Fundamentality and the Dynamical Approach to Relativity*

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1 Introduction

I'm going to talk about the dynamical approach to relativity, Harvey Brown's view about how we should understand the geometry of relativity theory.

*This paper is a (lightly edited) transcript of a talk given at the conference "The Foundation of Reality: Fundamentality, Space and Time." I am very grateful to Anna Marmodoro, David Glick and the other organisers of the conference for the invitation, and to Patrick Dürr for preparing a draft transcript. At the time of writing these notes (28 June 2023), the audio of the talk is available at: http://media.philosophy.ox.ac.uk/metaphysics/FUND_Pooley.mp3.

This topic turns out to be an ideal case study for questions concerning how fundamentality and explanation interact.

Let me start with two slogans.

(1) Rods and clocks do what they do because spacetime's geometry is what it is.

(2) The geometry of spacetime is what it is because rods and clocks do what they do.

You'll notice that in these slogans there is this key word "because". How we should think about this key word in these two slogans is not entirely straightforward. Roughly speaking, in the first slogan the "because" is something like a causal explanation "because". In the second one, it's something like a metaphysical or perhaps a logical explanation "because".

The view to be defended in this talk is: Rods and clocks do what they do because spacetime's geometry is what it is.

Let me say a bit more about the two views that these slogans are meant to encapsulate.

The first one is what Harvey Brown calls *the geometrical approach*. According to the geometrical approach, the geometry of spacetime's being what it is *in part* explains why (amongst other things) rods and clocks do what they do. The view is completely compatible with the geometry of spacetime not itself being fundamental. But it's certainly not the case, unless you are going to be caught in some vicious explanatory circularity, that the geometry of spacetime is not only not fundamental but also grounded in the behaviour of rods and clocks, or in the symmetries of the dynamical theories in terms of which the behaviour of familiar rods and clocks might otherwise be modelled and explained.

Contrast this view with *the dynamical approach*. I won't say much about it now. Part of the purpose of this talk is to push a bit further than is sometimes pushed what the approach might actually commit one to. But the key commitment is that the geometry of spacetime is what it is in virtue of the behaviour of rods and clocks. Here is a quotation from Harvey Brown illustrating this thought:

> The Minkowskian metric is no more than a codification of the behaviour of rods and clocks, or equivalently, it is no more than the Kleinian geometry associated with the symmetry group of the quantum physics of the non-gravitational interactions in the theory of matter. (Brown, 2005, 9)

The structure of what I'm going to say has four parts. I'm going to start by covering common ground. I take this common ground to be a whole bunch of commitments that Harvey Brown has pressed that we should accept. But they are commitments that the substantivalist, or the advocate of the geometrical approach, should have no problem accepting too. It's important to cover those in order to clear them away, so as not to get distracted by thinking that that's where the action is.

We'll then move on to look in turn at these key explanatory claims, the first that spacetime symmetries and the geometry of spacetime can explain the behaviour of rods and clocks in particular by explaining dynamical symmetries. We'll see why that's the thing to look at after we've covered the common ground. Then, we'll turn to what the dynamical approach advocate wants to say: that you can explain geometry in terms of dynamical symmetries. Finally, I will comment on the status of the metric in general relativity.

2 Common Ground

Here are four things that, I think, will not distinguish between someone who believes in the dynamical approach and an advocate of the geometrical approach.

The first is the claim that the principles of principle theories are not explanatory. (I had originally intended to say that principle theories are not explanatory but I became aware that exactly what one might mean by "principle theories" is ambiguous.)

Secondly, everyone should recognize that explanation is context dependent and in that sense pluralistic.[1]

The third area of common ground concerns particular explanations of characteristically relativistic phenomena and in particular explaining why, for example, a rod contracts when set into motion. Everyone should concede that explaining why a material rod of some specific type exhibits characteristically relativistic behaviour need not, and in some sense should not, appeal to the details of the dynamics. This means that the advocate of the dynamical approach is not saying that you have to appeal to details of the dynamical rod to explain this contraction.

Finally, everyone should acknowledge that, if you want to explain why

[1]Note added after talk: I now think that calling the phenomenon I'm interested in "context dependence" misrepresents it. The issue is that different albeit closely related explananda can call for very different explanations. Pseudo-debates can arise when the different explananda are not recognised as distinct.

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.

We are quickly going to review why all these things are true.

2.1 Principle versus Constructive Theories

Let me start by reminding you of the distinction between principle theories and constructive theories and point out the sense in which the principles in principle theories are not themselves explanatory.

According to Einstein, principle theories involve finding certain regularities in the phenomena from which you can go on to derive "a theory which will apply in every case" (Einstein, 1919). The idea is: Wherever these regularities in the phenomena hold, the thing that you can derive via the principle theory approach is guaranteed to hold and be true as well.

When you apply this to Einstein's 1905 derivation of the Lorentz transformations, the principles he has in mind as the regularities in the phenomena on which everything rests are, in addition to the isotropy of space and the homogeneity of space and time, the Relativity Principle and the Light Postulate. And the question then is: What is the best way of conceiving of "the theory which will apply in every case" that is derived from those principles? Although it may not have been exactly how Einstein originally would have presented it, now the best thing to say is: What one derives—the result of Einstein's 1905 derivation—is that *all the fundamental laws are Lorentz covariant*.

It's tempting to think that Einstein has a kind of shift of view between his 1905 position and later on, when he says: Here is what special relativity is all about – it's the Lorentz invariance of the fundamental laws. There is a sense in which, as he presents it, it's true that there is a shift. But there's a sense in which, materially, this doesn't represent a shift at all, because Lorentz invariance *just is* the main conclusion of the 1905 derivation.

Now contrast principle theories with constructive theories. In a constructive theory one builds a picture of complex phenomena out of some relatively simple postulates. Einstein went on to say that it's really constructive theories that provide you with the understanding of the phenomena in question. Of course, this is where you might mistakenly think that you need to provide a constructive theory of length contraction, if you're really to understand it. I'm going to claim that this is not the case, and that Brown agrees (or, at least, we've said things in the past that mean that).

Why should we say that the principles of principle theories are not explanatory? Let's consider length contraction. Figure 1 reproduces a slide that Minkowski used in his 1908 lecture. It depicts a now very familiar story.

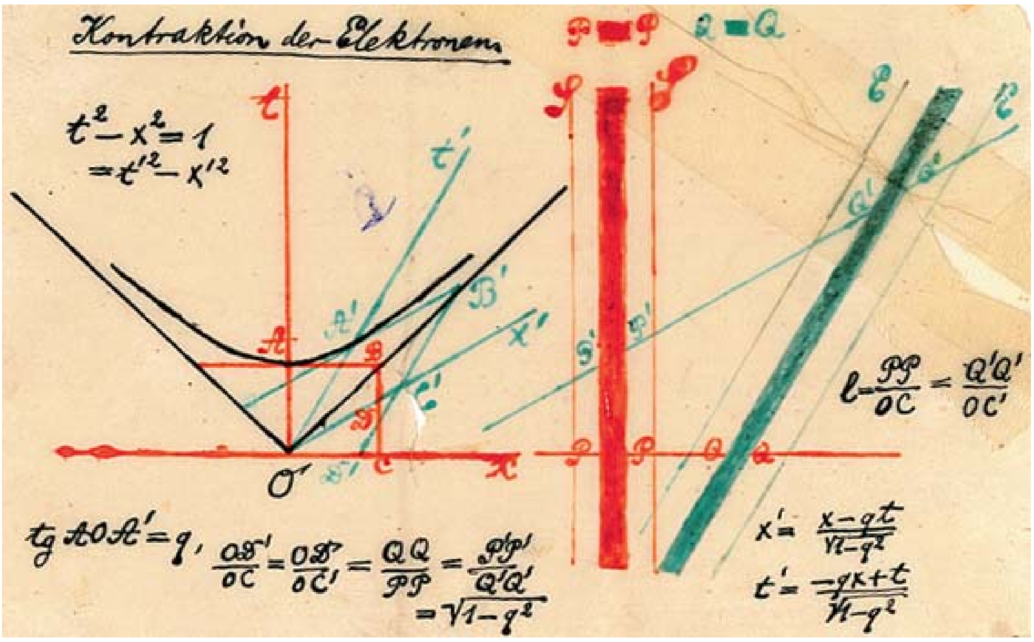

Figure 1: Minkowski's diagram

We are considering two rods, the red rod and the green rod, in relative motion. We are noticing that, according to Minkowski geometry, the length QQ is shorter than the length PP. This fact follows, according to Minkowksi geometry, if $Q'Q'$ is the same length as PP, which is just to say: the two rods are identical except for their relative motion.

Let's think about this from the perspective of deriving that this is how things look, having first derived the Lorentz transformations from Einstein's principles. In the context of thinking about relativity as a principle theory, you might ask: What explains what? In particular, I suggest, 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. To say they were, would be to say that the Relativity Principle (this phenomenological fact that you can't perform an experiment to detect your state of motion) and the phenomenological fact that whenever you measure the speed of light you find that it's independent of the speed of the source, that those two facts explain why rods contract. Surely, if anything, it's the other way around. It's the fact that rods contract that explains why what you measure about light in one frame is the very same as what you measure in a frame that is moving uniformly with respect to the first frame.

However, if one turns to what was *derived* from the principles, namely the Lorentz covariance of the laws, it does seem that you can appeal to this to explain certain facts about length contraction.

2.2 Context Dependence

On to the contextual nature of explanation. Here is a quote from Yuri Balashov and Michel Janssen about what it takes to explain length contraction:

> Length contraction is explained by showing that two observers who are in relative motion to one another and therefore use different sets of space-time axes disagree about which cross-sections of the 'world-tube' of a physical system give the length of the system. (Balashov and Janssen, 2003, 331)

This is really just Minkowski's explanation. Take the green rod. Someone at rest with respect to the green rod is using $Q'Q'$ as its cross-section. Someone at rest with respect to the red rod and using that system of coordinates is going to take QQ to be the length of the rod. These are different lengths in Minkowski geometry. Therefore, the observers disagree about the length of the rod.

Now the advocate of the dynamical approach says: Of course, this is a perfectly fine explanation. No one thinks you can't appeal to Minkowski geometry to explain things.[2] But, of course, in doing that, one is taking it for granted that those material systems conform to and obey and exhibit and manifest Minkowski geometry. You might ask: What explains *that*? How come these material systems do that?

Going back to Einstein's thought that, in order to understand a phenomenon one has got to come up with a constructive theory of it, you might be tempted to go down the following route. You might think that what you need to do is to provide, in the style of Bell (1976), a constructive derivation of the behaviour of the particular system you are considering in terms of that system's detailed dynamics.

Here is what Brown and I said in a paper a while ago:

> The truly constructive explanation of length contraction involves solving the dynamics governing the structure of the complex material body that undergoes contraction. (Brown and Pooley, 2006, 82)

[2]For example, exactly this was said by advocates of the dynamical approach in Brown and Pooley (2006, 79).

I stand by this. *If* you want to explain length contraction *constructively*, i.e., if you want to provide a constructive theory, this is what you will end up doing. In the paper just cited, immediately after the passage just quoted, we go on to concede that "there are, of course, many contexts in which such an explanation may not be appropriate, contexts that call for a purely geometrical explanation." What we were concerned to stress was that such geometrical explanations are not constructive theory explanations in Einstein's sense.

Let's consider the type of derivation that Bell gestures at. Suppose that, given the exact equations that describe the system, one could derive that it behaves in a particular way and, in particular, length-contracts when you boost it in a particular way. I would claim that such a derivation is genuinely explanatory. It's a canonical example of the kind of thing that is done in physics all the time to explain stuff in a constructive context.

However, this isn't to concede something that goes against the geometrical approach because, according to someone who thinks that fundamentally geometrical structure is not to be derived in terms of dynamical symmetries—that it's some primitive part of the structure of the world—this kind of story, the Bell-type story, is appealing implicitly to geometrical structure.

I'll leave it as an exercise for you to think through how that works in the case of something like Bell's story about length contraction. Consider instead Newton's Bucket and the way in which inertial structure figures in a causal explanation of why the water becomes concave when the bucket is spinning. It's *not* the fact that somehow there is a mysterious law that, when things spin, there is some inertial force that pushes water up the sides. (Nick Huggett (1999, 135–6) makes this point nicely.) We've got the standard physical story about why the water becomes concave if the bucket is spinning: if it's *absolutely* spinning—a fact that, according to the advocate of the geometrical approach, we understand in terms of primitive inertial structure—there must be net accelerative forces towards the centre of the bucket. To get those, you need the right kind of pressure gradient, and the equilibrium configuration would have to involve the concave surface.

What this illustrates is that, in order to be sympathetic to the advocate of the geometrical approach, you have to think through the standard story, typically given in standard inertial coordinates, and recognize all the places where, if you were an advocate of the geometrical approach, geometrical facts, or facts that are defined in terms of geometrical structure, are featuring in your constructive explanation.

2.3 Symmetries as Explanans

There is an obvious sense in which the constructive explanation is not very good because, when I have given this complex derivation in terms of, say, electromagnetism, that my particular system will Lorentz-contract, I haven't given myself any reason to think that another body will Lorentz-contract in exactly the same way, even if it's described by the very same laws, because maybe it involves a different equilibrium configuration of a different state of material. But of course, Lorentz contraction *is* a universal phenomenon. So, this kind of explanation, although I think it's genuinely explanatory, is missing out something big.

Here is how Brown and I made this point in 2006:

> [I]n many contexts, perhaps in most contexts, one should not appeal to the *details* of the dynamics governing the microstructure of bodies exemplifying relativistic effects when one is giving a constructive explanation of them. *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. To appeal to any further details of the laws that govern the cohesion of these bodies would be a mistake. (loc. cit., 82, emphasis in the original)

In fact, it isn't right to call such explanations "constructive explanations."[3] The basic point made in this passage is correct, however: Assume that there are stable bodies, bodies that function like rods. It is the fact that the laws governing their constitution (whatever they are) are Lorentz covariant that explains why the bodies contract. (We'll see in a moment exactly how that explanation goes.) It would be a mistake to think that you actually have to appeal to the dynamical details if you are in the business of explaining Lorentz contraction as a universal phenomenon.

I say that all this is (or should be) common ground between the advocate and the dynamical approach and that of the geometrical approach. Here, at least, is someone who seems to think exactly the same thing. I take the

[3]This is also something acknowledged in our 2006 paper, albeit with more caution than I now think warranted: "[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" (Brown and Pooley, 2006, 83).

following quotations (anonymous for now!) to be an illustration of the things that I've just gone through.

Our anonymous author starts by noting that an appropriately accelerated rod undergoes "a sort of real physical contraction, which is itself caused by the interatomic forces binding the rod together into a rigid body." Why does this happen? Noting that one could explain this in terms of a Bell-style analysis, case-by-case, the author stresses that one can also give a general argument: "The key to a general analysis lies in the notion of a *rigid* body" where by "rigid body" is just meant a body that has "an *equilibrium state* that it tends to maintain in the face of (sufficiently small) external forces" and to which it returns when the external forces have been removed.

Our author concedes that "complete physical understanding of an equilibrium state would require a complete account of the internal structure of the rigid system, both its composition and the forces among its parts" but they maintain that "even absent such a detailed account, we can make some general assertions about rigid bodies in any Special Relativistic theory" such as, for example, that they will Lorentz-contract.

Here is how that story goes (I quote our author):

> Suppose a system has an equilibrium state that it tends to maintain when it is free of external forces (and hence in inertial motion). Let's call the equilibrium state S_{EQ}. In a given Lorentz coordinate system, such as the rest frame of the system, S_{EQ} will have a particular coordinate dependent description...
>
> Now consider a *different* physical state S' related to S_{EQ} as follows: S' has the same coordinate-based description relative to a *different* Lorentz coordinate system as S_{EQ} has relative to its rest frame.

Note that at this point it is not *assumed* that S' is physically possible: it's just a different kinematically possible state for this object that's defined, via its coordinate description with respect to a Lorentz-boosted coordinate system, in the way described. As the author notes, S_{EQ} and S' count as "corresponding states" in Lorentz's sense.

Back to the story:

> It follows, for *any* relativistic force laws, that S' will also be an equilibrium state, and that a system near the state S' and free from external forces will tend to go into the state S'. For... the laws of physics take exactly the same coordinate-based form when stated in a coordinate-based language in any Lorentz coordinate system... So the behavior of S' described in terms of the new

> Lorentz coordinates will be identical to the behaviour of S_{EQ} described in terms of the old coordinates. ... So, if initially the system is disposed to return to S_{EQ}, after the appropriate physical boost, it will be disposed to return to S'. We don't even need to know the forces that bind the system together.

To recap: We have the state S_{EQ}, an equilibrium state, and we are considering a particular coordinate system K, in which the system gets a particular description. We then consider K', a Lorentz-boosted coordinate system, and a different state S', which is defined as the state whose description with respect to K' is exactly the same as S_{EQ}'s description with respect to K. The key claim was: If the fundamental laws are relativistic (i.e. if they are Lorentz invariant and so take "exactly the same coordinate-based form when stated in a coordinate-based language in any Lorentz coordinate system"), then S' will be an equilibrium state, and if you perturb the system in the right kind of way, it will end up in the state S', and because it's exactly like S_{EQ}, but described with respect to a Lorentz boosted coordinate system, that's a Lorentz-contracted state.

This advocate of the dynamical approach is none other than Tim Maudlin, who, you might think, is one of the clearest proponents of the geometrical approach.[4] So, what's going on? Well, I've said that all these claims should never have been controversial to either side. Of course, there is more to the story and this now gets us to the second part of the talk: the spacetime explanation of dynamical symmetries.

3 The Spacetime Explanation of Dynamical Symmetries

Here is the key omission from the quotations from Tim Maudlin I was giving. When I said "if the fundamental laws are relativistic...", we have a story that Brown would be completely happy to embrace if we read that merely as the claim that the laws are Lorentz invariant. That's not how Tim Maudlin puts it. So, here is his statement of what it is for a law to be relativistic: "The fundamental requirement of a relativistic theory is that the physical laws should be specifiable using only the relativistic space-time geometry. For Special Relativity, this means, in particular, Minkowski space-time" (Maudlin, 2012, 117).

Having said this, he goes on to say why the claim above about S' and S_{EQ} with respect to the Lorentz-boosted coordinates K and K', follows. Here is

[4] The quotations come from Maudlin (2012, 116–8).

why it is true: The laws of physics take exactly the same coordinate-based form, when stated in coordinate-based language in a Lorentz coordinate system, because (1) according to the "fundamental requirement" the laws can only "advert" to Minkowski geometry (which is just to say that there are no other fixed structures other than Minkowski geometry that you might use to define or express your laws). And (2) the Lorentz coordinate systems just are the coordinate systems in which Minkowski geometry takes a particularly simple form. The distance relationships of Minkowski geometry are encoded in the coordinate system in just the way Euclidean distances are encoded in Cartesian coordinates. So what Maudlin is saying is that the symmetries of spacetime explain the symmetries of the laws, if the symmetries of the laws are taken to be just the transformations between coordinate systems in which the laws take an especially simple form.

This commitment that the symmetries of spacetime explain the symmetries of the laws, is endorsed by Balashov and Janssen. They put the choice to us:

> Does the Minkowskian nature of spacetime explain why the forces holding a rod together are Lorentz invariant or the other way around? ... Our intuition is that the geometrical structure of space(-time) is the *explanans* here and the invariance of the forces the *explanandum*. (Balashov and Janssen, 2003, 340)

They don't say any more and they appeal to intuition, which is perhaps unfortunate. But we've just seen there is much more one can say.

So, can we make sense of the claim that dynamical symmetries are what they are because the symmetries of spacetime are what they are? Harvey Brown's reaction to the claim is: "Here we are the heart of the matter. It is wholly unclear how this geometrical explanation is supposed to work" (Brown, 2005, 134–5). I take it that the story we have looked at already in the Maudlin quote gives us more than an indication of how it is supposed to work. Before talking about it a bit more, let me give you some other quotations from Brown which give voice to the intuition that appeal to spacetime structure as explanatory is getting the cart before the horse, in particular with respect to inertial structure.

Here is Brown on force-free motion:

> Force-free... bodies conspire to move in straight lines at uniform speeds while being unable, by fiat, to communicate with each other. It is probably fair to say that anyone who is not amazed by this conspiracy has not understood it.(Brown, 2005, 14–5)

I agree that this 'conspiracy' is something that you might want to try to explain. You might think you can explain it by postulating inertial structure and then saying that it's a law-like feature of the world that the motion of bodies not under the action of forces has to be adapted to that inertial structure in the right kind of way. Brown's reaction to that proposal is:

> To appeal… to the action of a background space-time connection in which the particles are immersed… is arguably to enhance the mystery, not to remove it. There is no dynamical coupling of the connection with matter in the usual sense of the term. (loc. cit., 142)

Now, it's certainly true that if we were thinking that the explanation has to go via some kind of 'action' of spacetime on the force-free bodies, then that's not something we're familiar with from physics. But I take it that's not how the explanation goes. Brown's conclusion is:

> It is simply more natural and economical—better philosophy, in short—to consider absolute space-time structure as a codification of certain key aspects of the behavior of particles (and/or fields). (loc. cit., p. 25)

Let's just stick with pre-relativistic physics. The plots in Figure 2 below, taken from Barbour (1999, 84), show the behaviour of three bodies moving under the action of gravitation. Here, we see what work inertial structure is doing in Newtonian theory, standardly understood.

All of these plots involve initial configurations that, with respect to relative distances and the rates of change of these relative distances, are identical. So, in terms of what you might think the relational initial data should be, they have the very same initial state. And yet you see that they evolve and can evolve in all of these very different ways.

The Newtonian has a very neat and elegant explanation of that. In addition to the spacetime structure carried by the material bodies themselves (i.e., the relative distances and the temporal separations between instantaneous configurations) there is additional structure in the world, namely inertial structure, and it's in terms of that structure that Newton's laws appear to be formulated. We have a simple force law, only dependent on the relative separation of the bodies, but what the forces determine is the acceleration of the bodies. *Prima facie*, to understand what that means, we need the inertial structure. Just by helping ourselves to that simple, elegant extra bit of structure, we've got an elegant and simple explanation of all these complicated differences between these cases. *That's* the explanatory

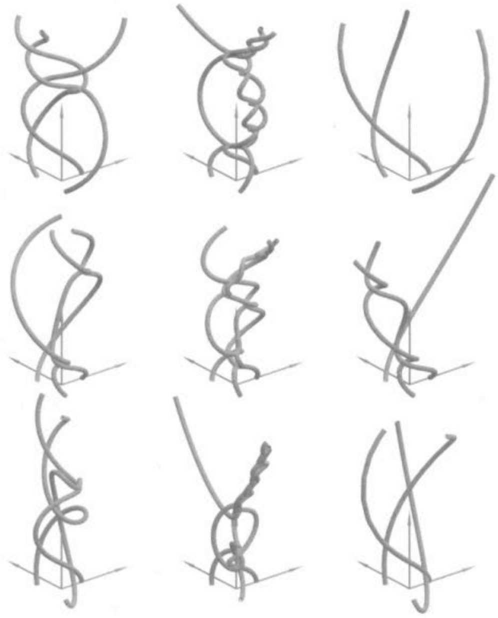

Figure 2: Different trajectories with identical relational initial data

work that inertial structure is doing for you. Just taking this at face value, inertial structure is playing an explanatory role in explaining the differences between these (initially) relationally identical configurations.

Let's go back to the explanation of Lorentz invariance in terms of Minkowski geometry. According to David Wallace in a recent paper (Wallace, 2019), the explanatory story I'm about to run through is the consensus view in foundational work on how the coordinate-based approach to representing physical theories is to be understood.[5] Wallace's paper is concerned with defending the legitimacy and (for certain conceptual purposes) the preferability of presenting theories in coordinate-based terms with respect to special coordinates, rather than thinking that everything has to be shoe-horned into the straightjacket of differential geometry.

Here is how the story goes: Suppose that we've got some fixed structure and we identify spacetime symmetries as just those maps from spacetime to itself that leave that structure invariant. One typically thinks that differential structure is part of spacetime structure, and that it's absolute structure – so, of course, these transformations will be diffeomorphisms – but we're considering the subgroup which leaves other richer structure, like Minkowski distances, invariant.

[5]My previous effort to articulate this explanation clearly can be found in Pooley (2013, 570–1).

Now, if the equations defining our theory are expressed with respect to coordinate systems related by an element of that symmetry group, then the numerical values of the components of the objects describing spacetime structure will take the same values in each coordinate system. That's true whatever the coordinates. We can pick an arbitrary coordinate system and then consider another coordinate system related to it by such a symmetry. However, it may be that our spacetime structure is very symmetric. There may be certain coordinate systems in which these components take very simple values. In particular, in the Minkowski spacetime case, we can make all the off-diagonal elements vanish and choose our coordinates so that the diagonal elements are either plus or minus one. In those coordinates, our equations apparently simplify and we have the standard coordinate-based way of writing the theory. Wallace's way of putting it is: "the standard coordinate-based way of writing a theory is to be understood simply as the differential-geometric theory described with respect to one of these simple coordinate systems" (Wallace, 2019, 126).

Here, I take it, is the geometrical advocate's explanation, in terms of the symmetries of spacetime structure, of why certain coordinate systems are preferred and why the form of the equations with respect to those coordinates will be invariant with respect to exactly the spacetime symmetry group.

3.1 Assessing the explanation

For the sake of the argument, let's grant the advocate of the geometrical approach, that there is in the world this structure that is not to be defined in terms of or reduced to dynamical symmetries, somehow otherwise understood. Let's suppose also that the laws are to be understood as certain relations that hold between the dynamical stuff in the world and that structure; that structure is being used to constrain what the dynamical possibilities are. If you grant that, then what we've just seen is that it follows ineluctably that if that structure has symmetries, i.e., if there are adapted coordinates in which it looks simple, then our coordinate-expression of the laws, so understood, will look simple in those coordinates. Those coordinates will be related exactly via the spacetime symmetries. So, in that sense, I take it, we have an explanation.

It's completely compatible with this way of deriving dynamical symmetries, so understood, that the dynamical symmetry group might in fact be a proper supergroup of the group of transformations that preserve spacetime structure, just if our laws use, in some intuitive sense, some but not all of the structure we have postulated. But if that's so, we are motivated to try to reformulate our laws in a way that doesn't involve postulating more structure than we really need. This is what Earman calls Symmetry Principle (SP1) (Earman, 1989, 46).

I'm not sure that advocates of the dynamical approach have directly engaged with the explanatory story just told. They have various examples, however, which are meant to give trouble for the general idea that you can explain dynamical symmetries in terms of postulated structure that has certain symmetries. Here are two that illustrate different kinds of problems.

1. **Jacobson-Mattingley Theory** is a theory that looks a bit like electromagnetism in a dynamical Lorentzian spacetime but there is an extra constraint which forces the vector field to be time-like (Jacobson and Mattingly, 2001). Now, if I've got an object that is time-like, and given that I'm in the context of a dynamical geometry, so that I'm only going to have locally preferred coordinate systems, it may be, given the nature of the dynamical object involved, that there are coordinates even more special than those that (simply) diagonalise the metric. If I've got a time-like vector field, at a point I can always choose a coordinate system that has that has that vector field as $(1,0,0,0)$. *But that's completely compatible with the story just told.* We can imagine a theory like this with a fixed Minkowski background, but a vector field that is constrained to be time-like, but is dynamical. What we'd then have is sets of global coordinates which are special and in which things look simple (just standard Lorentz charts) but at any point we could always choose a special coordinate system from amongst that set that makes things look even more special by adapting our time unit and time-axis to the value of $A_{\mu\nu}$ at that point.

2. **TeVeS**: Here is a theory where we've got a primordial metric and other (scalar and vector) fields, and, given the primordial metric and these other fields, we can define an effective metric (Bekenstein, 2004). The theory tells us that normal matter couples to the effective metric in just the way matter couples to the primordial metric in GR. If that's the case, then it will be coordinate systems picked out in the way we've just described by the effective, non-primordial metric that will be special. But notice that the story about why those coordinate systems are special is just the standard one that the advocate of the geometrical approach is giving.

The key thing to focus on in these examples is that the kind of mathematical-logical story that the advocate of the geometrical approach gives about special coordinate systems has potential variations, which you can think through in these different theories. They show that different cases can differ interestingly and significantly from each other. But I don't take them to be counterexamples to the basic idea. In particular, if we are in the nice simple context of flat Minkowski spacetime, then things work out exactly as the advocate of the geometrical approach says.

4 The Dynamical Explanation of Spacetime Geometry

What if we try to reverse the order of explanatory priority and take dynamical symmetries as explaining spacetime geometry? Let's go back to the quotation we had earlier: "The Minkowski metric is no more than the Kleinian geometry associated with symmetry group of the quantum physics of non-gravitational interactions in the theory of matter" (Brown, 2005, 9). For that to be a way of giving us a metaphysical picture, according to which spacetime geometry comes after symmetries, we'd better have an independent handle on what we mean by "symmetry group of the ... non-gravitational interactions in the theory of matter." We've reviewed what the geometrical advocate thinks the symmetry group is. That way of thinking about it isn't open to the advocate of the dynamical approach; they need an alternative.

In the paper already mentioned, David Wallace characterizes what he calls *the coordinate-based approach*. Here, to present a physical theory is to "give the field's equations in coordinates, and state that the theory is defined on a structured space and that the structure group is the symmetry group of the equations" (Wallace, 2019, 132). The basic idea is: You present certain equations governing the field that it is a theory of in some coordinates, and you say that this is a field on a space with a certain structure. What structure? It's the structure that is defined by a group of transformations. Which group of transformations? Just the group of transformations that is the symmetry group of the equations. So now we are taking the coordinate-expression of the equations as basic. In the geometrical approach, we had a kind of intrinsic characterization of the laws, expressed perhaps in terms of equations that could be written in the language of differential geometry. Note that one doesn't need to do that. It's important to separate the idea that we've got some intrinsic characterization of the laws from the idea that the right way to give that characterization is differential-geometric.

But here, in the coordinate-based approach, what we are doing is saying: No! Our primary way of presenting a theory takes equations presented with respect to a coordinate system as basic. Mathematically, this is perfectly legitimate and makes sense. You can consider, in particular, a space which is otherwise unstructured, except for the fact that I give you a bunch of preferred maps into R^N that are related by a particular transformation group. Then one asks: What are the structures one can define in ways using those coordinates which are invariant under the coordinate transformations that I have given you?

However, if I just give you just an equation and say it holds with respect to a special coordinate system, it's not completely straightforward to work out exactly what the other special coordinates systems are. In particular, it was a struggle over several decades to get to the fact, after Maxwell's equations were well-known, that their symmetry group was the Lorentz group. Perhaps more tellingly, suppose I give you the laws of Newtonian Gravity expressed in coordinate dependent form. We've got this potential field or maybe even a gravitational force field. If we treat this potential field as a scalar, or perhaps something that is defined up to an additive term, or if we take the gravitational force field as a vector field, then the symmetry of these equations is the Galilean group. But, if you think about those equations in a different way, and if you think of this field, not as one of those objects, but actually as the components of a connection in some special coordinates (or if you allow it to transform in a particular way) then your set of coordinates expands to the Maxwell group.

Here is another worry you might have about the coordinate-based approach. If we are identifying structure via coordinates, we are ruling out our ability to discern interesting differences between elements of that structure. Just take the Poincaré group plus scale transformations and ask: What is the structure that is invariant under those transformations? We might say: ratios of Minkowski distances. But we could instead have said: the non-qualitative two-place causal connectability relation, because in terms of that you can define everything else. Or you might take time-like straight lines, and nothing else, as basic. The point is: There is an awful lot that might be invariant but *some subset of it might sufficient to define the others*, and there might be interesting metaphysical reasons for preferring one subset over the others, exactly as those who advocate some causal theory of time believe (see, e.g., Winnie, 1977) or as is suggested by Maudlin's taking the structure of time-like lines as more fundamental (Maudlin, 2010).

I now move on to a more serious objection. If you take this presentation of the theory seriously, and you think of laws in a non-Humean way, then the resulting view, I suggest, has some very unpleasant features. It suffers from what Hartry Field calls "heavy duty Platonism". We are effectively saying: Here is how the world must be like; we've got some field, and it's constrained to evolve such that there is a map from it into the real numbers, such that under that description certain equations are satisfied. So, it looks like the real numbers are entering into the statement of law in a way that is unpleasant.

A way of doing it without numbers is to say: I'll postulate some Minkowski structure but, of course, we are then really back to the geometrical approach. To adapt a quote from Frank Artzenius, we are doing something like this: "Suppose I were to claim that the world is pretty much as you think it is, except that [chairs] do not exist. I then go on to claim that the true theory of the world is that there are no [chairs], and that the true theory of the world merely says that the things and properties and relations that there are (namely, everything other than [chairs] and their properties) are embeddable in a non-existing make-belief world which includes [chairs] and in which your favourite make-belief laws hold." (Arntzenius, 2012, p. 170)

On the view Artzenius is criticising, we have, for example, the pattern of people in the room, which we want to explain. Now the advocate of the analogue of the geometrical approach says: there are these chairs, and people can sit on them; the dynamically possible dispositions of people are constrained by these real things, chairs. But not according to the analogue of the coordinate-based approach. It says that 'the true theory of the world merely says that things and properties and relations other than chairs are embeddable into a non-existing make-belief world that includes [chairs].'

How can this be made more palatable? Be a Super-Humean! That makes it a little bit more palatable.

Minkowski can be considered as an advocate of something like this. Here is how he describes what Lorentz invariance is:

> From the totality of natural phenomena it is possible... to derive... a system of reference x, y, z, t ... by means of which these phenomena then present themselves in agreement with definite laws. But when this is done, this system of reference is by no means unequivocally determined by the phenomena. *It is still possible to make any change in the system of reference that is in conformity with the transformations of the group G_c, and leave the expression of the laws of nature unaltered.* (Minkowski, 1908, 79)

Imagine we've got this mosaic of the phenomena, not structured by the distance relations that the preferred coordinates define. We try out lots of different coordinates – and with respect to some—wow!—we get a description that satisfy some nice equations. Of course, satisfying these equations doesn't fix the coordinates. If I've got one coordinate system that works, any coordinate systems related to it via Lorentz transformations will do the same job.

Here, then, is the dynamical approach, described in a Humean way (see,Pooley, 2013, §6.3.2; Stevens, 2018):

- The basic ontology consists of some material fields. We imagine that the 'spatiotemporal' structure they have is topological or topological-differential.
- Then we only consider coordinate systems for that extended world that respect this structure and ask: Is it the case that with respect to some proper subset of these, the description we get of the material world looks nice and simple in that it satisfies some maximally simple equations?
- If the Humean mosaic of the differentially structured stuff is such that some nice simple equations that are Poincaré invariant are satisfied, then we've got a way of getting hold of dynamical symmetries independently of postulated spacetime symmetries.

5 Dynamical Geometry

We could pursue this super-Humean strategy in general relativity. Here is how it might go. We could tell the same story but now about special *local* coordinate systems: Local lorentz charts would be underpinned via patterns in the distribution of fields other than $g_{\mu\nu}$. That allows us to define at each point a Minkowski metric. Now taken together they give us back the Lorentzian metric field $g_{\mu\nu}$. Now we look again at the mosaic and we see that $g_{\mu\nu}$ satisfies Einstein's field equations.

But that's not the view advocates of the dynamical approach in fact take. They typically take $g_{\mu\nu}$ to be 'just another field'. If one does that, one ends up saying things that, I claim, the advocate of the geometrical approach will agree with (*cf* Pooley, 2013, 578).

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