Geometrical constructivism and modal relationalism: Further aspects of the dynamical/geometrical debate

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Abstract

I draw together some recent literature on the debate between dynamical versus geometrical approaches to spacetime theories, in order to argue that (i) there exist defensible versions of the geometrical approach; (ii) these versions of the geometrical approach can provide constructive explanations (in the sense of Einstein) of dynamical effects; (iii) light can be shed upon different relationalist views about spacetime which have been articulated in the context of this debate by appeal to the distinction between modal versus non-modal relationalism.

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1 Introduction

This paper regards the debate between 'dynamical' versus 'geometrical' approaches to spacetime theories, as initiated by Harvey Brown and Oliver Pooley in [8, 9], and later developed by the former in his book, *Physical Relativity* [6]. The debate is subtle and intricate, mixing issues of ontology, explanation, and operationalism—but the central question at hand is easy to state: "Does spacetime's having the structure that it does explain the dynamical behaviour of matter, or vice versa?" According to advocates of the dynamical approach, dynamics explains spacetime structure; according to advocates of the geometrical approach, the reverse is true.

In order to make progress in establishing which of these two answers is to be preferred, philosophers have made important clarifications along a number of distinct axes. First, different relevant senses of explanation—see [15, 16, 19].[1] Second, the role of Einstein's principle/constructive theory distinction in this debate—see [2, 22, 55]. Third, the interplay between this debate and the substantivalism/relationalism debate—see [2, 39]. Fourth, the articulation of different possible versions of the geometrical approach—see [45].

My purpose in the present paper is to show how the above four themes can be brought together in illuminating ways. First, I make explicit how two of the most prominent advocates of the 'geometrical' approach—Michel Janssen [24] and Tim Maudlin [29]—fit into the above framework. Second, I show that the dynamical/geometrical debate can be decoupled from the substantivalism/relationalism debate, thereby nuancing claims by Acuña [2]. Third, I argue that defensible versions of the geometrical approach *can* qualify as constructive theories, in the sense of Einstein. Fourth, I show how a classical distinction between 'modal' versus 'non-modal' relationalism can be brought to bear in articulating the difference between the views of Janssen and Brown. Although this paper is—I concede!—'one more epicycle' on an already-turgid literature, in my view there remain important points to be made on these matters; the ever-increasing size of the literature on these issues is testament first-and-foremost to the depth and subtlety of the issues at hand.

2 Two debates

In this section, I discuss the overlap between the dynamical/geometrical and substantivalism/relationalism debates; along the way, we will see where the views of Maudlin [29] and Janssen [24]—two notable advocates of the geometrical approach—diverge.

2.1 Dynamical and geometrical approaches

I begin with the dynamical/geometrical debate, following the presentation given in [45]. On this setup, the dynamical approach involves two claims:

[1] It is now widely agreed that, whatever is the relevant sense of explanation at play, it must be non-causal. See [48] for a recent collection on 'explanation beyond causation'.

1. Fixed fields (i.e., fields fixed identically in all kinematical possibilities of a given theory—cf. [40, p. 115]), such as the Minkowski metric field η_{ab} of special relativity, are to be ontologically reduced to the symmetries of the dynamical equations governing matter fields (the dynamical view is, therefore, a modern form of relationalism—cf. [39, §6.3.2]).[2,3]
2. Ontologically autonomous metric fields, such as g_{ab} in general relativity, do not have their 'chronogeometic significance'—i.e., are not surveyed by physical measurement apparatuses—of necessity (i.e., in all solutions of any theory in which they appear).[4]

Focussing on (2), advocates of one version of the opposing *geometrical approach* to spacetime theories would state that ontologically autonomous metric fields, such as g_{ab} in general relativity, *do* have their chronogeometic significance necessarily. This version of the geometrical approach was labelled the 'unqualified geometrical approach'. However, in [45], it was argued that this particular version of the geometrical approach is not viable—precisely because there exist problem cases for such a view, in which one has a metric field g_{ab} in one's theory, but that structure is not surveyed by physical rods and clocks. For this reason, I focus in the remainder of this paper on what was argued in [45] to be a viable version of the geometrical approach—which was dubbed the 'qualified geometrical approach'. This version of the geometrical approach (which, for brevity in the ensuing, I simply refer to as the 'geometrical approach') accepts (as with the dynamical approach) (2), but rejects the ontological reduction presented in (1). That is to say, this version of the geometrical approach rejects the *particular form* of relationalism embodied in (1): the reduction of spacetime structure to dynamical symmetries (but, at this point, the possibility is still open that the advocate of the geometrical approach might embrace *other* forms of relationalism—see below); moreover, while the advocate of this version of the geometrical approach concedes that a given piece of spacetime structure does not, absent further stipulations, explain the particular behaviour of matter fields, it may be the case that, *once certain further stipulations are laid down*, legitimate such explanations can be given. For example, if one stipulates that in special relativity matter may only couple to the Minkowski metric field (see, for example, Maudlin: "... the fundamental requirement of a relativistic theory is that the physical laws should

[2]Two points here. First: throughout this paper abstract index notation is deployed. Second: the notion of a fixed field is still rather unclear in the literature; I have included an appendix on this notion, to which I refer the reader for further details. Reference to the notion of a fixed field is part of my reconstruction of the dynamical approach, but I think that other classes of objects—in particular, those which violate what Brown calls the 'action-reaction principle' (cf. [7])—would serve equally well. (I thank an anonymous referee for prompting me to write the appendix on fixed fields.)

[3]There are many open questions here. For example: which symmetries are relevant—'internal' or 'external'? (For discussion of this issue, see [14].) Second: how is this ontological reduction supposed to work? (To my mind, one of the most promising approaches is the generalised Human strategy defended in [50].) Third: how is the dynamical view supposed to distinguish between spacetimes without symmetries? And fourth: can the ontological reduction aspect of the dynamical view be extended from geometry to topology? (For discussion on this matter, see [30, 32].)

[4]This latter point is what Butterfield calls in [11] 'Brown's moral.'

be specifiable using only the relativistic space-time geometry. For Special Relativity, this means in particular Minkowski space-time." [29, p. 117]), *then* (according to the advocate of this version of the geometrical approach) that spacetime structure may be appealed to in legitimate explanations of the behaviour of matter.[5]

As a presentation of the dynamical and geometrical views, the above will suffice for my purposes in this paper—and to be explicit: in my view, the qualified geometrical approach is the *correct* way of thinking about the geometrical approach (see [45] for extensive argumentation to this end). Before proceeding to discuss the substantivalism/relationalism debate and its interaction with the above views, though, let me make one remark on general relativity. I have sympathies with the authors of [39, 45] when they claim that, in general relativity—which features no fixed fields—the dynamical and geometrical approaches (at least when the geometrical approach is taken in the defensible form articulated above) *do not differ*: both maintain that the metric field does not have its chronogeometric significance necessarily, and since there are no fixed fields in that theory, the ontological reduction of (1) does not apply.[6] For this reason, in the remainder of this piece I focus on the dynamical/geometrical debate in the context of theories with fixed fields—in particular, special relativity.

2.2 Substantivalism and relationalism

According to substantivalism, spacetime is an ontologically primitive element of reality; by contrast, according to relationalism, spacetime is ontologically reducible to (relations between) material bodies/fields. (For a virtuoso survey of the substantivalism/relationalism debate, see [39].) In [45], it was assumed that advocates of the geometrical approach must be substantivalists, and—in light of (1) above—advocates of the dynamical approach (at least in the context of theories with fixed fields, such as special relativity) must be relationists. Such views are echoed by Acuña, who writes, "Janssen's view is connected to a form of substantivalism, whereas

[5]Or at least, legitimate explanations of the symmetry properties of the dynamical equations governing matter. Even for a geometrician who makes the above further stipulations, there is no guarantee that there will exist (say) stable rods and clocks capable of surveying the piece of spatiotemporal structure under consideration (see [45]). This is the distinction between what is referred to in [47] as 'theoretical spacetime' (that structure whose symmetries coincide with the symmetries of the dynamical laws) and 'operational spacetime' (that structure surveyed by rods and clocks). One case in which theoretical spacetime seems to come apart from operational spacetime is massive scalar gravity—a case study explored extensively by Pitts [34, 35, 36, 37]. To be clear though: I do *not* regard such cases as being problematic for the viable versions of the geometrical approach discussed in this paper, which seek to appeal to certain pieces of geometrical structure to explain (whether constructively or not—see below) *some* aspects of the dynamics (in particular, the symmetries of the laws), and not (as made clear above) why that structure has chronogeometric significance (and, moreover, do so *without* assuming that said structure is the 'One True Geometry', thereby evading concerns raised in [37]).

[6]Oliver Pooley has argued to me that advocates of the dynamical approach should extend (1) to dynamical fields also—so that they should seek to ontologically reduce the metric field of general relativity also to the dynamics of matter. Though I agree in principle with this verdict, it remains to be shown in detail how this strategy can be realised in practice: Pooley has suggested a generalisation of the Humean strategy articulated in [50].

Brown's view assumes a form of relationalism." [2, p. 1]. As Acuña himself acknowledges, such claims stand in tension with Brown and Janssen's own written statements on the matter. For example, Janssen writes:

> I claim that Minkowski spacetime explains Lorentz invariance. For this to be a causal explanation, Minkowski spacetime would have to be a substance with causal efficacy. Like Brown, I reject this view ... . Minkowski spacetime explains by identifying the kinematical nature (rather than the cause) of the relevant phenomena. [24, p. 28]

And Brown writes:

> ... my book is not designed to be a defence of a Leibnizian/Machian relational view of space-time ... . Although I have sympathies for this view, in my opinion the dynamical version of relativity theory is a separate issue and can be justified on much wider grounds, having essentially to do with good conceptual housekeeping. [6, p. ix]

While ultimately I am essentially in agreement with Acuña on these matters, it is worth reflecting further upon how his claims regarding the substantivalism/relationalism debate interact with the above passages from Janssen and Brown. Beginning with Janssen, one might initially think that Acuña is not correct, for at this juncture the option seems to remain open that one might construe spacetime structure not as being an *entity* of some kind, but rather as being (e.g.) some kind of modal *constraint*, of the form 'the only possible laws are Poincaré invariant laws'.[7] If one thinks that such modal constraints (more on which later) are (i) consistent with a relationalist thesis, (ii) can do the explanatory work that advocates of the geometrical approach require of them, then it would seem that Acuña's claim that a geometrician *must* be a substantivalist is incorrect. Indeed, Janssen proposes (though ultimately rejects) just one such option, when he writes,

> Borrowing the language of Lange [27], one could say that the requirement of Lorentz invariance expresses a meta-law. A meta-law cannot be derived from mere ordinary laws (although there might be a deeper explanation of it). Even though it would serve my purposes, I hesitate to avail myself of Lange's machinery, as I fear that its operation commits one to the questionable notion of nomic necessity. [24, p. 3, n. 7]

So, Langian meta-laws seem to open the possibility of 'spacetime structure as modal constraints', and thus to open the door to 'geometrical relationalism'. What would Acuña say about such cases?

In his paper, Acuña distinguishes between substantivalism and what he calls 'absolutism'. While according to substantivalism, spacetime is an independent entity in one's ontology, according to absolutism—a more general thesis—spacetime is an independent entity, or *structure*, or (my extrapolation) *constraint*. Substantivalism is, therefore, one particular brand of

[7] Cf. [28].

absolutism.[8] Acuña's point here is that Janssen's other suggestions for the form that a geometrical approach could take are still *absolutist* theses.

I agree with Acuña (anticipating terminology which I recall to the reader in the following section) that any constructive version of the geometrical approach must be absolutist. However, it is in these terms that Acuña should have made the point—for he himself acknowledges that there exist possible non-substantival geometrical views! Thus, his claim that "Janssen's view is connected to a form of substantivalism" does not follow, even on Acuña's own terms, unless one pays careful attention to the qualifier, "a form of".

Turn now to Acuña's claims that advocates of the dynamical view are committed to relationalism. On this, Acuña writes,

> On the other hand, Brown's dynamical interpretation of special relativity is certainly committed to a relationist eliminative stance. The reason is that if a substantivalist version of his argument is asserted, the result is an ontological fluke. [2, p. 6]

Acuña has in mind here that, if the advocate of the dynamical approach reifies the metric field (e.g. the Minkowski metric field of special relativity), then it is unclear how or why the dynamical behaviour of matter should advert to this structure: they seem, rather, to be two independently-turning wheels in one's ontology. Here, however, it is worth noting two points. First, this is *exactly* the account which is given by the advocate of the dynamical approach in the context of general relativity—insofar as she does not take this to be problematic for her view in the context of general relativity, nor should she take this to be problematic for her view in the context of special relativity (see [6, 45]).[9] Second, insofar as an advocate of the dynamical approach does buy into a substantivalist thesis, she is rejecting (1)—in light of this, it is not obvious that there is any difference between this view and the most defensible version of the geometrical approach—recall the discussion of general relativity in §2.1. The real issue, then, is whether one thinks that (1) is an essential aspect of the dynamical view: transparently, Acuña thinks just this.[10] On the other hand, a dynamicist who denies this will disagree with Acuña that the dynamical approach presupposes relationalism.

2.3 Situating geometricians

Having now presented the dynamical and geometrical approaches, and shown their interactions with the substantivalism/relationalism debate, it becomes straightforward to distinguish different authors within the geometrical camp. First consider Maudlin. In his book, *Philosophy of Physics:*

[8] Although Acuña writes, "It would be better to use the term 'absolutism' for this basic thesis, so that 'substantivalism' would refer to a particular form of the absolutist thesis." [2, p. 6], I am not convinced that 'absolutism' is better nomenclature—it is too reminisient of the closely-related issue of the existence of 'absolute objects' in spacetime theories (cf. [33]). Perhaps simply 'generalised substantivalism' would be a better choice.

[9] It is worth pointing out that Acuña is not the only author to observe this cosmic coincidence—it was labelled in [46] the 'second miracle of relativity'.

[10] And I have sympathies with him—cf. footnote 6 above.

Space and Time [29], he writes,

> If we accept that in a vacuum there is no physical structure, except for the structure of space-time itself, then the behaviour of light in a vacuum implies that *the geometry of spacetime alone determines the trajectory of the light rays*. That is, given any point in the space-time p, the structure of space-time ought to fix where light emitted from that p (in any possible direction) will go. [29, p. 68] (Emphasis in original.)

This passage shows a clear commitment to a substantivalist thesis (witness the discussion of spacetime in the absence of matter—in which case, the former cannot be reduced to the latter). On the other hand, consider Janssen: we have already seen that, at least in print, Janssen is agnostic on the substantivalism/relationalism debate. However, as will be explained in the following section, if Janssen is to be regarded as offering a *constructive* explanation of the behaviour of matter in terms of spacetime structure, he must be understood as being an absolutist, in Acuña's sense (note, though, that this still does not commit Janssen to substantivalism).[11] It is this issue—connected closely to Einstein's famous distinction between constructive and principle theories—which we must now consider in detail.

3 Constructive relativity

In my view, the qualified geometrical approach is the correct way of articulating the geometrical approach. However, the view should not be construed as entailing a commitment to substantivalism. Rather, what I think should be said is the following: *if* a geometrician seeks to offer a *constructive* explanation of relativistic effects, then she must be an absolutist (which, recall again, is a broader thesis than substantivalism). However, even non-absolutist geometricians can offer certain kinds of explanations of relativistic effects—albeit not constructive explanations.

It is these issues which I consider in this section. I begin by recalling Einstein's distinction between principle and constructive theories, before arguing that absolutist geometricians can offer constructive explanations. I then consider the possibility of non-constructive geometrical explanations. I close by questioning Acuña's claim made at [2, p. 8] that not every principle theory (again in the sense of Einstein) need have an associated constructive theory.

3.1 Constructive explanations

It is well-known to authors engaged in the dynamical/geometrical debate that, in a 1919 article in *The Times*, Einstein drew a distinction between 'principle theories' and 'constructive theories' [18]. The former are theories which take phenomenologically well-grounded regularities, raise them to the status of postulates, and derive further empirical consequences therefrom. By contrast, the latter are theories which seek to provide an account of the physical goings-on underlying those observed regularities.

[11] On this matter too I am in agreement with Acuña [2].

The paradigm example of a principle theory is thermodynamics (proceeding as it does to explore the empirical consequences of its laws—themselves codifications of phenomenological regularities); the paradigm example of an associated constructive theory is the kinetic theory of gases—see [6, ch. 5] for further discussion.

The question to be addressed now is: can spacetime offer a *constructive* explanation of the dynamics of matter? Brown argues that such is not the case—and, therefore, that the geometrical approach fails. But is this too fast? In answering this question, it is useful to begin by reminding ourselves of what it is that both parties to the debate at hand might be seeking to provide a constructive explanation *of*. The answer (focussing, to repeat, on special relativity in the ensuing) is, two things in particular:

A. The existence of stable rods and clocks ...

B. ... which manifest special relativistic effects, such as length contraction and time dilation.

It is also useful to distinguish *constructive* explanations—explanations of physical phenomena which appeal to some underlying physical quantities, objects, or structures—from *fundamental* explanations—explanations of physical phenomena which appeal to the *fundamental microdynamics* governing the physical systems under consideration. Now, if we wish to explain both (A) and (B)—and therefore provide what Brown and Pooley call an *untruncated* account of the origins of special relativistic effects, then one must provide a fundamental explanation. As Brown and Pooley write:

> ... it is worth noting that the possibility of the existence of rods and clocks likewise does not follow from the mere assumption that all the fundamental laws are Lorentz covariant. It is only a full-blown quantum theory of matter, capable of dealing with the formation of stable macroscopic bodies that will fill the gap. [8, p. 10]

In a subsequent paper, Brown and Pooley continue:

> *Granted that there are stable bodies*, it is sufficient for these bodies to undergo Lorentz contraction that the laws (whatever they are) that govern the behaviour of their microphysical constituents are Lorentz covariant. It is *the fact that the laws are Lorentz covariant*, one might say, that explains why the bodies Lorentz contract. [9, p. 82] (Emphasis in original.)

The latter position here is what Brown and Pooley dub, coopting Bell's original terminology [3], the *truncated Lorentzian pedagogy*. The idea is that a certain assumption about the fundamental microdynamics—namely, that the laws governing matter fields are Poincaré-invariant—suffices to explain (B), and thereby to provide an explanation of special relativistic effects. This, however, does not provide a *fundamental explanation* of all aspects of the existence and behaviour of special relativistic bodies—for the full details of the fundamental dynamics are excluded; these details would be needed in order to explain (A). The first point I want to make here is the following: ultimately, all parties should agree that a fundamental explanation of special relativistic effects—accounting for both (A) and (B)—would be desirable.[12] Indeed, as Pooley puts it,

> [E]veryone should acknowledge that, if you want to explain why such a rod functions as a rod in the first place, you will have to say something about the details of its dynamics. The advocate of the geometrical approach, in particular, certainly shouldn't disagree with that. [41, p. 3]

The real issue at stake here (and the issue upon which I focus in the remainder) is whether the dynamical and geometrical explanations of (B)—i.e., the truncated Lorentzian pedagogy versus appeals to spacetime structure, respectively—can qualify as constructive explanations for (B).

Can spacetime structure also provide a constructive explanation (if not a fundamental explanation) of special relativistic effects, as the geometrician claims? My answer (*pace* Brown) is: *yes*.[13] Suppose first that one is a substantivalist about (in special relativity) Minkowski spacetime. Then, one can point to the coupling of matter fields to this spacetime structure to explain why the laws have the form that they do, and therefore explain why matter exhibits special relativistic effects (cf. [45, §5.2.1]).[14] Indeed, the same account will go through if such spacetime is construed as a modal constraint of the kind discussed above; it will, indeed, go through (I claim) on any reasonable absolutist account of spacetime structure. Indeed, in a sense such constructive explanations are 'one level deeper' than the constructive explanations offered by advocates of the dynamical approach: they explain why the fundamental laws governing matter have certain symmetries, and in turn explain why special relativistic effects are observed. (Note, though, that this is not to deny that appeal to the symmetries of dynamical laws is itself a legitimate constructive explanation.[15]) On

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[12]One attempt to give such an explanation was provided in [44].

[13]Frisch remarks that "it is not clear how well Janssen's classification of a spacetime account as constructive sits with Einstein's own characterization of constructive theories as building "up a picture of the more complex phenomena out of the materials of the relatively simple formal scheme from which they start out."" [22, p. 179] Frisch is correct here, if we read Einstein as identifying constructive and fundamental explanations. We have, however, already seen that these two notions can be teased apart.

[14]In the context of universally coupled massive scalar gravity (see [35, 36, 34, 37]), or bimetric theories more generally, one might be able to point to one metric field (say η_{ab}) to explain some aspects of the laws—but perhaps not all of them, in light of the coupling of the other metric field. This I take to be compatible with the qualified geometrical approach.

[15]That symmetry properties of dynamical laws can provide a suitable basis for constructive explanations is questioned in passing by Brown and Pooley, who write:

> [O]ne might be tempted to deny that explanations which appeal to an explanans as non-concrete as the symmetries of the laws are genuinely constructive explanations. In other words, it turns out that there are even fewer contexts than one might have at first supposed in which length contraction stands in need of a constructive-theory explanation. [9, pp. 82-83]

I do not agree with this verdict: the truncated Lorentzian pedagogy appeals to certain *aspects* of the fundmental dynamics in order to explain special relativistic effects. I do not see what more could be required of a (partial/truncated—not fundamental) constructive explanation.

Perhaps one reason for hesitance here is that one might think that, if symmetries of the laws can offer certain constructive explanations of dynamical effects, then, since spacetime structure codifies those dynamical symmetries, spacetime structure, *even for advocates of the dynamical view, or for non-absolutist geometricians*, would have to be understood as offering constructive explanathe other hand, suppose that one advocates the geometrical approach, but is a relationalist about spacetime. Then, I claim, one can *still* take spacetime to explain the form of dynamical equations, and so the behaviour of matter, but this will not be a *constructive* explanation—for there is no *element of reality* (whether an entity, as for substantivalists, or a constraint etc., as for other absolutists) which can be appealed to in order to account for why matter manifests special relativistic effects. Acuña hits the nail on the head here, when he writes:

> Now, if spacetime is not conceived as a self-standing entity, but as the formal encoding of spatiotemporal behavior, how can it be the constructive explanation of anything? The very definition of a constructive explanation requires that if spacetime is to be assigned a constructive explanatory role, then it must be hypostatized. If we accept Janssen's view that Minkowski spacetime structure encodes the standard spatiotemporal behavior of physical systems, it cannot be the constructive explanation of Lorentz invariance unless it is regarded as the reality that somehow determines such a behavior. [2, §4.1]

I am in full agreement will this point; I will turn to the kinds of non-constructive explanation which can be offered by geometrical relationists in the following subsection. What the discussion thus far highlights, therefore, is that many different kinds of constructive explanations are possible—in particular:

(a) Partial dynamical constructive explanations—e.g., appeals to the symmetry properties of the fundamental laws.

(b) Fundamental dynamical constructive explanations—e.g., appeals to the full dynamics governing matter, and how such dynamics are amenable to the construction of stable rods and clocks.

(c) Partial geometrical explanations—e.g., appeals to hypostatised spacetime structure and its coupling to matter to explain *why* dynamical laws have the symmetry properties that they do; or, appeals to modal constraints (such as Langian meta-laws) to explain why laws have the symmetry properties that they do.

Dorato has pointed out that there is no inconsistency in maintaining that both (a) and (c) can qualify as legitimate constructive explanations:

> Brown neglects the possibility that the relativistic 'deformations' could be given different explanations, one in terms of the four-dimensional structure of Minkowski spacetime, and one in terms of dynamical phenomena (provided that the latter are really to be counted in). [15, p. 100]

tions for certain dynamical effects. One natural response here is simply to deny that constructive explanation is conserved under codification. Then, on the dynamical view, although the symmetries of the laws can feature in constructive explanations, spacetime structure (which codifies those symmetries) cannot; similarly for non-absolutist geometrical views. In my view, such a response is reasonable: only aspects of the formalism of a theory with direct correlates in physical reality can offer constructive explanations. For dynamicists and non-absolutist geometricians, spacetime structure does not *itself* feature in constructive explanations—it only does so *qua* codification of certain other aspects of the physics.

Naturally, I agree with Dorato on this matter. With all of the above in hand, we must now take up the question of non-constructive geometrical explanations.

3.2 Non-constructive geometrical explanations

An advocate of the geometrical view does not have to embrace absolutism about spacetime structure, so long as she relinquishes the goal of providing a *constructive* explanation of relativistic effects. However, as discussed above, other varieties of explanation for such effects are available to relationalist geometricians. Consider, for example, the *structural explanations* of Dorato and Felline, following in turn the lead of Clifton:

> What are, however, structural explanations? A minimal definition of structural explanations was briefly provided by Rob Clifton:
>
> > We explain some feature B of the physical world by displaying a mathematical model of part of the world and demonstrating that there is a feature A of the model that *corresponds* to B. ([13, p. 7], our emphasis) [16, p. 3]

In our case, the fact that the Poincaré symmetries of the dynamical laws are captured in the structure of Minkowski spacetime—in the sense that the isometries of Minkowski spacetime *just are* the Poincaré symmetries—means, for Dorato and Felline, that the latter may be appealed to in explaining those dynamical symmetries, and so in turn in explaining the relativistic effects manifested by physical bodies. In my view, this is a completely legitimate sense of explanation—but note that there is no claim here that what is being offered is a constructive explanation. Indeed, Brown and Pooley seem to concede all this, when they write:

> Specific features of Minkowski geometry are appealed to in an explanation of why, *relative to surfaces of simultaneity orthogonal to the world-tube of R, S* is shorter than R whereas, *relative to surfaces of simultaneity orthogonal to the world-tube of S*, it is R that is shorter than S. [Footnote suppressed.]
>
> In our opinion these constitute perfectly acceptable explanations (perhaps the only acceptable explanations) of the explananda in question. But it is far from clear that they qualify as *constructive* explanations. [Footnote suppressed.] [9, p. 9]

So: structural explanations are one kind of non-constructive explanation which might be appealed to by relationalist geometricians.

A second natural way of cashing out non-constructive—but nevertheless legitimate—geometrical explanations would be to appeal to Friedman's thesis of explanation as unification [20]. The point here would then be that Minkowski spacetime, say, provides a codification and unification of otherwise disparate relativistic effects, and of the fact that otherwise disparate laws all manifest the same symmetry properties.[16] It is hard to deny that

[16]This latter aspect was referred to in [46] as the 'first miracle of relativity'—cf. footnote 9.

spacetime structure can be unificatory in this sense—and thus, it is hard to deny that spacetime structure can be explanatory, given Friedman's account of explanation.

Thus, there are at least two non-constructive accounts of explanation in science, to which geometricians (including relationalist geometricians) can appeal.[17] Given this, I now move on to consider the related issue of the explanatory value of principle theories versus constructive theories. Both Brown and Pooley [8, p. 74] and Janssen [24, p. 38, n. 27] dismiss principle theories as being explanatorily inferior to constructive theories. Insofar as principle theories cannot (it seems—see below) offer constructive explanations, I agree with them. However, there are many other senses in which principle theories *do* offer legitimate explanations of physical phenomena—perhaps explanation in one of the two senses considered above.[18] For example, consider Einstein's 1905 principle theory presentation of special relativity, proceeding from the starting point of the relativity principle and light postulate [17]. It is hard to deny that this theory, *qua* expression of universal Lorentz invariance, unifies and encompass a vast range (indeed, all) of special relativistic phenomena—thus, it is hard to deny that this principle theory offers explanation in the Friedmannian unificatory sense (the same, indeed, could be said for thermodynamics). Once again, authors have already made exactly this point. For example, Van Camp writes:

> Constructive theories are grounded in their ability to offer causal-mechanical explanations of phenomena, a type of scientific explanation most prominently advocated by Salmon [49].
>
> Principle theories are also explanatory. The primary function of a principle theory is tied to the explanatory role it plays through unification. The theory of explanation as unification was first advanced by Friedman [21] and has been developed since by Kitcher [25]. [55, pp. 23-24]

Ultimately, then, Frisch's final word on the matter here is entirely appropriate:

> There may be important differences in the depth of understanding that various kinds of explanation can provide, but it seems to me to be a mistake to deny that principle or purely phenomenological theories can provide any explanations at all. [22, p. 179]

Let me close here with the following observation. If (as per the above) one takes Einstein's 1905 principle theory of special relativity (derived from the relativity principle and light postulate) to be the statement of universal

[17] Others may be extracted from [48].

[18] On this point, Pooley states that "the principles of principle theories are not explanatory" [41, p. 3], and subsequently suggests that "it would clearly be a mistake to think that the principles from which you derive something in the 1905 derivation are explanatory of what you go on to derive, in particular length contraction" [41, p. 5]. I agree: the *principles* of principle theories (e.g. the light postulate) need not be explanatory (here: explanatory of special relativistic effects), but the principle theory *itself*, which is *derived* from those principles (and thus is logically weaker—in the context of special relativity, the principle theory is just that of universal Lorentz invariance of dynamical laws) *can* be explanatory of the relevant set of dynamical facts.

Lorentz invariance of dynamical laws, *and* one thinks (as we have also seen above) that the symmetries of the laws can offer constructive explanations, then one is committed to principle theories as offering (partial/truncated—not fundamental) constructive explanations of special relativistic effects. Although counter-intuitive, this is an under-appreciated aspect of principle theories which, in light of the foregoing discussion, I am willing to accept.

3.3 The need for constructive explanations

Acuña also calls into question Brown and Janssen's shared motivations to identify a constructive version special relativity—denying the following thesis (which he labels P_2):

> P_2: For every realm of phenomena E for which there is a theory of principle T_p, there is a constructive theory T_c. [2, p. 7]

Clearly, if one denies in this way the claim that for every principle theory there is an associated constructive theory, then one denies that it is always appropriate to seek constructive explanations of physical phenomena.[19] Acuña justifies said denial of P_2 as follows:

> Since it [P_2] is a universal principle that refers to contingent cases, we cannot characterize it as empirically vindicated, no matter how strong its inductive support may be. Thus, in terms of its truth, P_2 is highly problematic, if decidable at all. In turn, the absolute demand for a constructive interpretation of special relativity is as questionable as the truth of P_2—there just may not be a constructive counterpart for special relativity as a theory of principle. [2, p. 8]

My concern with this denial is straightforward. We have already seen that one kind of constructive explanation is a fundamental explanation. But to deny, then, that we should always think that to any given principle theory there is an associated constructive theory amounts to denying that we should always think that to any given principle theory there is an associated *fundamental* theory. But the belief that there is indeed an ultimate, fundamental theory of physics is something that many—including myself!—would be unwilling to give up on; indeed, many would regard this belief as a necessary condition for scientific realism. In denying P_2, Acuña appears, therefore, to side with e.g. Cartwright's 'dappled world' conception of science and scientific laws—one of the central tenants of which is the denial of the claim that there must necessarily exist a fundamental theory of physics [12]. Perhaps this is something that Acuña would be willing to accept—but these controversial consequences of his views deserve to be made explicit.[20]

[19] In light of the foregoing discussion, according to which even principle theories may sometimes be understood as offering (partial/truncated—not fundamental) constructive explanations of physical phenomena, it might be preferable to prefix 'constructive theory' in discussions of P_2 with (e.g.) 'deeper'. I will pass over this subtlety in the remainder of this section.

[20] Cartwright should not necessarily be understood as objecting to P_2; rather, she should only be understood as objecting to the stronger claim that there is always a fundamental theory associated with any given theory; this denial is consistent with P_2. What is true, however, is that the denial

After calling into question P_2, Acuña subsequently argues that, once the project of providing a constructive underpinning of special relativistic phenomena is abandoned, the connection between Minkowski spacetime and Poincaré invariance of the dynamical laws is seen to be bidirectional and analytic: they are "two sides of the same coin" [2, p. 1]. I agree with Acuña here, insofar as it is a mathematical fact that the isometries of Minkowski spacetime *just are* the Poincaré transformations (cf. [2, p. 9, n. 12]), and that, once the constructive project is abandoned, questions of relative explanatory priority become moot. Nevertheless, it is worth stressing that one will *not* be led to this conclusion if one rejects the antecedent of Acuña's conditional—that is, insofar as one continues to seek a constructive explanation of special relativistic phenomena.

Myrvold correctly highlights that, for advocates of the dynamical approach, spacetime symmetries *just are* dynamical symmetries.[21] However, it is important to keep in mind that authors such as Brown are still seeking a constructive explanation of special relativistic effects—and, for that reason, still regard dynamical symmetries as *conceptually prior* to spacetime symmetries. For this reason, although the *mathematical* arrow between Minkowski spacetime and Poincaré symmetries is bidirectional, the *explanatory* arrow remains monodirectional, for Brown. It is only when one—like Acuña—*rejects* the call for constructive explanations, that one can state that the mathematical arrow is the only arrow of any significance, and therefore that Minkowski spacetime and Poincaré symmetries constitute "two sides of the same coin". Thus, when Myrvold writes that "If talk of spacetime structure codifies facts about dynamical symmetries, there can be no question of explanatory priority of one over the other" [2, p. 14], he is (as he himself proceeds to admit) more in line with Acuña than Brown. In short, then, neither Acuña nor Myrvold should not be identified as allies of the dynamical approach in the sense of Brown, insofar as both reject the explanatory priority of dynamical symmetries over spacetime structure.

4 Modal and non-modal relationalism

For substantivalists, space and time exist as ontologically autonomous entities in the world; for relationists, by contrast, space and time are (in some way to be articulated) ontologically reduced to (relations between) material bodies/fields. This much is extremely well-known in the philosophical literature; what is slightly less known is one particular historical subdistinction *within* relationalism—*viz.*, the distinction between 'modal' versus 'non-modal' relationalism. According to modal relationalism, space and time are ontologically reduced to (relations between) matter bodies/fields *in all possible worlds*. By contrast, according to non-modal relationalism, space and time are ontologically reduced to (relations between) matter

of P_2 entails the denial of this stronger claim. Thus, while Acuña and Cartwright might disagree on P_2, they nevertheless agree on the stronger claim. My thanks to Nic Teh for discussion on this point.

[21] Although Brown himself would not place much weight upon the importance of the nomenclature 'spacetime'—cf. [10, §3.1].

bodies/fields *in the actual world*. Certain historical players have been identified as falling into one or the other of these two camps—for example, Leibniz and Barrow have been identified as modal relationists (see e.g. [5, pp. 173-185], [52, §4.5]); Locke (more controversially) has been identified as a non-modal relationist (see e.g. [52, §7.3]).

In this article, I do not seek to engage with the interesting exegetical project of ascertaining which historical authors fall into which of the above categories in the substantivalism/relationalism debate. Rather, I wish to highlight that the modal versus non-modal relationalism distinction affords an interesting lens through which to view the distinction between the positions of Brown versus Janssen—at least when the latter is understood as construing Minkowski spacetime as a 'modal constraint'. For Brown—a thoroughgoing empiricist—Minkowski spacetime is nothing but the codification of the symmetries of the dynamical laws governing matter fields *in the actual world*.[22] By contrast, one way of reading Janssen as a relationalist would be to take him to be maintaining that Minkowkski spacetime is a codification of the symmetries of the dynamical laws governing matter fields *in all possible worlds*. Now, in response to this version of Janssen, an advocate of the dynamical approach might still respond: "*why* is Minkowski spacetime the codification of dynamical symmetries in all possible worlds?"—what is it about the structure of modal space which constrains this to be so? Just as with Langian meta-laws, such a view might face the charge of obscurity (cf. [10, §3.2])—especially *qua* putative absolutist constructive theory. Nevertheless, the foregoing remains an interesting option for the geometrician to consider.

5 Conclusions

Let me summarise the points made in this paper in the following bullet points:

- What is dubbed in [45] the 'qualified geometrical approach' is the correct way to think about the geometrical approach.
- Advocates of this version of the geometrical approach need not be substantivalists.
- But: if advocates of this version of the geometrical approach seek *constructive* explanations of relativistic effects, then they must be absolutists, in Acuña's sense.
- Absolutist geometricians can offer constructive, but not fundamental, explanations of relativistic effects.
- Advocates of the truncated Lorentizian pedagogy on the dynamical approach can also offer constructive, but not fundamental, explanations of relativistic effects.
- Both parties should agree on the value of fundamental explanations.
- Non-absolutist geometricians can still offer explanations of relativistic effects—*just not constructive ones*.

[22] At least assuming that the actual world is special relativistic!

- Janssen's view of Minkowski spacetime as a 'universal kinematical constraint' may be understood as an absolutist thesis akin to modal relationalism; Brown's view of spacetime structure is akin to non-modal relationalism.

Many of these insights I owe to others. But it is, I hope, of sufficient value to have them in one place, and to connect them with other aspects of the literature, to justify the existence of this paper. The short story here is that, *pace* Brown, there are many ways to make sense of a viable geometrical approach—whether one that can offer constructive explanations of relativistic effects, or not.

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A Fixed fields

The notion of a 'fixed field' was introduced to the philosophical literature in [40, p. 115] and [4, fn. 137]; however, there remain several aspects of this notion deserving of further clarification. The purpose of this appendix is to address these matters.

A.1 Definition

Before getting to the definition of a fixed field, it will be valuable to recall, following [54], two maps which may be used to compare models $\mathcal{M} = \langle M, O_1, \ldots, O_n \rangle$ and $\tilde{\mathcal{M}} = \langle M, \tilde{O}_1, \ldots, \tilde{O}_n \rangle$ of some theory $\mathcal{T}$, where M is a differentiable manifold and the O_i and $\tilde{O}_i$ are various geometric objects defined on M:

- $1_M : M \to M$, which is the identity map on M. This is the unique map such that, given any other map $\gamma : M \to M$, $\gamma \circ 1_M = 1_M \circ \gamma = \gamma$.
- $\psi : M \to M$, which is a diffeomorphism (i.e., a smooth map with smooth inverse) taking all geometric objects O_i on M to the pushforward geometric object, $\psi_* O_i$.[23]

[23]As Weatherall stresses, "according to the theory of smooth manifolds, diffeomorphism is the standard of isomorphism for manifolds; just as other mathematical objects are only defined up to isomorphism, manifolds are only defined up to diffeomorphism" [54, p. 335]. Thus, supposing that we have two models $\mathcal{M}$ and $\tilde{\mathcal{M}}$ related by some diffeomorphism ψ, but not by the identity map 1_M (so that they are, indeed, numerically distinct mathematical objects), we are not usually concerned with interpreting those models in a way which 'cuts finer' than diffeomorphism. Here is not the place to discuss Weatherall on the hole argument and representation in physics—I refer the reader to [42] for further discussion.

The notion of a fixed field on M will be defined using 1_M. The rough idea behind this definition is the following:[24] when one compares two kinematically possible models $\mathcal{M} = \langle M, F_1, \ldots \rangle$ and $\tilde{\mathcal{M}} = \langle M, \tilde{F}_1, \ldots \rangle$ using 1_M, F_1 and $\tilde{F}_1$ agree on field values at each $p \in M$. With this in mind, one could define a fixed field as follows:

> For a given theory $\mathcal{T}$ with class of kinematically possible models $\mathcal{K}$, $\mathcal{T}$ has a *fixed field* if and only if, in each $k_i \in \mathcal{K}$, there is a field F_{k_i} (with non-trivial associated transformation rules), such that there is a (possibly infinite) set $S := \{F_{k_1}, F_{k_2}, \ldots\}$, with one element for each $k_i \in \mathcal{K}$, and each element of S agrees on the field values at each $p \in M$, when the identity map on M, 1_M, is used to compare the elements of $\mathcal{K}$. In particular, the elements of S identify this fixed field in each model.

If one uses this definition, then a fixed field is indeed 'fixed identically in all kinematically possible models' (cf. [40, p. 115]).

Note, importantly, that fixed fields have non-trivial transformation rules associated with them. Thus, objects such as the Levi-Civita alternating symbol ϵ_{abcd}, and the Kronecker delta $\delta^a{}_b$—which are 'confined objects' in the sense of Pitts [33, §2]: they do not change at all, under any transformation—are not fixed fields.[25] Fixed fields, by contrast to objects such as these, are such that, for any diffeomorphism d, if $\langle M, F_1, \ldots \rangle$ is a kinematically possible model of $\mathcal{T}$, then $\langle M, d_* F_1, \ldots \rangle$ is (in general) *not* a kinematically possible model of $\mathcal{T}$ (cf. [40, p. 116]). In brief, then, since fixed fields still have non-trivial transformation properties, objects like the Levi-Civita symbol and the Kronecker delta do not count as fixed fields.

To be completely clear here, take another example: that of the Levi-Civita *tensor*, $\tilde{\epsilon}_{abcd} := \sqrt{-g}\epsilon_{abcd}$, in general relativity. This is the product of the Levi-Civita symbol ϵ_{abcd} (a confined object) and the square root of the metric determinant, $\sqrt{-g}$. The latter is constructed from the metric field g_{ab}, which is not a fixed field; accordingly, $\sqrt{-g}$ is also not a fixed field (a diffeomorphism, for example, will redistribute this scalar density on the manifold). In light of this, $\tilde{\epsilon}_{abcd}$ is also not a fixed field.

A.2 Diffeomorphism invariance

One of the benefits of working with fixed fields is that it allows one to draw a conceptual distinction between *general covariance* and *diffeomorphism invariance*—and thereby to make some progress in distinguishing (say) special from general relativistic theories.[26] Following Pooley, define these two notions as follows:

> A formulation of a theory $\mathcal{T}$ is *generally covariant* if and only if the equations expressing its laws are written in a form that holds with respect to all members of a set of coordinate systems that are related by smooth but otherwise arbitrary transformations. [40, p. 115]

[24] In this appendix, I'll use O_i for generic geometric objects on M; F_i for fixed fields on M; and D_i for dynamical (i.e., non-fixed) fields on M.

[25] Pitts, in turn, drew the notion of a confined object from [53].

[26] One is only here entering rather than leaving the weeds—see [40, 43] for discussion.

> A theory $\mathcal{T}$ is *diffeomorphism invariant* if and only if, if $\langle M, F_1, \ldots, F_m, D_1, \ldots, D_n \rangle$ is a solution of $\mathcal{T}$, then so too is $\langle M, F_1, \ldots, F_m, d_* D_1, \ldots, d_* D_n \rangle$, for all $d \in \mathrm{Diff}\,(M)$. [40, p. 117]

A theory formulated in the coordinate-independent language of differential geometry *a fortiori* holds in all coordinate systems. Since all theories admit of such a formulation, the above notion of general covariance is trivial—this is a modern-day version of the Kretschmann objection.[27] Note, however, that special relativistic theories formulated in terms of fixed fields will *not* be diffeomorphism invariant (see [40, p. 117] for the details, and [43, ch. 2] for further discussion)—so there is room to adjudicate, using fixed fields, that theories are generally covariant, but not diffeomorphism invariant.

Note that this definition of diffeomorphism invariance is to be applied only to the objects specified directly in the models of the theory under consideration: this avoids concerns that theories such as general relativity might not in fact come out as diffeomorphism invariant, if they can be shown to contain a derived fixed field. For example, consider some geometric object O in a model $\mathcal{M}$ of some theory $\mathcal{T}$, and a geometric object $\tilde{O}$ in a model $\tilde{\mathcal{M}}$ of $\mathcal{T}$, in which all geometric objects are related to their corresponding geometric objects in $\mathcal{M}$ by a diffeomorphism d, so that we have $\tilde{O} = d_* O$. Now, in $\tilde{\mathcal{M}}$, define a derived geometric object $\tilde{O}' := (d_*)^{-1} \tilde{O} = (d_*)^{-1} d_* O = O$; clearly, if one considers such an $\tilde{O}'$ in all models of $\mathcal{T}$, one will be able to find derived fixed fields in theories where, intuitively, one would not wish fixed fields to appear. The workaround, as mentioned above, is to consider only the fixed fields specified directly in the kinematically possible models of whatever theory is under consideration—although there is, indeed, a worry that this move is *ad hoc*: see [43] for further discussion.

A.3 Comparison with absolute objects

The notion of an *absolute object* was first articulated by Anderson [1], and subsequently developed by Friedman [21].[28] For our purposes, we can take an absolute object to be an object fixed (up to isomorphism) in all dynamically possible models of the theory under consideration (see [40, p. 124]). Now, one reasonable question to ask is: why bother to work with fixed fields, rather than absolute objects? To make this more concrete: is there any reason to favour (i) a formulation of a special relativistic Klein-Gordon theory, with kinematically possible models $\langle M, \eta_{ab}, \varphi \rangle$, formulated using a field field η_{ab} and with dynamically possible models picked out by $\eta_{ab} \nabla^a \nabla^b \varphi = 0$ (with ∇ compatible with η_{ab}), to (ii) a formulation of a special relativistic Klein-Gordon theory, with kinematically possible models $\langle M, g_{ab}, \varphi \rangle$ and with dynamically possible models picked out by $g_{ab} \nabla^a \nabla^b \varphi = 0$ (with ∇ compatible with g_{ab}) and $R^a{}_{bcd} = 0$?[29] One reason to do so is in order to be able to articulate a difference between general covariance and diffeomorphism invariance—see above. Another

[27] See [26] for Kretschmann's original paper, and e.g. [51, §2.1] for some recent discussion.

[28] For further discussion, see [23, 33].

[29] In [40], these theories are dubbed **SR1** and **SR2**, respectively.

is that these objects play subtly different roles in the adjudication of the *background independence* of a given theory—see [43] for discussion on this point.

But even if one retains a preference for working in terms of absolute objects rather than fixed fields, the choice does not, as far as I can see, matter for the purposes of this paper (at least if attention is restricted to the fundamental fixed fields in a theory—see above): there is a natural sense in which formulations of theories which use one brand of object rather than another are equivalent (see [40, p. 122] and [43, ch. 2])—and indeed, the important content of my claims in the body of this paper regarding theories formulated in terms of (fundamental) fixed fields would transfer to theories formulated in terms of absolute objects.

A.4 Fixed fields and primitive identities

An anonymous referee has objected to the notion of a fixed field, on the grounds that, in order to make sense of this notion, one seems to be forced to an unpalatable commitment to the primitive transworld identity of spacetime points (needed, the thought goes, in order to be able to *identify* such a field as being fixed). To this latter point I reply as follows: it is not correct to say that the notion of a fixed field relies on the transworld identity of spacetime points, in a physical sense. The notion of a fixed field does rely on comparing models of the theory under consideration not with a map which realises an isomorphism (ψ, in the terminology used above), but rather with the identity map associated with one of the models (1_M). This, however, is all mathematics; once one has used such a map to identify whether a field is fixed or not (and so the kinematical possibilities of one's theory), one is still faced with the project of *interpreting* the models of one's theory, so constructed: and one *may* interpret them without a commitment to the primitive (transworld) identities of spacetime points, *à la* the sophisticated substantivalist.[30]

Of course, the notion of a fixed field does presuppose that we are able to construct mathematical models of a given theory which feature the very same manifold M (composed, ultimately, of the very same set-theoretic constituents); it also presupposes that it is legitimate to speak of maps between models such as the identity map 1_M. Both of these may legitimately be scrutinised: here, we enter difficult philosophical waters regarding the identity conditions of mathematical objects.[31] I recognise these concerns as legitimate, but set them aside: what I wish to point out here is that this is a separate issue from an apparent commitment to the primitive (transworld) identity of spacetime points.

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