssen views universal Lorentz covariance—taking the general principle to be the common origin of particular matter theories' Lorentz covariance. I have noted that Brown allows for something like a continuum between principle and constructive explanations [3, p. viii], and so Brown's view does indeed seem to be in line with Frisch's—and hence, by the latter's argument, with Janssen's.

Finally, I note that Janssen is very explicit about his reasons for defending his particular viewpoint, stating: "I want to argue that the orthodox version of this physical theory is preferable to the alternative proposed by Brown because it provides better guidance for further research" [12, p. 6]. This underscores my point that the preceding discussion has little to do with Brown's ontological position with respect to space-time. It is difficult to say which epistemic approach is more amenable to guiding further research; however, Brown's approach has a powerful moral that those pushing the frontiers of physics would do well to mark. This is the subject of Section 2.3—the final section in this discussion of Brown's central thesis.

2.3 The moral

In his 2007 paper [7], Butterfield distills what he calls "Brown's moral"[18] out of the larger discussion. He broadly states the moral as follows:

> We can think of the moral as having two aspects, "negative" and "positive". It will be clearer to start with the negative aspect, since the positive aspect explains it. Negatively, the rough idea is that we should not simply postulate that a quantity in a physical theory has (chrono)-geometric significance. The point here is not just that it would be wrong to infer from a quantity's being called a metric that it mathematically represents (what the theory predicts about) the readings of rods, and-or clocks and-or other instruments for measuring lengths and time-intervals. That is obvious enough: after all, a quantity might be given an undeserved, even tendentious, name. But also: we should not infer from the fact that in the theoretical context, the quantity is mathematically appropriate for representing such behaviour, that it does so.[7, pp. 16-17]

Conversely, the positive formulation of the moral is expounded in my Section 2.1.4: that the chronogeometric nature of the metric "is an interpretation the metric *earns* on the basis that rods and clocks are made out of physical material which behaves in a dynamical way that reflects the metric."

The upshot of Brown's moral is that we miss some deeper insights by positing ideal rods and clocks as if they were of a different species than the

[18] Which, to keep track of our growing stack of eponyms, is essentially what Brown identifies as Bell's reformulation of Lorentz's (but really FitzGerald's) pedagogy.

rest of the matter that may inhabit space-time. There is nothing wrong, *per se*, with discussing particular phenomena in terms of broad, general principles. In the context of length contraction, Brown and Pooley note that such general, geometrically-guided approaches are "perfectly acceptable explanations (perhaps the only acceptable explanations) of the explananda in question"[5, p.9]. However, what such explanations lack is the intuition that comes from seeing how such a phenomenon arises constructively due to a series of elementary interactions as described by an observer *solely in his or her own frame*. That is the essence of Bell's approach: that while it isn't *necessary* to use any one inertial frame of reference to describe the physics (for that would be to privilege that frame above the others), it is *sufficient* to use only one frame, for each is just as good as another.

This is a point missed by Janssen, who argues that "if an effect can be defined away by a mere change of convention about how to slice Minkowski space-time, then that effect is purely kinematical"[12, p. 63]. Janssen's view suggests that we must examine a phenomenon from a co-moving reference frame in order to understand what is "really" going on. Conversely, Brown's moral is a consequence of taking Einstein's Principle of Relativity really and truly seriously.

The lesson, then, from all this talk of kinematics vs. dynamics, principles vs. constructions, and so on, can be concisely stated: relativity, viewed as part of a broader investigation into the quantum field theoretic nature of particles, is fundamentally a theory about matter and how it interacts. While it may be formulated in terms of space-time geometry, viewing it as a theory *about* space-time geometry is putting the cart before the horse [5, p. 12]. Building a picture of relativistic physics from the bottom-up in this manner does, admittedly, lack some of the elegant conciseness of Minkowski's geometric formulation. Nonetheless, as John Bell said: "The longer road sometimes gives more familiarity with the country [1, p. 77]."

3 Simultaneity in special relativity

Having broadly examined Brown's views on space-time, along with some of the responses these views have received, I move on to a narrower question: what does in mean in special relativity when we say that, according to a particular frame, two distant (i.e. space-like separated) events are simultaneous? It is of fundamental importance in relativity that the labelling of such events as simultaneous must, at the very least, be frame dependent. Otherwise, the invariance of the speed of light and the rejection of absolute motion as a meaningful concept are mutually inconsistent [3, p. 96]. However, one can go further and propose that even the assignments of simultaneity *in a particular reference frame* is purely a convention.

Both a conventional and a non-conventional interpretation can be squared with relationalist views of space-time such as Brown's—though the latter would presumably require the attribution of spatiotemporal properties to matter *à* la Frisch—but the former is Brown's own view [*Id.*] and does seem to be the more natural interpretation to pair with Brown's space-time ontology. However, I will argue that the conventional view weakens Brown's moral to some degree, lending some support to Norton's accusation of operationalism.

I explore this issue in detail in Section 3.1. First, referring to Brown and to a review article by Allen Janis [11], I sketch the background of the conventionality debate in Section 3.1.1. Then, in 3.1.2 I discuss a key theorem taken by many to support the non-conventional position, as well as Brown's response. Section 3.1.3 examines the relationship between the conventionality thesis and Brown's constructive approach to relativity. Finally, I discuss in Section 3.2 the impact of these questions on how SR is taught—ultimately bringing us back to Bell and his pedagogical concerns.

3.1 The conventionality question

3.1.1 Background

The debate regarding the conventionality of simultaneity for distant events has—like most of the ideas discussed in this paper—its antecedents in the early days of SR's formulation. Indeed, it predates SR and was considered by Poincaré even before the problems with a universal (i.e. acceptable to all inertial observers) definition of simultaneity were realized [11, Sec. 1]. Einstein was aware of the problem in 1905 and viewed it as a conventional choice, necessary to derive the relations that allow transformations between reference frames:

> If at the point A of space there is a clock, an observer at A can determine the time values of events in the immediate proximity of A by finding the positions of the hands which are simultaneous with these events. If there is at the point B of space another clock in all respects resembling the one at A, it is possible for an observer at B to determine the time values of events in the immediate neighbourhood of B. But it is not possible without further assumption to compare, in respect of time, an event at A with an event at B. We have so far defined only an "A time" and a "B time." We have not defined a common "time" for A and B, for the latter 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. [Emphasis preserved from the original German] [8]

Einstein's solution, then, was to establish a constructive procedure for synchronizing distant clocks. We take a ray of light to leave from A at "A time" t_A, to be reflected at B at "B time" t_B, and to be received back at A at t'_A. Then Einstein (and generations of physicists after him) defined the clocks at A and B to be synchronized if

$$t_B - t_A = t'_A - t_B. \tag{2}$$

This is referred to by Brown as the "Poincaré-Einstein convention" [3, p. 46] for synchronizing clocks. According to this synchronization procedure, the times t_B and $\frac{t_A+t'_A}{2}$ are assigned to be simultaneous in this reference frame (that is, the frame in which the transmission and receipt of the light ray at A occur at the same location). Brown notes further that while the postulates of SR only imply that the two-way, or round trip, speed of light be invariant, the Poincaré-Einstein convention is equivalent to requiring the $one - way$ speed of light be invariant and equal to the two-way speed, c [3, p. 77].

This synchrony convention allows one to assign space-time coordinates to all events in an inertial reference frame. Even better, having established the convention, one can go about determining how to transform the coordinates of one frame into the coordinates of another (in a way that preserves the space-time interval, which we encountered back in Eq. 1) and thus arrive at the familiar Lorentz transformations[19].

However, let us take a step back and consider what happens if we do not fix the one-way speed of light to be invariant as c. As SR still requires the invariance of the two-way speed of light, this is equivalent to allowing the one-way speed of light to be anisotropic.[20] Following Brown [3, p. 96], we can do this by considering a new set of coordinates for a particular inertial frame:

$$\tilde{\vec{x}} = \vec{x} \tag{3}$$

$$\tilde{t} = t - \vec{k} \cdot \vec{x} \tag{4}$$

[19] To quote an old professor of mine from an undergraduate course on electrodynamics: "It was a big problem in physics in the nineteenth century to figure out the symmetries of Maxwell's equations. They were not invariant under the Galilean transformations like the rest of known physics—big problem! But Poincaré thought long and hard and he answered this important question. Poincaré worked out how the laws of electromagnetism transform: they're called the 'Lorentz transformations'. Sometimes ... history is unkind."

[20] In the case of Einstein's synchronization procedure, this amounts to allowing the light to take longer to go from A to B than to go from B to A, or *vice versa*.

where $\vec{k}$ is a constant vector.[21] Then, using $\tilde{c}_+$ for the one-way speed of light from A to B (with respect to the new coordinates) and using $\tilde{c}_-$ for the speed from B to A, it is straightforward to show that:

$$\tilde{c}_\pm = \frac{c}{1 \mp ck} \tag{5}$$

where $k = |\vec{k}|$. Equivalently, we can define, for notational convenience, a parameter $\epsilon = \frac{1}{2}(1 + ck)$, according to which:

$$\tilde{c}_+ = c/2\epsilon \tag{6}$$

$$\tilde{c}_- = c/2(1-\epsilon) \tag{7}$$

In the original synchronization procedure, let the round trip time of the light ray's travel, $t'_A - t_A$, be denoted T. Then according to our new coordinates:

$$t'_B = t'_A + \epsilon T. \tag{8}$$

This parameter ϵ provides a convenient short-hand for discussing the effect of assigning different anisotropies to the one-way speed of light. Clearly, the Poincaré-Einstein convention is equivalent to the choice $\epsilon = 1/2$. However, we might ask what happens to the two-way speed of light for a different choice of ϵ. Denote the distance from A to B (in this frame) as L. Then the new two-way speed of light, $\tilde{c}$, is:

$$\begin{aligned}\tilde{c} &= \frac{2L}{L/\tilde{c}_+ + L/\tilde{c}_-} = \frac{2}{1/\tilde{c}_+ + 1/\tilde{c}_-} \\ &= \frac{2c}{2\epsilon + 2(1-\epsilon)} = c\end{aligned}$$

Hence, this new simultaneity convention is consistent with the invariance of the two-way speed of light.[22] All the dynamical predictions of SR that follow from this round-trip invariance, such as clocks running at different rates, are reproduced exactly with no dependence on the synchrony convention chosen [11, Sec. 1]. Indeed, one may even be a bit perverse and choose different synchrony conventions for different frames with no ill effects—and a judicious exploitation of this fact even allows one to eliminate the relativity of simultaneity and one-way time dilation between particular frames [3, p. 105][17, p. 386].

[21] Strictly speaking, it need not be constant. However, the analysis is simpler if it is and still demonstrates the main point.

[22] That is, this calculation shows the new convention is consistent with two-way speed of light being c. The invariance of $\tilde{c}$ then follows the invariance of c in the old coordinates.

The question, then, is whether the one-way speed of light can be directly measured, thus settling experimentally what value ϵ should take; the answer appears to be 'no' [11, Sec. 2]. The issue is that, unlike a round-trip measurement, performing a one-way measurement invariably requires the use of clocks at different locations. Determining a time interval using two different clocks requires the clocks to be suitably synchronized. Thus far, the only synchronization procedure I have discussed is equivalent to choosing a convention for the one-way speed of light, leading us in a full circle. So, the question may be rephrased: is there a different, *convention-free* scheme to synchronize distant clocks that we can avail ourselves of?

One of the most widely discussed schemes is the slow transport of clocks. This procedure starts with two clocks in immediate proximity to one another at A which—as Einstein noted above—allows direct synchronization. Then one clocked is moved very slowly,[23] so as to avoid corruption of the synchronization by time dilation, until it reaches B. The conclusion: the clocks at A and B are synchronized and no assumptions have been made about the one-way speed of light. Janis notes one issue with this is that "until the clocks are synchronized, there is no way of measuring the one-way velocity of the transported clock" [11, Sec. 3]. Alternative, more elaborate schemes, have been proposed but critics have claimed, Janis notes, that "nontrivial conventions are implicit in the choice to synchronize clocks by the slow-transport method" [*Id.*]. Ultimately, there hasn't yet been a clock synchronization scheme, purported to be free from either the one-way speed of light convention or an equivalent convention, that has satisfied the proponents of conventional simultaneity.

In the absence, then, of an empirical test of the one-way speed of light, attempts have been made to deduce the nature of simultaneity in SR with reason alone (in conjunction with already-known empirical facts). Hans Reichenbach, whom the ϵ-notation is due to, argued that it is incoherent to unambiguously assign simultaneous times by any means to events that are causally disconnected from one another [11, Sec. 1]. For a long time, this view was the orthodox position. However, a landmark theorem was proved by David Malament in 1977 in which the standard (Poincaré-Einstein) synchrony and the basic light-cone structure of SR were shown to be intimately connected. Malament's work caused a resurgence of interest in the non-conventional interpretation of simultaneity.

[23] In theory, 'very slowly' means 'in the limit of vanishing velocity'. In practice, the precision of one's measuring devices would dictate what is slow enough.

3.1.2 Malament's theorem and Brown's response

As far as the importance of Malament's work goes, Brown characterizes the theorem as "a result which virtually single-handedly managed to swing the orthodoxy within the philosophy literature from conventionalism to anticonventionalism." [3, p. 98]. The foundation for the theorem was a program undertaken by Cambridge physicist Alfred Robb who, in a series of papers from 1911 to 1936, set out to axiomatize the geometry of special relativity. That is, he aimed to do for Minkowski space-time what Euclid (and, later, Hilbert) had done for familiar three-space. Robb succeeded in defining a notion of orthogonality in (3+1)-dimensional space-time that, it was later observed, was tied to the Poincaré-Einstein convention of $\epsilon = 1/2$:

> Specifically, imagine an inertial world-line W and any point p on W; then the set of points q such that the straight line joining p and q is orthogonal to W in Robb's sense turns out to be just the set of all points simultaneous with p according to the Poincaré-Einstein convention in the inertial rest frame of the free particle whose world-line is W. [*Id.*]

The key aspect of Robb's result is that his axiomatization of space-time—and, by extension, his notion of orthogonality—was constructed *solely* from the causal connectibility of space-time points. That is, from the light-cone structure of Minkowski space-time, which is convention-free.

While an important result in its own right, Robb's work on space-time orthogonality does not, by itself, have any implications for the simultaneity debate. That one *can* define a simultaneity relation in terms of the causal structure of space-time is interesting, but the question is whether one can do so *uniquely*. For, if other values of ϵ are also compatible with the causal structure, then we are right back where we started. Malament's theorem was an affirmative answer this question, in which he "argues that standard synchrony is the only simultaneity relation that can be defined, relative to a given inertial frame, from the relation of (symmetric) causal connectibility" [11, Sec. 4]. That is, Malament demonstrated that Robb's notion of orthogonality to the world-line W is the only non-trivial one that can be defined solely using W itself and the causal structure of space-time [3, p. 98].

Janis cites a number of subsequent authors who took Malament's theorem to conclusively settle the simultaneity issue [11, Sec. 4]. However, he also emphasizes that conventionalists have criticized Malament's result for a number of reasons.[24] A conservative reading of the situation would be

[24]Noting, though, that at least some of the reasons are flawed.

that the implications of Malament's theorem are controversial. Of particular importance to my discussion is Brown's response to Malament:

> Why should we consider defining simultaneity just in terms of the limited structures at hand in the Grunbaum-Malament construction, namely an inertial world-line W and the causal, or light-cone structure of Minkowski space-time? Part of the answer is already obvious in Malament's paper: W is taken to represent an inertial observer, and we are after all talking about simultaneity relative to such an observer. But in the real world there is a lot more structure for the observer to observe: is none of this relevant? [3, p. 100]

Brown asks us to consider the world for which Malament's theorem is proved, consisting solely of W and the light-cone structure.

Recall, when I examined Norton's critique of Brown in Section 2.2 that he describes a world like Malament's and claims that it poses a serious problem for a constructivist's view of time:

> The constructivist must, in effect, say that the entire scenario envisaged is impossible. The notion that there can be a part of spacetime without matter or a part of spacetime with static matter is simply a confusion on the part of realists. It makes no sense to talk of time in such scenarios.[14, p. 832]

Norton is unimpressed by this defence, dismissing it as "extreme operationism". And yet, as Norton must be aware[25], this is *precisely* the "out" Brown employs:

> The Malament world is so utterly different from ours, I think it is legitimate to ask whether it even contains time at all. It is not enough to say that being four-dimensional, the space-time manifold therein has time built into it. We are doing physics, not mathematics ... The conformal light-cone structure is in itself timeless. It has no non-trivial dynamics. Supposedly there is also a particle or observer in motion, but in motion relative to what? There can only be one answer: in relation to the space-time manifold. But if Malament's world is anything at all like ours, this is not a notion that today, after the lesson of Einstein's hole argument has finally sunk in, is widely regarded as physically meaningful. [3, pp. 100-101]

[25] Again, since he refers in his footnote 13 to the very excerpts of Brown that I am quoting.

Norton appears to view his accusation of operationalism in this defence as a sort of *reductio ad absurdum*. So, the next question is whether Norton is right and Brown's dismissal of Malament necessitates a departure from physical realism well beyond just Brown's objections to an independent space-time. "The assertion," according to Norton, "must be that it makes no sense to speak of times elapsed unless a clock or the change in some material process actually measures the times elapsed" [14, p. 833]. I think that, at the very least, the accusation is overstated. Brown's reason for denying the passage of time in this scenario is not the absence of a physical clock along W; it is the total absence of *anything* that could be considered dynamical in this scenario. While there may be elements of operationalism in Brown's views—particularly with respect to the immeasurable one-way speed of light—it is not clear to me that Norton has established: (a) the operationalism is as radical as he alleges, and (b) that even if it is, that such a categorization should be seen as a genuine problem for the ontology.

3.1.3 Tension with constructive relativity

There is a sense in which Brown's moral is in tension with the conventionality interpretation of simultaneity. It is not a contradiction *per se*, but I would argue that Brown's moral is weakened a bit by the conventionality thesis. It will be helpful to re-examine a comment from Brown and Pooley's 2001 paper (quoted first in Section 2.2.1) to illustrate what precisely they hope to accomplish with a constructive approach to relativity that emphasizes dynamical interactions over geometry:

> The geometrical features of the objects that are assumed, and appealed to, in these [space-time based] explanations are similar in status to the postulates of principle theories. They do not, *directly*, concern the details of the bodies microphysical constitution. Rather they are about aspects of their (fairly) directly observable macroscopic behaviour. And this reflection prompts an obvious question: why do these objects obey the constraints of Minkowski geometry? It is precisely this question that calls out for a constructive explanation. [4, p. 9]

This emphasis on understanding how relativistic behaviour arises from from a detailed understanding of the microscopic matter (or at least the *possibility* for such an understanding in Brown's truncated Lorentzian pedagogy) suggests a criterion for what makes for a good constructive explanation: a degree of uniqueness. Of course, in relativity this is necessarily a frame-dependent criterion. Bell's entire point was that a complete physical understanding of relativistic kinematic phenomena may be obtained from dynamical laws in a *single* frame. Still, *within* that single frame, it would be preferable to have a constructive explanation that is free from arbitrary conventions. Otherwise, precisely how meaningful is it to say that certain macroscopic behaviour occurs *because* of a particular set of microscopic interactions if those interactions only happen on paper after the physicist has adopted a particular convention?

Of course, this hints at the lesson of general covariance in GR: we should not rely on coordinates to actually explain anything. Rather, relativity is best understood as the theory of invariants: explicitly local measurements made by observers of proper time and distance, sent and received light signals, and so on. Hence, there is one point on which Brown's thermodynamic analogy fails. When Boltzmann showed that entropy arises from statistical considerations of large numbers of particles, his derivation of entropy was constructive in a very literal sense. Certain concrete events were happening at a low level of the description and yielding certain system-wide behaviours at a higher level description. The conventionality of simultaneity makes such a literal understanding problematic for how relativistic geometry constructively arises from the interactions of Lorentz covariant matter. This is not a catastrophic problem for Brown's moral; however, it does seem to dull the point a little.

3.2 Pedagogical consequences: how not to teach special relativity

To conclude these interpretational discussions, I briefly return to the pedagogical concerns of John Bell. An introduction to SR typically involves a selection from a standard canon of so-called "paradoxes". Each involves a scenario in which the features of SR seem, when naïvely misapplied by the beginning student, to lead to a contradiction between two or more reference frames. Of course, they are not true paradoxes: without fail, when the mechanics of SR are applied *correctly* the apparent contradiction vanishes. The didactic utility of the paradoxes is to essentially illustrate Bell's point: that one can use any inertial frame in which to do the analysis.

Despite their non-paradoxical nature in actual fact, there still remains a question in many cases of how to actually *do* the analysis in a given frame in a way that satisfies the student that relativity is consistent. Consequently, the paradoxes have been some of the most popular subjects for academic articles on SR for as long as they have been around. The so-called "twin paradox" in particular has received a great deal of attention due to its connection with the relativity of simultaneity. I have already outlined in Section 2.1.4 what a valid reading of length contraction is according to Brown's view: according to a frame in which length contraction of some object is measured[26] to occur, there is a genuine physical contraction due to the Lorentz covariance of the matter composing the object. As a result of Brown's views on simultaneity's conventionality, he cautions that "explanations of synchrony-independent phenomena in SR that rely crucially on the relativity of simultaneity are not fundamental [emphasis removed] [3, p. 105]." Hence, it is worthwhile to consider what one should replace such explanations *with*. After a few remarks about the postulates of SR, I will comment on several pedagogical approaches to the twin's paradox and discuss the implications of Brown's perspective.

3.2.1 Postulates

At the heart of SR are two postulates: the principle of relativity and the light principle. As we have seen, some other milder assumptions—like spatial isotropy—are needed to derive the familiar mathematics of SR; however, the real heart of the theory lies in these two postulates. The first, the principle of relativity, is powerful, far reaching, and yet extremely simple. In his 1905 paper, Einstein phrased it thus:

> The laws by which the states of physical systems undergo change are not affected, whether these changes of state be referred to the one or the other of two systems of co-ordinates in uniform translatory motion. [8]

This is often equated with the equivalent—but slightly more intuitive—statement that there is no experiment that can be performed by someone moving inertially that will allow them to determine if they are at rest.

The second postulate has lent itself to a great deal more confusion than the first. Referring again to Einstein:

> Any ray of light moves in the "stationary" system of co-ordinates with the determined velocity c, whether the ray be emitted by a stationary or by a moving body. [*Id.*]

This is, on the surface, not a surprising claim. Water waves emitted from a boat travel at the same speed through the water regardless of whether

[26] The distinction between 'measured' and 'observed' is an important one here. The latter may imply visual inspection which, due to the finite propagation of light, brings its own set of complications.

the boat is in motion through the water. It is only absent a medium for the light to travel through—as the water waves travel through the water—that the statement takes on an unusual flavour. Einstein's innovation was to avoid any talk whatsoever about the medium. The luminferous aether, which invariably is discussed as historical context when SR is taught, is indeed superfluous in his formulation. However, he doesn't explicitly reject it; he doesn't need to.

Despite the light principle's rather humble statement, Brown notes that:

> It is often wrongly claimed that Einstein's light postulate is the stronger claim that the light speed is invariant across inertial frames. The advantage of his postulate as it stands is that it is logically independent of the [principle of relativity]. This meets an obvious desideratum in a semi-axiomatic derivation of the new kinematics of the type Einstein was constructing. [3, p. 76]

It is only when the two postulates and Maxwell's equations of electrodynamics are taken together that the universal invariance of the speed of light is deduced[27]: not as a postulate of SR but as a theorem.

Given how singularly bizarre the constancy of c appears to new students (and even to seasoned veterans) due to its divergence from the everyday experience of velocity, it is somewhat comforting that it need not be baldly asserted as a postulate, but rather may be deduced from less shocking principles. From a pedagogical standpoint, it is advisable to keep in mind the distinction between the light postulate and the invariance of the speed of light. With respect to Brown's emphasis on dynamical theories over abstract principle theories, beginning with principles that are even more abstract than strictly necessary is not an auspicious start for guiding students to a constructive understanding of relativity's postulates.

3.2.2 The twin paradox

Scenario: Alice and Bob are twins living on earth. They synchronize their watches and Bob gets into a spaceship and flies off at $0.8c$ to Planet X four lightyears away (in Alice's frame). Once he gets there, he immediately turns around and flies home at the same speed. At this point, Alice will have aged ten years. However, time dilation means that during the trip, Alice will determine that Bob's clocks run slower than hers by a factor of 0.6. Hence, she deduces that when Bob returns, he will have aged a mere six years. And

[27] It is obviously the absence of a 'Maxwell-like' frame-independent expression for water waves that prevents the same conclusion being reached for water waves off a boat.

yet, from Bob's perspective, it was Alice who was in motion and whose clocks should have run slower. Thus, Bob reasons, Alice should be the younger twin when he returns. Who is right?

As is well-known, Alice is correct. However, why is Bob wrong? The simple answer is that Bob's trip requires two separate inertial frames—he does not stay in the same one the whole time—since he turns around to come home. Hence, his naïve application of the time dilation formula to Alice is incorrect. If we determine what the twins actually see by having them exchange light pulses throughout the trip, we find, using the relativistic Doppler shift, that their observations are consistent with Alice aging ten years and Bob aging six years. However, suppose we are willing to treat Bob's frame with the subtleties it requires and wish to explicitly calculate what happens in *Alice's* (inertial) frame according to *Bob's* (non-inertial) frame.

Redhead and Debs [17] note two common approaches: treating Bob's acceleration like a pseudo-gravitational field and using the relativity of simultaneity. They dismiss the former explanation on the grounds that clever reformulations of the problem using a third twin or alternative space-time topologies eliminate Bob's need to accelerate to return home. Nonetheless, in all these variants some asymmetry between Alice and Bob remains (or there would be a genuine paradox!). Brown characterizes the latter explanation as an illustration of his cautionary note against simultaneity-based explanations in SR:

> A common example concerns the clock retardation effect, or twins paradox, where it is claimed that at the point of turn-around of the travelling clock, the hyperplanes of simultaneity suddenly change orientation and the resulting 'lost time' accounts for the fact that the clocks when reunited are out of phase. It is worth bearing in mind that the clock retardation effect, like any other synchrony-independent phenomenon in SR, is perfectly consistent with all the non-standard transformations in this section, including those which eliminate relativity of simultaneity. [3, p. 105]

Redhead and Debs agree with Brown on this point—that the standard simultaneity explanation cannot be a really fundamental account of Alice's aging faster, on average, than Bob (as Bob computes everything). However, they take the idea in a direction that does not square well with Brown's constructive relativity. They demonstrate how different simultaneity conventions (i.e. different choices for ϵ) lead to radically different step-by-step accounts of what happens during Bob's trip; and yet, the end result is always the same: Bob ends up four years younger than his sister. They do not conclude from the smorgasbord of possible (but contradictory) narratives based on different simultaneity conventions that the whole approach is a dead-end. Rather, they develop a geometric illustration for the convention-dependence of the sequence of events. They ascribe genuine explanatory import to the geometric picture: "The overall twins aging is *caused by the horizontal dash line segment* over which, from the Earth, the traveler's clock stands still [emphasis mine] [17, p. 388]." This is precisely the fetishization of geometry that Brown argues against when rejecting Minkowski's formulation of SR as genuinely constructive.

The conclusion compatible with Brown seems to be a minimalist one: it simply does not make sense to ask how Bob's and Alice's aging proceeds alongside one another. The only invariants in relativity, after all, are those measured locally. Unfortunately, it is not easy see, then, how one could provide the sort of *constructive* explanation for Alice's age that is valid according to Bob. This is the weakening of Brown's moral I noted in Section 3.1.3.

It is simpler, at least, to constructively explain in Bob's frame why his trip takes six years, and not ten: in both inertial frames he occupies during the trip, the distance from earth to Planet X is length contracted. We can use the usual constructive explanation provided by Bell for this, with one caveat: there is no intervening matter between the two planets that can contract inter-atomically. Hence, the best we can do is accept that both planets may together be described by some (very complicated) quantum state; that is, we can follow Butterfield's sketch of the *truncated* Lorentzian pedagogy outlined in Section 2.1.4.

4 Concluding remarks

A great deal of ground has been covered in this paper, and much of it far more briefly than the material merits. The references, of course, provide fertile ground for the keen reader who desires to delve more deeply into the issues I have discussed. While the majority this paper is an overview of other peoples' arguments for and against various theses, my own positions are present to varying degrees throughout (through both editorial selection and explicit comments on particular claims). It is my hope that somewhere in this survey there is some novel thinking—however minor it may be.

There is, naturally, ongoing dispute in the literature about nearly every topic I have examined in the preceding pages. One cannot help but read the concluding remarks of Redhead and Debs' 1996 paper on the twin paradox with a bit of amusement at the wild optimism:

> Perhaps the method discussed in this paper, the conventionality of simultaneity applied to depicting the relative progress of two travellers in Minkowski space-time, will settle the issue of the twin paradox, one which has been almost continuously discussed since Langevin's 1911 paper. [17, p. 391]

If there is central point to this somewhat disconnected overview of several distinct issues, it is Brown's moral, discussed in Section 2.3. It is a worthwhile reminder that we physicists are ultimately engaged in the business of matter theories and should be cautious about leaving our roots behind. Brown's view, that matter is fundamental with respect to geometry in relativity theory, is clearly a contentious one. What should be less contentious is which of the two is the physicist's ultimate object of study. There is, I think, great wisdom in the suggestion that if having an understanding of matter and its motions, interactions, and deeper intricacies is the ultimate goal of a physicist, then matter itself is a sensible place to start.

5 Acknowledgments

I am indebted to Dr Jeremy Butterfield and Dr Adam Caulton for their immensely helpful conversations and criticisms about this essay. Their patience with some of its earlier drafts is particularly laudable. I gratefully acknowledge the conversations with my Part III colleague Mr Kane O'Donnell as we both explored this unfamiliar territory. Furthermore, I owe much thanks to Dr Christopher Brookes, Director of Studies for mathematics students at Corpus Christi College, Cambridge, as well as to the college's graduate tutorial office. Corpus Christi's academic, financial, and pastoral support during the period of this essay's composition meant a great deal to me.

I am, in addition, grateful to the Natural Sciences and Engineering Research Council of Canada, whose funding has supported my Part III Mathematics studies at Cambridge and, by extension, this essay.

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