Arguments from scientific practice in the debate about the physical equivalence of symmetry-related models

Abstract

In the recent philosophical literature, several counterexamples to the interpretative principle that symmetry-related models are physically equivalent have been suggested (Belot 2013, Belot 2018, Fletcher 2020). Arguments based on these counterexamples can be understood as arguments from scientific practice of roughly the following form: because in scientific practice such-and-such symmetry-related models are treated as representing distinct physical situations, these models indeed represent distinct physical situations. In this paper, a strategy for analysing arguments of this type is presented and applied to the examples that can be found in the literature. I argue that if we are exclusively interested in models understood as representing entire possible worlds (not their subsystems), arguments from scientific practice should involve some additional assumptions to guarantee that they are relevant for models understood in this way. However, none of the examples presented in the literature satisfy all these additional assumptions, which leads to the conclusion that arguments from scientific practice based on these examples do not undermine the interpretative principle that different symmetry-related models represent the

same possible world.

1 Introduction

A symmetry of a physical theory is a transformation of some kind that induces a bijection on the set of models of that theory (models of the theory always get transformed into models and non-models into non-models). There are different types of symmetries, the most familiar of which are spatio-temporal symmetries consisting of a transformation of the spatio-temporal variables in terms of which the equations of a theory are written. However, this is not the only type of symmetry (see, e.g., Belot 2013:3-6 for a classification). Consequently, various questions about symmetries always need to be disambiguated by specifying what kind of symmetries we are talking about.

In all standard cases of physical theories, symmetry transformations form a group that acts on the space of models. As a result, symmetries induce an equivalence relation between models because of the correspondence between mathematical features of groups and equivalence relations: the existence of an identity element leads to reflexivity, the existence of inverse elements leads to the relation being symmetric and the group composition leads to its being transitive.

This is a purely mathematical fact, but one can further ask what the significance of these equivalence classes is (cf. Castellani 2003). One idea in the literature is that the presence of symmetries indicates that a theory contains a "surplus structure", and, therefore, for each equivalence class all its elements are physically equivalent. This idea can be made precise in several ways (see section 2). The crucial observation is that models of a given theory can be understood either as representing entire possible worlds (which I will call the "world-interpretation") or only subsystems of possible worlds (which I will call the "subsystem-interpretation"). I will focus on two formulations of the idea that symmetry-related models are physically equivalent that will be called SYM-ONE-Q-W and SYM-ONE-H-W. Both concern models understood as representing entire possible worlds (which is indicated by "W"). The first of them takes into account only qualitative identity ("Q"), whereas the second one also concerns haecceistic identity ("H").

In the recent philosophical literature, several counterexamples against the idea that symmetry-related models are physically equivalent have been presented (Belot 2013, Belot 2018, Fletcher 2020). Arguments based on these examples can be regarded as instances of arguments from scientific practice and have roughly the following form: because in scientific practice such-and-such symmetry-related models are treated as representing distinct physical situations, these models indeed represent distinct physical situations. The aim of this paper is to provide a strategy for analysing arguments of this form (understood as arguments against SYM-ONE-Q-W or SYM-ONE-H-W) and to apply it to the cases put forward by Belot and Fletcher. The general idea of my argumentation is that although these examples show that certain models under the subsystem-interpretation represent distinct physical situations, this is not sufficient to show that the same models under the world-interpretation represent distinct possible worlds.

The following is the plan for this paper. In section 2, I will consider different formulations of the interpretative principle that symmetry-related models are physically equivalent. Two of them, SYM-ONE-Q-W and SYM-ONE-H-W, will be chosen for further investigation. Section 3 will provide a rough version of the form of arguments from scientific practice against these interpretative principles. In section 4, potential counterexamples to these principles presented in the literature will be reviewed. Section 5 will provide some refinements to the form of arguments from scientific practice established in section 3, which are indispensable for them to work against SYM-ONE-Q-W or SYM-ONE-H-W. These refinements will lead to a list of conditions that counterexamples need to satisfy for the argument to go through, which will be presented at the beginning of section 6. The rest of this section will be devoted to checking whether the counterexamples reviewed in section 4 satisfy all these conditions, with the conclusion being that none of them does. Section 7 will provide a short summary of the results.

2 Formulation of the problem

The idea that symmetry-related models are physically equivalent can be made precise in different ways. These more precise formulations may be called "interpretative principles". Under the first and rather liberal view, it is assumed that each model can represent various physical situations, but for two symmetry-related models the sets of these situations are the same:

> **SYM-MANY**: Two models of a physical theory are symmetry-related iff they can represent the same possible physical situations.

Assuming that any model can represent only one possible physical situation, SYM-MANY leads to a more restrictive interpretative principle:

> **SYM-ONE**: Two models of a physical theory are symmetry-related iff they can represent the same possible physical situation.

In what follows, I will focus on the latter, more restrictive principle (i.e., SYM-ONE). All counterexamples that will be reviewed in section 4 are supposed to undermine SYM-ONE. Some of them could also be used to argue against SYM-MANY. However, if we succeed in showing that they fail to undermine SYM-ONE, there will be no reason to also consider SYM-MANY, which is the weakening of SYM-ONE.

The proper understanding of these interpretative principles requires certain precisifications. First, in both principles there is an implicit universal quantification over physical theories and, for a given physical theory, over all its models (as will also be the case for all their later refinements).[1]

Second, we should say something about the word "can" used in the formulation of these principles. Its usage follows Fletcher (2020). This terminology is equivalent to the terminology of representational capacities of models (Weatherall 2018:332, Fletcher 2020:230) and of models being well-suited to represent something (Belot 2013). Why do we talk about what models can represent instead of talking about what models do in fact represent? What does it mean that a model can represent something but does not actually represent it? These are important questions, but it seems to me that giving an answer to them here would settle too much in advance about our topic, so I will leave the word "can" unanalysed here, as do the authors I am referring to. However, I will give three examples, which at least will show that in certain contexts the distinction cannot be collapsed.

Example (i): imagine that we are using models of n-particle classical mechanics to represent a subsystem of the universe, taking our laboratory as a reference frame. With a reference frame fixed, there is a unique answer to the question about the positions and momenta of our n particles, so exactly one model actually represents them in this context. However, other models also *can* represent this physical situation, and if we changed

[1] To avoid any confusion, below I present the formulation of our principles that is more explicit formally:

> **SYM-MANY**: For any physical theory T and for any two models of T, M_1 and M_2: M_1 and M_2 are symmetry-related iff there is a class of possible physical situations P such that both M_1 and M_2 can represent all and only possible physical situations belonging to P.
>
> **SYM-ONE**: For any physical theory T and for any two models of T, M_1 and M_2: M_1 and M_2 are symmetry-related iff there is a possible physical situation such that both M_1 and M_2 can represent it and they cannot represent any other possible physical situation.

the reference frame, this "can represent" would become "actually represent" for one of these models.[2]

Example (ii): it might happen that the same model represents different physical situations with different degrees of adequacy (i.e., it represents some physical situation with a certain degree of adequacy and some other with a smaller or larger degree). This sense of "can" will not be of my interest here, so whenever I say that two models can represent certain possible physical situations (or possible worlds), this means that they can represent them with the same degree of adequacy. In other words, in all principles considered here, the degree of adequacy of representation is treated as fixed.

Example (iii): it might happen that the same possible world contains two qualitatively identical subsystems. Then one can use the same model to represent each of them. If we do not require the representations to be fully adequate, the subsystems do not need to be exactly qualitatively identical, it suffices if they are appropriately similar. For example, the Schwarzschild model of GR can be used to represent different black holes. However, if in a given context it represents one of them, it does not represent any of the others in that context (cf. Roberts 2020:5).

As the focus of this paper is the defence of the most restrictive version of SYM-ONE and the distinction between "can represent" and "actually represent" is interesting only in the case in which a model can represent more than one physical situation (or possible world), the lack of specification of the exact meaning of "can" will not be threatening for our conclusions.[3]

[2]Transformations here are understood passively, so this is not in conflict with what is claimed on p. 8.

[3]Does this mean that if a model can represent exactly one physical situation/possible world, then it always actually represents it? Not necessarily so, as we might choose to not represent anything physical by this model and consider only its mathematical features, or change our standard representative conventions (to take an extreme example, one can use all models of classical mechanics to represent the Eiffel Tower). In all our considerations we assume that the standard representative conventions are used (i.e., $\vec{x}$ in classical mechanics represent positions, $\vec{E}$ in electrodynamics represents the electric field, an so on).

Third, there is an implicit assumption that the general type of the system is fixed. The need for this assumption arises because different physical phenomena can sometimes be described by the same equations. For example, the harmonic oscillator equation accurately describes both a mass on a spring and an electric circuit (this is Fletcher's 2020:234-235 example). However, this is not the type of difference we are interested in.

Fourth, it is often the case that a model does not represent all features of a physical situation. Therefore, "representing a physical situation" should be understood modulo features of this situation that are not expressible in a given theory.

Fifth, the term "physical situation" is ambiguous, as a (possible) physical situation can be thought of as either encompassing the entire (possible) world or as encompassing only a subsystem of a (possible) world. At first glance, it is not obvious whether this distinction is important for our considerations, as one can make the following conjecture:[4]

> **S-W-Equivalence**: Whatever interpretative principles are true for models under the world-interpretation, they are also true for these models under the subsystem-interpretation, and the other way around.

I think that this conjecture is false; at least for spatio-temporal symmetries there are good reasons for thinking that the way in which the models under the world-interpretation represent is in certain important respects different than the way in which (mathematically identical!) models under the subsystem-interpretation represent. This is because in the subsystem case, certain physical objects not represented explicitly in the model can nevertheless be represented implicitly,[5] and spatio-temporal symmetries can be thought of as changing the relations of the explicitly represented subsystem of the universe to these implicitly represented objects. Let

[4] Cf. Fletcher (2020:236), who claims that breaking this equivalence would be *ad hoc*.

[5] This idea is mentioned by Caulton (2015:158, footnote 17) and discussed in more depth by Pooley (2017:136–140) and Luc (2021).

us take as an example classical mechanics and translations, which are dynamical symmetries of this theory. Consider a model of classical mechanics consisting of n objects (either point-like or extended). What is the meaning of the positions and momenta of these n objects written in a particular reference frame? If we are interested only in the relations between these n objects (i.e., in the internal features of the n-body system), then only frame-independent information is relevant. However, if we think of our reference frame as being associated with an observer O[6], then positions and momenta in this particular reference frame are positions and momenta relative to O. As O is assumed to be a real physical object, these positions and momenta are physically meaningful quantities. What changes when we perform a translation? It depends on whether it is understood actively or passively. Under the passive understanding of the translation, it amounts to changing a reference frame to another one, which may be associated with another observer O'. If there is indeed such an O', then positions and momenta in this new reference frame are positions and momenta relative to O'. Under the active understanding of the translation, it amounts to changing positions and momenta of n bodies composing our system relative to O (so the observer does not change here). This translation-related model represents a different physical situation than the original one, as some physical quantities are different than in the original situation (namely, the relations of our n explicitly represented objects to implicitly represented O). Therefore, under the subsystem-interpretation of the models of classical mechanics and under the active interpretation of translations, translation-related models can be thought of as representing different physical situations—even though not intrinsically different, as they differ only in their relations to the implicitly represented observer associated with the reference frame. This interpretation is valid only in some contexts (in particular, there must indeed be

[6] By an "observer" I mean just any reference object, not necessarily human, which is a common way of speaking in physics.

some physical object located at the origin of the reference frame), but the existence of such contexts should be uncontroversial.

What would change if our two translation-related models were interpreted as representing entire possible worlds instead of representing subsystems? In that case, all the physical objects existing in each of these worlds would be represented explicitly in the respective models, so there would be no further objects that could be represented implicitly. Therefore, translations (understood actively) would no longer admit an interpretation of the changes of relations of the system represented explicitly in the model to some external physical object represented implicitly. One can still maintain that translation-related models under the world-interpretation represent distinct possible worlds, but this view in the case of the world-interpretation is much less attractive than in the case of the subsystem-interpretation (and in fact it is rarely encountered in the contemporary literature; cf. Pooley 2013, Belot 2018:948). If this analysis is correct, then translation-related models under the subsystem-interpretation can represent different possible physical situations, whereas under the world-interpretation they cannot represent different possible physical situations, which is a failure of S-W-Equivalence.[7]

Therefore, we need to decide whether we want to consider our interpretative principles concerning symmetries in the version for entire worlds, in the version for subsystems or in both versions. In our example of trans-

[7]S-W-Equivalence would be more plausible if we were restricted to the internal features of the represented subsystems and abstracted away from their relations to any external, implicitly represented physical objects. This way of thinking can be further supported by observing that the whole idea of implicit representation may sound rather suspicious—one could claim that either something is represented explicitly in the model or is not represented at all. However, the concept of implicit representation seems to be needed to account for how models under the subsystem-interpretation are actually used. When scientists test the predictions of a physical theory, they make observations and measurements in a particular reference frame (often called "laboratory reference frame"), which is associated with a real physical object (namely, the concrete laboratory, where these scientists are working) and is often not represented explicitly in the model. This way of thinking is also visible in textbook presentations of classical mechanics and special relativity, where reference frames are usually associated with "observers" (see, e.g., Gregory 2006:40, 259; Morin 2008:509-512; Rindler 1982:7). Therefore, I assume that the idea of an implicitly represented reference object and its (also implicitly represented) relations to the explicitly represented system is tenable.

lations in classical mechanics, SYM-ONE fails (at least in some contexts) for models thought of as representing subsystems, but (arguably) SYM-ONE is still true for (mathematically the same!) models thought of as representing entire worlds, because our way of justifying that symmetry-related models under the subsystem-interpretation do not represent the same physical situation is not available under the world-interpretation (there are no objects outside of the universe to which the universe might be related). This suggests that SYM-ONE can be true only for models under the world-interpretation, if at all. For models under the subsystem-interpretation it is false, and no sophisticated examples are needed to show this; the simplest ones (such as translations in classical mechanics) suffice. However, most of the disputants seem to have in mind models under the world-interpretation, as the debate is often framed in terms of possible worlds. Therefore, in this paper only the versions of SYM-ONE and SYM-MANY for entire possible worlds will be considered.

The sixth ambiguity in our interpretative principles (i.e., SYM-MANY and SYM-ONE) concerns how the "sameness" of physical situations should be understood: do only qualitative differences matter or also haecceistic ones? To complicate this even more, (merely) haecceistic differences can be of two types. The first type is when two qualitatively identical situations contain the same individuals but differ in which properties each individual possesses. The second type is when two situations are qualitatively the same and contain different individuals (of course, "different" in a non-qualitative sense)—that is, there is at least one individual involved in one of these situations but not in the other. The first sense of haecceistic difference is more common in the philosophical discussions about space-time. For example, Pooley (2013) defines haecceistic difference in the following way: the difference between two possible worlds is merely haecceistic if "they differ only over which space-time points instantiate which of the particular features common to both worlds". The second sense of haecceistic difference is difficult to find in the debates about space-time (I think this is because the issue of possible worlds differing merely haecceistically emerges in the philosophy of physics in the context of symmetry transformations, which seem to generate such worlds, but they do not change the range of what there is), although it is a vital option for more general metaphysical debates. For example, the view that it is not necessary what there is (called "contingentism", see, e.g., Williamson 2013:2) allows that there are possible worlds differing haecceistically in the second sense (on its own, contingentism does not entail this thesis—to get an entailment here, one would need to add some other metaphysical assumptions). However, if we are interested in the subsystems rather than entire worlds, the second type of haecceistic difference is something not only conceivable but (arguably) sometimes actually encountered: two numerically distinct subsystems of the same world can be represented by the same model provided that they are qualitatively identical (cf. example (iii) on p. 6).[8] As both qualitative and haecceistic differences are worth studying, in what follows I will consider two versions of SYM-ONE:

> **SYM-ONE-Q-W**: Two models of a physical theory under the world-interpretation are symmetry-related iff they can represent the same possible worlds and these possible worlds do not differ qualitatively (at most, they differ haecceistically[9]).
>
> **SYM-ONE-H-W**: Two models of a physical theory under the world-interpretation are symmetry-related iff there is exactly one possible world they can represent.

As previously, these principles involve an implicit universal quantification over theories and over models.[10] According to SYM-ONE-Q-W,

[8] Perfect qualitative identity is rather rare, but sufficient qualitative similarity in the relevant respects is quite common—without it, scientific experiments would not be repeatable.

[9] Both types of haecceistic differences mentioned in the main text are taken into account here.

[10] Therefore, the more explicit formulation of these principles is as follows:

two symmetry-related models under the world-interpretation represent the same possible world understood qualitatively, but still the worlds represented by these two models can differ haecceistically. In contrast, according to SYM-ONE-H-W, two symmetry-related models under the world-interpretation represent the same possible world in both a qualitative and a haecceistic sense (that is, in all possible senses of sameness).[11] SYM-ONE-Q-W follows from SYM-ONE-H-W[12] but not the other way around. This means that one can reject SYM-ONE-H-W without rejecting SYM-ONE-Q-W, but rejecting SYM-ONE-Q-W amounts to rejecting SYM-ONE-H-W as well. Some of the counterexamples that will be considered in section 4 are supposed to undermine only SYM-ONE-H-W, but others are supposed to also undermine SYM-ONE-Q-W. As my aim will be to show that both kinds of counterexamples are unsuccessful, I will focus mainly on the stronger principle, SYM-ONE-H-W; if any of the coun-

SYM-ONE-Q-W: For any physical theory T and for any two models of T, M_1 and M_2: if M_1 and M_2 are considered under the world-interpretation, then M_1 and M_2 are symmetry-related iff there is a class of possible worlds P such that the elements of P do not differ qualitatively, and both M_1 and M_2 can represent all and only possible worlds belonging to P.

SYM-ONE-H-W: For any physical theory T and for any two models of T, M_1 and M_2: if M_1 and M_2 are considered under the world-interpretation, then M_1 and M_2 are symmetry-related iff there is a possible world such that both M_1 and M_2 can represent it and they cannot represent any other possible world.

[11]SYM-ONE-H-W is not a haecceistic principle in the sense of presupposing the truth of haecceitism (i.e., the view that there can be merely haecceistic differences). It might be true even if there is no such thing as merely haecceistic differences at all. "H" in SYM-ONE-H-W indicates that in contrast to SYM-ONE-Q-W, it excludes that two symmetry-related models under the world-interpretation can represent possible worlds differing haecceistically. According to SYM-ONE-H-W, such models can represent possible worlds that differ neither qualitatively nor haecceistically, which is equivalent to saying that there is exactly one possible world they can represent (for possible worlds to be different, they need to differ either qualitatively or haecceistically).

[12]This is easy to show, but let me do this once. Assume SYM-ONE-H-W, that is, for any physical theory T and for any two models of T, M_1 and M_2, under the world-interpretation: M_1 and M_2 are symmetry-related iff there is exactly one possible world that M_1 and M_2 can represent. Now, take any theory T and any pair of its models, M_1 and M_2, considered under the world-interpretation. Assume that M_1 and M_2 are symmetry-related. By SYM-ONE-H-W, there is exactly one possible world that M_1 and M_2 can represent (call it w). Therefore, the class of possible worlds that can be represented by M_1 is the same as the class of possible worlds that can be represented by M_2—namely, this is the singleton that has w as its only element (and of course the elements of this class differ at most haecceistically—in fact, they do not differ at all, as there is only one element). As we have considered an arbitrary T and an arbitrary pair of its models, this establishes SYM-ONE-Q-W.

terexamples were successful, this principle surely would be undermined. However, I think that counterexamples directed specifically against SYM-ONE-Q-W are more interesting in the context of the existing literature because this interpretative principle is much less commonly discussed.[13]

Analogously, one can precisify SYM-MANY:

> **SYM-MANY-Q-W**: Two models of a physical theory under the world-interpretation are symmetry-related iff they can represent the same possible worlds (neither qualitative identity nor qualitative distinctness of these worlds is assumed).
>
> **SYM-MANY-H-W**: Two models of a physical theory under the world-interpretation are symmetry-related iff they can represent the same possible worlds and these possible worlds do not differ qualitatively (at most, they differ haecceistically).

As always, these principles involve an implicit universal quantification over theories and over models.[14] Observe that SYM-MANY-H-W implies SYM-MANY-Q-W but not the other way around and that SYM-MANY-H-W is the same as SYM-ONE-Q-W.

Seventh, the above formulations are still ambiguous, as long as we do not provide the exact meaning of the term "symmetry". The most liberal definition could be that any permutation of the set of models of

[13]For example, in the literature about the Hole Argument, it is usually assumed that there are no qualitative differences between possible worlds represented by two models related by the hole diffeomorphism, and the discussion concerns the question whether they represent two possible worlds differing only haecceistically or just one possible world (and what the consequences of each option for the ontology of space-time are). See, for example, Pooley (2013, section 7) and references therein.

[14]Therefore, the more explicit formulation of these principles is as follows:

> **SYM-MANY-Q-W**: For any physical theory T and for any two models of T, M_1 and M_2: if M_1 and M_2 are considered under the world-interpretation, then M_1 and M_2 are symmetry-related iff there is a class of possible worlds P such that both M_1 and M_2 can represent all and only possible worlds belonging to P.
>
> **SYM-MANY-H-W**: For any physical theory T and for any two models of T, M_1 and M_2: if M_1 and M_2 are considered under the world-interpretation, then M_1 and M_2 are symmetry-related iff there is a class of possible worlds P such that the elements of P do not differ qualitatively, and both M_1 and M_2 can represent all and only possible worlds belonging to P.

a theory counts as a symmetry (cf. Belot 2013:3). If even the weakest of our interpretative principles (i.e., SYM-MANY-Q-W) was true for this understanding of symmetries, it would mean that for any physical theory all its models can represent the same possible worlds (because for any two models there exists a bijection on the set of models that exchanges them). This would make the theory unable to make distinctions between physical situations it is supposed to make and eventually render it useless. Therefore, this definition is clearly too liberal, and we need some more restrictions on our notion of symmetry. An additional support for this need comes from the analysis of the examples of symmetries actually discussed in physics (see, e.g., Sundermeyer 2014), which contain much less than the set of all bijections on the set of models.

We are left with two questions. First, how should the set of bijections on the set of models be restricted to get the extension of the word "symmetry" that is closer to physicists' use? Second (and more importantly for our topic), for what understanding of symmetries are the interpretative principles such as SYM-ONE-Q-W and/or SYM-ONE-H-W true? I will not provide general answers to these questions. I will restrict my consideration to dynamical symmetries (i.e., symmetries preserving the theory's dynamics), so that whenever two models will be said to be "symmetry-related", it will mean "related by a dynamical symmetry". All interpretative principles will be considered only for dynamical symmetries. This choice will not be fully argued for here, but it is dynamical symmetries that are most often considered by physicists and philosophers (in particular, all examples reviewed in section 4 are of this type), and there are some general arguments for the thesis that precisely these symmetries should be regarded as associated with physical equivalence (see, e.g., Baker 2010, Wallace 2019 and Dewar 2022, section 6.2).

One could try to define symmetries simply by means of our interpretative principles. Such a definition would look as follows: "a symmetry of a theory T is any transformation of T that satisfies S", where S is SYM-ONE-H-W, SYM-ONE-Q-W, SYM-MANY-H-W or SYM-MANY-Q-W. This would give us four (in fact three, because of the equivalence of SYM-ONE-Q-W and SYM-MANY-H-W) senses of the word "symmetry". However, I think this is not a good strategy for defining symmetries. First, it is not commonplace in the literature, so it would be revisionary. Second, it makes the chosen principle analytically true and in this way trivially resolves the debate about which of our interpretative principles (if any!) is true. Of course, one could then ask which transformations of a given theory are symmetries in the sense of our new definition, so the debate could reappear in a different wording. However, I think that it is more natural to start with the notion of symmetries defined in a way that is independent of our interpretative principles and then ask which of them (if any) are true for symmetries understood in this way.

3 The form of arguments from scientific practice

In the recent philosophical literature on symmetries (e.g., Belot 2018, Weatherall 2018, Fletcher 2020), the idea emerged that interpretative principles such as SYM-ONE or SYM-MANY (and all their versions) should be analysed in the context of scientific practice in which the models these principles are about are actually used. In particular, philosophical discussions should take into account and respect how scientists use the relevant concepts. For example, Belot (2018:946) begins his paper, entitled *Fifty Million Elvis Fans Can't be Wrong* (which is also the title of Elvis Presley's album), by stating:

> About some things—such as the fact that Elvis was peachy-keen—his legions of fans could not be wrong, just because

> there were so many of them. Similarly, might makes right whenever a large group of people uses a word or a concept in a certain way—if you are interested in providing an account of how that word or concept works, then you had better be able to accommodate their use (not necessarily only their use, of course). With this in mind, I take another look here at some classic questions about the counting of possibilities from the philosophy of space and time, paying special attention to how physicists count possibilities.

Belot wants to approach the problem of "counting possibilities" (i.e., whether a symmetry transformation applied to some model leads to a model that is only mathematically different or whether in addition it represents a different possibility than the one represented by the original model) by focusing on how physicists themselves count possibilities. And he claims that indeed in some cases they do this differently than some philosophers. For example, later in the same paper, he argues that because physicists distinguish between gauge symmetries and physical symmetries and regard shifts in classical physics as an example of the latter, shifts should be thought of as generating new possibilities (i.e., in our terminology, as violating some versions of SYM-ONE). His reasoning goes as follows (Belot 2018:954):

> So there doesn't seem to be much room to deny that there is a large community of people according to whose modal concepts shifts generate new possibilities—large, that is, relative to the number of philosophers interested in these matters. So shiftless philosophers are engaged in the revisionary project of trying to construct new modal concepts to replace ones in common use. That may well be worthwhile. But unless they are imagining converting physicists to the use of these new concepts (a tall order, to say the least), they will still end up facing the problem

> the rest of us face: trying to construct an analytic framework to make sense of a collection of modal concepts according to which shifts generate new possibilities—since they will, presumably, still want to make sense of the modal concepts actually in use.

Fletcher (2020) sympathises with Belot's ideas and applies them to the case of relativistic physics (2020:241-242):

> I am assuming that the representational capacities of a mathematical model have an intentional component, in the sense that they depend on how its users intend it to be a part of a larger class. And I have taken the relevant class of users to be mathematicians and mathematically grounded scientists. (...) Any attempt to impose considerations extrinsic to that community's, whether philosophical or mathematical, would be misplaced in understanding how representation and equivalence work in general relativity, especially if the physicists outnumber philosophers and mathematicians.

Again, in making judgements concerning representation and equivalence we are advised to look at how scientists using the mathematical structures under consideration think of the way these structures represent physical situations, as well as which of these structures they treat as equivalent and which not.

How exactly arguments for or against interpretative principles (such as various versions of SYM-ONE and SYM-MANY) should be formulated is a subtle issue. Here, based on the papers referred to above (and especially on the above quotes), I will propose only a rough formulation:

> Let T be a physical theory[15] and M_1, M_2 be its two models related by a dynamical symmetry.

[15] A theory here is understood in a rather fine-grained way. In particular, if T_1 and T_2 have different dynamical equations, then they count as different theories. For example, classical mechanics would not count as a theory under this understanding but rather as a family of theories.

Premise: In scientific practice, models M_1 and M_2 are regarded as representing distinct possible physical situations.

Conclusion: Models M_1 and M_2 represent distinct possible physical situations.

This rough formulation will be sufficient for us to understand various examples of such pairs of models (see section 4), which according to some authors satisfy the premise of this reasoning and therefore satisfy the conclusion, which leads them to reject either SYM-ONE-H-W or SYM-ONE-Q-W or both (depending on whether the distinctness of physical situations is understood only haecceistically or also qualitatively). However, as our discussion in section 2 should have made clear, in this version of the argument from scientific practice there appear ambiguities similar to those present in the initial formulations of SYM-ONE and SYM-MANY. Therefore, this form of reasoning will require certain amendments (see section 5), and I will argue that once these amendments are added, it becomes very dubious that the counterexamples indeed undermine either SYM-ONE-H-W or SYM-ONE-Q-W (see section 6).

4 Potential counterexamples to SYM-MANY and SYM-ONE

In the recent philosophical literature, some authors argue for SYM-MANY but insist that it should not be strengthened to SYM-ONE; some others argue that even SYM-MANY is contestable. Their arguments are based on counterexamples to these principles, which will be presented in this section.

Belot (2018) argues for SYM-MANY[16], at least for the examples he

[16] Is this SYM-MANY-H-W (which, to recall, is identical to SYM-ONE-Q-W) or SYM-MANY-Q-W? I think that Belot (2018) has in mind the former, although he does not consider this distinction explicitly. In any case, I am interested in saving the strongest of our interpretative principles, SYM-ONE-H-W, so it does not matter what the exact target of the

analyses in that paper (spatio-temporal symmetries in classical mechanics and isometries[17] in General Relativity). Let me invoke the following three examples.

Example 1 (Belot 2018:953-956): a system of n point particles in classical mechanics. It is uncontroversial that if such a system is treated as a subsystem of a larger system, then to specify a configuration completely one needs $3n$ parameters.[18] However, if there exists nothing besides these n particles, then what is represented by the model is a global history of the world and not a history of its subsystem. In accord with SYM-ONE, fixing a configuration requires less than $3n$ numbers because configurations that differ only by a translation or rotation of the whole system are not genuinely different. However, physicists seem to avoid this identification according to SYM-ONE. Belot suggests that this is because they treat rotations and translations as physical symmetries and not as gauge symmetries (cf. his quotation in section 3). He insists on the significance of this distinction both for subsystem models and global models, so if he is right, then the interpretative principles he establishes should be valid both under (what I call) the subsystem-interpretation and under the world-interpretation of models (but this is precisely what will be questioned in section 6).

Example 2 (Belot 2018:956-958): monopoles. This example is supposed to strengthen the previous one. According to Belot, this is the case where physicists indeed care about the distinction between global states and subsystem states and are interested in counting possibilities. In the previous case (i.e., example 1), one can argue that physicists are

counterexamples as conceived by the authors was. Therefore, to avoid overinterpretation, in this section I will discuss the counterexamples in terms of less precise principles SYM-ONE and SYM-MANY (understood as holding for models under the world-interpretation).

[17]Isometries are understood here as diffeomorphisms extended to the metric via the push-forward operation. This should be distinguished from a narrower sense of isometries, according to which they are a subset of diffeomorphisms that do not change the metric.

[18]Here, Belot is interested in the configuration space of the system and not in the phase space.

interested only in subsystems of larger systems, so this is why they do not treat translation- and rotation-related models as equivalent. Here, according to Belot, physicists explicitly think of the states as global.

Monopoles are magnetically charged particle-like solutions to the Yang-Mills-Higgs equations. In the physics literature, there are two ways of counting the number of states of a system of n static monopoles that disagree about the role of global phase but agree that fixing a position of each monopole requires $3n$ parameters. This implies that according to them, configurations differing only by a translation or rotation of the whole system are treated as physically non-equivalent.

Example 3 (Belot 2018:964-970): solutions of General Relativity (GR) that are asymptotically flat at spatial infinity. Typically, they represent (approximately) isolated systems, such as a single star. A pair of isometric solutions of this type can differ by a time translation at spatial infinity, so they are not equivalent despite being symmetry-related; according to Belot, equivalent solutions must differ at most by an isometry that is asymptotic to the identity at spatial infinity. In Belot's own words (2018:965-966):

> Faced with such a pair of isometric solutions (= same pattern of events, instantiated differently on [space-time] V) that are not gauge equivalent (= they are capable of representing distinct physical possibilities), we conclude that there is a pair of physical possibilities that similarly differ by a temporal translation at spatial infinity. So, contrary to the common wisdom among philosophers, two general relativistic worlds can differ only as to when things happen, in the sense of differing by a time translation at infinity.

In his earlier work, Belot (2013) argues for a more radical thesis that in some cases even SYM-MANY[19] is not accurate. Below there are two

[19]It seems that it is SYM-MANY-H-W (equivalent to SYM-ONE-Q-W) that is questioned

of his examples.

Example 4 (Belot 2013:7): the one-dimensional harmonic oscillator. Solutions describing systems of this kind have the form $q(t) = A \cos t + B \sin t$. The symmetries of this system are such that any solution can be transformed into any other (there is a one-parameter group that changes the value of A and another that changes the value of B).[20] Belot argues that if SYM-MANY was true, then this theory would not be able to discriminate between situations that are clearly physically different, for example, between a permanently immobile oscillator and "the oscillator continually sproinging around."

Example 5 (Belot 2013:7): linear homogeneous partial differential equations. In physics, the heat equation, the wave equation, and the source-free Maxwell equations are of this type. Again, any two solutions are related by a symmetry, so SYM-MANY seems to make impossible distinguishing, for example, between a field in its ground state and propagating waves carrying energy.

Weatherall (2018) argues for the "only if" part of SYM-MANY (i.e., if two models are symmetry-related, then they can represent the same physical situations). According to him, issues of this type should not be settled on the basis of metaphysical arguments but by purely methodological ones. His main assumption is that "the default sense of 'sameness' or 'equivalence' of mathematical models in physics should be the sense of equivalence given by the mathematics used in formulating those models" (2018:3). For example, in GR mathematical objects are Lorentzian manifolds and Weatherall claims that if they are related by an isometry, then they should be regarded as physically equivalent—simply because in mathematics, the standard of identity for Lorentzian manifolds is an isometry.

here—that is, according to Belot, we have to do with qualitative differences here.

[20] For technical details, see Wulfman and Wybourne (1976).

Fletcher (2020) explores further consequences of Weatherall's ideas.[21] He argues against the "only if" part of SYM-ONE (i.e., if two models are symmetry-related, then they represent the same physical situation; he calls it "Representational Uniqueness by Mathematical Equivalence"), against the "if" part of SYM-MANY (i.e., if two models represent the same physical situations, then they are symmetry-related; he calls it "Representational Distinctness by Mathematical Inequivalence") and for the "only if" part of SYM-MANY (i.e., if two models are symmetry-related, then they can represent the same physical situations; he calls it "Representational Equivalence by Mathematical Equivalence"; this is the claim that he attributes to Weatherall). He uses the following two examples.

Example 6: Schwarzschild space-times (Fletcher 2020:236-239). This is a family of solutions of GR used to represent black holes. Outside the Schwarzschild radius r_S, the line element of such a space-time is given by:

$$c^2 ds^2 = \left(1 - \frac{r_S}{r}\right) c^2 dt^2 - \left(1 - \frac{r_S}{r}\right)^{-1} dr^2 - r^2(d\theta^2 + \sin^2\theta d\phi^2). \quad (1)$$

The family is therefore parametrised by one parameter $r_S \in (0, \infty)$. Fletcher observes that each mathematical Schwarzschild space-time can represent any physical Schwarzschild space-time (with any r_S) through an appropriate choice of units. This is clearly in conflict with SYM-ONE[22] because it implies that any model can represent only one physical situation (since irrespective of what we mean by symmetries, every model is symmetry-

[21] Strictly speaking, Fletcher's examples do not use the term "symmetry" but rather the term "isomorphism." Two models are isomorphic if they are structurally the same (the precise meaning of this depends on what we count as structure in a given context). Being isomorphic is a different relation than being symmetry-related, as it does not appeal to the theory's dynamics. For our current purposes, this distinction will not matter much because all examples considered here involve symmetry-related models (even if the author's main focus is on their being isomorphic), so they may serve as counterexamples to SYM-ONE, even if the original purpose of invoking them was slightly different. However, in general the distinction between symmetry and isomorphism is worth bearing in mind.

[22] Here SYM-ONE-Q-W seems to be relevant, as the difference in the value of the Schwarzschild radius is a qualitative one.

related to itself).

The same family of models can be used to argue against the "if" part of SYM-ONE. Consider the homothety $g_{ab} \mapsto Cg_{ab}$ with $C > 0$, $C \neq 1$. This is a transformation within the family of Schwarzschild space-times. By an appropriate change of variables, the transformed metric can be put into the original form, so models related by this transformation are suited to represent the same physical situations. However, these space-times are not isomorphic to each other as Lorentzian manifolds (i.e., they are not isometric) because their Schwarzschild radii (which are coordinate independent) are numerically distinct. Therefore, Fletcher concludes that the RHS of SYM-ONE is satisfied, but the LHS is violated.

Example 7 (Fletcher 2020:237-238): relativistic swerve scenario (an analogous classical example, put to the same purpose, can be found in Belot 2013:949-950). Consider Minkowski space-time with one particle that stays at rest until some moment when it starts to accelerate at a constant rate in some direction. More specifically, its motion is described by:

$$
\begin{aligned}
t(\tau) &= \begin{cases} \tau & \text{if } \tau \leq 0, \\ (c/a)\sinh(a\tau/c) & \text{if } \tau > 0, \end{cases} \\
x(\tau) &= \begin{cases} c^2/a & \text{if } \tau \leq 0, \\ (c^2/a)\cosh(a\tau/c) & \text{if } \tau > 0, \end{cases} \\
y(\tau) &= z(\tau) = 0,
\end{aligned} \tag{2}
$$

where τ is the particle's proper time and a is a constant acceleration. According to Fletcher, the users of relativity theory "would intend for the theory to endorse that the particle could have swerved (at the same acceleration) in another direction, even at another time, than it did in the above model" (2018:10). But such alternative models are related by a symmetry (namely, time translation) to the original one, so according to SYM-ONE, they are not genuinely different possibilities. Therefore, modal intuitions of the theory's users seem to be in conflict with the "only-if" part of SYM-ONE.[23]

5 The form of arguments from scientific practice revisited

In the previous section, we saw certain counterexamples to SYM-ONE-Q-W and SYM-ONE-H-W. As suggested in section 3, the reasoning based on these counterexamples takes the following (rough) form:

> Let T be a physical theory and M_1, M_2 be its two models related by a dynamical symmetry.
>
> **Premise**: In scientific practice, models M_1 and M_2 are regarded as representing distinct possible physical situations.
>
> **Conclusion**: Models M_1 and M_2 represent distinct possible physical situations.

As suggested earlier (in section 3), this form of arguments from scientific practice is too rough for the purpose of undermining SYM-ONE-H-W or SYM-ONE-Q-W. First, recall that in some cases the representation relation can work quite differently depending on whether we consider the models under the subsystem-interpretation or the world-interpretation. However, the argument has been formulated in terms of "physical situations", which is an ambiguous concept. In order to transform it into a valid argument against SYM-ONE-H-W or SYM-ONE-Q-W, we need

[23] Is the difference here meant to be qualitative or quantitative? Perhaps each answer is defensible. On the one hand, the shape of the trajectory of the particle is the same in all models, which may incline one to think about physical situations represented by these models as qualitatively the same, differing only in how they are embedded in space-time. On the other hand, the difference in direction is something that can be observed, which suggests that it is qualitative. Our analyses in section 6 will apply to both cases, so the answer does not matter here (cf. footnote 16), but this ambiguity is interesting on its own.

to additionally assume that the models under investigation can be understood as representing entire possible worlds and not only their subsystems. This leads to the following modification of the form of the argument:

> **Premise 1**: Models M_1 and M_2 admit the world-interpretation, that is, they can be used to represent possible worlds.
>
> **Premise 2**: In scientific practice, models M_1 and M_2 are regarded as representing distinct possible physical situations.
>
> **Conclusion**: Models M_1 and M_2 under the world-interpretation represent distinct possible worlds.

This is still not a sufficient refinement because it is possible that M_1 and M_2 are used in scientific practice only under the subsystem-interpretation, even though they admit the world-interpretation. If this is the case, then Premise 2 is made true solely by the use of these models under the subsystem-interpretation, from which nothing follows about these models under the world-interpretation. However, our conclusion concerns the world-interpretation. The following version of the argument closes this gap:

> **Premise 1**: Models M_1 and M_2 admit the world-interpretation, that is, they can be used to represent possible worlds.
>
> **Premise 2**: In scientific practice, models M_1 and M_2 under the world-interpretation are regarded as representing distinct possible worlds.
>
> **Conclusion**: Models M_1 and M_2 under the world-interpretation represent distinct possible worlds.

I think that this argument as it stands is still not convincing enough. This is because scientific practice is not always reliable. It is currently well known that some actual practices of scientists are methodologically flawed. A more promising move is to appeal not just to any scientific practice but to those practices that enjoy predictive success (at least to a good approximation). Scientists can be trusted in their grasp of what the world looks like only when their theories are predictively successful and only to the extent that is really needed to explain this success.

Does this methodological stance constitute a divergence from the way of thinking of philosophers who use arguments from scientific practice, such as those referred to in this paper? Perhaps to some extent it does, but I believe that this is a difference at the level of declarations rather than in spirit. I suppose that philosophers who trust arguments from scientific practice do so precisely because science in general does enjoy many predictive successes.[24] Therefore, I assume that those philosophers (including the authors whose works are discussed in this paper) would not object to the following modification of the argument from scientific practice:

> **Premise 1**: Models M_1 and M_2 admit the world-interpretation, that is, they can be used to represent possible worlds.
>
> **Premise 2**: In scientific practice, models M_1 and M_2 under the world-interpretation are regarded as representing distinct possible worlds.
>
> **Premise 3**: Predictive success of the scientific practice mentioned in Premise 2 relies on Premise 2 being true (i.e., this practice would be less predictively successful if models M_1 and M_2 under the world-interpretation were not regarded as representing distinct possible worlds).
>
> **Conclusion**: Models M_1 and M_2 under the world-interpretation represent distinct possible worlds.

The role of the additional step (i.e., Premise 3) is to ensure that we do

[24] One could suggest that philosophers focusing on the way in which scientists use certain terms are interested in capturing scientific practice as such, irrespective of whether it is empirically successful or not. This might be a worthwhile pursuit, but as here we are ultimately interested in reaching some metaphysical conclusions, the question whether a given scientific practice is empirically successful is clearly relevant.

not base our judgement concerning representational equivalence of certain models on scientific practices that do not contribute to the predictive success of science. As long as they do not contribute to the predictive success of science, we do not have a sufficient basis for believing in their metaphysical relevance. The burden of proof is on the side of these scientific practices.

However, our Premise 3 requires even something more—not only that the scientific practice on which we base our judgements concerning representational equivalence is predictively successful but also that this success is dependent on regarding models M_1 and M_2 as representing distinct possible worlds within this scientific practice. It should be clear why this stronger condition should be imposed. One can imagine the following situation (and this seems to even be the typical case in physics). Scientists use models of a certain theory to represent subsystems of the world; they treat symmetry-related models as representing different physical situations; on this basis, they generate predictions that turn out to be empirically successful; and they admit that models of the same theory can be used to represent entire possible worlds and not only their subsystems but actually never use the models of this theory to represent the entire world in a way that is predictively successful. In such a case, we are not allowed to infer our conclusion, irrespective of whether scientists think of symmetry-related models under the world-interpretation as representing distinct possible worlds or the same possible world. This is because their thinking in either of these two ways does not have any impact on the predictive success of their scientific practice, and it is precisely this success that enables us to make inferences from scientific practice to claims about the physical reality.

Of course, even in this final form the argument is fallible in the sense that its conclusion can be false even though all the premises are true. This is because the argument is not a deductive one. Instead, it is similar to arguments for scientific realism based on the inference to the best explanation, although these two types of arguments establish different types of conclusions. For example[25], an argument for the existence of electrons can be formulated in the following way:

> **Premise 1**: The existence of electrons is postulated in scientific practice.
>
> **Premise 2**: Predictive success of the scientific practice mentioned in Premise 1 relies on Premise 1 being true (i.e., this practice would be less predictively successful if the existence of electrons was not postulated within it).
>
> **Conclusion**: Electrons exist.

The conclusion here concerns the existence of objects of a certain kind, whereas in our argument it concerns the reality of certain differences—if models M_1 and M_2 under the world-interpretation represent distinct possible worlds, then there is a physical difference that corresponds to the mathematical difference between M_1 and M_2. Alternatively, the conclusion of our argument concerns what physically possible worlds there are—if models M_1 and M_2 under the world-interpretation represent distinct possible worlds, then there are possible worlds differing in the way that is captured by the mathematical difference between M_1 and M_2.

In light of this similarity between the two arguments, it seems plausible that at least those who find the argument for scientific realism reliable should also endorse the argument from scientific practice as formulated in this section. This does not mean, however, that scientific realists should reject any of our interpretative principles. By "reliability" I mean a "softer" counterpart of validity (the latter concept is appropriate for deductive arguments only). However, a valid argument does not need to

[25] Here, I understand scientific realism only as an affirmation of the existence of objects denoted by theoretical terms, but in general it comprises several theses about scientific theories (see, e.g., Psillos 1999).

be sound; that is, its premises can be false and then the conclusion is not established. I will argue that precisely this is the case for our argument from scientific practice—it is reliable, but its premises are false, so it does not constitute the reason for rejecting any version of SYM-ONE.

One can object to my analogy between the above argument and the argument for scientific realism by pointing out that the conclusions they establish are of different kinds: the conclusion of the former is about what a theory says, whereas the conclusion of the latter is about whether a theory is true. In response, let me repeat that these arguments, as I understand them here, are both about what there is (where "is" extends to the realm of mere possibility, so in the broad sense of "is"). Despite the fact that the conclusion of our argument from scientific practice is formulated in terms of representation, its main gist is not semantic (what represents what) but metaphysical (what there is to be represented—are there distinct possible worlds, whose differences are captured by mathematical differences between two symmetry-related models?). Concerning the argument for scientific realism, I have already mentioned that here I am interested only in its existential content (see footnote 25).

6 Saving SYM-ONE-H-W

For the analysis of potential counterexamples to our interpretative principles (i.e., versions of SYM-ONE and SYM-MANY), I propose the following strategy that is based solely on the content of these principles and on the final form of arguments from scientific practice established at the end of section 5. If two models of a given theory T, M_1 and M_2, are regarded in a certain scientific practice as representing different physical situations, then before we reject some of our interpretative principles, we should answer the following questions:

(a) Does the theory T have well-formulated dynamics?

(b) Are M_1 and M_2 related by a dynamical symmetry?

(c) Do models M_1 and M_2 admit the world-interpretation? (needed for Premise 1)

(d) Are models M_1 and M_2 *under the world-interpretation* regarded as representing distinct possible worlds in this scientific practice? (needed for Premise 2)

(e) Does the theory T contribute to the predictive success of science? (needed for Premise 3, part 1)

(f) If so, does this contribution rely on using models M_1 and M_2 under the world-interpretation? (needed for Premise 3, part 2)

(g) If so, does this contribution rely on regarding models M_1 and M_2 under the world-interpretation as representing distinct possible worlds? (needed for Premise 3, part 3)

If the answers to all the above questions are "yes", then we are entitled to reject some of our interpretative principles. If the sets of possible worlds represented by M_1 and M_2 are different, then we reject SYM-MANY-Q-W. If these sets are the same and the possible worlds belonging to them differ qualitatively, then we reject SYM-MANY-H-W (which is identical to SYM-ONE-Q-W). Finally, if the sets are the same and possible worlds belonging to them differ only haecceistically, then we reject SYM-ONE-H-W. Of course, whenever we reject some interpretative principle, we also need to reject all principles that imply it.

Let me explain briefly where these conditions come from. In condition (a) by "well-formulated dynamics" I mean what most of physicists and philosophers of physics would call just "dynamics". The adjective "well-formulated" is only to exclude explicitly cases that are dynamical in the sense of involving some representation of change, but where this change is not calculated from any equations of motion. For example, Ptolomean and Copernican models of celestial bodies surely describe motion, so they are dynamical in the looser sense, but they lack dynamics in the proper sense. This is in contrast to Newtonian celestial mechanics, which has a well-formulated dynamics (consisting of Newton's second law and Newton's law of gravitation). I do not claim that the only way to build a physical theory is to base it on a well-formulated dynamics in this sense. Condition (a) is introduced here only because it is needed to pose our question about SYM-ONE and SYM-MANY (recall that in section 2 we decided to use the term "symmetry" in the sense of "dynamical symmetry"). If it is violated, then one cannot even ask which transformations are dynamical symmetries of the theory T, and, *a fortiori*, one cannot apply to models of T any of our interpretative principles concerning symmetries (because they assume that the set of the dynamical symmetries of the theory is specified). Therefore, the sense of "well-formulated" is not normative here, this adjective is only used to differentiate between theories for which the questions about SYM-ONE and SYM-MANY are well-posed from those for which they are ill-posed due to the lack of dynamics of an appropriate kind.[26]

The condition (b) should be obvious, as our interpretative principles concern models related by dynamical symmetries, so models that are not related by dynamical symmetries are irrelevant for their truth or falsity. The remaining criteria, (c)–(g), are indispensable for the argument from scientific practice (in its final formulation established at the end of section 5) to go through; it is indicated in brackets to which premise of the argument a given condition corresponds.

One can object that these conditions are too strong in a way that begs the question against a denier of our interpretative principles. If there is only one world our predictions are about (i.e., the actual world), then—the objection might go—it is logically impossible for (g) to be satisfied, as it requires using M_1 and M_2 to represent different possible worlds in

[26] This condition might not seem to be worth mentioning, but it will turn out that it is not satisfied in example 7.

a predictively successful way. I agree that (g) is very difficult to satisfy, but this is not logically impossible, even under the assumption that our predictions concern only one of the possible worlds. To say that M_1 and M_2 represent different possible worlds is to say that there is a physical difference between possible worlds represented by M_1 and M_2 (i.e., there is some proposition p that is true about one of these worlds but not about the other, where p might be either a qualitative or only a haecceistic claim), so one can reasonably ask which of these two worlds (if any) is the actual one (i.e., whether in the actual world p is true). If this physical difference between worlds represented by M_1 and M_2 is detectable, then it is in principle possible to try to answer this question empirically. In such a case, distinguishing between M_1 and M_2 contributes to the predictive success of science, even though at most one of them represents the actual world.[27]

Below, I will argue that none of the potential counterexamples to SYM-ONE-Q-W and SYM-ONE-H-W presented in section 4 satisfies all the above conditions, which means that they do not succeed in undermining these interpretative principles.

In example 1, physicists' reluctance to identify solutions differing only by translation or rotation can be explained by the fact that they always

[27] The issue of the observability of differences between symmetry-related models is analysed in the literature about the so-called direct empirical significance of symmetries (see, e.g., Kosso 2000, Brading and Brown 2004, Greaves and Wallace 2014, Teh 2016). Significantly for our discussion, only models under the subsystem-interpretation are considered in this context. As Brading and Brown (2004:646) write (emphases mine):

> We maintain that the direct empirical significance of physical symmetries rests on the possibility of effectively isolated subsystems that may be actively transformed with respect to the rest of the universe. (...) The example of Galileo's ship also illustrates that observing a symmetry involves two observations (...) we first observe the transformation, which involves *transforming a subsystem* with respect to some reference that is itself observable, and we then observe that the *symmetry holds for the subsystem.*

According to this quote, we can observe the difference between two symmetry-related states because they are related to some reference objects, which is possible only if these states are not the states of the universe as a whole. This suggests that the primary or even the only way in which a difference between two symmetry-related models can contribute to the predictive success of science is not available if the models are used to represent the universe as a whole, which provides a reason to doubt that condition (g) can ever be satisfied.

assume an implicit laboratory reference frame (cf. footnote 7), and, therefore, what they have in mind when counting possibilities are models under the subsystem-interpretation and not under the world-interpretation. Therefore, this example seems to violate our condition (d).[28]

At this point, one could ask further: *if* condition (d) was satisfied, would some of the remaining conditions, (e)–(g), block the argument of the opponent of SYM-ONE? Starting with (e), classical mechanics has undoubtedly made a contribution to the predictive success of science (it is even one of the most important theories in the whole history of science). However, our understanding of a theory here is more fine-grained, so condition (e) should be understood as being not about classical mechanics, but (in example 1) its version dealing with n point-like particles. Under this specification, condition (e) is still satisfied—for example, our Solar System can be (approximately) represented by models of this theory in a predictively successful way.

Our next question is whether there are any predictively successful applications of n-particle classical mechanics to the universe *as a whole* (i.e., whether our condition (f) is satisfied in this example). How might one argue for or against a claim of this kind? To show that condition (f) is satisfied, it suffices to provide an example of such successful application. In contrast, showing that it is not satisfied seems more challenging. I think that there are two main possible argumentative strategies. The first strategy is to analyse the history of science, especially of cosmology (including, of course, the most recent history), make a list of theories whose models have been in fact used to represent the universe as a whole with predictive success, and to check that the theory under investigation is not on this list. This might seem to be difficult, because for the argument

[28] Answering this question decisively would require some empirical studies of the physicists' community (e.g., based on questionnaires). The issue is complicated by the fact that the users of these models might not even have any definite views concerning the preferred understanding of such models under the world-interpretation.

to go through, we need to make sure that our list is complete, which appears to be impossible because of the vast amount of relevant publications. However, in fact the list does not need to be fully complete, as theories can be divided into similarity classes; then it is sufficient to check that our list is complete with respect to the similarity class, to which the theory under investigation belongs. Furthermore, instead of investigating all publications in a given field of science, one can restrict to its authoritative textbooks and reviews, which are expected to mention all theories whose predictive success is not controversial (of course, they may express the fact of the predictive success of a theory using different words). The second strategy is to find some features of a theory under investigation that make its models unsuitable to represent the universe as a whole. If it possesses such features, then we do not need to engage in the historical work, as such a theory is in principle not able to satisfy condition (f). It should be stressed that the second strategy, if it is successful, gives us more than we need—the argument from scientific practice is an argument from the actual scientific practice, so showing that it does not work requires only showing that no practice it calls for has taken place, not that it is impossible.

Concerning the first approach, in contemporary cosmology there is one theory that is regarded as standard and currently best established, namely, the so-called ΛCDM model[29] that is based on FLRW metric, which is the solution to Einstein's equations under the assumptions of spatial homogeneity and isotropy. Many other theories are considered,[30] but none of them enjoys enough predictive success to replace the current

[29]The word "model" in this expression is used not in the sense that is usual in this paper (i.e., a model of a theory), but as a name for a kind of a theory.

[30]See, for example, Krasiński (1997), Bojowald (2015), Joyce et al. (2015), Bull et al. (2016), Kragh and Longnair (2019). They can be classified in relation to the standard cosmological model in the following way: (1) GR solutions different than FLRW (inhomogeneous and/or anisotropic), (2) modifications of GR, (3) alternatives to GR, (4) theories that are supposed to be more fundamental than GR and expected to reduce to GR in appropriate limits, (5) semi-classical theories that are between GR and theories of category (5). The boundaries between these categories are not expected to be sharp.

standard cosmological model.

It is difficult to find in contemporary cosmology any uses of classical mechanics to represent the universe as a whole. Newtonian theory is still regarded as predictively successful with respect to smaller systems such as the Solar System (which is not the subject of cosmology but of celestial mechanics; see, e.g., Fitzpatrick 2012), although even in this case GR enables better precision. The only exception I managed to find are papers by Milne (1934) and McCrea and Milne (1934), which reproduce to a certain extent the predictions of GR expanding universe model within Newtonian mechanics. These works have been later revived by Callan, Dicke and Peebles (1964), and are mentioned in some cosmology textbooks, such as Sciama (1971:101–110) and Weinberg (1972:474-475); a recent popularizing treatment is Jordan (2005). It should be stressed that this is not the case of classical mechanics making any predictive contribution to cosmology on its own, but rather an attempt to reproduce *partially*[31] the predictive success of GR in cosmology within the framework of classical mechanics. In any case, the mentioned works do not discuss the issue of differences between symmetry-related models, so even if we counted them as an evidence for condition (f) being satisfied by classical mechanics (which seems to me rather far-fetched), this would be not enough to establish (g).

In the case of example 2, condition (d) cannot be easily dismissed, as Belot in his presentation explicitly suggests that here physicists really treat the states as global. If he is right, then condition (d) is satisfied here. Therefore, we need to examine conditions (e)–(g). Even though there are no empirical signs of the existence of fundamental magnetic monopoles, this theory does contribute to the predictive success of science, as certain systems have effective quasi-particle excitations with magnetic charges (Castelnovo, Moessner and Sondhi 2008, Rajantie 2012), so condition (e)

[31]The liminations of the Newtonian approach compared to GR are discussed, for example, in Sciama (1971:110–117), Weinberg (1972:475) and Jordan (2005:653).

is satisfied. However, this is surely predictive success only with respect to subsystems of the universe, not the universe as a whole. In cosmology, monopoles appear as a theoretical prediction for the early universe, but as it is not confirmed, various mechanisms have been proposed that account for their absence (this is known as the ,,monopole problem"; see, e.g., Guth 1981, Lazarides 2006). Even if monopoles turned out to be significant in cosmology, n-monopole models would not be suitable to represent the entire universe (even approximately), as we know already that most of its matter content are objects that are not monopoles. Therefore, we can conclude that example 2 violates condition (f).

It should be stressed that this line of reasoning does not exclude that (i) n-monopole models can be thought of as representing entire possible worlds instead of possible worlds' subsystems and (ii) n-monopole models do contribute to the predictive success of science. In other words, the violation of condition (f) is consistent with conditions (c) and (e) being satisfied. However, the argument from scientific practice against SYM-ONE-Q-W or SYM-ONE-H-W requires something more than the conjunction of (i) and (ii) being true. It requires that the usages of a theory where its models are interpreted *as global* contribute to the predictive success of science, and here this is not the case. If n-monopole models contribute to the predictive success of science, they do so only under the subsystem-interpretation.

One can object that the modal content of physical theories is also important for their predictive success, so not only models that represent the actual universe should be regarded as contributing to this success. If so, the objection might go, the fact that n-monopole models are not suitable for representing the actual universe as a whole is not sufficient for showing that in this case condition (f) is not satisfied.

In response, let us observe that our conditions (e)–(g) are about a theory, not about models. The last two conditions mention models, but it is a theory that is assessed with respect to having or lacking predictive success. Therefore, one can consistently claim that the modal content of physical theories contributes to the predictive success of science, while maintaining that only theories that have models representing (a part or the whole of) the actual universe contribute to the predictive success of science. But why should one endorse the latter claim? Surely, not all theories contribute to the predictive success of science and the boundary needs to be drawn somewhere. Where exactly it should be drawn depends on our views on *how* the modal content of physical theories might contribute to their predictive success. This is a difficult topic and it cannot be fully analysed here, but let me make some brief comments.

I think that the modal content of a theory is important for its predictive success primarily because the specification of the content of any particular model requires other models to be formulated—representing something is always excluding something else. For example, to represent a particle as having a velocity $\vec{v}$, we need to define the whole space of vectors representing possible velocities, as without that it is impossible to make any meaningful claim about velocity. Even if only a single model of a given theory represents (a part or the whole of) the actual universe, it could not do this without all other models of this theory being defined, so these other models, which represent merely possible but not actual physical situations, contribute in this way to the predictive success of this theory. Other theories are not needed for this purpose, so their contribution to the predictive success of science cannot be justified merely by their being related in some way to some theory that is predictively successful.

Another way in which the modal content of a theory may be relevant for its predictive success is the use of modal claims in scientific practice. It seems that scientists often make claims that concern not only the actual happenings, but also what is physically possible or what would happen in a given situation if the conditions were different (e.g., what would be the measured value of some quantity if the experimental arrangement differed from the actual one in such-and-such way). The issue of empirical testing of such claims is famously controversial (as we always observe only the actual course of events and never merely possible ones), but crucial for our assessment of their importance for the predictive success of physical theories. I do not have space here for analysing this topic in detail, but it seems to me that the role of these modal claims can be reconciled with our thesis that only theories that have models representing (a part or the whole of) the actual universe contribute to the predictive success of science. This is because the laws of a theory (in the form of its dynamical equations) are arguably the basis for all its modal claims (e.g., something is physically possible according to a theory T iff it is allowed by the laws of T) and it is precisely these laws that individuate theories in our sense. Therefore, starting from a theory with a model that represents (a part or the whole of) the actual universe, such a theory is sufficient to explain the truth of its modal claims and no appeal to other theories (postulating different laws) needs to be made.

Even if these principled and abstract arguments are not fully convincing, in our attempt to draw the boundary between theories that contribute to the predictive success of science and those that do not, we can still appeal to science itself. If a given theory is not mentioned in the authoritative textbooks and reviews in the relevant field of science as predictively successful, then it surely is not regarded as such by the (majority of) scientific community, so we can conclude that it does not satisfy condition (e). Similarly, if a given theory is mentioned as predictively successful, but not as predictively successful in representing the universe as a whole, then we can conclude that it does not satisfy condition (f) for any of its models. These conclusions are as strong as the authority of the sources we rely on and may change with the change of consensus in science, but this way of justifying seems to be appropriate in our context, as the argument we analyse is an argument from scientific practice. If some theory is not mentioned in authoritative textbooks and reviews in the relevant field of science as being predictively successful, then the burden of proof should be on the person who claims that it does satisfy condition (e). Similarly, if some theory is not mentioned in authoritative textbooks and reviews in the relevant field of science as being predictively successful in representing the universe as a whole, then the burden of proof is on the person who claims that it does satisfy condition (f) for some of its models. As we already observed, in our case the relevant field of science is cosmology, and we do not find in its authoritative sources any proclamation of n-monopole theory being predictively successful in representing the universe as a whole. The same holds for examples 3–7, so below I will not repeat this claim in the analysis of these examples, but I will only discuss issues that are specific to each case.

Example 3 also violates either condition (d) and/or (f). Even though the GR models in question seem to be most naturally interpreted as representing entire possible worlds, as they cover the entire space-time, they also admit the subsystem-interpretation. Otherwise, they would be unsuitable to represent anything in the actual world, as the actual world does not consist of a single star (or any other single massive body). However, such models are used in scientific practice to represent subsystems of the actual world and successfully so. In such applications, it is assumed that there are physical objects in the region that in the model is represented as empty, but the influence of these objects on the star under investigation is to a good approximation negligible. Taking this into account, we need to conclude that in all predictively successful applications these models are used under the subsystem-interpretation, even though this might not be obvious at first glance.

What about the family of inertial frames at infinity mentioned by Belot? My proposal is that they can be interpreted as implicitly representing some physical observers. Of course, if we take “infinity” literally, then this is not an admissible interpretation because the measurement cannot be at an infinite distance from the measured object. However, “infinity” makes sense here as an idealisation—the actual observers are at a finite distance from the star, but their influence on it is negligible, almost as if they were infinitely far away. Granting this interpretation, two such models differing by a temporal translation at spatial infinity indeed represent two distinct physical situations but only because the world besides the explicitly represented single star includes (among other objects) physical observers located far away from this star. Surely, for us it makes a difference whether we observe a star at 2 AM or at 3 AM (because our environment changes in the meantime, and we—the observers—change as well), even if the modelled star (in its intrinsic aspects) remains the same throughout this period. Therefore, it is the difference in what is implicitly represented (and in the relations of what is explicitly represented to what is implicitly represented, which are themselves also only implicitly represented) that makes two such models physically inequivalent. However, as noted earlier (cf. the discussion of S-W-Equivalence in section 2), this way of thinking cannot be extended to the same models under the world-interpretation.

The situation is very similar in the case of examples 4 and 5. Predictive success of physics does not rely on using models representing a single harmonic oscillator or an electromagnetic field that is not accompanied by any matter under the world-interpretation. Any successful uses of such models treat them as representing only some part of the physical world. Therefore, condition (f) is not satisfied here. Importantly, if we extended any of these models by including more objects, the transformations mentioned by Belot would no longer be symmetries of the new dynamics.[32] With the addition of some new physical objects, the states

[32]The same is true for another of Belot’s (2013) examples that I did not mention in section

of the harmonic oscillator with different A and B will become distinguishable, and the same for different states of the electromagnetic field. To take a simple example, consider the family of harmonic oscillators with $B = 0$—that is, described by $q(t) = A \cos t$. Then, the symmetries that change the value of A are simply changes of the amplitude of oscillations. However, the newly added object can be thought of as a rod, with respect to which this amplitude can be measured. For different values of A, the fraction between the amplitude of oscillations and the length of the newly added object will be different. Therefore, the differences between various values of A will become physically meaningful; but also transformations changing them would no longer be applicable to the dynamics of the extended system, so the breakdown of physical equivalence is accompanied by the breakdown of the symmetry. A similar story can be told, *mutatis mutandis*, in the electromagnetic case.

Additionally, in the case of the harmonic oscillator, it is not clear whether these models admit the world-interpretation at all because the equation of motion involves force, which perhaps should have some physical source, but it is not included in the theory.[33] If this is so, then example 4 violates even condition (c).

My diagnosis of Fletcher's example 6 is also similar. Schwarzschild models are mathematical structures suitable for representing subsystems of the universe and they can also represent entire possible worlds other than the actual world, but all their predictively successful applications in scientific practice have been made under the subsystem-interpretation. Schwarzschild solution is used in physics in a predictively successful way to represent neighbourhoods of large massive bodies (e.g., of the Sun in the calculation of the precession of Mercury; see, e.g., Wald 1984:136–148) or to represent black holes with zero electric charge and zero angular

4, namely, a generalised symmetry of the Kepler problem associated with the conservation of the Lenz-Runge vector.

[33] Recall that theories here are understood in a fine-grained way; cf. footnote 15.

momentum (see, e.g., Wald 1984:298–299). Therefore, again condition (f) is violated and perhaps also (d).

There remains a question about the proper understanding of models with different Schwarzschild radii. I think, contra Fletcher (2020:236), that the analysis of this case should violate what I have called S-W-Equivalence (see section 2), that is, the assumption that models under the world-interpretation and under the subsystem-interpretation represent physical situations in the same way. Schwarzschild models under the world-interpretation can be regarded as representing the same possible world (i.e., the difference in Schwarzschild radii can be thought of as a mere descriptive redundancy). However, if in the universe there is some other object, then the fraction between the Schwarzschild radii and the length of this object is a physically meaningful quantity, so under the subsystem-interpretation models with different Schwarzschild radii represent different physical situations.[34]

For the second part of Fletcher's analysis of example 6 that involves homotheties to work against SYM-ONE (presumably against SYM-ONE-Q-W, cf. footnote 22), one would need to assume that only isometries can count as symmetries of the family of models under investigation. However, our understanding of symmetries as transformations leaving invariant the theory's dynamics (see section 2) allows us to count homotheties as symmetries of Scharzschild space-times, as they are transformations of the metric that do not change the form of Einstein's equations for this particular family of space-times. Therefore, perhaps this Fletcher's example shows that models can have the same representational capacities without being isomorphic[35] (if we agree that isometry is a criterion of isomorphism

[34]The subtlety here is that the metric in GR is sensitive to the presence of material objects, so after the introduction of some such objects the metric would no longer be exactly Schwarzschild. However, it could still be approximately Schwarzschild; otherwise, such models would not be applicable to the actual world. The fact that a model represents a physical situation only approximately does not on its own undermine the viability of questions about the way in which it represents.

[35]Which is in fact what he wanted.

here), but it does not show that models can have the same representational capacities without being symmetry-related.

Concerning example 7, one can object that it does not involve well-formulated dynamics (i.e., it violates condition (a)). We are presented only with a stand-alone family of trajectories (given by equation (2)) instead of a full-blown physical theory with dynamical equations such that the presented trajectories are their solutions. Without having such equations, we cannot even ask what the dynamical symmetries of this theory are, so, *a fortiori*, we are not even able to pose the question of whether some of our interpretative principles hold here. Furthermore, if an acceleration is present, this means that there is some force acting on a particle, which indicates that perhaps this could not be a global model (condition (c)), similarly as in the case of example 4.

Can example 7 be reformulated so that it will satisfy our condition (a)? This would require finding some dynamical equations, the solutions of which are swerve trajectories. They would need to be indeterministic, as at least some of these solutions do not differ before certain time. This is rather difficult, as typically considered dynamical equations have a unique solution. However, there are exceptions, one of which is the so-called Norton's dome (Norton 2008). In the case of Norton's dome, an object is placed at the top of a dome and the dynamical equations allow both that it will stay there forever and that it will start to move at an arbitrary time (the latter corresponds to Fletcher's swerve). However, there are no predictively successful scientific practices using anything similar to Norton's dome, so it is likely that a modified version of example 7 satisfying condition (a) would violate condition (e). This means that an argument from scientific practice based on this example cannot succeed.[36]

The above analysis shows that SYM-ONE, if understood as holding for models under the world-interpretation (i.e., as SYM-ONE-Q-W or SYM-

[36] This does not mean that Norton's dome cannot be used in the argumentation for which it was devised—namely, that Newtonian mechanics is not fully deterministic.

ONE-H-W), is not so easy to undermine by means of arguments from scientific practice. It is consistent with successful scientific practice, at least if we are considering the examples reviewed in section 4 and many others that are similar to them.

7 Summary

The aim of this paper was to present a strategy for analysing potential counterexamples to interpretative principles concerning symmetries, such as SYM-ONE-Q-W and SYM-ONE-H-W. This strategy consists of checking seven conditions, (a)–(g), which need to be satisfied for the argument appealing to such counterexamples to go through. It has been argued that none of the potential counterexamples reviewed in section 4 satisfy all seven conditions, so we do not have at our disposal a good argument from scientific practice undermining SYM-ONE-Q-W or SYM-ONE-H-W.

Does this mean that these interpretative principles should be regarded as true? The argumentation of this paper supports this conclusion (or at least keeps it "on the table" as a live option), but of course it is not sufficient for showing this. Perhaps one can find some other counterexamples that satisfy all conditions (a)–(g). This would be difficult, as models of physical theories are usually used to represent only subsystems of the universe; and if they are used to represent the entire universe, the predictive success of such scientific practices can be independent of any assumptions concerning the representational (in)equivalence of symmetry-related models. It is also conceivable that arguments of a different kind can be important in this debate; I just do not take them into account here, as this paper is devoted solely to analysing arguments from scientific practice.[37]

[37]A note on the recently made distinction between the "interpretational" approach and the "motivational" approach to symmetries (Møller-Nielsen 2017, Read and Møller-Nielsen 2020) is in place here. According to the interpretational approach, we can regard two symmetry-related non-isomorphic models (call them M_1 and M_2) as representing the same possible physical situation solely on the basis that they are symmetry-related, whereas according to the motivational approach, the fact that two non-isomorphic models are symmetry-related

To definitely exclude the possibility of violations of SYM-ONE-Q-W and SYM-ONE-H-W, we should not restrict ourselves to discussing potential counterexamples to these principles but formulate some positive argument for these principles, which seems to be rather difficult.[38]

References

[Baker (2010)] Baker, D.J. (2010). Symmetry and the Metaphysics of Physics. *Philosophy Compass* 5(12), pp. 1157-1166.

[Belot (2013)] Belot, G. (2013). Symmetry and equivalence. In: Batterman, R. (ed.) *The Oxford Handbook of Philosophy of Physics*, Oxford: Oxford University Press, pp. 318-339.

[Belot (2018)] Belot, G. (2017). Fifty Million Elvis Fans Can't be Wrong. *Noûs* 52(4), pp. 946-981.

[Bojowald (2015)] Bojowald, M. (2015). Quantum cosmology: a review. Reports on Progress in Physics, 78(2), 023901.

should only motivate us to find a modified theory in which the counterparts of these two models (call them M_1' and M_2') will be isomorphic. Once such a new theory has been found, we are entitled to regard M_1 and M_2 (as well as M_1' and M_2') as representing the same physical situations; but prior to this, we should rather treat M_1 and M_2 tentatively as representing different physical situations. According to the authors, this is because without the modified theory we do not have "a metaphysically perspicuous characterization of the (putative) reality underlying symmetry-related models" (Møller-Nielsen 2017:1258). Therefore, this is an example of an argument not referring directly to scientific practice but rather based on a general view on what one should expect scientific theories to provide. My claims in this paper are tangential to this "interpretational" vs. "motivational" debate. The question I am interested in here is whether various potential counterexamples to SYM-ONE-Q-W or SYM-ONE-H-W succeed in undermining them. If we found any counterexamples to SYM-ONE-Q-W, then *both* interpretational and motivational approaches would turn out to be wrong, as then there would be positive cases of non-isomorphic symmetry-related models that represent distinct possible worlds, so we should neither interpret them as representing the same possible world, nor should we be motivated to find a modified theory (counterexamples to SYM-ONE-H-W that are not counterexamples to SYM-ONE-Q-W would not have such an effect, as they would concern haecceistic differences only, so the models involved would be isomorphic). If, as I claim, these counterexamples are unsuccessful in undermining SYM-ONE-Q-W and SYM-ONE-H-W, then it is still left open whether one should take an interpretational or motivational attitude towards symmetry-related non-isomorphic models.

[38] But see Baker (2010), Wallace (2019) and Dewar (2022, section 6.2) for such attempts.

[Brading and Brown (2004)] Brading, K. A. and Brown, H. R. (2004). Are gauge symmetry transformations observable? British Journal for the Philosophy of Science 55(4):645-665.

[Bull et al. (2016)] Bull, P. et al. (2016). Beyond ΛCDM: Problems, solutions, and the road ahead. Physics of the Dark Universe, 12, 56–99.

[Callan, Dicke and Peebles (1965)] Callan, C., Dicke, R. H. and Peebles, P. J. E. (1965). Cosmology and Newtonian Mechanics. American Journal of Physics, 33(2), 105–108.

[Castellani (2003)] Castellani, E. (2003). Symmetry and equivalence. In: Brading, K. and Castellani, E. (ed.), *Symmetries in Physics: Philosophical Reflections*, Cambridge: Cambridge University Press, pp. 425-436.

[Castelnovo, Moessner and Sondhi (2008)] Castelnovo, C., Moessner, R. and Sondhi, S. L. (2008). Magnetic monopoles in spin ice. Nature 451:42–45.

[Caulton (2015)] Caulton, A. (2015). The role of symmetry in the interpretation of physical theories. *Studies in History and Philosophy of Modern Physics* 52:153–162.

[Dasgupta (2016)] Dasgupta, S. (2016). Symmetry as an Epistemic Notion (Twice Over). *The British Journal for the Philosophy of Science* 67(3), pp. 837-878.

[Dewar (2022)] Dewar, N. (2022). *Structure and Equivalence.* Cambridge: Cambridge University Press.

[Fitzpatrick (2012)] Fitzpatrick, R. (2012). *An Introduction to Celestial Mechanics.* Cambridge: Cambridge University Press.

[Fletcher (2020)] Fletcher, S.C. (2020). On Representational Capacities, with an Application to General Relativity. *Foundations of Physics* 50:228-249.

[Greaves and Wallace (2014)] Greaves, H. and Wallace, D. (2014). Empirical Consequences of Symmetries. British Journal for the Philosophy of Science 65(1):59-89.

[Gregory (2006)] Gregory, R. D. (2006). *Classical Mechanics.* Cambridge: Cambridge University Press.

[Guth (1981)] Guth, A. H. (1981). Inflationary universe: A possible solution to the horizon and flatness problems. Physical Review D, 23(2), 347–356.

[Hawking and Ellis (1973)] Hawking, S. W., and Ellis, G. F. R. (1973). *The Large Scale Structure of Space-time.* Cambridge University Press, Cambridge.

[Jordan (2005)] Jordan, T. F. (2005). Cosmology calculations almost without general relativity. American Journal of Physics, 73(7), 653–662

[Joyce et al. (2015)] Joyce, A., Jain, B., Khoury, J., and Trodden, M. (2015). Beyond the cosmological standard model. Physics Reports, 568, 1–98.

[Kosso (2000)] Kosso, P. (2000). The empirical status of symmetries in physics. British Journal for the Philosophy of Science 51(1):81-98.

[Krasiński (1997)] Krasiński, A. (1997). *Inhomogeneous Cosmological Models.* Cambridge: Cambridge University Press.

[Lazarides (2006)] Lazarides, G. (2006). Basics of inflationary cosmology. Journal of Physics: Conference Series, 53, 528–550. doi:10.1088/1742-6596/53/1/033

[Luc (2021)] Luc, J. (2021). Context-dependence of statements about quantities and symmetries, unpublished manuscript.

[McCrea and Milne (1934)] McCrea, W. H., and Milne, E. A. (1934). Newtonian universes and the curvature of space. The Quarterly Journal of Mathematics, os-5(1), 73–80.

[Milne (1934)] Milne, E. A. (1934). A Newtonian expanding universe. The Quarterly Journal of Mathematics, os-5(1), 64–72.

[Møller-Nielsen (2017)] Møller-Nielsen, T. (2017). Invariance, Interpretation, and Motivation. *Philosophy of Science* 84(5), pp. 1253-1264.

[Morin (2008)] Morin, D. (2008). *Introduction to Classical Mechanics with Problems and Solutions*. Cambridge: Cambridge University Press.

[Norton (2008)] Norton, J. D. (2008). The Dome: An Unexpectedly Simple Failure of Determinism. *Philosophy of Science* 75(5):786-798.

[Pooley (2013)] Pooley, O. (2013). Substantivalist and Relationalist Approaches to Spacetime. In: Batterman, R. (ed.), *The Oxford Handbook of Philosophy of Physics*, Oxford: Oxford University Press.

[Pooley (2017)] Pooley, O. (2017). Background Independence, Diffeomorphism Invariance and the Meaning of Coordinates. In: Lehmkuhl, D., Schiemann, G. and Scholz, E. (Eds.), Towards a Theory of Spacetime Theories, Birkhäuser, pp. 105–143.

[Psillos (1999)] Psillos, S. (1999). *Scientific Realism: How Science Tracks Truth*. London and New York: Routledge.

[Rajantie (2012)] Rajantie, A. (2012). Magnetic monopoles in field theory and cosmology. Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences, 370(1981):5705–5717.

[Read and Møller-Nielsen (2020)] Read, J. and Møller-Nielsen, T. (2020). Motivating dualities. *Synthese* 197:236-291.

[Rindler (1982)] Rindler, W. (1982). *Introduction to Special Relativity*. Oxford: Clarendon University Press.

[Roberts (2020)] Roberts, B. (2020). Regarding 'Leibniz Equivalence'. *Foundations of Physics*, doi: 10.1007/s10701-020-00325-9.

[Sciama (1971)] Sciama, D. W. (1971). *Modern Cosmology*. Cambridge: Cambridge University Press.

[Sundermeyer (2014)] Sundermeyer, K. (2014). *Symmetries in Fundamental Physics*. Springer.

[Teh (2016)] Teh, N. J. (2016). Galileo's Gauge—Understanding the Empirical Significance of Gauge Symmetry. Philosophy of Science 83(1):93–118

[Wald (1984)] Wald, R. M. (1984). *General Relativity*. Chicago: The University of Chicago Press.

[Wallace (2019)] Wallace, D. (2019). Observability, redundancy and modality for dynamical symmetry transformations. http://philsci-archive.pitt.edu/id/eprint/18813

[Weatherall (2018)] Weatherall, J.O. (2018). Regarding the 'hole argument'. *The British Journal for the Philosophy of Science* 69(2), pp. 329-350.

[Weinberg (1972)] Weinberg, S. (1972). *Gravitation and Cosmology: Principles and Applications of the General Theory of Relativity*. New York: Wiley.

[Williamson (2013)] Williamson, T. (2013). *Modal Logic as Metaphysics*. Oxford: Oxford University Press.

[Wulfman and Wybourne (1976)] Wulfman, C.E. and Wybourne, B. G. (1976). The Lie group of Newton's and Lagrange's equations for the harmonic oscillator. *Journal of Physics A: Mathematical and General* 9(4):507-518.