The Hole Argument and Determinism(s)

Neil Dewar

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This paper does two things. First, it reviews the recent debate between Halvorson and Manchak (2022) and Menon and Read (2024), looking for a reading of the former that is sympathetic to the concerns of the latter. Second, it considers whether there is a notion of determinism for spacetime theories that is adequate for the purposes of (Halvorson & Manchak, 2022); it concludes that there is not, but that we learn much of interest by considering the question.

In his (2018), Weatherall proposed that the Hole Argument outlined by (Earman & Norton, 1987) was the result of a mathematical confusion and did not warrant the considerable amount of ink and time that had been expended on it. This has made a lot of people very angry and been widely regarded as a bad move. It has also had the inevitable result of inducing philosophers to expend further ink and time debating Weatherall's views about the Hole Argument, and the philosophical questions that have arisen as a result. This paper continues that trend.

It takes as its starting-point a recent pair of papers from the post-Weatherall literature on the Hole Argument: "Closing the Hole Argument" (Halvorson & Manchak, 2022) and the response, "Some Remarks on Recent Formalist Responses to the Hole Argument" (Menon & Read, 2024). Our first order of business will be to try and mediate somewhat between these two, by explicating Halvorson and Manchak's project in a manner that is sensitive to the concerns raised by Menon and Read. This project will lead us into some reflections on how to think about determinism in General Relativity (GR).

Halvorson and Manchak (2022) present the Hole Argument as having the following schematised form:

(A) Substantivalism.

(B) Some mathematical facts.

(C) Pernicious indeterminism.

Their primary concern is with the nature of the second premise, (B):

> ...what mathematical claim is supposed to serve as the second premise of the hole argument? To state it abstractly, the claim is:
>
> (B1) There are relativistic spacetimes X and Y, a proper open subset O of X, and an isomorphism $\phi : X \to Y$ that changes things in O but not outside O.[1]

We will follow (Halvorson & Manchak, 2022) in taking a *relativistic spacetime* to be a manifold M equipped with a Lorentzian metric g. The question, therefore, is how to formalise this claim—and in particular, the idea of an isomorphism that "changes things in O but not outside O". Halvorson and Manchak propose two ways in which this claim might be formalised. The first proposal is that ϕ changes things relative to some "standard of comparison" isomorphism ψ. This yields:

> (B2) There are relativistic spacetimes X and Y, a proper open subset O of X, and isomorphisms $\psi : X \to Y$ and $\phi : X \to Y$ such that $\phi|_{X\setminus O} = \psi|_{X\setminus O}$ but $\phi|_O \neq \psi|_O$.[2]

Here and throughout, $\phi|_O$ denotes the restriction of ϕ to O (and the same, *mutatis mutandis*, for $\psi|_O$, $\phi|_{X\setminus O}$, etc.). The second proposal is to restrict to the special case $Y = X$, and use the identity map 1_X as the standard of comparison. This yields:

> (B3) There is a relativistic spacetime X, a proper open subset O of X, and an isomorphism $\phi : X \to X$ such that $\phi|_{X\setminus O} = 1_{X\setminus O}$ but $\phi|_O \neq 1_O$.[3]

Note that (B3) really is just a special case of (B2): so if (B3) is true, then (B2) follows.

However, even (B2) and (B3) are not wholly unambiguous, since each of them makes use of an as-yet-unanalysed notion of "isomorphism". So now we need to ask what we should take an isomorphism to be in this context. Halvorson and Manchak argue (for reasons that we will return to) that we should use the standard notion of isomorphism between relativistic spacetimes—namely, *isometry*.

Recall that, given relativistic spacetimes (M, g) and (M', g'), an *isometry* from (M, g) to (M', g') is a diffeomorphism $\phi : M \to M'$ such that $\phi^* g' = g$. Here, ϕ^* is the

[1](Halvorson & Manchak, 2022, §5)
[2](Halvorson & Manchak, 2022, §5)
[3](Halvorson & Manchak, 2022, §5)

pullback, which may roughly be thought of as an operation mapping a metric g' on M' to a metric $\phi^* g'$ on M, such that the value of $\phi^* g'$ at $p \in M$ corresponds to the value of g' at $\phi(p) \in M'$. Note that this is indeed only a rough definition, since we have not made precise what "corresponds to" means: unless $p = \phi(p)$, then we cannot make straightforward identity-claims between the value of the metric $\phi^* g'$ at p and the value of the metric g' at $\phi(p)$. Still, the rough notion will be good enough for our purposes. We will also sometimes want to use the notion of the *pushforward*, which is the inverse of the pullback: again in rough terms, for $\phi : M \to M'$, the pushforward maps any metric g on M to a metric $\phi_* g$ on M' such that the value of g at p corresponds to the value of $\phi_* g$ at $\phi(p)$. One can show that ϕ is an isometry from (M, g) to (M', g') iff ϕ is a diffeomorphism from M to M' such that $\phi_* g = g'$. For rigorous definitions of pullback, pushforward, and isometry, see (Malament, 2012, §1.5).

If we do as Halvorson and Manchak suggest and take "isomorphism" in (B2) and (B3) to be "isometry", then we obtain:

(B2*) There are relativistic spacetimes X and Y, a proper open subset O of X, and isometries $\psi : X \to Y$ and $\phi : X \to Y$ such that $\phi|_{X \setminus O} = \psi|_{X \setminus O}$ but $\phi|_O \neq \psi|_O$.

(B3*) There is a relativistic spacetime X, a proper open subset O of X, and an isometry $\phi : X \to X$ such that $\phi|_{X \setminus O} = 1_{X \setminus O}$ but $\phi|_O \neq 1_O$.

However, Halvorson and Manchak employ a theorem due to (Geroch, 1969) to prove the following result:

Theorem 1. Let (M, g) and (M', g') be relativistic spacetimes. If ϕ and ψ are isometries from (M, g) to (M', g') such that $\phi|_O = \psi|_O$ for some non-empty open subset O of M, then $\phi = \psi$.[4]

Slightly more informally: if two isometries disagree at all, then they disagree on every nonempty open set.[5] This result immediately rules out (B2*), and hence (B3*). Thus,

[4](Halvorson & Manchak, 2022, §5)

[5]Menon and Read (2024), in my view, somewhat mischaracterise the significance of Theorem 1. They present its dialectical role as being to refute a claim they label `Distinct Isometries`: "For any two isometric Lorentzian manifolds [relativistic spacetimes], there is more than one diffeomorphism relating those Lorentzian manifolds which witnesses their being isometric." (p. 2) Now, `Distinct Isometries` is indeed false, but it is false for the much simpler reason that there exist relativistic spacetimes that admit no nontrivial spacetime symmetries. Rather, the point of Theorem 1 is to refute (B2*): stated in terms as similar as possible to `Distinct Isometries`, this is the claim that *there exist* two isometric Lorentzian manifolds for which there is more than one diffeomorphism relating those Lorentzian manifolds which witnesses their being isometric *and which agree on some nonempty open set.*

Halvorson and Manchak conclude, "there are no hole isomorphisms"—provided, that is, that isomorphisms are understood as isometries.

One natural response might be to think that this shows that—at least for the purposes of expounding the Hole Argument—the relevant standard of isomorphism is *not*, in fact, isometry. Perhaps we should instead have taken *diffeomorphisms* to be isomorphisms. After all, one might think, diffeomorphism is the standard notion of isomorphism between manifolds; and is not the target of the Hole Argument that version of substantivalism that views "the manifolds M of the models as representing spacetime"?[6] If we do this, and substitute "diffeomorphism" for "isomorphism" in (B2) and (B3), we obtain:

(B2**) There are relativistic spacetimes X and Y, a proper open subset O of X, and diffeomorphisms $\psi : X \to Y$ and $\phi : X \to Y$ such that $\phi|_{X \setminus O} = \psi|_{X \setminus O}$ but $\phi|_O \neq \psi|_O$.

(B3**) There is a relativistic spacetime X, a proper open subset O of X, and a diffeomorphism $\phi : X \to X$ such that $\phi|_{X \setminus O} = 1_{X \setminus O}$ but $\phi|_O \neq 1_O$.

(B2**) and (B3**) have the virtue (compared to (B2*) and (B3*)) of being true. However, as Halvorson and Manchak convincingly argue, they have the vice of being irrelevant. To take diffeomorphisms as isomorphisms is to disregard the metrical structures on our spacetime models—but those structures are what codify the physically relevant information about those models! One way to see this is to observe that (B2**) and (B3**) are equivalent, respectively, to the following claims:

(B2***) There are (metrisable) manifolds M and M', a proper open subset O of M, and diffeomorphisms $\psi : M \to M'$ and $\phi : M \to M'$ such that $\phi|_{M \setminus O} = \psi|_{M \setminus O}$ but $\phi|_O \neq \psi|_O$.

(B3***) There is a (metrisable) manifold M, a proper open subset O of M, and a diffeomorphism $\phi : M \to M$ such that $\phi|_{M \setminus O} = 1_{M \setminus O}$ but $\phi|_O \neq 1_O$.

So on this reading, the metrical structure turns out to be purely epiphenomenal to the content of (B2) and (B3). But insofar as we are interested in GR as a physical theory, it seems clear that (B2***) and (B3***)—and hence, (B2**) and (B3**)—therefore cannot be telling us much of interest.

To this extent, then, I agree with Halvorson and Manchak's analysis: no matter how the term "isomorphism" is understood in (B2) or (B3), one cannot obtain a claim that is

[6] (Earman & Norton, 1987, p. 518)

suited to serve as premise (B) of the schematised Hole Argument with which we began.[7] However, I submit that what this demonstrates is that the fact appealed to in premise (B) does not, in fact, fit the template of (B2) or (B3) at all. Rather, the relevant fact is the following:

(B4) There are relativistic spacetimes X and Y, a proper open subset O of X, a diffeomorphism $\psi : X \to Y$, and an isometry $\phi : X \to Y$, such that $\phi|_{X\setminus O} = \psi|_{X\setminus O}$ but $\phi|_O \neq \psi|_O$.

To get this from (B2), we have to do a non-uniform substitution of "isomorphism": we replace the first occurrence of "isomorphism" by "diffeomorphism", and the second by "isometry". The specific version of (B4) that is typically invoked in the Hole Argument is the following:

(B5) There are relativistic spacetimes (M, g) and (M, g'), a proper open subset O of M, and an isometry $\phi : (M, g) \to (M, g')$, such that $\phi|_{M\setminus O} = 1_{M\setminus O}$ but $\phi|_O \neq 1_O$.

This is a special case of (B4), just as (B3) was a special case of (B2); note, however, that rather than specialising to the case $X = Y$, we have instead expanded X and Y as (M, g) and (M', g') respectively, then specialised to the case $M = M'$.

Unlike (B2*) and (B3*), (B4) and (B5) are true; and unlike (B2**) and (B3**), they do not ignore the metrical structure on the relativistic spacetimes at issue. Indeed, it is not hard to see that (B5) is exactly the claim that Earman and Norton (1987) appeal to in proving the "Hole corollary" to their "Gauge Theorem". Thus, the mathematical considerations presented by Halvorson and Manchak (2022) do not bear on the Hole Argument as it is standardly understood in the literature.

This diagnosis of the situation is very similar to that offered by (Menon & Read, 2024), who also (p. 9) identify the crucial move in the Hole Argument as being that of "comparing any two isometric models of general relativity using diffeomorphisms which do not witness those models' being isometric".[8] Similarly, Luc (2024) holds that Halvorson and Manchak's use of Theorem 1 "is not applicable to situations that are

[7] Strictly speaking, we haven't fully demonstrated this claim, since we have not shown that *every* possible notion of isomorphism is unsuitable. However, it is highly implausible that any notion of isomorphism other than isometry or diffeomorphism could be relevant to the Hole Argument.

[8] Indeed, let us say that given relativistic spacetimes (M, g) and (M, g'), a *hole isometry* is an isometry $\phi : (M, g) \to (M, g')$ like that in (B5): i.e. one such that $\phi|_{M\setminus O} = 1_{M\setminus O}$ but $\phi|_O \neq 1_O$, for some open $O \subset M$. Then (B5) essentially coincides with the following claim, that Menon and Read identify as a key premise of the Hole Argument:

> `Hole isometry`: Given a metric manifold (M, g), there exists a distinct metric manifold (M, g') such that a (non-trivial) hole isometry [. . .] exists between them.[9]

relevant for the Hole Argument", because (in the notation above) they only consider the case where the relativistic spacetimes X and Y are identical, rather than the case where the underlying manifolds (of two distinct relativistic spacetimes) are identical.

All of this, then, raises a concern that Halvorson and Manchak's analysis is simply misdirected: they sought to show that the Hole Argument had no underlying mathematical fact that could support it, but were in fact talking about something that was not the Hole Argument. However, the dialectical situation here is more subtle than that. That this is not the whole story is indicated by the following: Halvorson and Manchak do, in fact, explicitly consider the possibility of invoking (B5)! In a footnote, they make the following remark:

> In the hole argument, the relevant spacetime models are of the form $X = (M, \phi^* g)$ and $Y = (M, g)$. In this case, one might propose the map $1_M : X \to Y$ as the default for 'does not change things'. However, 1_M is a morphism in the category **Lor** [i.e. is an isometry from $(M, \phi^* g)$ to (M, g)] only if $g = (1_M)^* g = \phi^* g$, in which case $X = Y$. If $X \neq Y$, then $1_M : X \to Y$ is not even a physical equivalence, and so it is not a good standard for 'does not change things'.[10]

This contrasts directly with the above reading of the Hole Argument. On that reading, the Hole Argument is indeed based on taking the case where $X \neq Y$, but using 1_M to compare X and Y nevertheless—notwithstanding that it fails to be a physical equivalence. Thus, Halvorson and Manchak's rejection of (B5) is not a mere oversight, but a conscious decision.

Why, then, do Halvorson and Manchak do this? Why do they reject (B5) as a possible underpinning for the Hole Argument? The reason, I suggest, is that they follow Weatherall (2018) in regarding that reading as unviable. After all, compare the quotation above to the following remark of Weatherall's:

> When we say that (M, g) and $(M, \phi_* g)$ are isometric spacetimes, and thus that they have all of the same invariant, observable structure, we are comparing them relative to ϕ. Indeed, we must be because, as in the previous example, there is no sense in which 1_M either is or gives rise to an isometry. In other words, relative to 1_M, (M, g) and $(M, \phi_* g)$ are not equivalent, physically or otherwise.[11]

[10] Halvorson and Manchak, 2022, §5

[11] Weatherall, 2018, p. 336. Again, I have adjusted the notation for reasons of consistency. (I note one nontrivial aspect of the adjustment: Weatherall's notation distinguishes between a diffeomorphism ϕ and the isometry $\tilde{\phi}$ it induces, whereas my notation does not.)

Thus, Halvorson and Manchak agree with Weatherall that comparing isometric spacetimes (M, g) and (M, g') using a non-isometry such as 1_M is somehow illegitimate—at least insofar as one wishes to regard (M, g) and (M, g') as physically equivalent. More generally, on this view, one cannot pick and choose different standards of isomorphism at will. If one has declared oneself to be working with relativistic spacetimes, then one should apply isometry as the standard of isomorphism; conversely, if one insists on invoking diffeomorphisms, then that means one is working with manifolds instead. In this specific case, given that the practice of relativistic physics requires the structure of relativistic spacetimes (not that of mere manifolds), then isometries rather than diffeomorphisms are the appropriate comparisons to use.[12]

I do not wish to go into the question of whether this view of the practice of mathematical physics is warranted or not.[13] Rather, let us remain focused on the question of what it is that Halvorson and Manchak (2022) are seeking to do. Based on the above, I suggest that their aim is *not*, in fact, to engage with the Hole Argument as it is standardly construed in the literature. On their view, that argument has been refuted already by Weatherall (2018). Rather, their goal is to show that once Weatherall's views on mathematical practice are accepted, there is no way to *resurrect* the Hole Argument, by finding a modified premise (B) which could raise concerns about indeterminism *whilst also being immune to Weatherall's critique.*

This means that criticising Halvorson and Manchak for not capturing the Hole Argument is somewhat beside the point; what they are considering are arguments related to the standard Hole Argument, but distinct from it. However, it does follow that at least implicitly, their view is that were Theorem 1 to be false, then GR would turn out to be indeterministic after all (even if we accept Weatherall's account). What I will do in the remainder of this paper is argue that this view is mistaken. Of course, mathematical counterfactuals are widely acknowledged to be rather peculiar. So the question we will instead ask is: for spacetime theories in general, if a theory fails to satisfy (the analogue of) Theorem 1, does it follow that it is indeterministic?

Toward this end, it will be helpful to introduce some terminology. Suppose that we

[12]This gives a natural way to see where the two lines of argument that (Pooley & Read, 2021) attribute to Weatherall come from. The "equivocation argument" trades on the insistence that one must be consistent in the choice of a standard of isomorphism; the "structuralism argument" employs the extra supposition that the correct choice for relativistic physics is that of isometry (which for Weatherall, is equivalent to the claim that the correct mathematical objects to use in relativistic physics are relativistic spacetimes).

[13]This question has been discussed extensively in the recent literature: see Arledge and Rynasiewicz (2019), Bradley and Weatherall (2022), Fletcher (2020), Pooley and Read (2021), and Roberts (2020), and references therein.

have some theory T, which picks out some class of mathematical objects as models of T. Following (Geroch, 1969) and (Halvorson & Manchak, 2022),

Definition 1. A theory T is *rigid* with respect to regions of type S and maps of type K if the following condition holds: for any models $\mathfrak{M}, \mathfrak{M}'$ of T and for any type-K maps $f, g : \mathfrak{M} \to \mathfrak{M}'$, if there exists some type-S region O in $\mathfrak{M}$ such that $f|_O = g|_O$, then $f = g$.

Thus, Theorem 1 demonstrates that GR is rigid with respect to open sets and isometries. On the other hand, GR is not rigid with respect to open sets and diffeomorphisms: this is what makes (B2**) and (B3**) true. The relativisation of Definition 1 to regions of type S is inspired by (Butterfield, 1988); the relativisation to maps of type K is inspired by (Luc, 2024). Here and throughout, I make the following assumptions about the criteria of "being of type S" and "being of type K". First, I assume that the property of being a type-S region is invariant under isomorphism: so given a type-S region O in $\mathfrak{M}$ and an isomorphism $f : \mathfrak{M} \to \mathfrak{M}'$, the image $f[O]$ in $\mathfrak{M}'$ is a type-S region. Second, I assume that for any choice of K, the maps of type K form a group under composition.

So, our question is then: what is the relationship between rigidity and determinism? Halvorson and Manchak do indeed appear to regard rigidity as a kind of criterion for determinism. Their paper discusses what they call "Property R", which (in my terms) corresponds to rigidity with respect to initial segments and isomorphisms. After introducing a putative notion of determinism that they call "MLE determinism" (which we shall return to shortly), and showing that a particular pair of toy theories T and T^+ fail to be MLE-deterministic, they say the following (emphasis mine):

> The question before us is whether a theory's failing the MLE criterion is a sign that that theory is not as deterministic as a theory could be. *We think not.* [...] In particular, the theories T and T^+ [are MLE-deterministic] even though both of them are *deterministic in the following precise sense*:
>
> Proposition: The theories T and T^+ have Property R.
>
> [...]
>
> Of course, there are dynamical theories that lack Property R; and *these theories, we claim, are genuinely indeterministic.*[14]

This suggests that rigidity, for Halvorson and Manchak, constitutes a form of determinism: in particular, being rigid (or at least, having Property R) is a sufficient condition

[14] Halvorson and Manchak, 2022, §6.

to consider a theory deterministic, and lacking Property R is a sufficient condition to consider a theory indeterministic.

So, we should assess whether this is indeed the case. To do so, we need to get clearer on what determinism means. Unfortunately, as even a casual acquaintance with the literature on the Hole Argument will make clear, there are many competing definitions. What I propose to do, therefore, is work through some candidate definitions of determinism—and show that no such candidate can support Halvorson and Manchak's conclusion.

In general, the idea of determinism is the following: a theory is deterministic if, among the possibilities it describes, there do not exist two possibilities that agree at some earlier time but disagree at some later time. The challenge is how to make this idea formally precise. As a first step toward doing so, I will do two things. First, I will drop the time-asymmetry implicit in this definition: so theories where the future underdetermines the past will also count as "indeterministic". This is just for convenience, as it means we are not restricted to considering theories equipped with a temporal orientation. Second, I will follow Butterfield (1987) in relativising determinism to regions of a certain kind, rather than trying to fix on one characterisation of what constitutes a "time". (This will also facilitate comparisons between our proposed definitions of determinism and the above definition of rigidity.)

We start with an exceptionally strong definition of determinism—one that is adapted from what Halvorson and Manchak (2022, §6) call "MLE determinism", short for "Montague-Lewis-Earman determinism". I will call this "identity determinism", since it effectively demands that a part of a model (of the appropriate kind) uniquely determines the identity of the model as a whole.

Definition 2. A theory T is *identity-deterministic* with respect to regions of type S if, for any models $\mathfrak{M}$ and $\mathfrak{M}'$ of T, if there exists some type-S region O of $\mathfrak{M}$ such that O is also a type-S region of $\mathfrak{M}'$ and $\mathfrak{M}|_O = \mathfrak{M}'|_O$, then $\mathfrak{M} = \mathfrak{M}'$.

The problem is that—as Halvorson and Manchak observe—no theory constructed in the standard fashion is deterministic in this sense. In particular, suppose that a theory T has at least one model $\mathfrak{M}$ containing some proper subregion R of type S, and that T's class of models is closed under isomorphism. Then T is not identity-deterministic with respect to regions of type S. To see this, just take any element of $\mathfrak{M}$'s underlying base set that is not an element of R, and replace it with some object not identical to any other element of $\mathfrak{M}$'s base set. The resulting structure $\mathfrak{M}'$ is distinct from $\mathfrak{M}$ but isomorphic to it, and so $\mathfrak{M}'$ is also a model of T and R is also a type-S region of $\mathfrak{M}'$ (per our assumptions); yet $\mathfrak{M}$ and $\mathfrak{M}'$, by construction, are such that $\mathfrak{M}|_R = \mathfrak{M}'|_R$.

Identity-determinism expresses a particularly strong kind of haecceitistic determinism: an identity-deterministic theory would be such that knowing which points make up some particular region of a model (and the structures on those points) is sufficient to determine *which other points make up the rest of the model.* So theories fail to be identity-deterministic so long as they allow the user some degree of lassitude in choosing the base set of the models. Of course, in principle one could construct a theory that does not permit that degree of lassitude: perhaps a theory all of whose models are set on $\mathbb{R}^4$, where that is understood as *one specific* set in the set-theoretic universe. But still, it certainly seems plausible that one might want to recognise a theory as deterministic even if it does not impose such stark set-theoretic restrictions on how models get to be constructed. Moreover, it seems rather odd to think that such restrictions are relevant to determinism. That is, as Halvorson and Manchak (2022, §6) put it, "a theory can fail to be MLE deterministic [i.e. identity-deterministic] simply because the language of set theory allows the same situation to be described in different ways. (That is precisely what set theory is doing: constructing new sets.)" So identity-determinism does indeed seem to be what they call a kind of "trivial semantic indeterminism".

Evidently, then, identity-determinism is not the kind of condition that Halvorson and Manchak have in mind. Can we do better? Let us turn next to a condition on determinism inspired by the discussion in Butterfield (1987, 1988, 1989). Like our definition of rigidity, we will give this definition a twofold relativisation, to both regions of type S (following Butterfield's own discussion) and to regions of type K (again, following Luc—though this will also prove helpful for capturing several of Butterfield's definitions in one fell swoop).

Definition 3. A theory T is *Butterfield-deterministic* with respect to regions of type S and maps of type K if, for any models $\mathfrak{M}$ and $\mathfrak{M}'$ of T and any type-K map $f : \mathfrak{M} \to \mathfrak{M}'$, if there exists some type-S region O of $\mathfrak{M}$ such that $f[O]$ is also a type-S region of $\mathfrak{M}'$ and $f|_O$ is an isomorphism from $\mathfrak{M}|_O$ to $\mathfrak{M}'|_{f[O]}$, then f is an isomorphism from $\mathfrak{M}$ to $\mathfrak{M}'$.

Butterfield (1987, 1988, 1989) considers several definitions that correspond to special cases of Butterfield-determinism. The definitions "Dm1" in (Butterfield, 1987) and "Dm" in (Butterfield, 1988) correspond to Butterfield-determinism with respect to diffeomorphisms. The definition "Dm0" in (Butterfield, 1988) corresponds to Butterfield-determinism with respect to isometries. The definitions "Dm1" in (Butterfield, 1988) and "Dm1" in (Butterfield, 1989) correspond to Butterfield-determinism with respect to absolute-structure-preserving diffeomorphisms. Note that the moniker of "Butterfield determinism" is, however, unfair in the following sense: in the cited texts, Butterfield does not in fact endorse Butterfield-determinism (in any of its several varieties) as a plausible criterion of determinism. However, since I already have a name for the criterion of determinism that Butterfield endorses (which we will get to in due course), and I cannot think of a pithy alternative for this condition, the label will have to do.[15]

Butterfield-determinism is closely related to rigidity. To discuss this relationship, it will be helpful to define the notion of a *model* (rather than just a theory) being rigid.

Definition 4. A model $\mathfrak{M}$ of a theory T is *rigid* with respect to regions of type S and maps of type K if for any type-K maps $f, g : \mathfrak{M} \to \mathfrak{M}$, if there exists some type-S region O such that $f|_O = g|_O$, then $f = g$.

That this definition is appropriate is indicated by the following proposition.

Proposition 1. A theory is rigid iff every model of that theory is rigid.

Proof. The left-to-right direction is immediate from Definition 4. For the converse, suppose that T is not rigid. Then there exist models $\mathfrak{M}, \mathfrak{M}'$ of T and type-K maps $f, g : \mathfrak{M} \to \mathfrak{M}'$ such that $f \neq g$ but $f|_O = g|_O$ for some type-S region O. Now consider $h := g^{-1} \circ f$, which must also be a type-K map (by our earlier assumption that the type-K maps form a group). Then $h|_O = 1_O$, but $h \neq 1_{\mathfrak{M}}$. Yet $1_{\mathfrak{M}}$ is also a type-K map (by the same assumption), and therefore the model $\mathfrak{M}$ is not rigid. □

We then have the following proposition.

Proposition 2. Suppose that T is Butterfield-deterministic with respect to regions of type S and maps of type K. Then every model $\mathfrak{M}$ of T is either non-rigid with respect to regions of type S and isomorphisms of models, or is rigid with respect to regions of type S and maps of type K.

Proof. Suppose that there were a model $\mathfrak{M}$ of T which is not rigid with respect to maps of type K, but is rigid with respect to isomorphisms. Then there exist type-K maps $f, g : \mathfrak{M} \to \mathfrak{M}$ such that $f \neq g$, yet for some type-S region O of $\mathfrak{M}$, $f|_O = g|_O$. Now consider $h := g^{-1} \circ f$, which by assumption is also a type-K map. Since $h|_O = 1_O$, we have that $h|_O$ is an isomorphism from $\mathfrak{M}|_O$ to $\mathfrak{M}|_O$, and that $h[O] = O$ is also a

[15] Another option might be "Montague-Earman determinism", since Butterfield cites both Montague and Earman as having inspired—and he, thinks, as being advocates of—this definition. However, this risks confusion with Halvorson and Manchak's use of "Montague-Lewis-Earman determinism" to refer to what we above called identity-determinism.

type-S region of $\mathfrak{M}$. Since $\mathfrak{M}$ is rigid with respect to isomorphisms, the fact that $h \neq 1_{\mathfrak{M}}$ means that h is not an isomorphism from $\mathfrak{M}$ to $\mathfrak{M}$. So we have a type-K map that is an isomorphism on a type-S region but not a global isomorphism; hence, T is not Butterfield-deterministic. □

Thus, each model of a Butterfield-deterministic theory must be of one of two kinds. On the one hand, it may be non-rigid with respect to isomorphism. This is possible only if it exhibits a large amount of symmetry: it must admit some non-trivial automorphism which is the identity on some type-S region. If this is not the case, then it must be rigid with respect to type-K maps. The larger the class of type-K maps, the harder it will be for the model to be rigid with respect to them: so, for a given theory, Butterfield-determinism will increase in logical strength as one expands the class of type-K maps, and decrease as one shrinks it. Indeed, an immediate corollary to Proposition 2 is that *any* theory is Butterfield-deterministic with respect to isomorphisms (and regions of any type): for any given model of the theory will then either be non-rigid with respect to isomorphisms (so satisfying the first conjunct) or rigid with respect to isomorphisms (so satisfying the second conjunct).

Toward the other extreme, Butterfield-determinism with respect to diffeomorphisms is a very strong condition. For most reasonable choices of S, given a manifold M, there will exist diffeomorphisms from M to itself that are the identity on some type-S region R of M, but not the identity overall. Any model set on M will therefore be non-rigid with respect to diffeomorphisms; so if it is to be a model of a Butterfield-deterministic theory, it must be non-rigid with respect to isomorphisms, and hence highly symmetric in the sense noted above. For example, a model consisting of a constant scalar field on a manifold is non-rigid with respect to isomorphisms; it follows that a theory for which every model is a constant scalar field on a manifold is Butterfield-deterministic. However, any theory admitting a model lacking this kind of symmetry will fail to be Butterfield-deterministic. For example, consider a theory whose sole model is that of a single particle at rest in Newtonian absolute space. Then a diffeomorphism that acts as the identity up to some arbitrary time, but does not preserve the particle worldline globally, will instantiate a violation of Butterfield-determinism with respect to diffeomorphisms and initial segments.[16] This example also demonstrates the implausibility of taking Butterfield-determinism with respect to diffeomorphisms as a criterion of determinism:

[16] cf. the observation in Butterfield (1988, p. 68) that if a theory admits a model lacking non-trivial global automorphisms, then that theory is not Butterfield-deterministic with respect to diffeomorphisms. This observation follows as a corollary to the above: if a model has no automorphisms, then *a fortiori* it has no automorphisms that are the identity on some type-S region.

it is surely very plausible that this theory should be considered deterministic with respect to initial segments.

So let us next consider an intermediate variety of Butterfield-determinism, by taking the type-K maps to be those maps that preserve spacetime structure. We will follow (Butterfield, 1988) in referring to such maps as "isometries" (in a mild sacrifice of accuracy for the sake of readability). Butterfield notes that the corresponding definition of determinism is more restricted—*and that the reason for this is that rigidity with respect to isometries is much more readily obtained than rigidity with respect to diffeomorphisms*:

> [Butterfield-determinism with respect to isometries] works very well in that it delivers intuitively right verdicts about whether determinism holds, for some familiar theories [...] Namely, theories using a classical spacetime with or without absolute rest, and theories using Minkowski spacetime; where the regions S are time-slices, or thin sandwiches, across the manifold. In such theories, the postulation of a fixed framework means that there are isometries between any two models. And more important, the restricted quantifier [i.e., the restriction from diffeomorphisms to isometries] eliminates unwanted violations of determinism of the kind that plagued [Butterfield-determinism with respect to diffeomorphisms]. The main reason is that in all three spacetimes, there is a unique extension of an isometry on a sandwich to a global isometry. That is: an isometry between two models is determined by its restriction to a sandwich. [17]

One has to be a little careful here: the above may give the impression that rigidity with respect to isometries *entails* Butterfield-determinism with respect to isometries. This is not the case: consider a theory of a scalar field on Minkowski spacetime that admits two models, one in which the field vanishes everywhere and the other in which the field vanishes everywhere apart from the forward light cone of some point p, in which it takes the value 1. This theory is Butterfield-indeterministic with respect to isometries and hyperplanes of simultaneity: there exists an isometry from the one model to the other that is an isomorphism on some hyperplane of simultaneity, but not an isomorphism between the models as a whole.[18]

Read carefully, though, we see that Butterfield is in fact claiming only that the rigidity of the spacetimes with respect to isometries facilitates the non-triviality of Butterfield-

[17]Butterfield, 1988, p. 69. Note that Butterfield (1987) makes a similar argument for Butterfield-determinism with respect to diffeomorphisms; as the 1988 paper acknowledges, that argument is erroneous (John Norton is credited for the correction).

[18]Incidentally, note that both models are rigid with respect to isometries. This demonstrates that the converse to Proposition 2 does not hold.

determinism with respect to isometries. Proposition 2 makes it clear why this is the case. If a theory is set on a spacetime that is rigid with respect to isometries, then (by definition) its models will be rigid with respect to isometries; so it is at least possible for the theory to be Butterfield-deterministic with respect to isometries without every model needing to be highly symmetric (i.e. non-rigid with respect to isomorphisms).

Since we are interested in the Hole Argument, we might naturally ask: is GR Butterfield-deterministic with respect to isometries? The answer to this question depends a little on what form one takes the models of GR to have. If one takes models of GR to be of the form (M, g), as we have been doing throughout this essay, then it is trivial that GR is Butterfield-deterministic with respect to isometries: for then isometries are isomorphisms of models, and as noted earlier, Butterfield-determinism with respect to isomorphism is trivial. If one takes models of GR to be of the form (M, g, T) satisfying the Einstein field equations (where T is the stress-energy tensor), then the answer is still affirmative, again for somewhat trivial reasons. The Einstein field equations fix the value of T as a function of g; hence, any isometry is an isomorphism, so Butterfield-determinism is assured. Finally, suppose that one takes models of GR to be of the form (M, g, Φ), where Φ represents the various matter fields (so the stress-energy tensor is assumed to be some function of g and Φ). Then the Einstein field equations alone do not guarantee that Butterfield-determinism with respect to isometries holds. Whether that is the case will depend on the dynamics governing the Φ fields. Nevertheless, determinism for GR is at least possible.

So: could Butterfield-determinism with respect to isometries be what Halvorson and Manchak have in mind? There are two problems with such an interpretation. The first is dialectical: Butterfield-determinism is an awkward fit for Halvorson and Manchak's general mathematical and philosophical orientation. Note, in particular, that it employs exactly the sort of mixing and matching of standards of comparison that Halvorson and Manchak—at least on the interpretation I gave of them earlier—are at pains to decry. The second problem is that Butterfield-determinism with respect to isometries is not, it turns out, much more plausible as a definition of determinism than Butterfield-determinism with respect to diffeomorphisms.[19]

To demonstrate this, consider any theory set on Leibnizian spacetime that admits some model whose matter content is not invariant under (all) Leibnizian transformations: for example, a theory whose sole model (up to isomorphism) describes a single particle tracing a continuous, differentiable and timelike trajectory through Leibnizian spacetime. Now consider a Leibnizian transformation f that is the identity on some plane of absolute

[19] Again, I stress that Butterfield agrees with this verdict.

simultaneity, but elsewhere acts non-trivially on the particle trajectory. Then f is an isometry that is an isomorphism on that simultaneity surface, but is not an isomorphism overall; hence, this theory is Butterfield-indeterministic with respect to isometries and simultaneity surfaces. Yet upon inspection, it is not so clear that this theory should really be regarded as indeterministic. It is indeterministic only with respect to the issue of *which* points of spacetime the particle's worldline lies on. The two models that instantiate the violation of Butterfield-determinism are, by construction, isomorphic to one another. So as far as the qualitative properties of the spacetime go, they do not describe different possibilities. The kind of indeterminism here therefore seems to be merely haecceitistic.

This conclusion is not novel. Indeed, this kind of case was first discussed by Stein (1977), who did not take it to show that any theory on Leibnizian spacetime must be indeterministic, but rather the importance of identifying the physical content of isomorphic models:

> It must not be thought that this argument demonstrates the impossibility of a deterministic Leibnizian dynamics; the situation is, rather, that all of the systems of world-lines that arise from one another by automorphisms have to be regarded as objectively equivalent (i.e., as representing what is physically one and the same actual history).[20]

Weatherall (2020) puts the point as follows:

> On Stein's view, a deterministic Leibnizian dynamics would determine future states up to isomorphisms of Leibnizian space-time. This is because to determine future states up to isomorphism is precisely to determine only those facts that are expressible within Leibnizian space-time. Earman, by contrast, appears to demand more than this of a (deterministic) Leibnizian dynamics. He seems to require that such a dynamics determine, at least, unique worldlines for particles.[21]

Note that as Weatherall indicates, Earman (1986) does not follow Stein in regarding this theory as deterministic; indeed, Weatherall argues that this case was the foundation for Earman's presentation of the Hole Argument![22] Nevertheless, I take this to at least problematise Butterfield-determinism with respect to isometries enough to undermine its attractiveness to Halvorson and Manchak. Again, it is implausible that a standard

[20]Stein, 1977, p. 6

[21]Weatherall, 2020, p. 83

[22]Butterfield (1989) also discusses Earman's analysis of this case.

of comparison other than isometries or diffeomorphisms will be relevant to the case of GR; so at this point, we should move on from Butterfield-determinism altogether.

Can we do better? That is, can we find a more plausible definition of determinism? The consensus in the literature is that we can indeed. Go back to the rough-and-ready characterisation of determinism that we started with: a theory is deterministic if, whenever two possibilities agree at one time (more generally, in some type-S region), then they agree overall. In light of what we have said so far, the most natural way to cash out this notion of "agreement" is in terms of isomorphism if we are comparing models as a whole, or embedding (i.e. local isomorphism) if we are comparing parts of models. This motivates the following definition:

Definition 5. A theory T is *de dicto deterministic* with respect to regions of type S if, for any models $\mathfrak{M}$ and $\mathfrak{M}'$ of T, if there exists some type-S region O of $\mathfrak{M}$ such that there exists an embedding $f : \mathfrak{M}|_O \to \mathfrak{M}'$ (whose image $f[O]$ is also of type S), then there is an isomorphism $g : \mathfrak{M} \to \mathfrak{M}'$.

The motivation for the terminology of "de dicto determinism" (introduced in (Dewar, 2016)) will become clear shortly. This definition corresponds to Butterfield's preferred definition of determinism, labelled by him as "Dm2", in the papers discussed above (1987, 1988, 1989);[23] it also corresponds to the definition of determinism given by (Lewis, 1983). It is also referred to as "Definition 1" in (Belot, 1995), as "Lewis's analysis" in (Melia, 1999), and as "**Det2**" in (Pooley, 2022).

De dicto determinism has the virtue of vindicating (in a non-trivial manner) a sense in which GR is deterministic;[24] indeed, we will see shortly that GR is deterministic in a strictly stronger sense. It also avoids mixing and matching standards of comparison, so in principle should be acceptable to Halvorson and Manchak. However, it has been argued in the literature that it is too weak a conception of determinism to be plausible: in particular, that it classes certain intuitively indeterministic theories as deterministic.

For example, consider the following theory T.[25] It has one model up to isomorphism, which describes the history of single cylinder on a disk. Initially, the cylinder stands on the central point of the disk; at some time t, however, the cylinder falls over so that it lies flat on the disk. We can take T to be representing a tower, standing on a flat and empty planet, which suddenly topples over. Since T has only one model, it is necessarily

[23] This endorsement is later recanted, in favour of "de re determinism" (to be discussed below); see (Gomes & Butterfield, 2023).

[24] Contra what is claimed by Butterfield (1989); the error in the argument is discussed by Landsman (2023) and Cudek (2023), and acknowledged in (Gomes & Butterfield, 2023).

[25] This example is based on one originally given by Wilson (1993). It is discussed by various authors: see Arntzenius (2012), Belot (1995), Brighouse (1997, 2008), and Melia (1999).

de dicto deterministic. However (the thought goes), there is something indeterministic about T: for, prior to time t, there is no way to tell in what direction the cylinder will topple.[26] As Melia puts it:

> Now, are there not many ways in which the tower could have fallen? Surely, given the symmetry of the initial conditions, the tower could have toppled in a different direction and come to rest upon some other part of the planet. It didn't happen—but it might have.[27]

There is a natural way of strengthening de dicto determinism that captures the sense in which the theory T is indeterministic. This goes as follows:

Definition 6. A theory T is *de re deterministic* with respect to regions of type S if, for any models $\mathfrak{M}$ and $\mathfrak{M}'$ of T, if there exists some type-S region O of $\mathfrak{M}$ such that there exists an embedding $f : \mathfrak{M}|_O \to \mathfrak{M}'$ (whose image $f[O]$ is also of type S), then there is an isomorphism $g : \mathfrak{M} \to \mathfrak{M}'$ such that $g|_O = f$.

The difference is at the very end: we now require that an embedding entails the existence not only of a (global) isomorphism, but of an isomorphism *that is an extension of the embedding*. This definition corresponds to "Definition 2" in (Belot, 1995), the "second resolution" in (Melia, 1999), and "**Dm2+**" in (Cudek, 2023). Moreover, this definition captures the sense in which the tower theory T is indeterministic: there are many non-trivial embeddings of early stages of the tower into the sole model, but only one isomorphism from the model as a whole to itself.[28] GR, on the other hand, is de re (and hence, de dicto) deterministic with respect to Cauchy surfaces—provided, at least, that we restrict its class of models to just the *inextendible* models. This fact is a consequence of the Chouquet-Brouhat theorem: for proofs, see (Landsman, 2023) or (Malament, 2012, §2.10).

Dewar (2016) argues that there is not a productive debate about whether determinism is better captured by Definition 5 or Definition 6. Rather, the two definitions are held to capture slightly different senses in which a theory might be deterministic—two senses that correspond, in fact, to the two species of possibility for which these definitions are

[26] A wrinkle: there is a second potential source of indeterminism, namely indeterminism about *when* the tower will fall. Since this only reinforces the point at issue, however, we need not worry about this. (Alternatively, we could obviate the issue by adding a timer to the model, that assigns each time a particular real number (with the toppling time t being assigned the number 0). Provided that such a timer can be added without perturbing the rotational symmetry of the model, it will remove this second form of indeterminism.)

[27] (Melia, 1999, p. 649)

[28] Well, perhaps two—if we count the reflection of the model around the cylinder. Nevertheless, the point stands: not all embeddings are extendible to isomorphisms.

named. Recall that de dicto possibility concerns how things could have been for the world as a whole, whereas de re possibility concerns how things could have been for the individuals within the world. Similarly, de dicto determinism asks: given the state of the world at a time, how many ways could things play out for the world as a whole? De re determinism asks: given the state of the world and the individuals within it, how many ways could things play out for the world *and for those individuals*? This makes de re determinism sensitive to variations that de dicto determinism ignores: whether the tower lands on *this* part of the disk or *that* one makes a different to the parts of the disk (in that a tower lands on them, or doesn't); but it doesn't make a difference to the overall state of the world, which records merely that the tower landed on some part of the disk or other.

So, we have not one but two further definitions of determinism. Could Halvorson and Manchak appeal to these definitions in order to warrant the claim that rigidity expresses a form of determinism? First, the bad news: rigidity is logically independent of both forms of determinism.[29]

Proposition 3. Rigidity does not entail de dicto determinism (and hence, nor does it entail de re determinism).

Proof. Consider a theory with two models up to isomorphism, each of which is a two-element set. In the first model $\mathfrak{M}$, exactly one element satisfies the predicate F; in the second model $\mathfrak{M}'$, exactly one element satisfies the predicate G. This theory is rigid with respect to subsets: given any two isomorphic models there is a unique isomorphism between them, which entails rigidity.[30] However, the theory is not de dicto deterministic with respect to subsets: there exists an embedding of a subset of $\mathfrak{M}$ into $\mathfrak{M}'$, but no isomorphism between them. □

Proposition 4. De re determinism does not entail rigidity (hence, nor does de dicto determinism).

Proof. Consider a theory in the empty signature whose sole model up to isomorphism is a three-element set. This theory is de re deterministic with respect to subsets: any embedding (i.e. injection) of a subset of that model into itself can be extended to an isomorphism (i.e. bijection) from the model to itself. However, it is not rigid: given an embedding of any one-element subset of the model into the model, there are two extensions of that embedding. □

[29]Menon and Read (2024, p. 14) observe one half of this independence: that determinism does not entail rigidity, so that even if GR were non-rigid, it would not follow that it was indeterministic.

[30]The standard of isomorphism here is the standard one from model theory, i.e. an invertible mapping that preserves the extensions of predicates.

The examples in the above are very simple, of course, but it is not hard to see that the point will still apply for more properly physical examples.[31] Moreover, I contend that reflection on such examples gives further evidence that rigidity is not itself a form of determinism: the examples that speak to Proposition 3 strike me as genuinely indeterministic, and those that speak to Proposition 4 strike me as genuinely deterministic.

However, although rigidity is independent of determinism, that is not to say that there is not an interesting relationship between the two notions. In fact, there is a natural sense in which rigidity (with respect to regions of type S) can be thought of as *dual* to de re determinism (with respect to regions of type S). Consider a theory T, models $\mathfrak{M}$ and $\mathfrak{M}'$ of T, and some type-S regions O and O' of $\mathfrak{M}$ and $\mathfrak{M}'$ respectively. Now consider the class of all isomorphisms from $\mathfrak{M}$ to $\mathfrak{M}'$ that send O to O', and the class of all embeddings of $\mathfrak{M}|_O$ in $\mathfrak{M}'$ that send O to O'. Given any isomorphism f in the former class, then the restriction $f|_O$ will be a member of the latter class. So we obtain a map from the former class to the latter class; call this the *restriction map*.[32] Expressed in these terms, rigidity is the requirement that the restriction map be injective, and de re determinism is the requirement that the restriction map be surjective. Or, in more informal terms: given an embedding, de re determinism expresses the demand that there exist at least one extension to an isomorphism, whilst rigidity expresses the demand that there exist at most one such extension.

In light of this, it is very natural to consider rigidity and determinism alongside one another, even though they are distinct notions. For example, Theorem 2 of Landsman (2023) contains not only the claim that GR is de re deterministic, but also the claim that it is rigid (as Landsman notes). Similarly, Cudek (2023) considers augmenting his **Dm2+** (i.e. de re determinism) with the further condition that the extension be unique (i.e. that the theory be rigid as well). Finally, I suggest that this close relationship offers a natural explanation for Halvorson and Manchak's intuition that rigidity is a form of determinism: although (in my view) that intuition is misguided, it is not wholly off the mark.

To conclude, then: in this paper, I have sought to clarify and contextualise the claims made in (Halvorson & Manchak, 2022), in light of the critique of (Menon & Read, 2024). On the positive side, I have suggested that although their reconstruction of the Hole Argument does not capture that argument as it is understood in the literature, that is not in fact a problem—because the purpose of their paper is to consider instead

[31] For example, a stochastic theory on a rigid spacetime will suffice for the proof of Proposition 3, and a theory whose sole model is non-rigid (e.g. Leibnizian spacetime) will suffice for the proof of Proposition 4.

[32] This terminology is taken from sheaf theory.

a hypothetical alternative argument, one that might escape the concerns of (Weatherall, 2018). On the negative side, I have argued that this hypothetical argument is not in fact troubling—because it has the power to show only that GR is non-rigid, not that it is indeterministic. However, this negative conclusion is tempered by the fact that there are indeed interesting and non-trivial relationships between determinism and rigidity. More specifically, Butterfield determinism imposes rigidity-related constraints on a theory's models; and de re determinism, though logically independent of rigidity, is in fact dual to it. So we have learned things about the conditions we might impose on our physical theories—even if those are not the things that we set out to learn initially.

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