Paving the Way for A Middle Way on Theoretical Equivalence

Jill North*

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Abstract

There is a divide between two broad approaches to the nature of theoretical equivalence in physics, to what we mean when we say that two physical theories are fully equivalent, mere notational variants. On one side is a formal approach, according to which formal criteria play a pivotal role in questions concerning the equivalence of theories. On the other side is a resistance to formal approaches, to the point of regarding formal criteria as essentially or ultimately irrelevant to these questions. Without arguing for any particular account of theoretical equivalence or any one formal criterion, I defend the basic thought that theories' formal or mathematical or structural features, as well as formal relationships between these features of potentially different theories, can bear true physical significance. I then show how this paves the way for a middle way on theoretical equivalence—an account according to which formal criteria of some kind are necessary (but not sufficient) for theoretical equivalence in physics.

1. Introduction

There is something of a divide between two broad approaches to the nature of theoretical equivalence in physics, to what we mean when we say that two physical theories are fully equivalent, mere notational variants—that they say all the same things, but perhaps in different ways—as we often do say in physics.[1] Think of Lagrangian and Hamiltonian formulations of classical mechanics, classical electromagnetism in terms of fields versus the potentials, Schrödinger and

*Department of Philosophy, Rutgers University; j.north@rutgers.edu

[1]See Weatherall (2019a,b) for an overview that distinguishes between the two approaches. Wallace (2022, 2024) notes that on a certain view (what he calls a "math-first" approach, akin to the semantic conception of theories), scientific theories do not "say" anything but rather represent. I slide between both ways of talking. The current discussion is independent of any particular view on the nature of scientific theories.

Heisenberg formulations of quantum mechanics: all examples of theoretical formulations said to be fully equivalent, just different ways of stating one and the same theory.[2] The philosophical question concerns the nature of these equivalence claims: what criteria lie behind reasonable judgments of equivalence?

Philosophers generally agree that two theories must be empirically equivalent in order to be fully equivalent in a sense that is relevant to physics. (The theories must in some sense give rise to all the same empirical predictions. It is not my concern here to address the proper sense of empirical equivalence, and philosophers involved in the recent debate tend to leave this to one side.[3]) The divide concerns what else is required.

On one side is a formal approach. Physical theories are equivalent when they are formally or structurally or mathematically[4] equivalent in some way, in addition to being empirically equivalent. There has been a lot of work in recent years aimed at proving various formal results that are supposed to guide us toward the right notion of formal equivalence, the one that indicates when two physical theories are genuinely equivalent, and applying these results to yield verdicts on particular cases. (Proponents of formal accounts will often note that the proposed criteria are to be added to criteria of empirical equivalence, which are over and above any purely formal notion, in order to generate reasonable judgments of theoretical equivalence. Since it is the formal criteria that have been the focus of recent discussion, these are generally referred to as formal accounts.) There is disagreement as to which formal notion is the right one, but agreement in general outlook that some such notion is pivotal to questions concerning theoretical equivalence in physics.[5]

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[2] These are examples of theoretical formulations standardly regarded by the physics community as equivalent. One could dispute these verdicts; I do not take a stand on particular cases here.

[3] This is not to say there has been no recent discussion of this; see Wolf and Read (2023).

[4] I use these terms interchangeably. Accounts wielding such features are all versions of the formal approach.

[5] Traditional ideas include logical or definitional equivalence (Glymour, 1971, 1977, 1980; Quine, 1975). More recent ideas and applications involve Morita equivalence, categorical equivalence, or some other type of structural equivalence (North (2009, 2021); Halvorson (2012, 2016, 2019); Curiel (2014); Barrett (2015a, 2018, 2019, 2020); Rosenstock et al. (2015); Barrett and Halvorson (2016a,b, 2017, 2022); Rosenstock and Weatherall (2016); Weatherall (2016a,b, 2017, 2021); Halvorson and Tsementzis (2017); Teh and Tsementzis (2017); Hudetz (2019); Nguyen et al. (2020); Dewar (2022); Jacobs (2023a); March (2024a,b); Bradley (2025); Gajic et al. (2025); March et al. (2025); Weatherall and Meskhidze (2025)). It is not always made clear in these discussions whether formal plus empirical criteria are intended to be sufficient for theoretical equivalence or merely necessary, though the impression often conveyed is that they are sufficient (sometimes the impression is that formal criteria on their own suffice). Regardless, formal or structural criteria play a central role. (They also seem to play a significant role in

On the other side is a resistance to formal approaches. The resistance might note that physical theories themselves consist of more than just their formal apparatus (and empirical content), so that judgments concerning the equivalence of theories must take into account more than just their formal features (plus empirical content). Philosophers have pointed to cases of physical theories that are equivalent in various formal and empirical respects, but nonetheless seem to differ in what they say about the world, so that the theories cannot be fully equivalent in a sense that matters to physics. The recent laser focus on defending one or another formal criterion has been misguided.[6]

I believe there are good reasons to be wary of taking formal criteria, even when explicitly added to empirical criteria, to be sufficient for theoretical equivalence, reasons that have been given in the literature: formal features cannot be the whole story when it comes to theoretical equivalence in physics. However, there is at the same time a danger of going too far in the other direction, and taking the formal results being generated to be essentially or ultimately irrelevant, effectively beside the point—as has been suggested to varying degrees in the literature arguing against the formal approach. This attitude is bolstered by an idea coming from the philosophy of science literature on models and representation (essentially that the representational devices we use in science do not in themselves have content; more on this below).

I wish to advocate for a middle way. In the midst of a growing recognition that formal notions on their own are inadequate for indicating when two physical theories are genuinely equivalent (even assuming the theories are empirically equivalent; I drop explicit mention of this from now on), it is important to be reminded of a basic thought that has been driving the formal approach, which is that we often take theories' mathematical or formal or structural features to bear true physical significance, and with good reason. More, a formal equivalence of some kind does seem to be necessary, if not sufficient, for theoretical equivalence in physics. (A formal inequivalence of the right kind signals a theoretical inequivalence.) And since it is not immediately clear what type of formal equivalence is the right kind, this is something it is worthwhile investigating, in ways that philosophers in the formal camp have been doing.

My aim is to defend that thought guiding the formal approach. Some may

claims concerning duality, a different though not unrelated notion: see de Haro and Butterfield (2018); Butterfield (2021); de Haro (2021); de Haro and Butterfield (2025).)

[6] See Sklar (1982); Ruetsche (2011); Coffey (2014, 2024); van Fraassen (2014); Nguyen (2017); Maudlin (2018); Bradley (2021); Butterfield (2021); North (2021, ch. 7); Teitel (2021b); Wilhelm (2024). Some of these discussions more or less explicitly support a notion that Weatherall (2019a,b) calls "interpretational equivalence," where "two theories are equivalent just in case they have the same interpretation."

regard the main point I will come to in this vein as all but obvious, yet it has been lost sight of in recent discussions, and warrants explicit defense; for it has implications for the theoretical equivalence debate that have not been recognized. I am not here going to advocate for any particular middle-way account or any one formal criterion (I will not be entering into debates over which formal criterion is the right one). My advocacy for a middle way will be more indirect. I aim to pave the way for such an account, by arguing that a middle way of some kind—an account that takes formal criteria of some sort to be necessary (albeit not sufficient) for theoretical equivalence in physics—must be correct. And I do this not by developing in detail any particular formal requirements on equivalence (though some will in the course of things be gestured at), but by defending the basic thought that theories' formal features, and formal relationships between these features of potentially different theories, can virtually in themselves bear physical significance. (The "virtually" will be explained in due course.) Not only does this undercut the main objection leveled at the formal approach (the objection is discussed in section 2, the undercutting in section 3), but it also supports the necessity of a formal sense of equivalence to full-fledged theoretical equivalence (section 4). (I do not here defend the claim that formal criteria are insufficient for theoretical equivalence, taking this to have been adequately done in the literature, so that the plausibility of a middle way rests on a defense as to their necessity.) It is therefore reasonable for a formal notion of equivalence to play the significant role that it does seem to be playing throughout physics,[7] and the recent philosophical attention being paid to this notion is warranted.[8]

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[7]Compare Wallace: "Physics practice is replete with the idea that two apparently-different theories are 'equivalent'"; "It is implausible that theoretical equivalence is in all circumstances an entirely formal matter" (and not just because empirical equivalence is also required); "Nevertheless, a formal notion of equivalence seems to play at least some large role in the use of equivalence in physics" (2022, 352–3).

[8]Whereas Teitel says, "Relegating formal criteria to mere necessary conditions makes it mysterious why such considerable energy has been exerted on these positive results" (2021b, 4130–1), I demur. If philosophical arguments concerning classical mechanics are to be trusted (North (2009, 2021); Curiel (2014); Barrett (2015a, 2019); cf. Hunt et al. (2025)), for example, then some theoretical formulations regarded as equivalent by the physics community are not genuinely equivalent, because they are not equivalent in a certain formal sense. This is a significant result. (For one, it suggests additional cases of underdetermination to be wrestled with.) The idea that formal criteria are philosophically interesting and worthwhile, even if not the entire story, is defended by de Haro (2021) and de Haro and Butterfield (2025), who further argue for a middle-ground account of the type I pave the way for here; see also Hudetz (2019); Dewar (2019, 2022, 2023a); Wallace (2024).

2. Representational practice

Philosophers who argue against the formal approach tend to regard theories' formal or mathematical features as essentially or ultimately irrelevant to questions of theoretical equivalence, on the grounds that it is some other ingredient—typically something along the lines of an interpretation—that is truly responsible for reasonable judgments concerning equivalence in physics. The result is that formal criteria "tell us little to nothing about genuine or bona fide equivalence," in the words of Neil Dewar (2022, 3) (who defends the significance of formal criteria to these questions).

The idea that formal criteria are ultimately irrelevant is held, implicitly or explicitly, by many of those arguing against the formal approach and cognate ideas, and has recently been made explicit by Trevor Teitel (2021b). The core problem, as far as the physical significance of any formal feature or result is concerned, is that no representational device, in particular no mathematical or formal tool, in itself has physical content—we must stipulate that it does, and what content it has. More, any such device can be stipulated into representing anything in physics we choose. After all, as Shamik Dasgupta (2011, 134) puts it of the mathematical models we use in physics, these things are "our tools not our masters." Indeed, we can in principle use anything we like to represent anything at all, just by stipulating that we are going to so use it. Teitel, following Hilary Putnam (1983), calls this idea trivial semantic conventionality: it is the thesis that "any representational vehicle can in principle be used to represent the world as being just about any way whatsoever" (2021b, 4125).

And if a theory's formal or mathematical aspects or devices on their own do not say anything about physical reality—if in themselves these things do not represent the world as being any particular way or other—-then no (formal) relationship between such aspects of potentially different theories can tell us about any similarities or differences in what the theories are saying about physical reality; in other words, about whether the theories are equivalent in the relevant sense. After all, no purely formal result can say whether we, the masters of our representational tools, have decided to use certain devices to represent particular physical situations, let alone whether we have decided to use them to represent the same or distinct physical situations. We can, if we like, use two different devices that are mathematically inequivalent in some sense to represent the very same physical situation, just by stipulating that we are doing this; in which case two theories that differ (only) in which of these devices they employ will be equivalent, they will say all the same things in different ways, just because we have effectively decided that they do, irrespective of any mathematical inequivalence between them. Conversely, we may choose to use the same mathematical device to represent two very different physical situations. Two physical theories, each of which employs this selfsame device, do not by virtue of this mathematical similarity then say all the same things in any physically significant sense—we have just stipulated that they do not.

Consider as well that there is nothing incoherent in the idea of someone else's making fairly different representational choices from one's own. In an example from Teitel, imagine a community for which, as a matter of their longstanding representational practice, only positive numbers can be used to represent the norms of timelike vectors; so that a relativistic theory formulated in terms of a metric with one Lorentzian signature versus the opposite—these metrics being different mathematical objects that we, in our community, regard as amounting to a mere difference in notation, not any true physical difference—are seen by this other community as essentially different theories, characterizing very different kinds of spacetime. Such a choice may strike us as odd, but there is nothing inherently wrong with it, should that community decide to do things that way for whatever reason.

Thus, not only are we the masters of our representational tools, where the holding of a representation relation between a chosen tool and an aspect of physical reality is a matter of mere stipulation on our part, but the only things constraining this practice are pragmatic factors, "the needs of the representation users, rather than…essential features of the artifacts themselves," in the words of Craig Callender and Jonathan Cohen (2006, 76). (Callender and Cohen are concerned with scientific representation (which they argue is stipulational in nature), not theoretical equivalence per se, but the point is taken here.)

The result is that investigations into theories' formal or structural or mathematical aspects cannot on their own reveal anything of physical significance. If such things are at all relevant to questions of theoretical equivalence in physics, this can only be by virtue of substantial interpretive stipulations, since the apparent implications of any formal feature or result can be overridden by mere stipulation. In Kevin Coffey's words, "formulations themselves do not stand in relations of equivalence"; they "are equivalent interpreted one way, and inequivalent interpreted another," so that "questions of theoretical equivalence ultimately reduce to questions of interpretation" (2014, 835, 837). Formal considerations are ultimately irrelevant, they are essentially beside the point, given the decisive role played by interpretation. For again, regardless of any formal similarities or differences that may be shown to hold between two formal or mathematical apparatuses, we are always free to stipulate that we are going to use these things differently from what the formal considerations seem to suggest. Any formal criterion thus "starts to look like a sideshow," as Dewar puts it: theoretical formulations are simply "equivalent if they are assigned the same interpretation, and inequivalent if they are assigned different interpretations" (2022, 62). Formal or structural features cannot on their own be relevant to theoretical equivalence in physics—they certainly cannot be playing the pivotal role the formal approach takes them to be playing—for it depends entirely on the specifics of how we choose to interpret them.[9]

When proponents of formal accounts claim to be proving things about the capacities of different devices to represent various physical situations, therefore—as when Sarita Rosenstock, Thomas Barrett, and Jim Weatherall conclude, of two formulations of general relativity, that, "the two theories have precisely the same mathematical structure," in a sense they demonstrate in their article, "and thus, we claim, the same capacities to represent physical situations" (2015, 315)[10]—they cannot really be proving any such thing. For by trivial semantic conventionality, any representational device (including any mathematical formalism used to state a physical theory) trivially has the capacity to represent anything at all, by means of the appropriate stipulations, and no purely formal result concerning the relationship between different such devices can touch this fact. As Teitel puts it, "all representational vehicles considered on their own have the *same* 'capacities to represent physical situations' because of semantic conventionality: namely, the capacity to represent just about any physical situation whatsoever" (2021b, 4137).

The idea that a theory's mathematical or formal devices or aspects in themselves lack physical content—the idea animating the thesis of trivial semantic conventionality and concomitant objection to the formal approach—is implicit in, and bolstered by, a view that is popular in the literature on scientific models and representation. This is the view that scientific representation is not what Mauricio Suárez calls "radically naturalized," where "whether or not representation obtains depends on facts about the world and does not in any way answer to the personal purposes, views or interests of enquirers" (2003, 226). Although there is disagreement as to the correct account of scientific representation, there is wide agreement in the rejection of any radically naturalized account. The reason is a general acknowledgment that the holding of a representation relation

[9]Coffey writes that, "it's perfectly consistent with my account for there to be some instances in which formal considerations suggest theoretical equivalence in the absence of any specific interpretation" (2014, 837–8). However, given what he says elsewhere in his article, alongside his general thesis that theoretical equivalence is interpretive equivalence, it is not entirely clear this is consistent. Perhaps the thought is that formal considerations may hint at this without indicating anything more conclusive; but then questions of equivalence still ultimately boil down to questions of interpretation, formal criteria being effectively irrelevant.

[10]Compare Weatherall (2016a); Hudetz (2019); Fletcher (2020); Dewar (2023b); Weatherall and Meskhidze (2025); cf. Nguyen (2017).

between a given device and target depends essentially on the user's purposes, the intended audience, the context, and other such factors that are external to the intrinsic natures of the device and target.[11] A variety of things may factor into a representation, and these may include such intrinsic features, but whether a representation holds is never purely a matter of such things. As James Nguyen puts it, "Mathematical structures, and models more generally, do not represent without some sort of intentional act of model users associating model facts with target claims" (2017, 994). Notice how this dovetails with trivial semantic conventionality. If the various representational devices we use in science do not in themselves have content—if (as per trivial semantic conventionality) it is freely up to the users of the representation to stipulate this—then it seems as though no radically naturalized account can be correct. (Discussing the literature on models and representation further would take me too far afield, though I will touch on it at points.)

Strands of other literatures employ this idea, too. Consider the debate over wavefunction realism: whether the mathematical wavefunction appearing in standard formulations of quantum mechanics directly represents a physical field. Opponents will often point out that the mere appearance of a mathematical object in a theoretical formulation does not mean that it must directly represent something physically real, and more generally that a theory's mathematical formulation does not on its own entail what the physical world is like.[12] As Tim Maudlin says, "We can only be sure what a piece of mathematics is supposed to represent (if anything) by being told by the expositor of a physical theory" (2018): it takes the users of a mathematical formalism to endow it with physical content. Claims to the effect that the mathematical models we use in physics do not come with ready-made physical content have recently been waged in the debate over spacetime substantivalism.[13] Nguyen (2017) uses the idea in arguing against formal accounts of theoretical equivalence specifically. These discussions, too, rely on the thought that mathematical devices in themselves lack physical content, the core idea animating the claim that no purely formal feature or result can be significant to questions of theoretical equivalence in physics.

In all, representational devices, including the mathematical structures or formalisms we use to state physical theories, do not in themselves possess physical

[11]This is a theme running through recent literature on scientific representation. See Suárez (2010); Boesch (2015); Frigg and Nguyen (2021); Nguyen and Frigg (2022) and references therein.

[12]See for example Maudlin (2010); Allori (2013); Goldstein and Zanghì (2013); Bokulich (2020).

[13]See Dasgupta (2011); Pooley and Read (2025); Teitel (2021a); Pooley (2022). Jacobs (2023b) is a response that is in the spirit of the present paper.

content. It is up to us to decide, to stipulate, what representational tools we are going to use and what we are going to use them for. And there are no constraints on this practice coming from the inherent nature—including the formal features—of the tools themselves; the only things governing the choice of representation are factors extrinsic to that nature. When it comes to theoretical equivalence in physics, formal criteria, which can always be overridden by means of the relevant representational stipulations, must be essentially irrelevant.

3. Representing physical theories

The above points about our representational practice are to an extent unassailable. It is certainly true that formal or mathematical features or tools in themselves do not say anything or other about the nature of the physical world. A stipulation on our part is required to connect the mathematics to the world, to indicate what it represents. There is as well a lot of leeway in what representational stipulations we can make, the choice often being made on pragmatic grounds.

It does not follow, however, that formal criteria must be essentially irrelevant to a physically significant notion of theoretical equivalence, so that any account championing a formal sense of equivalence is bound to fail. The reason, we will see, is that when it comes to representation in physics—particularly the mathematical representation or formulation of a physical theory, the object of attention in the theoretical equivalence debate—there are limits to the thesis of trivial semantic conventionality. This is something that has not been made explicit in these discussions, perhaps because at first glance it seems insignificant or obvious or irrelevant. Yet bringing the point to light has larger implications, for it reveals that a theory's formal aspects can have physical significance independently of particular interpretive assumptions. This in turn opens the door to the physical significance of formal relationships between potentially different theories, thereby paving the way for a middle way on theoretical equivalence—all the while granting that the formal tools we use in physics do not in themselves say anything about the nature of physical reality, so that we can only assert the physical significance of any formal feature or result with some care.

To see the point, imagine that you are a physicist working with a classical Newtonian physics, a physics broadly of particles moving around and interacting by means of forces in three-dimensional space. (Assume substantivalism for ease of discussion.) Suppose that you want to represent this physics mathematically. You want to use some mathematical apparatus or other to describe the various physical systems and situations and interactions it characterizes.

Now imagine that I hand you the set of integers, and I say: go ahead, use this as your mathematical device. After all, we just learned that we can use any representational device we like to represent anything at all, including anything at all in physics. So go ahead and create a mathematical representation of this physics using (only) the integers. Afterwards, we can go back and evaluate whether there are any pragmatic reasons for rejecting this particular choice of representational tool. But that will only tell us about our representational practice and what tools happen to be useful for us in this context. It won't reveal anything about the inherent nature of the physics we aim to represent or of the device we are using to represent it.

Notice that it is going to be very, very difficult for you to make this particular choice of representation. In fact, it is impossible. You cannot represent different locations in three-dimensional space, or particle masses, or inter-particle forces, using only integers. There simply aren't enough integers available to represent all the different possible values of these quantities, and the different states these systems could be in. In this case, you cannot stipulate your way into using this as your representational vehicle. Of course, for any *particular* situation, for some particular configuration of Newtonian particle positions and momenta, you will be able to represent it using the integers (even a single integer), by stipulation. But the integers won't work for a mathematical representation of the theory as a whole. There is no general stipulation you could make under which arbitrary Newtonian systems or situations can be represented by integers.

Nor is this representational failure merely a matter of pragmatics or other sorts of extrinsic factors having to do with us. It's not that it would be *more difficult for us* to use the integers than (say) the real numbers; or that if we had different aims and interests, or possessed sufficient ingenuity and determination,[14] or were in a different context, then we could do so. We can't—it is not possible. The reason does not have to do with us, but flows directly from the intrinsic nature of the representational vehicle, on the one hand, and the physics, on the other. The integers simply do not have the right structure for a Newtonian physics, and no amount of stipulation on our part can make this be otherwise. Similarly, it is not the case that the integers do represent this physics, interpreted one way, and do not represent it, interpreted another way. This mathematical apparatus simply cannot do the representational job, regardless of any such further interpretive stipulations one might claim to make.

Now, this is assuming that we want distinct entities or aspects of the formalism to represent distinct physical situations; that for each different possible value of a physical quantity, and each different possible physical state, there is a distinct mathematical entity or feature available to represent it—a distinct mathematical

[14]The phrase is from Swoyer (1991, 450).

entity corresponding to each possible mass or spatial location, for example. It is assuming, in other words, that we do not allow the mapping from the different possible physical states or quantity values, to the mathematical entities we are going to use to represent them, to be many-to-one, as in the case of Newtonian physics and the integers. (The mapping should instead be injective.) But this seems like a very mild assumption. Indeed, it seems a minimal condition of adequacy on the mathematical representation of a physical theory that it be capable of characterizing distinct physical situations by means of distinct aspects of the representational vehicle. It is certainly hard to imagine having anything like a physics without it.[15]

I have been referring to the mathematical formalism we use to state a physical theory as a representational device (and more generally, using a theory's "formulation" and "representation" interchangeably). This is something of a departure from the literature on scientific models and representation, where it is often suggested that the mapping from the represented item (the target system) to the representational device (the model) need not be one-to-one, and is instead onto. The focus in that literature is on individual systems and our models of them that invariably leave something out, via idealizations or simplifying assumptions or abstractions, so that not all features of the target will be represented in the model.[16] The subject matter here is the formalism for stating an entire physical theory, for which we do require that different physical situations be representable by distinct aspects of the formalism, that no possible physical situation is left out; and which need not more generally be construed in terms of models. (I remain neutral on how to construe a theory's formalism or structure in an effort to remain neutral on what the correct formal criterion will be; recall n. 1.)

Here, then, is the limit or caveat to the thesis of trivial semantic conventionality. It is not the case that we can in principle stipulate that any vehicle be used to represent anything at all in physics—that we can bring about "representational relations between arbitrary relata by dint of mere stipulation" (Callender and Cohen, 2006, 74)—since not any candidate vehicle will be capable of representing the physics in question. In particular, not any formal or mathematical device can be used to formulate a given physical theory. For not any such device will have the discriminatory expressive resources necessary to capture all the distinctions in the physics; not any device will satisfy what we may call the cardinality

[15] de Haro and Butterfield (2025, 11.3.4) defend a similar idea they call the "univocality of the interpretation of physical theories." They regard this as a desirable feature rather than something more like a condition of adequacy.

[16] The literature is vast and I cannot do it justice here. See Downes (2011, 2021); Frigg and Nguyen (2020, 2021); Nguyen and Frigg (2022); Frigg and Hartmann (2025) and references therein.

constraint.

Relatedly, it does not follow from the fact that a representational device in itself lacks physical content that we cannot say it has the *inherent capacity* to represent various physical situations—something which, we saw, may be said by proponents of the formal approach in arguing that different theories do or do not have the same such capacities. The integers, after all, by their very nature lack the capacity to represent a Newtonian physics. More generally, certain formal or mathematical devices, by their very nature (because of "essential features of the artifacts themselves," in the earlier words of Callender and Cohen), will be incapable of representing the physics in question; and no mere stipulation on our part will allow us to circumvent this.

You might respond that the thesis of trivial semantic conventionality reveals that we *can* stipulate the integers into being our representational tool in any given case. After all, nothing prevents us from simply declaring: "I hereby decree that I will use (only) integers for articulating the physics of Newtonian systems." What is really going on then is not a failure of representation, but a *mis*representation, a representation that "portrays its target as having features it doesn't have" (Frigg and Nguyen, 2021). Such a stipulation would reveal that we are mistaken about the nature of the target, portraying the continuous nature of the space of states and physical quantities and physical space as though they are discrete—a case of inaccurate representation, not a failure of representation altogether.

Above I proposed a condition of adequacy on the mathematical representation or formulation of a physical theory. This seems a minimal condition on such a representation, not just an accurate one. We could not even use the formalism to characterize and distinguish among all the different systems the theory claims to govern if the constraint were not satisfied—a failure to represent the theory.[17] You might *say* that you are going to represent a Newtonian physics using only the integers; you would not thereby succeed.[18] (If you are unconvinced by this,

[17]It is if anything a case of mistargeting, arguably also a failure of representation. See Suárez (2003), who however regards mistargeting as a type of misrepresentation. I refrain from entering into the deep waters of accounts of scientific representation, albeit the above may hold implications for such an account. (Some of what I say aligns with the structural account, but there are also differences.)

[18]Imagine I were to write down "$\mathbb{Z}$" and decree that this symbol will be my device for representing Newtonian physics. It is hard to see this as a mere misrepresentation, portraying the physics as possessing features it doesn't have; it is hard to see how it counts as a representation of the physics at all (compare Boesch's (2017, 978) case of scientists drawing a star and stipulating that it represents predator-prey relations). This example may seem more closely tied to pragmatic failures than the kind of mismatch I am pointing to, but it helps underscore the idea that it cannot be reasonable to require of an account of theoretical equivalence that it render a certain verdict given *any* representational conventions: something like our usual conventions must be

you should at least grant that the badness of the representation flows from the intrinsic nature of the integers and the physics and the relationship between them, rather than pragmatic or other sorts of extrinsic factors that can be overridden by means of appropriately different aims or stipulations on our part. Either way, the source of the difficulty is the integers' inherent nature or structure.)

You might continue to think the cardinality constraint concerns our own peculiar situation, so that it must be possible to stipulate the integers into being one's representational tool given relevantly different aims and circumstances. After all, if we did not care about physics or having a physics in any recognizable form, then we could stipulate that we are going to use the integers for Newtonian physics (imagining, for the sake of argument, that making such a stipulation does not itself amount to having an interest in physics!). But this is just to point to the fact that physics as an endeavor depends upon our own situation, in the sense that God would have no need for a physics, let alone any particular representation of it. It does not follow from the fact that physics is the kind of thing that finite creatures like ourselves are particularly interested in and have a use for that the cardinality constraint has to do only with us and our circumstances. Consider that the cardinality constraint does not just let creatures like ourselves represent a given theory, but any inquirer—even God or a Laplacean demon could not get by with the integers for Newtonian physics. The inability of the integers to serve as a representational tool intuitively does not have to do with us and our own parochial situation (except insofar as physics itself does), but flows from the inherent mismatch between the mathematical apparatus and the physics.

We can now update the point about the nature of our representational practice, explicitly taking into account the limit to trivial semantic conventionality. Thus: any representational device whatsoever can in principle be used to represent anything in physics whatsoever, simply by means of the appropriate stipulations—(caveat:) so long as the device has the requisite discriminatory expressive powers, allowing distinct things in the physics to be represented by distinct aspects of the device; so long as it satisfies the cardinality constraint. In particular, when it comes to the representation or formulation of a physical theory (the focus of the theoretical equivalence debate), any formal or mathematical apparatus can be used, so long as it is able to characterize distinct physical situations by means of distinct things in the apparatus. Then, from among all the representational vehicles satisfying this constraint (from among all those that can serve as a representational tool), we may use any one, by stipulation.

The cardinality constraint may seem hardly worth mentioning. You might

held fixed. (Compare Jacobs (2023b) on reasonable representational conventions.) I thank an anonymous referee for the example and for this way of thinking about it.

respond that the underlying idea is already very familiar from discussions surrounding the Newman objection, Putnam's model-theoretic argument, and the like.[19] It may even be the sort of thing that proponents of trivial semantic conventionality and cognate ideas have in mind when they say that "just about" any device can be used to represent anything as being "virtually" any way at all.[20]

However, making this explicit brings further implications to light. For the fact that the integers cannot be used as a representational vehicle in this case reflects something about the theory and the type of physical reality it describes (it reflects in particular the continuous nature of the basic physical quantities): the fact that the integers cannot furnish a mathematical representation is physically significant. That is to say, the formal or mathematical or structural features of a theory's representational devices (such as the continuous nature of any mathematical apparatus used to formulate a Newtonian physics and to represent the various systems it describes)—these features virtually on their own, independent of substantial and detailed interpretive stipulations—can have real physical significance.[21] The only sort of stipulation needed is a minimal one to the effect that we are going to use such-and-such formal device to represent some physics; or, in a nod to the integers case, that we are going to try to use such-and-such device to represent some physics, in this case a broadly Newtonian physics. That much is needed to establish the representational connection between the mathematical device and the physics.[22] No detailed interpretive claims beyond that are required, as though the only reason we cannot employ the integers is for want of such a stipulation. (In particular, no stipulation regarding which features of the mathematics are physically significant, and which not, will allow this. Consider, by way of contrast, how we can formulate the physics of classical electromagnetism in terms of the potentials by means of just such a stipulation.)

To put it another way, it is not just pragmatic or other sorts of extrinsic factors that constrain our representational choices in physics—factors that allow us complete latitude to do as we like in our choices of representation, subject only to our aims in the context, and where any representational tool can be chosen by mere stipulation—but inherent features of the candidate vehicle and target

[19]I would suggest it is not quite the very same thing, but set such exegesis aside.

[20]That said, this on its own does not touch the point with respect to the formulation or representation of an entire physical theory. It is also worth emphasizing that even if that was the intent, the discussions have not been explicit about it. (Jacobs (2023b, 5) says that Teitel (2021b) points something like this out; however, Teitel does not in fact do that in his paper.)

[21]"Can have": some such features, e.g., those depending on arbitrary choice of unit or gauge, will be merely conventional. I do not address here the large question as to which ones these will be. See North (2021) for one account.

[22]This is granting the thought that representation in science is not radically naturalized in the sense of section 2. One could reject this (French, 2003); I do not discuss this here.

physics. The formal features of the representational tools we use in physics are not all entirely up to us, but are constrained by the nature of things. As a result, we can learn about that nature by examining those features.

So there are limits to the thesis of trivial semantic conventionality when it comes to representation in physics, particularly the mathematical formulation of a physical theory. (I do not claim to draw conclusions for scientific representation in general (n. 17) much less representation more broadly.) And it does not follow from this thesis, the explicitly limited one, that there can be no physical significance to any purely formal feature or result; in particular, that nothing physically significant can possibly follow from claims concerning the representational capacities of various formal or mathematical devices. The inherent inability of the integers to furnish a mathematical representation of Newtonian physics gives the lie to that claim. More generally, for any physical theory, there are going to be constraints on the kinds of mathematical devices that can be used to formulate it, given minimal conditions of adequacy on the representation of a physical theory. There are going to be limits to trivial semantic conventionality, the cardinality constraint being one.[23] Plausibly, there will be others, we will see.

But first, you might wonder whether the above case really makes contact with the philosophical question at issue. That case concerns the capacity of a particular mathematical apparatus for representing some physics, where the conclusion that it lacks the capacity flows from the fact that the physics has been antecedently specified. Yet the interesting philosophical question—whether two physical theories are genuinely equivalent, and what it is that underlies this judgment—arises for theoretical formulations in cases where we do not already know the full nature of the physics, and furthermore seems orthogonal to the representational capacities of a given device for one of them. After all, if we already had in hand two fully-specified accounts of the nature of physical reality according to each of two theoretical formulations, then the question of their equivalence, of whether they "say all the same things," would not arise. This would be on their face, and more to the point, the representational capacities of certain devices for either one would be neither here nor there.

The case makes contact with the philosophical question, however, first of all by undercutting the basis of the chief objection to the significance, if not the very relevance, of formal criteria to theoretical equivalence; namely, that since formal or mathematical devices in themselves lack physical content, nothing physically significant can follow from examining different such devices and the

[23] Compare Jacobs (2023b, 5–6). See Boesch (2017) on a different constraint on representation coming from the social nature of science (compare Wallace (2024, 15)); see also Hall and Ramírez (2024). See Dewar (2024) for a different concern about trivial semantic conventionality.

relationships among them. In addition, the physics as described is akin to the kinds of cases debated in the literature. Philosophical debate revolves around theoretical formulations that have been equipped with some claims as to their respective laws or ontology or some such aspects (if not to the extent of already having settled all the interesting questions about equivalence)—theoretical formulations that count as candidate theories of physics in the first place. They are instances of what Laura Ruetsche (2011, ch. 1) calls partially interpreted theories. (It does not make much sense to inquire into the physical equivalence of two bare mathematical structures.) Thus, it is a live question in the philosophical literature whether the traditional formulation of Newtonian gravitation and geometric Newtonian gravity are theoretically equivalent, even though all participants to the debate make some assumptions regarding their respective laws and ontology and the connection to empirical data. Even so, there is debate as to whether these should be regarded as the same theory, differently stated; where it has been argued within the formal camp that even though they might seem to make differing ontological claims, we learn from the relevant formal considerations that they are in fact different descriptions of the same underlying physical reality.[24] The cases discussed here are like this: partially interpreted theories, whose (in)equivalence with other partially interpreted theories is open to investigation, and for which the representational capacities of various formal devices may well play a role, for reasons we will continue to see.

4. Onward toward a middle way

The above has implications for the theoretical equivalence debate beyond the undercutting of a chief objection to the formal approach. For if a theory's formal or structural or mathematical features can virtually on their own be physically significant ("virtually": grant that we are using, or aiming to use, a given mathematical apparatus to represent some physics), reflecting inherent aspects of the physical reality being characterized by the theory, then (formal) relationships between these features of potentially different theories can be physically significant. They can reflect similarities and differences in the nature of the physical reality being characterized by each of the theories.

Thus if, for example, one physical theory, but not another, can (on pain of satisfying the cardinality constraint) be formulated in terms of the integers, then this indicates something physically significant about the two theories and the nature of the worlds they depict. It indicates that the theories cannot be fully equivalent in a sense that is relevant to physics: they are saying different

[24] See Weatherall (2016a). Cf. Knox (2014); North (2021, ch. 7); Coffey (2024).

things about the nature of physical reality. Recall that no mathematical apparatus with the cardinality of the integers can be used to formulate a Newtonian-style physics and to represent the various systems it governs. It follows that another theoretical formulation that has the cardinality of the integers—another theory that can be formulated in terms of the integers—cannot be equivalent in the relevant sense: it must be depicting a different kind of physical reality, with different kinds of physical systems.

To put it differently, take two theoretical formulations, one in terms of integers, the other in terms of real numbers. Suppose that each of these satisfies the cardinality constraint, and that the latter cannot be reformulated in terms of the integers without violating it. For two theoretical formulations such as these, it is not the case that they are equivalent interpreted one way, and inequivalent interpreted another (in the earlier words of Coffey), so that whether the theories are equivalent depends on detailed and substantial interpretive stipulations on our part. For it is not open to interpret the formulation given in terms of real numbers in such a way that it is equivalent, in the relevant sense, to the one given in terms of integers. In a sense that is significant for physics, these are not theoretically equivalent, regardless of any further interpretive claims one might (try to) make.[25] (You might *say* that you are going to interpret these as representing the same physics; you would not in the relevant sense succeed.)

In other words: these fail to satisfy a necessary condition for theoretical equivalence in physics. This is a substantive conclusion concerning theoretical equivalence drawn on the basis of the intrinsic natures of two mathematical structures and the relationship between them (in this case, their cardinality mismatch), given a minimal condition of adequacy on the mathematical formulation of a physical theory (alongside the barest of stipulations that we are using, or are aiming to use, these mathematical apparatuses to represent some physics). More generally, if two theoretical formulations fail to be equivalent in the relevant structural or formal or mathematical sense, then they will fail to be equivalent in a sense that is significant for physics: they must be depicting different kinds of physical realities. The formal inequivalence suffices for this conclusion.

Although the cardinality constraint and the conclusion for a theory that cannot be formulated in terms of the integers vis-à-vis one that can are reasonably

[25]Coffey might seem to agree: "if formal considerations are relevant, my claim is that their relevance obtains in virtue of how they shape interpretive judgments. The fact that particular structural relationships obtain between different formalisms may guide or constraint [*sic*] how those formalisms are interpreted, and thus whether they're judged theoretically equivalent" (2014, 837). As noted above (n. 9), it is not clear that this is compatible with the rest of his discussion; but more importantly, this points to the significance of formal criteria in a way that is not being sufficiently acknowledged.

clear, it is not very hard to see how things can generalize from here. Let me discuss one further example and gesture at a few more to show this. I do not have room to explore these or other cases in detail (it would take another paper to fully investigate these cases and the requirements on theoretical formulations that appear to be implicated in them, let alone others) but the following will suffice to convey the gist. The verdicts on these cases will be less clear-cut; yet regardless of whether you agree with these particular conclusions, it should be clear there are going to be some such further constraints on the mathematical or formal tools we can use to represent a physical theory, and hence some such further limits to trivial semantic conventionality, with corresponding implications for theoretical equivalence.

Suppose again that you are working with a broadly Newtonian physics, now conceived of as set in four-dimensional (classical) spacetime. Again you want to represent this physics mathematically. Imagine that I hand you a bare topological structure or topological space, and I say: go ahead, create a mathematical representation of this physics using this mathematical device. After all, we can stipulate anything into being a representation of anything else; there are no constraints on our choices of representation other than pragmatic ones, in particular none coming from the inherent natures of the vehicle and target; and so on.

Once again, this representational stipulation is not going to be possible, and again not because of things having to do with us and our own peculiar aims and circumstances, but because of an inherent mismatch between the nature of the device, on the one hand, and that of the physics, on the other.

Topology is often referred to as "rubber sheet geometry." It concerns the features of a space that are invariant under continuous transformations—stretching or squeezing, without tearing or pasting, as though the space is a rubber sheet. A topology will specify whether a given curve is continuous, for instance, since you cannot alter the continuity of a curve just by stretching and squeezing the space: continuity is a topological invariant. But a topology does not have the wherewithal to say whether a given curve is straight: straightness is not a topological invariant, but alters under continuous transformations. There is no notion of "straight versus curved line," in other words—the straightness of curves is not defined. A topology does not sort curves into those that are straight and those that are not; that requires further structure (an affine structure).

Newtonian physics possesses a law of inertia, which says that a body travels with constant velocity unless acted on by a net external force. An object behaves differently according to this law depending on whether its motion is inertial; that is, depending on whether its spacetime trajectory is straight. This requires that there be facts about the straightness of trajectories, and yet a topology lacks such facts. This mathematical apparatus is therefore unable to supply a representation of this physics. A topology simply does not have the right nature or structure for a physics with a law of inertia, and no representational stipulation on our part can make this be otherwise. (You might say that you are going to use a bare topology; you would not thereby succeed.)

Another way to think of it. Newtonian physics distinguishes between a world in which a particle travels on a straight spacetime trajectory and there are no net external forces acting on it (which is a possible world or situation according to this physics), and a world in which a particle does not travel on a straight trajectory even though it is not subject to any net external force (which is not possible). Since a topology does not say whether a given trajectory is straight or not, since the straightness of trajectories is simply not defined, this mathematical structure is unable to distinguish between worlds or situations that are physically possible, and those that are not; a theory that is mathematically formulated in terms of (only) this structure will then be unable to sort worlds or situations into those that are possible and those that are not. Given the condition that the formulation or representation of a physical theory must be able to distinguish between situations that are allowed by the theory and situations that are not—which seems like a minimal condition of adequacy—it follows that we will be unable to formulate this physics using only a topological structure. More generally, again: there are constraints flowing from the inherent nature of the physics and any candidate mathematical apparatus for representing it, given minimal conditions of adequacy on the representation of a physical theory, which cannot be overridden simply by means of certain interpretive stipulations on our part. (Keep in mind once again that the claim concerns the formulation of an entire physical theory, the representation of a whole body of physics, not any one particular system.)

It follows that if one physical theory can be formulated using only a topological structure, and another theory cannot be (given the relevant conditions of adequacy), then the two theories are not equivalent in the relevant sense: they are saying different things about the nature of physical reality. It is not the case that one theoretical formulation that employs only a topological structure, and another that additionally possesses, because it requires, an affine structure, are equivalent interpreted one way, inequivalent interpreted another, in a sense that is relevant for physics. The formal or structural inequivalence suffices for a genuine theoretical inequivalence, whatever other interpretive stipulations you might claim to make.

Other potential examples should come to mind. Imagine trying to formulate a classical (nonrelativistic) theory using the mathematics of a Minkowski spacetime structure. Classical physics assumes there are facts as to which events are simultaneous. Minkowski spacetime lacks the absolute simultaneity structure needed for such facts: there is no preferred simultaneity slice structure. This mathematical structure does not sort sets of events into those that are simultaneous and those that are not; the simultaneity of events is not defined. It is therefore incapable of being used to formulate such a physics,[26] and any other physical theory that can be so formulated (Einstein's special relativity, say) will fail to be equivalent in the relevant sense. Or think of trying to formulate an Aristotelean-style physics, according to which certain objects naturally move toward a particular spatial location, using the mathematical structure of a homogeneous space. You will not be able to. Characterizing objects' motions requires reference to a preferred spatial location, which this mathematical structure lacks. Any other physical theory that can be so formulated will then fail to be equivalent in the relevant sense.

These are further cases for which we will be unable to create the mathematical representation in question, and not for the kinds of extrinsic factors that vanish given appropriately different aims or stipulations or other such things on our part, but because of an inherent formal or structural mismatch between the mathematical apparatus and the physics. (The mathematical apparatus in particular lacks certain structure required by the physics.) However, these cases are not as clear-cut, turning on further issues I cannot sufficiently address here, such as how to construe the relevant notion of structure, how to compare different structures, and the nature of frame- or coordinate-dependent features or facts (simultaneity is not absolute in Minkowski spacetime, but there is a frame-dependent notion; a preferred origin can be specified relative to a chosen coordinate system).[27] The constraints on representation that are in play are also less immediate, and warrant further exploration.[28] Yet whatever the details, the

[26]If we were to *add* structure, the formalism could do the job. The point is that *this* mathematical structure is incapable; adding to it amounts to adopting a different kind of structure. See Barrett (2015b) for a comparison of classical (both Galilean and Newtonian) and Minkowski spacetime structures (cf. Bradley (2021)). The above point is perhaps easiest to see for Newtonian spacetime, which, Barrett shows, possesses strictly more structure than Minkowski spacetime, in a precise sense. Although Galilean spacetime both possesses more and less structure (in that sense) than Minkowski spacetime, it remains the case that the latter lacks a preferred simultaneity slice structure that the former possesses.

[27]See North (2021) for one discussion of these matters (according to which mere coordinate-dependent features are not part of a given structure (recall n. 21); cf. Barrett (2022a,b), Barrett and Manchak (2024a,b). One assumption I have been making is that there is a fact about the intrinsic structure of a space, as opposed to a conventionalism or anti-realism about this. This is compatible with different ways of spelling out what that structure amounts to, which I remain neutral on for present purposes; different accounts can be found in Sider (2011, 3.4); Wallace (2019); North (2021); Dewar (2022, 2023b). See Dürr and Read (2024) for a recent defense of conventionalism.

[28]Fletcher (2020); Jacobs (2023b); Hall and Ramírez (2024); de Haro and Butterfield (2025); North (2026) are recent discussions in philosophy of physics touching on representational

general point remains. For any physical theory, there are going to be constraints on the formal or mathematical devices we can use to represent it, constraints that flow from inherent features of a candidate device and the physics in question, given minimal conditions of adequacy on the representation of a physical theory. It will then follow that one physical theory that can be formulated using a given device, and another one that cannot be (given those constraints), are not equivalent, in a sense that is significant for physics.

None of this is to say that a formal equivalence suffices for theoretical equivalence in physics—it doesn't, for reasons given by Teitel (2021b) and others (n. 6). Nor is it to say that any viable theoretical formulation must in some sense "match" in all respects the structure of the physics. Take classical electromagnetism formulated in terms of the potentials. There is excess mathematics in the formalism, which we stipulate as bearing no direct physical significance, a stipulation that moreover renders it equivalent to a formulation in terms of fields. We often use without trouble mathematical devices that have a different structure from the physics, simply by making the appropriate interpretive stipulations. Relatedly, theoretical formulations that differ mathematically in some way can oftentimes be rendered equivalent simply by means of such stipulations (as in the case of classical electromagnetism).[29]

It is to say that theories' formal or mathematical or structural features are significant; indeed, that a formal criterion of some kind is necessary for theoretical equivalence in physics. (Not every pair of mathematically inequivalent formulations can be rendered theoretically equivalent simply by stipulation.) The relevant type of formal inequivalence, whatever it turns out to be, suffices for theoretical inequivalence.

5. Conclusion

Representational devices—among them the formal or mathematical tools we use in physics, including the formalisms we use for stating physical theories—do not in themselves possess physical content. Endowing them with such content requires the appropriate stipulations on our part. There is moreover a lot of leeway in the kinds of tools we can use and the representational stipulations we can make.

Even so, we cannot stipulate anything into representing anything at all in

constraints in physics.

[29]Unlike the previous cases involving too little structure, in a case of excess mathematical structure, it seems the relevant representational stipulations can generally override the mismatch. This is an interesting difference worth exploring.

physics: some formal or mathematical devices, by their very nature, will be incapable of representing the physics in question. The formal features of the devices we use in physics are not all completely up to us, but are constrained by the nature of things. (I would go further, though I cannot pursue this here. Thus: not only will some devices be incapable of representing the physics, but some will be incapable of doing so as well as others, in a non-pragmatic sense—as Teitel's imagined community from section 2, while neither being incoherent nor making things particularly difficult for themselves, is still making a worse representational choice, given the nature of the physics.[30])

The result is that theories' formal or mathematical or structural features can be reflective of the nature of things in a way that is significant to questions of theoretical equivalence in physics. For they can reflect physically significant things about a theory and the kind of physical reality it describes, and its relationship to other theories and the kinds of realities they describe, even in the absence of substantial and detailed interpretive assumptions. It is just that, since the formal devices we use do not in themselves possess physical content, and since there will generally be different such devices we can use, we must remember to pay careful attention to the representational stipulations we will invariably make. This is the warning flag raised by trivial semantic conventionality we should heed, even if the unrestricted thesis, when it comes to the representation or formulation of physical theories, is not quite right.

A final, all-too-brief note on something I cannot explore here, but which further underscores the significance of formal criteria in this context. Hans Halvorson (2012, 2016) has argued that scientific theories "have structure," they are structured entities. This is as opposed to the traditional semantic and syntactic conceptions, according to which scientific theories are "flat," unstructured things (bare collections of models or sets of sentences, respectively). Any scientific theory, after all, will make claims concerning the relations that hold between different sentences or models of the theory, claims that are integral to the theory's predictive and explanatory power. An account of scientific theories must take these sorts of claims, which can be captured by an appropriate structure on the set of models or sentences, into consideration. (This is not exactly Halvorson's argument for the idea, but is in its spirit.)

Halvorson's insight lends further support to the idea that we ought not overly demote the significance of formal results when it comes to theoretical equivalence in physics. Physical theories do not exclusively consist of a mathematical structure, so that reasonable verdicts of equivalence cannot be based solely on that structure (a formal equivalence is insufficient for theoretical equivalence).

[30]See North (2026) for discussion.

Nonetheless, any physical theory will possess a structure, representable by various mathematical devices. So it is highly relevant to consider (the representations of) these structures, and the relations between them, when investigating the equivalence of theories. An account of theoretical equivalence must make room for such things to play a significant role, allowing in particular for a structural mismatch of the right sort to signal a theoretical inequivalence. Exactly which formal considerations are the right sort remains an open question, but the way has been paved for taking something of the kind into account.

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