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Direct Empirical Status of Theoretical Symmetries in Physics

Thèse réalisée par
Valeriya CHASOVA

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Membres du jury :

Alexandre GUAY (UCLouvain), promoteur et secrétaire du jury
Michel GHINS (UCLouvain), lecteur
Guido BACCIAGALUPPI (Utrecht University, Pays-Bas), lecteur extérieur
Simon FRIEDERICH (University of Groningen, Pays-Bas), lecteur extérieur
Jean-Michel COUNET (UCLouvain), président

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Place Blaise Pascal, 1 bte L3.03.11, 1348 Louvain-la-Neuve, Belgique www.uclouvain.be/fial

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Summary

The work provides a novel ontological analysis of direct empirical status (DES), i.e. of the relationship between theoretical symmetries from physics and empirical symmetries, where the latter are symmetries in the world similar to Galileo's ship thought experiment.

Part I explains why study the ontology of theoretical symmetries in physics in general, provides a survey of some of the usual ways of studying that topic and presents DES briefly in that context. Part II recounts and analyses in detail the four main texts on DES, namely [Kosso 2000], [Brading and Brown 2004], [Healey 2009] and [Greaves and Wallace 2014], and provides some details of my own account of DES. Part III presents my account in more generality and more fully, explaining in particular how to formalise the notion of empirical symmetry, why use the empirical approach rather than the theoretical approach when establishing DES, what theoretical symmetries with the observational DES look like in the empirical approach, whether the global/local distinction matters for DES and how gauge symmetries in the sense of observationally complete theoretical symmetries are linked with DES.

What follows from my account is that empirical symmetries mostly have relational nature and need references to be established; that there is an infinity of properly global, properly local and mixed theoretical symmetries which have the observational DES with respect to a given empirical symmetry; and that which theoretical symmetries have a stronger ontological DES may depend on whether gauge symmetries in the sense above have an independent ontological significance, i.e. match with unobservable transformations and differences in the world.

Part I. Ontology of theoretical symmetries in physics

This work is devoted to the ontology of theoretical symmetries in physics as it arises from the analysis of their direct empirical status (DES). The aim of Part I is to provide a motivation, background and framework for this analysis.

Chapter I. 1. Ontology and science

This chapter explains what the ontology is and why it matters, how it can be studied with the help of science and why concentrate on the ontology of theoretical symmetries in physics more precisely.

Section I. 1. 1. Ontology and epistemology

Ontology in a non-deflationary sense is a study of the question of what exists in our world, as well as the results of such a study, i.e. a list of *entities* (which can be objects, properties, relations and so on) which exist in our world and a description of the way in which they exist. Elaborating the ontology is surely a task of prime importance for humans because it has direct implications in the form of actions we can perform and in the form of events which can happen to us, both being determined by what there is in the world.

To be sure, there have been proposals in the history of philosophy according to which a substantial ontology is unattainable, e.g. because we cannot know things by themselves but only through their relations to us (as Immanuel Kant thought) or because we can only know regularities but not 'laws of nature' in any substantial sense (as David Lewis thought). However, these are just some answers as to what is knowable about the ontology (and as to what there is) and as such they do not undermine the quest for the ontology but on the contrary presuppose it. It has also been suggested that the ontology in a non-deflationary sense is unnecessary for practical purposes, e.g. because what matters is our engagement in the existence of things rather than that existence itself (V.W.O. Quine thought along these lines). However, arguably such a position is untenable if not only our decisions to act but also things happening to us are taken into account.

To find out what there is in the world we can resort to *the potential means of access to the world* such as observation, introspection and inference. To be genuine means of access to the world the potential means of access should be *veridical* in the sense of conveying us information about the world reliably. Situations where their veracity is contested are called *sceptical scenarios* in epistemology (philosophy of knowledge). If such a scenario obtained, the non-veridical potential means of access would systematically make us think that the world is what it really is not, and so we could not have any knowledge of the world (and its ontology) on their basis. This would be too gloomy a perspective which moreover clashes with the common sense view according to which we are systematically able to get some knowledge of the world via what is mentioned above as the potential means of access to the world. Probably by these reasons those who describe sceptical scenarios usually find how to dispel them too. For instance, René Descartes in *Meditations on First Philosophy* suggests that various potential means of access to the world may become fallacious due to the action of an evil demon. However, he then shows how to derive the veracity of these potential means of access from the existence of God and the existence of God from the veracity (unattainable to the action of the evil demon) of the introspective inference "I think, therefore I exist". Similarly, Hilary Putnam [1981] considers the supposition that we are *brains in a vat* manipulated by an evil scientist so that to make us think of being humans exploring the world. However, he argues next that this supposition is self-refuting given that our words have the ability to refer. (Putnam's aim is the analysis of meaning, and his argument relies on an externalist approach to meaning; to extend his argument to the analysis of knowledge, one may use of an externalist approach to knowledge.)

Thus there are answers to sceptical scenarios which refute putative dangers for the veracity of different potential means of access at once, and so make possible the study of the ontology via all these means. However, such answers do not guarantee that a given potential means of access is always veridical or that all the potential means of access are equally reliable.

Observation is a potential means of access to the world which primarily consists in the use of our vision. However, it is usually understood more broadly as the use of any of our senses. It is further debated whether observation should also comprise different kinds of assisted use of senses (e.g.seeing through glasses or through a microscope). In any case, at least the unassisted use of vision provides a clear case of observation.

Among the potential means of access mentioned above observation is of particular importance for the study of entities external to oneself. Indeed, introspection suits mostly for studying oneself instead. Meanwhile, inference is secondary in that inferences rely on premisses and these need not be inferences themselves. So what inferences serve for depends on the content of their premisses, and likewise whether inferences are veridical depends on whether a means of getting their premisses is (valid inferences being truth-preserving, a conclusion of an inference is true if its premisses are). Actually, it is observation that can confer to inference both its relevance to the external world and its veracity. For statements describing observations can serve as premisses to inferences. And besides, one can argue that observation is systematically veridical because observations can be shared by different persons, seem independent from ourselves and can have an impact on us. If so, we should take it that in most cases our observations do inform us reliably about what there is in the world. Thus observation can be considered of a fundamental importance when learning about the ontology of the world external to ourselves.

However, observation by itself only informs us of that part of the external world which is accessible to it. There are different ways of dividing entities according to how remote they are from actual observation. For instance, we can divide them into those being observed, those already observed, those that will be observed, those that could be observed but neither are, nor were, nor will be observed, and those that could not ever be observed. It is usual however to frame this spectrum in terms of a simpler dichotomy between *observable* and *unobservable* entities, with the border separating entities that could not ever be observed from the remaining kinds. This dichotomy captures at least the difference between the kinds of entities on the opposite sides of the spectrum. This is the difference between entities whose existence can be established primarily through veridical observation and entities whose existence cannot be established without a primary reliance on means other than veridical observation. There can be entities of the latter kind, because that we cannot access some entities by observation does not imply that they cannot exist. Therefore, for a proper study of the ontology one needs to determine how to go beyond observation so that to learn whether unobservable entities exist and if yes what they are more precisely. At the same time, one also needs a way of generating more diverse observations which would extend our knowledge of observable entities beyond what we could learn by performing observations in everyday life alone.

Section I. 1. 2. Ontology of science

Ontology of science is a branch of ontology that studies how science relates to (and informs us about) the world. This domain devotes attention to science rather than say to pseudoscience or art, and even within science it usually concentrates on natural and exact sciences rather than humanities. For sciences such as physics and biology are more tied to our world than fiction, more rigorous than astrology, more general than history, and thus more likely to extend our knowledge of the world in reliable and substantial ways. Moreover, it is usual to distinguish within a scientific theory between *observational consequences*, i.e. predictions about what can be observed, and *theoretical underpinnings*, i.e. a theoretical basis from which observational consequences are deduced and which does not itself constitute an observational consequence. So a scientific theory has a potential to advance our knowledge both of observable entities, via its observational consequences, and of unobservable entities, via its theoretical underpinnings, and so is able to address both issues we encountered at the end of the previous section. This makes ontology of science a domain worthy of study.

One general question to answer within this study is what kind of link between a scientific theory and the world would be able to inform us about the ontology. There are different views on this question, which are in part conditioned by different views on the question of what a scientific theory is. The most popular approaches to the latter question in the XX (and the beginning of the XXI) century are the syntactic approach and the semantic approach. According to *the syntactic approach*, a scientific theory is a collection of statements, i.e. has a linguistic nature. One possibility for conceiving its ontologically relevant link to the world is then via the notion of *truth*, which can itself be understood in different ways (including a popular non-deflationary understanding of being true as being in a suitable correspondence relationship with the world). According to *the semantic approach*, a scientific theory is rather a collection of *models*, where these again can be understood in various ways including non-linguistic ones (the common point being that models are deemed to be more concrete than theoretical axioms and equations while still being more abstract than phenomena). In this case the link to the world can be thought of as a realisation or an instantiation of the theory by the world or as a representation of the world by the theory, depending on whether one considers this link in the direction from the theory to the world or vice versa. Until we will be able to choose among these and possibly other options, I will just call a *matching* the link between a theory and the world of relevance to the study of the ontology, so that not to presume what this link should amount to. I will also say that a theory or its parts are *ontologically significant* if the relevant matching between them and the world happens to obtain.

Next, one may study which observational consequences of a scientific theory are ontologically significant. There are plenty of cases where the ontological significance of observational consequences is recognised as having received a definite evaluation, i.e. as being either confirmed or disproved. This is because in many cases it has been possible to identify observational consequences of a theory, arrange empirically the conditions under which according to the theory a prediction making part of its observational consequences is expected to be realised, and conclude as to whether our observations ensuing from that empirical arrangement agree with the prediction in question. The positive outcome of such tests serves as the main basis for accepting a scientific theory, and thus encourages its further applications which may lead in turn to technological progress. In addition, however, these tests greatly extend our knowledge of observable entities by providing us with many new contexts of observation which we had not thought of arranging if we were not driven by science.

On the other hand, in many cases establishing the ontological significance on the observable level is far from being straightforward. One reason is that it may be hard to generate observational consequences within a theory. For instance, currently there are several approaches to quantum gravity which have not been elaborated enough for getting predictions. Another reason is that there may be practical difficulties with testing some predictions, for instance if this requires a complicated, expensive and highly sensitive equipment. Thus sometimes it may even take a century to test a prediction, as happened with gravitational waves predicted by general relativity. Besides, it may be unclear whether some part of a theory has observational consequences. For example, it seems that electromagnetic potential values had been thought not to have any observational consequences beyond those of electromagnetic field strength values until it was realised that the former values can be used on their own to predict the Aharonov-Bohm effect. One part of ontology of science consists in analysing this kind of issues and their ontological implications.

Another part studies whether a theory's theoretical underpinnings can inform us about unobservable entities. The main options are the following: theoretical underpinnings do not match with unobservable entities in the world; we cannot know whether they match with unobservable entities in the world; they do match with unobservable entities in the word. These options lead respectively to *scientific anti-realism*, *scientific agnosticism* and *scientific realism*. The first two positions often get combined with *scientific empiricism*, which puts a specific accent on the matching between observable entities and observational consequences. For instance, logical empiricists (who introduced the above-mentioned syntactic approach to scientific theories) both highlighted the importance of the latter matching for making scientific theories meaningful and denied as scientific anti-realists that there is a meaningful separate matching involving theoretical underpinnings and unobservable entities. They thus only matched theoretical underpinnings with observable entities via observational consequences. Scientific realists, by contrast, usually defend as much the matching between observable entities and observational consequences as the matching between unobservable entities and theoretical underpinnings. So, by contrast to scientific anti-realists and logical empiricists, scientific realists can be said to assign to theoretical underpinnings what I will call *an independent ontological significance*, i.e. an ontological significance that does not reduce to the ontological significance of observational consequences. Main difficulties for establishing scientific realism lie in proving that a theory is a reliable source of information concerning the ontology on the unobservable level and in extracting the ontology from a theory. The primary tool of the debate about whether and to what extent scientific realism holds are not observations but arguments (made on the basis of observations, in particular), i.e. inferences whose conclusions serve to advance the discussion.

Section I. 1. 3. The NMA, the PMI and the 'partwise' approach to the ontology

The most powerful argument in favour of scientific realism has been Putnam's [1975] *the No Miracle Argument* ('the NMA'). The premisses of the NMA are that the empirical success of the most empirically successful theories is best explained by these theories' being true in a substantial sense (unless one believes in a miracle), and that in our context the best explanation is the correct explanation. The NMA concludes from these premisses that theories which are the most empirically successful ones are true in a substantial sense. This suits well for the above-mentioned option of understanding the matching in terms of truth, but can also be reformulated to encompass other understandings of the matching.

Note that *the empirical success* of a theory is usually understood in terms of predictions, explanations and technological achievements, and that taking a theory's predictions to be successful means ascribing the ontological significance to its observational consequences. So in the NMA ontological significance of observational consequences (established itself with the help of the observation) is part of the basis from which the ontological significance of theoretical underpinnings is inferred.

Anti-realists also have strong arguments on their side. The most powerful argument against scientific realism has been Larry Laudan's [1981] *the Pessimistic Meta-Induction argument* ('the PMI'). The premisses of the PMI are that so far some highly empirically successful theories have been abandoned, and that what will happen with current or future theories is analogous to what has happened with past theories so far. From that the PMI concludes that current and future highly empirically successful theories will also be abandoned, where the relevant sense of being abandoned is taken to undermine the conclusion of the NMA.

Although the NMA and the PMI are major arguments in the realist-anti-realist debate, neither of them is conclusive. The NMA, for instance, has the form of an inference to the best explanation, or abduction. And abduction is a fallible form of inference, i.e. it does not guarantee the truth of the conclusion. Also, one may have doubts as to whether this kind of inference is valid in a metascientific context as ours as opposed to an observational context. As to the PMI, it has the form of an induction, and induction is also a fallible form of inference (as famously pointed out by David Hume). So it is possible that an ultimate empirically successful theory will be constructed which we will no longer abandon (such a (putative) ultimate theory plays an important role in the Humean conception of laws defended by David Lewis).

Still, we cannot deny that empirically successful theories happen to be abandoned, and so the PMI is right in that this fact has to be taken into account in one's approach to the ontology. One option for someone accepting the PMI (or at least the possibility of its application) is then to make a *conditional ontological analysis*, namely to study what would exist in the world if a given theory was true. But this is unsatisfactory, because this only allows us to ask and find out what the world could look like, not what it really is. Furthermore, the conditional analysis does not do justice to the NMA, while this argument does seem worthy of being addressed. On the other hand, accepting both the PMI and the NMA also leads to an unsatisfactory position when coupled with a *holistic approach to the ontology* which takes the ontological significance to be an attribute of whole theories. (This approach is particularly natural when one holds a *holistic view of scientific theories* advocated by Quine, among others, i.e. the idea that some properties, such as the fact of being matched with observations, concern whole theories rather than their parts.) For in that case one is led to attributing both the ontological significance (given the NMA) and its absence (given the PMI) to the same theory, thus getting a logical contradiction. Besides, the holistic approach to the ontology is actually inadequate in that usually in any application of a theory leading to a successful prediction it is not the whole theory that is used (e.g. not any initial conditions are relevant to producing a specific prediction), and so if one assigns the ontological significance on the basis of successful predictions, assigning it to a whole theory seems excessive.

John Worrall [1989] suggests another solution, namely to accept both the PMI and the NMA while taking them to apply to different parts of theories. Thus he proposes to distinguish between some parts that get abandoned and other parts that ensure empirical success and so have counterparts in the world. I see several reasons why this solution is better. In comparison with the conditional approach it is much more satisfactory in that it addresses the question of what the world really is, which is the main question of the ontology understood in a non-deflationary sense. Moreover, it attempts to answer this question in a way that leads to scientific realism, while the conditional approach only leads to a hypothetic ontology. Next, such a *'partwise' approach to the ontology* copes better with the problem of reconciling the PMI with the NMA than the holistic approach to the ontology. Indeed, in the 'partwise' approach no logical contradiction arises because the ontological significance and its absence are predicated of different parts of a theory rather than of the same theory. Besides, the 'partwise' approach is more adequate than the holistic approach in that it only assigns ontological significance to those parts of a theory that lead to successful predictions rather than to all the theoretical elements there are in a theory. In sum, reserving the ontological significance to certain parts of theories leads to a satisfactory version of scientific realism.

Worrall made some more suggestions in the same article [1989]. In particular, he claimed that the ontologically significant parts are theoretical structures (which is why his approach is called *structural realism*). This suggestion has revealed to be problematic in many respects, although some solutions have later been proposed (see in particular [Ladyman 1998], [French and Ladyman 2003] and the books [Ladyman and Ross 2007] and [French 2014] for a defence of this approach, as well as [Ainsworth 2010] and [Morganti 2011] for some critique). Besides, Worrall's interest, given the PMI, was more precisely in the question of which theoretical elements are retained through the theory change, i.e. when one theory gets abandoned in favour of another (in the same way in which e.g. Newtonian gravity is abandoned in favour of general relativity). Also, he was attributing the ontological significance on the basis of the NMA. However, the first of Worrall's proposals, namely that the ontological significance should be attributed to parts of theories rather than to whole theories, makes sense independently of whether the relevant parts come out to be structures or not, whether it is implemented within or outside the context of the theory change, and whether the attribution of the ontological significance is based on the NMA or not. (To see this just replace in my defence of Worrall's first proposal above the PMI with any reason to think that theoretical underpinnings may be abandoned and the NMA with any reason to think that theoretical underpinnings may carry ontological significance.) In this work I will thus be concentrating on that more general way of implementing the 'partwise' approach to the ontology.

Once a promising approach to studying the ontology is determined, the next question is which parts of which theories one should concentrate on. It will not be exaggerated to affirm that the question of ontology is most pressing in physics. Physics is a highly successful branch of science in terms of predictions, explanations and technological achievements, so this is a good candidate for a theory that gets something right about what there is at the unobservable level of the world and about how it works. At the same time, from all the branches of natural science it is physics that speaks of things most remote from our observations in terms of scales and spatiotemporal distances, and it is physics that is so much imbued with mathematics that surely its ontological significance cannot be evaluated without a thorough analysis. Therefore I will be concentrating on studying the *ontology of physics*, i.e. on determining which parts of physical theories match with counterparts in the world.

Next, there are many parts in a physical theory, such as (interpreted) variables, equations, spaces, operators and so on. My focus will be on a specific element of a physical theory, namely a theoretical symmetry. Theoretical symmetries are present in virtually any physical theory, including theories as different as classical mechanics and quantum mechanics, special relativity and general relativity, classical electromagnetism and quantum electrodynamics. Hence it is possible that by studying theoretical symmetries we will find out something universal to all these theories as well as perhaps to other theories including those yet to appear. Besides, theoretical symmetries involve other elements of which a physical theory is composed, such as equations or models that stay invariant under the action of a symmetry transformation. Thus we can also throw the light on the ontology of other parts of a given physical theory by studying the ontology of its theoretical symmetries. Moreover, there have been many suggestions, to be reviewed in the next chapter, as to why theoretical symmetries in physics have to be linked with the ontology. Therefore, studying theoretical symmetries should be particularly helpful for understanding the ontology of physics.

Chapter I. 2. Different contexts of study of the ontology of theoretical symmetries in physics

This chapter introduces some vocabulary and explains with its help how the ontology of theoretical symmetries in physics is usually addressed.

Section I. 2. 1. Constituents of a symmetry

In Ancient Greek a symmetry (συμμετρία) is literally an agreement of measures or commensurability (for different uses of this term in Ancient Greek see [Liddell and Scott 1940]). In various modern languages this term has two main meanings. In common speech it designates the fact for something in the world, such as a human face, to have similar parts. Meanwhile, in a technical sense it designates more abstract invariances in sciences such as physics and mathematics. I will say that something is *physical* if it belongs to the world and *theoretical* if it belongs to science, specifically physics. Common speech symmetries are thus *physical symmetries*, while scientific symmetries are *theoretical symmetries*. A theoretical symmetry is often defined as an invariance under a transformation (in a theoretical context). Transposed to a physical context, this definition also recovers well the extension of the common sense notion of symmetry. For instance, a face then comes out as symmetric because it is invariant under reflection along a vertical axis passing through its centre. Because of this it is admissible to take a *symmetry* to be an invariance under a transformation as much in theoretical as in physical contexts.

If symmetry is an invariance under a transformation, a symmetry must be constituted at least by a transformation, an item to be transformed and a resulting item such that it is to some extent identical to the original item. I will call a component of an item *a feature*, a transformation that preserves some feature *a symmetry transformation* with respect to that feature and two items linked by a transformation *states*. (In another more familiar meaning the term 'state' designates a collection of features of some item within some instant or period of time. In most contexts I will be discussing this usual meaning and the meaning just introduced will coincide in extension.) I will further call a state on which the transformation acts *the initial state* and a state resulting from the application of the transformation *the final state* with respect to that transformation. Thus *constituents of a symmetry* are a transformation and two to some extent identical states linked by it, or more precisely a transformation, an identical part of the states and possibly differing parts of the states.

Which features should be counted as belonging to the identical part and which ones as belonging to the differing parts of the states? Naturally, a feature should go to the former category if it is preserved under the action of a transformation and to the latter category if it is changed under the action of a transformation. However, only those features should be concerned that we are interested in preserving and can account for.

To determine which features get preserved and which change under the action of a transformation we should decide in particular what to do with the approximately preserved features. The latter features are deemed to be a hallmark of inexact symmetries as opposed to exact ones. Indeed, *an exact symmetry* is usually defined as a symmetry where certain features are exactly preserved, while *an inexact symmetry* is usually defined as a symmetry where these features are only approximately preserved. As it may be unclear whether approximately preserved features should count as identical or as differing ones, it may be unclear whether they should count as belonging to the identical part of the states or to the differing parts of the states. However, an approximately preserved feature on a coarse-grained account of what counts as a feature can often be divided into an exactly preserved feature and a non-preserved feature on a fine-grained account of what counts as a feature. In such cases *an exact symmetry* can be defined as a symmetry where certain (fine-grained) features are all exactly preserved, while *an inexact symmetry* is a symmetry where some of these (fine-grained) features are exactly preserved while others are not preserved at all. A consequence is then that former approximate features of inexact symmetries now provide some clear contribution to the identical part of the states and another clear contribution to the differing parts of the states. In other words, while *an exact symmetry* remains a symmetry with no differing parts in the states, *an inexact symmetry* now becomes a symmetry with differing parts in the states.

To be sure, approximately preserved features are not the only reason to have differing parts in the states. Another reason for this can simply be that we wish and are able to take into account what happens with a number of features under the action of a certain transformation and it happens that not all of these features get preserved by that transformation. Meanwhile, if it happens that none of these features get preserved by that transformation, this can be a reason for not recognising that as a symmetry at all, and this even while other features may happen to be preserved at the same time. Thus to determine whether we have a symmetry and whether its states are completely or only partially identical we should check not only how many features are preserved on a fine-grained account of what counts as a feature but also which of these features we are interested in as well as which among the features of interest we are able to take into account.

For example, consider, whether in a physical or in a theoretical context, a rotation of a square around its centre by other than a multiple of 90 degrees, with the environment of this square comprising a suitable reference object, e.g. a larger square. Such a rotation does not preserve the relations between the smaller square and its environment (e.g. distances and angles between the edges of the smaller square and the edges of the larger square) but does preserve the relations among parts of the smaller square (e.g. distances and angles between the edges of the smaller square). So it will not be a symmetry if we are only interested in preserving the first kind of relations, can be a symmetry with identical states if we are only interested in preserving the second kind of relations, and will be a symmetry with only partially identical states if we are interested in preserving both kinds of relations on a fine-grained account of what counts as a feature.

On the other hand, consider next not the rotation of a square by itself but a description of it. Obviously, whether this description will depict that rotation as a symmetry or not as well as whether the states will be depicted as completely identical or not will depend not only on which kinds of relations we are describing but also on which of them we are able to describe. This in turn may influence our understanding of whether the object of our description itself is a symmetry or not as well as whether its states are completely identical or not. For instance, in the example just given the usual practice is to concentrate on the first option and thus to take the rotation in question not to be a symmetry of a square. Probably this is because it is usual to resort to group theory to describe symmetries and this theory only captures well the first kind of relations mentioned above. However, with groupoid theory one can capture the second kind of relations too, so that it also becomes possible to describe our example as a symmetry with completely or partially identical states. Speaking more generally, groupoid theory thus enables one to capture more symmetries or to describe recognised symmetries more fully (on the advantages of using the latter theory for characterising symmetries see [Guay and Hepburn 2009]). In fact a groupoid is just a generalisation of a group (although a groupoid can itself be further generalised to a category). Indeed, a *groupoid G of transformations* in the sense of the groupoid theory in mathematics is a collection of transformations a, b, c, $\ldots \in G$ on a collection of objects, together with a partial composition operation * acting on these transformations, such that the collection of transformations is closed under the composition operation ($\forall\ a, b \in G$, $a*b \in G$), respects the associativity ($\forall\ a, b, c \in G$, $(a*b)*c = a*(b*c)$), has a left neutral transformation e_l and a right neutral transformation e_r ($\exists\ e_l, e_r$: $\forall a \in G$, $e_l*a = a = a*e_r$) and has an inverse transformation a^{-1} for each transformation of the collection ($\forall\ a \in G$, $\exists\ a^{-1}$: $a*a^{-1} = e_l$, $a^{-1}*a = e_r$). We get a *group of transformations* if there is just one object instead of a collection and provided $e_l = e_r$.

Another example where the choice of a formal means influences the classification of symmetries concerns the usual extensional mathematics (like the group theory and the groupoid theory) as opposed to the intensional mathematics (like the homotopy type theory). The intensional mathematics has means for tracking the history of states which the extensional mathematics lacks. So while the intensional mathematics may count as distinct the states linked by a non-trivial transformation (e.g. a clockwise rotation of a square by 90 or 360 degrees around its centre), except perhaps the cases where a non-trivial transformation can be decomposed into a transformation and its inverse (e.g. a clockwise rotation by 90 degrees followed by an anti-clockwise rotation by 90 degrees around the same point), the extensional mathematics may count such states as identical provided they preserve all the extensional features (as is the case e.g. for any rotations of a square by multiples of 90 degrees around its centre). Therefore, there are more possibilities for having symmetries with non-trivial differing parts in the intensional mathematics as compared to the extensional one.

If one wishes to study symmetries, it is natural to be interested in those features which get preserved by transformations. Why, however, should one also be willing to pay attention to features which change under the action of symmetry transformations and consequently to include them into the states of a symmetry as well? As we will now see, the main reason within our study of the ontology of theoretical symmetries is that because of such changes being concomitant to invariances under transformations in theoretical symmetries the ontological significance of these changes and so of symmetry transformations and so of theoretical symmetries themselves is unclear. Therefore, one cannot determine what the ontological significance of theoretical symmetries is without considering the invariances, the transformations and also the changes induced by them as constituents of a theoretical symmetry.

Section I. 2. 2. Spacetime symmetries

Subsection I. 2. 2. 1. Leibniz versus Newton and Clarke

Perhaps the most traditional problem about the ontology of theoretical symmetries goes back to the correspondence (first published in [Clarke 1717]) between Gottfried Wilhelm Leibniz (also spelled as Leibnitz) and Samuel Clarke, who represented the position of Isaac Newton. Leibniz's question is whether there can be different options as to what are the positions in space and time of all the things in the universe. Clarke and Newton admit this, because they postulate the absolute space and time in the world and envisage different possibilities as to where in these space and time a given thing may be located. However, Leibniz objects that on the one hand there is no evidence of things being in one location rather than another within the absolute space and time, because such putative options would be observationally indistinguishable (the absolute space and time being unobservable); and on the other hand, if there were such options, there would not be any sufficient reason for God to prefer one of them over the others, i.e. to place a thing in one location within the absolute space and time rather than in another. Therefore, Leibniz argues, there should not be any choice in locations to begin with, nor consequently any absolute space and time whose existence would sustain such a choice. What remains of space and time then are spatial and temporal relationships between all the things in the universe. So this is what for Leibniz space and time should be reduced to.

Leibniz and Clarke's debate is usually taken to concern whether space and time are absolute or relative. However, this debate also relates to the ontology of theoretical symmetries in the following way.

Firstly, there is a theoretical level in their discussion. The role of this theoretical level is played by different suppositions about where in the absolute space and time all the things are located.

Secondly, these suppositions can be organised into theoretical symmetries. Indeed, a way of obtaining a different position of a thing with respect to the absolute space and time would be to perform a spacetime transformation starting from the thing's actual position. Leibniz's examples are in particular spatial reflection and time translation of all the things in the universe. He considers those transformations which would preserve all the spatial and temporal relations between all the things in the universe and hence all the observable features. Such putative transformations are thus theoretical transformations which exactly preserve all the observational consequences, and so these theoretical transformation together with the preserved observational consequences constitute a theoretical symmetry. However, the theoretical symmetry here cannot be limited to the transformation together with what gets preserved. For the crucial idea in the debate is to analyse the putative unobservable changes which can be brought by the symmetry transformation in light of the invariance which can also be brought by it. Hence the theoretical symmetry here should be constituted more precisely by a spatiotemporal transformation, which acts on the hypothetical location of all the things in the universe, invariant observational consequences, which consist in predictions of observable features, and differing theoretical underpinnings, which consist in the theoretical differences in location before and after the transformation.

Thirdly, although Leibniz and Clarke's debate is primarily about what there is in the world, this debate also has consequences for whether and how theoretical symmetries just discussed match with the world. To be more precise this let us specify the consequences for our topic of each stage of the debate described above. I began by saying that Leibniz's question was whether there can be different options as to what are the positions in space and time of all the things in the universe. This has consequences for our topic because differing theoretical underpinnings of different theoretical states of a theoretical symmetry can only be matched with such options if there are such options to begin with. Next, I was saying that Clarke and Newton claimed that there are the absolute space and time in the world, the fact of the matter as to where in these absolute space and time all the things are located, as well as possibilities for all these things to be located elsewhere in these absolute space and time. Hence their position implies that the differing part of the theoretical underpinnings of one of the theoretical states can be matched with what obtains in the world, the differing part of the theoretical underpinnings of other theoretical states can be matched with what could obtain in the world and theoretical transformations inducing these differences between theoretical underpinnings can be matched with passages either from an actual location in the world to a possible location or from a possible location to another possible location. Let us call this the *Newtonian interpretation* of theoretical symmetries. Finally, I was saying that on Leibniz's view there are no possibilities as to where in the absolute space and time all the things can be located and hence no absolute space and time in the world. A consequence of this is that differing theoretical underpinnings of any theoretical state cannot be matched with anything in the world, and therefore likewise no theoretical transformation inducing these differences can be matched with any passage to an actual or a possible location. Let us call this the *Leibnizian interpretation* of theoretical symmetries. We see thus that the differences between the opinions of Clarke and Newton on the one hand and of Leibniz on the other hand as to what there is in the world lead to different conceptions about whether and how various constituents of a theoretical symmetry can be matched with the world.

If one measures *the strength of an interpretation* of a theoretical symmetry in terms of the quantity of constituents of this theoretical symmetry to which this interpretation attributes an ontological significance, the Newtonian interpretation will certainly be stronger compared to the Leibnizian one. For while both interpretations are compatible with assigning the ontological significance to identical observational consequences of theoretical states of a theoretical symmetry, only the Newtonian interpretation goes as far as assigning the actual ontological significance to another component of a theoretical symmetry in addition. Yet, even the Newtonian interpretation is still very weak in comparison to other possible interpretations. For as far as the remaining components of a theoretical symmetry are concerned the Newtonian interpretation is compatible with reserving a matching with what does obtain in our world to the differing theoretical underpinnings of just one of the theoretical states constituting a theoretical symmetry. Meanwhile, as said the differing theoretical underpinnings of other theoretical states as well as theoretical transformations inducing these differences may be linked instead with what can obtain in our world. And what can obtain in our world is something weaker than what does obtain in our world unless one postulates something like that possibilities are as real as actual situations or that possibilities must be realised in our world. Therefore, unless this kind of presuppositions is shown to be warranted one cannot consider the Newtonian interpretation as bringing a significant advancement in strength with respect to the Leibnizian interpretation.

Subsection I. 2. 2. 2. Earman and Norton versus Maudlin

Yet, even the small amount of strengthening brought about by the Newtonian interpretation is a source of huge problems. At least this is what follows from the famous hole argument formulated by John Earman and John Norton [1987] on the basis of the analysis by John Stachel of the general-relativistic work of Albert Einstein. The scope of the argument is however wider than just general relativity: it also includes for instance special relativity, Newtonian mechanics, Newtonian gravity and classical electromagnetism on a Newtonian or special-relativistic background [ibid., pp. 516-518]. As Leibniz and Clarke's debate, Earman and Norton's argument also concerns both the ontology of space and time and the ontological significance of spatiotemporal theoretical symmetries. However, this argument goes in the direction opposite to the one I have taken when analysing Leibniz and Clarke's debate, namely it proceeds from the denial of a matching between theoretical states of theoretical symmetries and putative locations in space and time to the denial of the existence of these space and time rather than the other way round. So here we have an example of what I have described above, namely a case where the analysis of the ontology of science clarifies what there is in the world.

Theoretical symmetries in Earman and Norton's case are constituted by two models of a theory linked by diffeomorphisms. A model for them consists of a manifold plus a certain collection of geometric objects on it. A diffeomorphism is a certain transformation. Usually it is understood in the passive sense. In this sense it consists in sending coordinates of points of a manifold to other coordinates. The novelty of the hole argument is to concentrate on the active sense instead. In this sense a diffeomorphism consists in sending points of the manifold to other points of the same manifold. By acting on points of a manifold or their coordinates diffeomorphisms induce changes for the collection of geometric objects on that manifold, so that we pass to a new model. However, these changes affect neither equations of motion, which say that certain geometric objects of a model vanish, nor observational consequences of a model. So two models linked by a diffeomorphism constitute a theoretical symmetry whether diffeomorphisms are understood passively or actively.

Earman and Norton also distinguish between a *substantivalist*, i.e. a person who believes that space and time exist over and above spatial and temporal relations between all the things in the universe, and a *relationalist*, i.e. a person who, as Leibniz, reduces space and time to spatial and temporal relations between all the things in the universe. To show how theoretical symmetries just described are related to these positions, Earman and Norton suggest that for a substantivalist a manifold should be a theoretical counterpart of space and time in the world. Therefore, on their view a substantivalist should match two models linked by an active diffeomorphism with two different actual or possible locations in space and time. Meanwhile, a relationalist should be able to match such models with the same situation in the world.

The following problem then arises for a substantivalist. Among diffeomorphically related models there are those which only differ within a bounded area that one may call a hole. This difference can be such that it only concerns the fate of geometric objects after a certain moment in the theoretical time. So there can be geometric objects in these models which are alike up to that moment of time but different to the future of this moment. Now a substantivalist should hold that one of these models rather than another matches with the actual world. But one cannot tell whether one or another model is the right one to be matched because they have all the same observational consequences and there is no other way to choose between them. Hence one cannot predict whether our world will evolve according to one or another of these models. This amounts to there being an epistemic indeterminism about the evolution of our world in time (which one could also try to extend to an ontological indeterminism, we may add). Moreover, this indeterminism is radical because there can be an arbitrary number of such holes. So Earman and Norton conclude that spacetime substantivalism should not be adopted for such a heavy price.

Although, similarly to Leibniz and Clarke's debate, the ultimate goal of Earman and Norton's argument is to clarify which kinds of space and time there are in the world, let us go back to those stages in their argument which concern the constituents of theoretical symmetries and their ontology, i.e. the matching between them and the world, to analyse in more detail the ontological interpretations of theoretical symmetries envisaged. In our terms the components of theoretical symmetries discussed by Earman and Norton are as follows: active diffeomorphisms play the role of symmetry transformations; models play the role of theoretical states; preserved equations of motion, observational consequences and to some extent geometric objects and manifolds play the role of the identical part of theoretical states; and differing connections of geometric objects with manifold points play the role of differing parts of theoretical states. With respect to the identical part of theoretical states a substantivalist's and a relationalist's ontological interpretations may to some extent coincide. For instance, both a substantivalist and a relationalist may match observational consequences of either model with observable features of phenomena if these are in a suitable agreement. On the other hand, even for the identical part of the states they are going to have some differences in interpretations. In particular, they will disagree as to what should be matched with a manifold. Besides, there is going to be a disagreement in the interpretation of the theoretical transformation and of the differences between the theoretical states induced by it. A substantivalist matches these differences with different actual or possible locations in some 'substantial' space and time supposed to exist in the world, and the transformation linking them with a passage between such locations. We recognise here the Newtonian interpretation of theoretical symmetries, except that in Newton's and Clarke's case the 'substantial' character of space and time was made a bit more precise by saying that they are absolute. A relationalist, on the other hand, does not match either the differing parts or the transformation with anything in the world. Unsurprisingly, we recognise the Leibnizian interpretation here. As said, the Newtonian interpretation is somewhat stronger than the Leibnizian one. The hole argument shows that this strengthening brings about unwelcome epistemic consequences.

At least, this follows if the hole argument is correct. Whether this is so is however a matter of debate. In particular, a notable critique of that argument was provided by Tim Maudlin in [1990]. There Maudlin first translates and analyses a passage from Aristotle's *Metaphysics* Z.3 to show that one should neither postulate bare haecceities (primitive essences) nor take all properties of an object to be accidental. Instead, one should admit that some properties (or rather relations, as it will turn out) are essential to some objects. Then Maudlin argues that Earman and Norton's argument commits the error of not respecting this principle when describing the position of a substantivalist. Indeed, on Maudlin's view the principle would be respected if spatiotemporal relations (or spatiotemporal relational properties) were taken to be essential to the identity of physical spacetime points whose existence is postulated by a substantivalist. But Earman and Norton presuppose that spatiotemporal relations are accidental to their identity, because they suppose that it is physically possible for a substantivalist to exchange physical spatiotemporal points and thus to change their spatiotemporal relations without changing the identity of these points. According to Maudlin the latter supposition comes from the fact that Earman and Norton take the physical spacetime of a substantivalist to be represented by a theoretical manifold rather than by a theoretical manifold plus a metric, which is one of the geometric objects on a manifold. For while a manifold accounts for spacetime points, a metric accounts for spacetime relations. And so taking only a manifold to represent the spacetime implies conceiving of spacetime points independently from their spatiotemporal relations. By contrast, Maudlin argues, by responding to a series of objections to his position, that the physical spacetime of a substantivalist should be represented by a theoretical manifold plus a metric. So Maudlin's position respects his version of Aristotle's principle because this position does not conceive of spacetime points in separation from their spatiotemporal relations. Now whether Maudlin's version of Aristotle's principle is respected or not matters for whether changes of spacetime relations that preserve spacetime points count as genuine physical possibilities: for a position of Earman and Norton's substantivalist that does not respect Maudlin's version of Aristotle's principle they do, while for Maudlin's own position that respects his version of Aristotle's principle they do not. But precisely such changes arise under the action of diffeomorphisms, because a manifold is preserved by them while a metric, as a geometric object, is not, and because a manifold is matched with spacetime points while a metric is matched with their spatiotemporal relationships. So Earman and Norton's substantivalist takes diffeomorphisms to link genuine physical possibilities, while Maudlin's substantivalist does not. But it is only when diffeomorphisms are taken to link genuine physical possibilities that the treat of indeterminism arises. Hence it arises for Earman and Norton's substantivalist but not for Maudlin's. In this way Maudlin's defence of substantivalism works. A curious and potentially problematic aspect of this defence is that it comes at a price of attributing to spacetime relationships a crucial role in the identity of spacetime points. In Maudlin's discussion it is a substantivalist that benefits from this move, but it can be turned to a relationalist's advantage. In particular, one may argue that it implies that spacetime points are not fundamental while spatiotemporal relations are, thereby obtaining a version of structural realism. This is an option that P.M. Ainsworth [2010, p. 55] finds sustained by quotes from Einstein and even Newton.

Section I. 2. 3. Similar objects

Subsection I. 2. 3. 1. Permutations of 'indistinguishable' objects

Manifold points can be an example of what can be called *'indistinguishable' objects*, i.e. objects that share the totality of their monadic and *n*-adic properties (or *properties* and *relations* respectively, for short). Other examples of 'indistinguishable' objects arise among classical particles and among quantum particles. Exchanges of objects, particularly of 'indistinguishable' objects, are called *permutations*. These transformations give rise to *permutation symmetries* because they preserve all properties and relations.

The main question usually discussed about 'indistinguishable' objects of a given kind is whether these should be identified, i.e. counted as the same object. This is usually understood as a question about what there is in the world, because it is usually presupposed that such object(s) exist(s) to begin with. But originally 'indistinguishable' objects come from theories and not from our experience, so this presupposition amounts to presupposing a matching between theoretical objects and the world. Correspondingly symmetries constituted by permutations of 'indistinguishable' objects are primarily theoretical symmetries, and this presupposition amounts to attributing them some ontological significance. Determining whether 'indistinguishable' objects of the same kind should be identified clarifies what this ontological significance consists in more precisely. In particular, it tells us whether theoretical states constituting a theoretical permutation symmetry can be matched with distinct physical states as well as whether counterpart physical permutation symmetries exist.

One option for what to do with 'indistinguishable' objects of the same kind is to take them to be identical and to be in the same physical state given that they have all the same properties and relations. An advantage of this option is that it does not postulate anything transcending properties and relations to distinguish between objects. Adopting this option is however unsatisfactory in that too many objects would then have to be identified. In particular, it seriously restricts possibilities by making impossible universes consisting of two or more 'indistinguishable' objects of the same kind (whether manifold points or particles or something else) as well as symmetric universes (where some objects may have different properties and relations but for each object with a given collection of properties and relations there is at least one other object with the same collection of properties and relations).

Another option is to take objects sharing all their properties and relations to be distinct because of them possessing some kind of haecceity or essence. This avoids the problem encountered by the first option, but runs into the problem the first option avoids. Indeed, the second option needs to postulate the existence of haecceities or essences in the actual world, which is not only considered as ontologically heavy but also is not ontologically parsimonious, thereby going against the so-called Ockham's razor by which one should not multiply (i.e. add to one's ontology) entities without necessity.

For some time it seemed that these two options exhausted viable answers to our question (for an example see [French and Redhead 1988] where it is claimed that, because the first option does not suit for quantum particles, the second option should be adopted for them). However, Simon Saunders [2003] suggested a third option based on previous work by Max Black and W.V.O. Quine. Namely, he proposed to take objects sharing all their properties and relations to be distinct if they are linked by a *symmetric irreflexive relation*, i.e. by a relation that they bear to each other but that each of them does not bear to itself.

What is good about this option is that it can cover manifold points as well as classical and quantum particles (for early applications to quantum particles see [Saunders 2006], [Muller and Saunders 2008], [Muller and Seevinck 2009]). This can be done using such symmetric irreflexive relations as the relation of being some spatiotemporal distance from each other and the relation of having a different spin. Besides, the third option does not face the problems mentioned above for the two previous options. It thus allows to get the outcome as in the second option while being limited to entities and kinds of entities admitted by the first option.

A problem with the third option noted by Saunders [2003] is that symmetric irreflexive relations do not allow to refer to one of objects linked by them without referring to another. The same, however, applies to *asymmetric irreflexive relations*, i.e. to relations that none of the objects entertains to itself but that one of the objects entertains towards another without the converse holding (example is the relation of being earlier than). Saunders calls objects *absolutely, relatively or weakly discernible* according to whether they differ by a monadic property, or share all monadic properties but differ by an asymmetric irreflexive relation, or differ by none of the latter but are linked by a symmetric irreflexive relation. Only absolutely discernible but not relatively or weakly discernible objects can be referred to separately from one another. However, at least this does not make the third option worse than the first option, because the first option amounts to the conjunction of the absolute discernibility and of a discernibility due to any difference in relations, and as the latter can be taken to include Saunders' relative discernibility, the first option will encounter the same problem. Yet, there may be other reasons why the third option can be thought of as worse or problematic. For instance, this option only works if one is able to show prior to applying it that certain symmetric irreflexive relations hold without presupposing that the objects which are going to play the role of relata for these relations are distinct (otherwise we get a circularity; on that see e.g. [Dorato and Morganti 2013, p. 596]).

While one may wish to choose among the three options on the basis of the advantages and disadvantages they have, one can also choose among them on the basis of one's conception of *individuality*, i.e. of what makes an object unlike all the others. Indeed, if one takes properties and/or relations as the basis of individuality, one will take objects that have the same properties and relations to be the same object, as in the first option; and if one takes haecceities or essences as the basis of individuality, one will take objects that have the same properties and relations to be distinct if they differ by haecceities or essences, as in the second option; while if one takes symmetric irreflexive relations as the basis of individuality, one will take objects that have the same properties and relations to be distinct if they are linked by such relations, as in the third option.

A concrete example where it is individuality that motivates counting objects as distinct may be discerned in Maudlin's [1990] reply to the hole argument. Indeed, although Maudlin mostly speaks of some properties or relations being essential rather than providing a basis of individuality, he does say that essential spacetime relations are what sustains the identity of spacetime regions [ibid., p. 545], so if asked whether spacetime regions or points are to be identified he could answer that they should not because of being individuated by spacetime relations. How this individuation occurs also is not in the focus of his discussion, but Saunders' relative and weak discernibility could help (indeed one may try to individuate spacetime points by the spatiotemporal relations of being earlier than and of being some distance from each other). Alternatively, the same means can be used by a relationalist about spacetime, or by a structural realist about both spacetime and particles as in [Ladyman 2007] (where relative and weak discernibility are considered as bestowing an individuality and relations are taken to be primary with respect to objects). Meanwhile, a substantivalist about spacetime may instead take spacetime points to be individuals because of them possessing haecceities or essences: indeed this had been a usual option prior to the time of Maudlin's defence. On all these ways of assigning individuality one would be led to counting the relevant objects as distinct instead of identifying them.

In general an inference from individuality to distinctness works because objects that are recognised to be individuals are necessarily counted as distinct. This means that for objects to be counted as distinct it is sufficient that they be recognised as individuals. On the other hand, it is not necessary that objects be recognised as individuals for them to be counted as distinct. So one may well count objects as distinct without recognising them as individuals. This is for instance what Saunders does in his article with F.A. Muller, where only absolutely discernible objects are recognised as individuals [Muller and Saunders 2008, p. 504], and yet the relative and the weak discernibility are used to count other objects as distinct. (Muller and Saunders also postulate [ibid.] that only individuals are distinguishable, although this is discussable. I leave the definition of distinguishability open, so I speak of 'indistinguishable' particles with inverted commas.)

Finally, it is worth noting that Saunders' original proposal [2003] was supposed to spell out Leibniz's *principle of identity of indiscernibles* ('the PII') according to which objects should be identified if they have all the same properties. Likewise in [Muller and Saunders 2008] all the three kinds of discernibility are linked to the PII. However, firstly as said the absolute discernibility for Muller and Saunders is a basis of identification (when it is absent) as well as a basis of individuality (when it is present), while the PII together with the relative and the weak discernibility are only bases of identification (the PII when it obtains, while the relative and the weak discernibility when they are absent). And secondly, while the PII, the absolute and the relative discernibility are only sensitive to the quantitative side, i.e. to whether there are any properties or relations that differ, the weak discernibility is sensitive to the qualitative side of relations linking objects, i.e. to what these relations are. So distinguishing between the three kinds of discernibility can hardly be reduced to spelling out the PII.

Summing up, having asked whether objects sharing all their properties and relations should be counted as the same object, we have got an affirmative answer from the supporters of the first option above, which reduces to the absolute discernibility in the sense of any difference in properties plus to a discernibility due to any difference in relations including Saunders' relative discernibility. Meanwhile, those happy with haecceities or essences or alternatively with the weak discernibility answer on the contrary that the objects in question need not be identified.

A consequence of this for the ontology of theoretical permutation symmetries is that theoretical states assigning all the same properties and relations to an object can be matched with more physical objects on the second and the third option in comparison to the first option, because these options allow more objects to exist than the first option does. This is a rather weak conclusion, and there are several reasons for us not getting something stronger. One is that objects that would fall under a given option need not exist. So, in particular, while the second and the third option allow that there exist more objects than the number allowed by the first option, they do not entail that there actually exist more objects than the number allowed by the first option. Another reason is that even if the second or the third option comes out as true by virtue of there existing several 'indistinguishable' objects of some kind, this may happen to have no incidence on a matching involving a given couple of theoretical states because it may happen that these theoretical states can only be matched with objects of some other kind and that there only exists a single object of that kind in our world. A third reason is that even if there exist several 'indistinguishable' objects of the kind relevant to given theoretical states, this does not entail any obligation for us to match distinct theoretical states with distinct objects but only a possibility for us to do so. A fourth reason is that even if on the contrary the first option comes out as true, there may be (and typically are) physical differences in properties and relations which do not have counterparts in given theoretical states, and so such theoretical states can also be matched with distinct physical objects which will moreover be physically different on the first option.

In addition to objects and theoretical states we need to consider what happens with transformations linking them. If two physical objects with the same properties and relations are actually the same object, a physical permutation linking them will be an identity transformation; if such objects differ by haecceities or essences, a physical permutation linking them will be a transformation that exchanges haecceities or essences, if such transformations are allowed in one's ontology; and if the objects in question are counted as distinct in virtue of being linked by a symmetric irreflexive relation, one may wish to still think of a physical permutation linking them as a genuine transformation, but it is not evident in that case what it would transform more precisely. Correspondingly a theoretical permutation between theoretical states attributing to an object the same properties and relations will be matched with one of these three transformations according to what the theoretical states linked by it are matched with.

Subsection I. 2. 3. 2. Objects with spatiotemporal differences

Having examined whether objects sharing the totality of their properties and relations should be counted as the same object, we may ask next whether objects differing by spatiotemporal properties and/or relations alone should be counted as distinct.

A first thing to say is that which spacetime differences exist depends on one's views about the ontology of spacetime. For a substantivalist more differences will exist because these will include those differences in absolute position in spacetime that preserve spatiotemporal relationships between all the things in the universe. Yet there are some differences on which both a substantivalist and a relationalist agree that they occur while disagreeing on their nature: these are differences in spatiotemporal relationships which for a substantivalist are consequences of differences in absolute position while for a relationalist they are not.

Next, it matters whether differences that one accepts as physical are spatial, temporal or spatial and temporal more precisely. For spatial differences the usual options are to take the original differing objects to be distinct objects or at worst distinct parts of the same system. The second option can be chosen if there is a suitable link between the two original objects, such as a correlation of physical states in the case of quantum objects (e.g. spin up for one object and spin down for the other). For temporal differences the second option usually is not resorted to, because parts of the same system are usually supposed to exist during the same time. The first option however suits again, as does the option of taking the original differing objects to be distinct physical states of the same object. Which of these options are chosen in a particular case depends on what one believes to be the evolution of the physical states of the two objects of interest in the period between the times of existence at which they are considered. If the earlier original object is believed to evolve into the physical state of the later original object and the later original object is believed to evolve from the physical state of the earlier original object within the period of time separating the two objects, then these objects are usually counted as the same object in different physical states as per the last option, and otherwise the first options is chosen. For instance, consider two particles of the same kind, both having the same spatial position but one existing at time t and another at time t+1. Such particles will be counted as two physical states (an earlier one and a later one) of the same particle if the first particle is believed to be at rest for the next unit of time and the second is believed to had been at rest for the previous unit of time. Finally, if differences are both spatial and temporal, the same options are usual as in the case of temporal differences alone. In sum, if there is any difficulty with choosing one option or another, this is primarily because of disagreements as to whether spatially separated objects can be counted as parts of a system and as to how objects evolve rather than because of differences in the kinds of entities one postulates and takes into account, as was the case for 'indistinguishable' objects.

Because of that the ontological significance of theoretical symmetries is rather straightforward to deduce as well. Here the symmetries usually considered are not those constituted by permutations but those constituted by transformations sending one of the original objects to another. In that case whether two original objects are taken to be two distinct objects or two distinct parts of a system or two physical states of the same object, we can match either theoretical state of such a theoretical symmetry with a different one of these two things, if theoretical spatiotemporal differences are taken into account, or we can match both theoretical states with one and the same of these two things, if theoretical spatiotemporal differences are neglected. Either way these are again possibilities rather than obligations. As to the theoretical transformation linking the two theoretical states, it will be matched with an identity transformation if the latter possibility is used, while in the case of the former possibility it will be matched with one of the following transformations: a passage from an object to a distinct one, for the first option of counting the original objects as distinct objects; a passage to a distinct part of a system, for the second option of counting them as distinct parts of a system; and a physical evolution of the same object, for the third option of counting them as distinct physical states of the same object.

A complication arising at the theoretical level is that in many theories including special and general relativity (but not the usual quantum mechanics) simultaneity is frame-dependent, and so what is a theoretical spatial difference in one reference frame becomes a theoretical spatiotemporal difference in another reference frame, and vice versa. The usual strategy is then to extend the option assigned in the case of a spatial difference to those cases with a spatiotemporal difference that can be obtained from the first case by transformations of frames that are taken to be symmetry transformations in the relevant theory.

Subsection I. 2. 3. 3. Objects with non-spatiotemporal differences

Michael Redhead raised the question whether objects differing by some internal properties and/or relations can be counted as identical. Here *internal* means non-spatiotemporal and is opposed to *external* in the sense of spatiotemporal. Redhead's example of objects for which this question is especially worth considering are a proton and a neutron. These differ by mass and electric charge, but share other features, so they can be considered as different physical states of the same particle called nucleon. Redhead calls an identification of objects "*the ontological unification*", so this would be one example of it. Redhead's question however is whether such a unification is legitimate in the case of objects differing by internal properties and/or relations, and his claim is that it is not.

Contrary to the usual discussions on 'indistinguishable' objects, Redhead is not deducing his answer from considerations about the world so that to next derive what consequences this has on the level of theoretical states, transformations and symmetries in terms of their ontological significance. Instead he proceeds the other way round, starting from the ontological significance of theoretical symmetries and deducing from this what obtains in the world. So in this sense those discussing 'indistinguishable' objects proceed like Leibniz and Clarke while Redhead proceeds like Earman and Norton. Thus Redhead provides another example where reflecting on theoretical symmetries may clarify what there is in the world.

One of Redhead's arguments against the idea that internal symmetries lead to the ontological unification is given in his [1988, p. 16], where Redhead suggests that for a theoretical symmetry to lead to the ontological unification the possibility of its passive interpretation is sufficient and may be necessary, while internal symmetries can only be given a meaningful active interpretation. He does not define the active/passive distinction there, but in his [1982, p. 82][1] he says that active transformations change one physical system into another while passive transformations redescribe the same physical system. In terms of matchings this means that *for Redhead a theoretical transformation* is *interpreted actively* when it is matched with a transformation of a physical system that changes the identity of that system, while a theoretical transformation is *interpreted passively* when it is matched with the fact that merely a description of a physical system changes. On these definitions the suggestion that for a theoretical symmetry to lead to the ontological unification the possibility of its passive interpretation is sufficient and may be necessary transforms into the suggestion by which for a theoretical symmetry to lead to the ontological unification it is sufficient

[1] In his later works Redhead refers to this text as published in 1983.

and may be necessary that one be able to interpret its theoretical transformation as a redescription of the same physical thing in different terms. Then the sufficiency claim seems right, because when a theoretical transformation is a mere redescription, the two theoretical states linked by it will be matched with the same physical state of the same physical object. However, the necessity claim seems wrong, because one can also obtain the ontological unification by interpreting the theoretical transformation in different ways intermediary between the active and the passive one. For instance, if the ontological unification also obtains when two different physical states are attributed to the same physical object, as the proton and neutron example [ibid.] suggests, one way to get it is by interpreting the theoretical transformation as a change in the physical state of the same physical object. Now even if Redhead was to amend his suggestion by including this and other intermediary ways of interpretation into his conditions under which the ontological unification may obtain, he would still deny that internal symmetries lead to the ontological unification. For as said in his [1988, p. 16] Redhead also suggests that internal symmetries can be given the active interpretation alone. But it is unclear why the latter suggestion would hold, in particular given that the definitions of being internal and of being actively interpreted seem completely unrelated. And no argument in support of the suggestion in question is provided in [ibid.]. So we do not have any reason at hand for this suggestion to hold, and so contra Redhead we can expect internal symmetries to lead to the ontological unification.

As to [1982, pp. 81-82], Redhead contrasts there two kinds of cases exemplified by the electromagnetic case and the proton and neutron case. The electromagnetic case consists in that theoretical electric and magnetic fields taken separately are not invariant under theoretical Lorentz transformations (i.e. spatial rotations and boosts) when these are interpreted passively. Meanwhile, a theoretical electromagnetic field strength is invariant under these transformations. Redhead's conclusion in then that we have an ontological unification of the electric and magnetic phenomena into the electromagnetic ones. On the other hand, for the proton and neutron case the relevant transformations are passages from a proton to a neutron and vice versa, and Redhead says that by contrast to the electromagnetic case the relevant transformations now are to be interpreted actively rather than passively. So on his view no unification of the proton and neutron into a nucleon obtains.

These reflections of Redhead are somewhat elliptic, but I guess the right reconstruction is this. Firstly, Redhead is not suggesting that the invariance of something theoretical depends on whether a theoretical transformation is interpreted passively or not, but simply wishes to know whether a given theoretical symmetry transformation can be interpreted passively, because he takes the ontological unification to be dependent on the latter condition. Secondly, he deems this condition to be satisfied by theoretical Lorentz transformations, and this results in him unifying the physical counterparts of theoretical electric and magnetic fields that vary under these transformations into a physical counterpart of a theoretical electromagnetic field strength that is invariant under these transformations. Therefore, the ontological unification in the proton and neutron case would have to function in a similar way, obtaining if theoretical passages from theoretical protons to theoretical neutrons and vice versa could be interpreted passively and resulting in us unifying the physical counterparts of a theoretical proton and of a theoretical neutron that are affected by these theoretical passages into a physical counterpart of a theoretical nucleon that presumably is not affected by these theoretical passages. Thirdly, Redhead thinks that his condition of being able to be passively interpreted is not satisfied by the theoretical passages in question, hence he claims that no ontological unification obtains in the proton and neutron case. Thus reformulated Redhead's reflections are consistent with what he says in [1988, p. 16], and fail by the same reasons as those mentioned in the analysis of the latter text above.

What is different in [1982, pp. 81-82] with respect to [1988, p. 16] are Redhead's positive claims concerning theoretical Lorentz transformations. In particular, as said he claims that these have the passive interpretation. Adding to this that theoretical Lorentz transformations are external transformations, we get that at least some external transformations on Redhead's views have the passive interpretation. Meanwhile, as I argued above Redhead is wrong about internal symmetries' not having the passive interpretation. Assuming then that Redhead is right about at least some external symmetries' having the passive interpretation, it follows that symmetries that have the passive interpretation can be as much internal as external. Therefore, we get that the internal/external distinction is irrelevant to whether a symmetry can have the passive interpretation, or at least that the internal or external character of a symmetry cannot by itself preclude it from having the passive interpretation. This is reasonable indeed, for the passive interpretation is just a change in conventions, and we are free to change conventions as we like.

Given what has just been said, it should seem puzzling that we have not seen the passive interpretation of transformations of external symmetries as changes in conventions when discussing them in relation to whether one has to identify physical objects that only have spatiotemporal differences. Instead, we have only seen that theoretical transformations constituting external symmetries can be interpreted as identities, or passages to a distinct physical system or a distinct part of a physical system, or changes of a physical state of the same physical system. This does not mean that something with my considerations just presented is wrong. Rather, this means that in the present context moving from the ontological significance of theoretical symmetries to what exists in the world has allowed us to notice a possibility of interpretation that was not evident when one proceeded the other way round. I will call a procedure where one moves from the theoretical level to the empirical level *the theoretical approach* and a procedure where one moves from the empirical level to the theoretical level *the empirical approach*. It is usual to implicitly employ the empirical approach in the discussion of 'indistinguishable' objects, and I further illustrated that approach by my discussion above of objects having spatiotemporal differences alone. But next we proceeded following Redhead via the theoretical approach instead, and that is where the extra possibility of interpretation came into light.

Comparing all the interpretations encountered in the two approaches, we also see that they overlap to some extent. Indeed, one option coming out from my discussion of objects having spatiotemporal differences alone was to interpret theoretical transformations as passages to a distinct physical system, and on the other hand Redhead was speaking of this same interpretation under the name of the active interpretation. However, while Redhead has only added to this the passive interpretation of transformations as changes in conventions, in my discussion above several other interpretations were considered consisting in interpreting transformations as identities, passages to a distinct part of a physical system or changes of a physical state of the same physical system. Does this mean that the empirical approach is more powerful than the theoretical approach? Hardly so. For the theoretical approach does not constrain us to use Redhead's binary active/passive distinction, and thus we can extend it into a variety of interpretations ranging from more active to more passive ones. This is where the other relatively active interpretations from the empirical approach can be regained, together with some new more passive interpretations by which a theoretical transformation can be matched with a passage to a distinct reference with respect to which a physical system is observed or with a change in the physical state of a reference that does not affect the identity of that reference. In the empirical approach, meanwhile, there does not seem to be a reason for matching theoretical transformations with changes in conventions and references while we are supposed to be interested in changes involving physical systems (or physical objects). And as there are thus more evident interpretations of theoretical transformations in the theoretical approach in comparison to the empirical approach, there are likewise going to be more such interpretations for theoretical states and for theoretical symmetries in the former approach in comparison to the latter. Thus in this case it is the theoretical approach that wins over the empirical approach rather than the other way round. On the other hand, there is no guarantee that in some other case the opposite situation will not obtain. Indeed, I will argue that for our topic of DES it is on the contrary the empirical approach that is more powerful than the theoretical approach. So which of the two approaches is to be used in which case matters at least from the epistemic perspective.

Section I. 2. 4. Ontology of gauge symmetries

Subsection I. 2. 4. 1. What are gauge symmetries

The internal/external distinction for theoretical symmetries that we have just discussed is not the only distinction that has been thought to have some impact on the ontological significance of different theoretical symmetries. One other distinction of that kind is the global/local distinction. There are several inequivalent definitions of this other distinction for theoretical symmetries. Those that will be of relevance to our discussion link the notion of being *global* to some kind of uniformity and the notion of being *local* to its absence. Among external symmetries examples of global symmetries are those relevant to Leibniz and Clarke's debate as well as those mentioned by Redhead, namely spatial and temporal translations as well as Lorentz transformations. Meanwhile, external local transformations are exemplified by theoretical symmetries we have encountered in the discussion of the hole argument, namely diffeomorphisms.

Besides, there is another distinction of potential relevance to the ontological significance which concerns observational consequences. This is the distinction between *observationally incomplete theoretical symmetries* constituted by theoretical transformations that do not preserve some observational consequences and theoretical states that differ in some of their observational consequences, i.e. are observationally inequivalent, and *observationally complete theoretical symmetries* constituted by theoretical transformations that preserve all the observational consequences and theoretical states that share all their observational consequences, i.e. are observationally equivalent. Examples of observationally complete theoretical symmetries are theoretical permutation symmetries of the kind relevant to the discussion of 'indistinguishable' objects, those theoretical symmetries constituted by spatial and temporal translations that are relevant to Leibniz and Clarke's debate, as well as those diffeomorphisms involved in the hole argument.

All the three distinctions just mentioned, namely the external/internal, the global/local and observationally incomplete / observationally complete, have been involved in particular in different understandings of the notion of *gauge symmetry*. As the latter notion designates a kind of theoretical symmetries whose ontological significance is especially controversial and has been a matter of extensive debate, it is important to know different ways in which this notion is usually understood as well as how they have arisen. This is what we will be considering in the present subsection.

Originally the term of gauge symmetry was used by Hermann Weyl [*1918*] *to designate a local external symmetry constituted by a scaling transformation*. This symmetry was introduced by him in an attempt to build a unified theory of electromagnetism and gravitation. But Einstein was quick to notice that this theory is empirically inadequate: its observational consequences do not match with what we observe. Subsequently Weyl built a more empirically successful theory, this time unifying electromagnetism with matter ([Weyl 1929]; on the development of that theory from the previous one see [O'Raifeartaigh 1997], [Afriat 2017]). That other theory featured a local internal symmetry constituted by phase and electromagnetic potential transformations. So *the name of gauge symmetry became associated with local internal rather than local external symmetries*. However, progressively many similarities were noticed between local internal symmetries such as Weyl's and the diffeomorphism symmetry of general relativity, which is a local external symmetry. To account for this the notion of gauge symmetry was extended to designate as much local internal as local external symmetries. As all theoretical symmetries are either external or internal or mixed in the sense of featuring both spatiotemporal and non-spatiotemporal transformations, *the notion of gauge symmetry thus began to comprise any local symmetries* independently of whether they feature spatiotemporal or non-spatiotemporal transformations. Meanwhile, similarly to the diffeomorphism symmetry Weyl's local internal symmetry was also an observationally complete theoretical symmetry. This is because the transformations constituting Weyl's symmetry were chosen so that they preserve a Lagrangian, and hence Lagrangian equations of motion, and hence observational consequences that can be derived from them. Because of that *the name of gauge symmetry also became associated with observationally complete theoretical symmetries*. Meanwhile, theoretical transformations of observationally complete theoretical symmetries as well as the theoretical differences induced by these transformations were often thought not to have any counterparts in the world because of not leading to observable transformations and to observable differences. Leibniz as well as Earman and Norton provide examples of this position. Because of that *the name of gauge symmetry become further associated with theoretical symmetries redundant in the sense of not having any ontological significance*.

In this way the term of gauge symmetry has acquired several intensionally different definitions. Fortunately, the extensions of those definitions partially coincide. That is, there are theoretical symmetries which are as much gauge in the sense of being local as gauge in the sense of being observationally complete. As we know, examples here are the diffeomorphism symmetry and Weyl's symmetry. However, the coincidence of extensions is not full. To see this, recall that symmetries relevant to Leibniz and Clarke's debate are external global yet also observationally complete. So they are non-gauge according to some of the meanings of the term 'gauge symmetry' but gauge according to another meaning.

Subsection I. 2. 4. 2. The gauge principle

Weyl's derivation of his second theory happens to be an instance of *the gauge principle*. Briefly, this principle can be described in modern terms as a procedure where, by passing from a global symmetry group to a local symmetry group, one obtains from a free matter theory (i.e. without interactions) a theory where a gauge field interacts with the matter. The groups involved here are (representations of) the groups of transformations from the group theory in mathematics which I presented above as a useful means of formalising symmetries. At the time Weyl was constructing his second theory, however, it was not yet apparent that the procedure he used can be thought of as a particular case of this general gauge principle which relies on the group theory. For Weyl himself his procedure rather consisted in building a theory of electromagnetic interactions by analogy with the theory of gravitational interactions which was general relativity. Thus, while the empirical success of Weyl's second theory provided a motivation to extend his procedure, it was unclear how this extension should be made to produce empirically successful theories of the remaining fundamental interactions, namely the strong and the weak interactions. So the first extension of Weyl's procedure to the strong interactions by C. N. Yang and R. L. Mills [1954] and in parallel by Ronald Shaw [1955], including in particular a new expression for the so-called gauge field strength $F_{\mu\nu}$, was rather obtained by the trial-and-error method. However, Ryoyu Utiyama [1956] showed that Weyl's and Yang and Mills' procedures are special cases of the gauge principle based on the group theory, and that moreover there is a derivation of interactions for general relativity which is another special case of the same principle. This appeal to the group theory allowed in particular to justify the use of the new expression for $F_{\mu\nu}$ in Yang and Mills' (and Shaw's) theory and explain that it differed from the expression for $F_{\mu\nu}$ in Weyl's theory due to the former theory being based on a *non-Abelian (non-commutative) group*, i.e. a group G' such that $\exists\, a, b \in G': a*b \neq b*a$ (on the development from [Weyl 1929] to [Utiyama 1956] see [O'Raifeartaigh 1997]). This also allowed to build a theory of weak interactions with the help of another non-Abelian group. Once obtained via the gauge principle, the theories of electromagnetic, strong and weak interactions have undergone some changes related in particular to the quantisation, the unification of the electromagnetic and the weak interactions and the introduction of the *symmetry* *breaking* (consisting in the absence of invariance of a particular solution of field equations under a transformation which preserves these equations themselves). In their modern form the theories of these three interactions constitute the Standard Model of particle physics. It is these theories (and their classical or semi-classical relatives) that are usually called *gauge theories*, but sometimes this term comprises general relativity as well. General relativity however remains somewhat disconnected from the other gauge theories, in particular because it is classical and uses Riemannian spacetime manifolds while the other theories have quantum versions and use Minkowskian spacetime manifolds, and it is hard as much to satisfactorily quantise general relativity as to formulate the other theories on Riemannian spacetime manifolds. In any case, current theories of all the four fundamental interactions enjoy impressive empirical success in terms of confirmed predictions, produced explanations and technological achievements. Important examples here are recent confirmations of predictions related to the Higgs boson at CERN and to the gravitational waves at LIGO. As the theories of all the fundamental interactions were either obtained or shown to be derivable from the gauge principle, the empirical success of these theories has often been credited to this principle. Also the group theory became a popular means of formalising symmetries because of the success of the gauge principle.

Let us see in more detail what this principle consists in. To implement it one starts with a free matter theory whose Lagrangian density (a quantity encoding the dynamics) is invariant under a global group of transformations and extends this group to a local group. A *group* of transformations and hence a symmetry constituted by it is usually said to be *global* if it essentially depends on one or more parameters and *local* if it essentially depends on one or more functions. So in the gauge principle one proceeds by replacing a parameter on which a group of symmetry transformations depends by a function. As a result the global symmetry group now becomes a subgroup of a local group. Usually the original free matter Lagrangian density is not invariant under the latter extended group of transformations, i.e. a transformation from the local group which is not in its global subgroup may bring about an extra term in that Lagrangian density. So one adds to it an interaction Lagrangian density featuring a gauge potential and makes this potential to transform under a local group of transformations in a way that brings about a compensating term. As a result the sum of the original free matter Lagrangian density and of the interaction Lagrangian density becomes invariant under the joint local transformations of matter and of the gauge potential. Next one adds a free gauge term to the Lagrangian density that is likewise invariant under the action of the local group of transformations. In this way the gauge principle allows one to obtain from a Lagrangian density of a free matter theory with a global symmetry group a total Lagrangian density of an interaction theory with a local symmetry group, where the latter Lagrangian density is a sum of the original free matter Lagrangian density, an interaction Lagrangian density and a free gauge Lagrangian density.

As an example consider the electromagnetic case. Here the free matter is taken to be originally described by a Lagrangian density which features the wavefunction ψ and which is invariant under the global phase transformation $\psi \rightarrow \psi' = \psi e^{iq\theta}$, where $e^{iq\theta}$ is a phase factor, q is usually interpreted as an electric charge (especially retrospectively after the application of the gauge principle is run to the end) and θ is an arbitrary parameter. A localised version of this transformation is obtained when the parameter θ is replaced by an arbitrary function $\theta(x_\mu)$ of theoretical spacetime points x_μ. To restore the invariance of the original Lagrangian density one adds an interaction Lagrangian density to it which features ψ together with the electromagnetic potential A_μ. When ψ transforms under the local group of transformations as just specified, A_μ is assumed to transform as $A_\mu \rightarrow A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$, where $\theta(x_\mu)$ is the same function as in the local phase transformations. Under this condition the sum of the original free matter Lagrangian density and of the interaction Lagrangian density will be invariant under the joint local transformations of phase and electromagnetic potential. To these Lagrangian densities is further added the free electromagnetic Lagrangian density $L_{EM} = -\frac{1}{4}F^{\mu\nu}F_{\mu\nu}$, built out of the electromagnetic field strength

$F_{\mu\nu}$ such that $F_{\mu\nu} = \partial_\mu A_\nu - \partial_\nu A_\mu$. The latter expression is invariant under the local electromagnetic potential transformations specified above, so $F_{\mu\nu}$ and hence L_{EM} are invariant under them as well. The sum of the original free matter Lagrangian density, the interaction Lagrangian density and the free electromagnetic Lagrangian density gives the total Lagrangian density.

Once a total Lagrangian density is obtained, we can get observational consequences with its help either on the classical or on the quantum level. On the classical level they can be obtained by constructing the Euler-Lagrange equations of motion out of derivatives of the total Lagrangian density and by solving these equations of motion for given initial conditions. To get observational consequences on the quantum level, we can firstly define an *action*, i.e. a real- or complex-valued quantity usually consisting (in the context of field theories such as gauge theories) of an integral over theoretical spacetime of a Lagrangian density sometimes multiplied by a constant, and then use Feynman's perturbative path integral approach. For the electromagnetic (and gravitational) interactions remaining on the classical level is enough to get successful predictions. On the other hand, the quantum analogue of the classical or semi-classical (i.e. with quantum matter) electromagnetic interaction theory, quantum electrodynamics, is extremely successful as well. By contrast, for the other interactions there are no classical empirically successful theories, and so the passage to the quantum level is necessary to get empirical success (see [Guay 2008, p. 351]).

Subsection I. 2. 4. 3. The gauge argument

As all the gauge theories lead to successful predictions in one way or another, it becomes possible to implement the standard move of a scientific realist, namely to use the ontological link between observations and observational consequences of given theories as a motivation for taking these theories to inform us about the ontology on the unobservable level. Given the crucial role of the gauge principle in formulating the gauge theories, in their case the realists' move is usually encoded in *the gauge argument*, according to which the empirical success of the gauge theories and the fact that they are based on the gauge principle justify the attribution of some ontological significance to theoretical symmetries involved in the gauge principle. This argument is very common, especially among physicists, but lots of philosophical work should actually be done before it can get through.

First, as the gauge principle is primarily a means of obtaining theories, one has to explain why it should be relevant to the ontology at all. This is not straightforward, because the heuristics and the ontology can well be disconnected. This becomes obvious when we note that there can be many means of obtaining the same (gauge) theory. For example, in their famous textbook on general relativity [1973] Charles Misner, Kip Thorne and J.A. Wheeler list six ways of obtaining Einstein's field equations for general relativity [ibid., box 17.2]. We cannot infer which of these ways, if any, is relevant for the ontology by checking which of them was actually used by Einstein.

Supposing that the gauge principle is indeed relevant for the ontology, another issue is that the outcome of its application is actually not unique. For instance, in the example of its application given above the familiar electromagnetism actually arises only if the so-called minimal coupling between the matter and the gauge field is chosen for the interaction Lagrangian density, while for other choices of the interaction term other outcomes are possible. To show that the gauge principle leads to a definite ontology one would thus have to find a choice criterion that would be reasonable from the ontological perspective.

Next, at least two crucial steps in the gauge principle are not sufficiently motivated. Firstly, it is unclear why one should pass from a global symmetry group to a local symmetry group. In particular, as Christopher Martin notes [2003, p. 44], the appeal to other notions of locality, such as the one used in special relativity, does not seem to be helpful here. Secondly, no convincing motivation is provided for the addition of the free gauge Lagrangian density. In particular, the addition of this term cannot be motivated by the requirement of invariance under a local symmetry group, because as mentioned above the sum of the free matter Lagrangian density and of the interaction Lagrangian density is already sufficient to get this invariance. The need for justifying both steps is recognised already in Yang and Mills' article [1954], but the justifications they provide (respectively [ibid., p. 192] and [ibid., p. 193, note added in proof]) are not enough to close the question.

On the other hand, there is a perspective from which the gauge principle looks highly natural, perhaps even trivial. This is when we take into account that the free matter we start with is supposed to be charged (or massive in the gravitational case). This may even be found to be expressed in our starting conditions, as seems to happen with q in the global phase transformations above which can be thought of as a charge even before the gauge principle is applied to yield electromagnetism. Moreover, the free matter may even be supposed to be charged in a way which corresponds to a certain kind of interactions. So when we build on this basis a theory of interactions corresponding to the kind of charge in question, this seems to just restore what was to some extent already there and of what the free matter theory is but an artificial and unfinished abstraction.

Supposing that this makes the gauge principle motivated, there can still be other impediments to assigning the ontological significance to symmetries involved in it as per the gauge argument. One obstacle consists in that often the gauge principle does not seem to be sufficient to warrant the empirical success. In particular, as said for the weak and strong interactions one can only get matching predictions after having added other ingredients such as the quantisation and the symmetry breaking. So one has to explain why in such cases the empirical success is to be credited to the gauge principle rather than to these other additions.

In particular, given the symmetry breaking one has to explain why the ontological significance should be assigned to symmetries rather than to their absence. One possibility is to use the fact that some of those physicists who have proposed the symmetry breaking ([Englert and Brout 1964], [Higgs 1964], [Guralnik, Hagen, Kibble 1964]) have also proposed alternative ways of arriving at the same predictions via unbroken symmetries ([Higgs 1966], [Kibble 1967]) (see [Struyve 2011] for a philosophical discussion). To be able to attribute the ontological significance to symmetries on this basis one has to show that the latter alternatives and not the symmetry breaking matter for the ontology.

Supposing finally that the attribution of the ontological significance to symmetries on the basis of the gauge argument is justified, one still needs to make precise what it is that gets the ontological significance there. In particular, one should say to which symmetries and to which constituents (among the transformations, the differing parts and the identical parts) the ontological significance is to be attributed.

A serious issue here is what to say about the transformations and the differing parts of those symmetries of interaction theories which already figure in free theories, be they matter or gauge ones. Examples here are global (in the case of the gauge argument) symmetry transformations of free matter, such as the global phase transformations above, as well as global and local transformations of free gauge fields, such as global transformations of electrostatic potential (which makes part of the electromagnetic potential) in classical electrostatics without matter and local transformations of electromagnetic potential (consisting again of transformations $A_\mu \rightarrow A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$ this time with any $\theta(x_\mu)$) in classical electrodynamics without matter. The problem with such symmetry transformations is that they usually preserve equations of motion of relevant free theories and hence their observational consequences. By that reason these transformations and the differences induced by them (such as a phase factor and a difference in value of a potential in the examples just given) are usually thought not to have any ontological significance. Thinking otherwise actually seems to lead us to an indeterminism akin to that of the hole argument. But if the gauge argument is taken to apply to these constituents of theoretical symmetries, and as a consequence they are assigned an independent ontological significance, this is what we have to address. Besides, we then have to explain why these constituents are responsible for the empirical success of interaction theories but have not elicited such an empirical success already in free theories (the observational consequences of the latter theories being insensitive to these constituents as just said).

On the other hand, reserving the ontological significance bestowed by the gauge argument for those transformations and differing parts of symmetries of interaction theories that do not already figure in free theories is likewise controversial. For one thing, in the electromagnetic case this would amount to assigning the ontological significance to those local transformations of the matter (and the differences induced by them) which are not in the global subgroup of symmetry transformations of the matter while not assigning any ontological significance neither to the latter transformations nor to any transformations of the gauge field. So we would find ourselves assigning the ontological significance to those transformations which are not symmetry transformations. Besides, we would thus credit the empirical success of interaction theories to those theoretical elements that do not have the form of elements responsible for the interaction (which elements would contain mentions of both the matter and the gauge field, examples being the interaction Lagrangian density or the total Lagrangian density or the local transformations of the matter and the gauge field when considered together).

It should be noted that the case of electromagnetism is special in that free electrodynamics with its symmetries was formulated by James Clerk Maxwell long before Weyl's interaction theory, while for the weak and the strong interactions interaction theories were on the contrary obtained prior to free gauge theories because the first theories in these domains were built using the gauge principle. So in the latter interaction theories one may wish to assign the ontological significance to transformations and differing parts not only of the complement of the global subgroup of symmetry transformations of the matter, but also of local symmetries of the gauge field. This would however mean that our ontological conclusions are different for the electromagnetic interactions on the one hand and for the weak and the strong interactions on the other hand because of the differences as to whether a free theory or an interaction theory was there first. But this is unsatisfactory, because the latter seems to be a contingent matter from which an ontological analysis should not depend. Besides, we may wish to have a unified treatment for the electromagnetic and the weak interactions in particular given that they are currently unified in a single theory.

As to the identical parts (invariants) of symmetries, in the interaction theories these are in particular the sum of the free matter Lagrangian density and of the interaction Lagrangian density; the free gauge Lagrangian density; the Euler-Lagrange equations of motion based on the total or free gauge Lagrangian density; actions based on the total or free gauge Lagrangian density; and the gauge field strength. At the very least one has to say whether all or only some of these theoretical elements should get the ontological significance on the basis of the gauge argument and why. One factor which can matter in this respect is how close different theoretical elements are to successful observational consequences. For instance, usually one cannot derive any observational consequences from Lagrangian densities without passing via the Euler-Lagrange equations or actions featuring these Lagrangian densities for that purpose. So one may argue that such equations or actions have more rights to benefit from the gauge argument than Lagrangian densities themselves because of the former theoretical elements being closer to observational consequences when compared to the latter theoretical elements.

In sum, the gauge argument is not hopeless and its analysis may be useful for the study of the ontology. However, this argument should be elaborated much more before it can be said to clarify what the ontology of theoretical symmetries is.

Section I. 2. 5. Indirect empirical status (IES)

The notion of *indirect empirical status* (hereafter *IES*) comes as we will see from the literature on direct empirical status (DES), in particular [Kosso 2000] and [Brading and Brown

2004]. There IES is described as something that first, is opposed to DES, and second, is exemplified by the case where a theoretical symmetry entails an empirically adequate theoretical conservation law via Noether's theorems. What other cases should also be included into IES is not very clear, but here I will take them to also comprise at least the case where a theoretical symmetry entails empirically adequate Euler-Lagrange equations via Hamilton's principle. This is because Noether's theorems and Hamilton's principle function in a similar way and apply to the same sort of theoretical symmetries, namely to theoretical symmetries of actions.

Subsection I. 2. 5. 1. Hamilton's principle and Noether's theorems

As we have just seen, in the gauge argument an independent ontological significance, i.e. an ontological significance that goes beyond the matching of observational consequences with observations, is ascribed to theoretical symmetries on the grounds that they serve, via the gauge principle, to derive theoretical elements responsible for empirically successful predictions. The arguments we will now discuss are of similar kind, but the role of the gauge principle is played there by *variational principles*, which consist in deriving useful results from the fact that a quantity is invariant (or *quasi-invariant*, i.e. invariant up to a divergence term) under a variation in values of some variable(s). Usually the quantity to remain (quasi-)invariant is an action, and the derived theoretical elements are either the Euler-Lagrange equations once again or the so-called *continuity equations*. The latter are equations stating that a derivative of some quantity vanishes. They serve in turn to derive integral theoretical conservation laws which can then be confronted with observations. Again, the empirical success of predictions derived from the Euler-Lagrange equations or from the continuity equations can then be appealed to as a basis for ascribing an independent ontological significance to (quasi-)symmetries of the action.

The variational principle which allows to derive from certain symmetries of an action the Euler-Lagrange equations of motion is called *Hamilton's principle*. *Noether's theorems* [Noether 1918] are further variational results which allow to derive continuity equations and conservation laws from other symmetries of an action and sometimes additional conditions. To see how these derivations are related we need some details about what the variation of an action consists in.

Firstly, there is a distinction between *dependent variables* and *independent variables*, i.e. variables whose values depend on values of other variables and those whose values the values of other variables depend on. This distinction is important for variations, because if a value of an independent variable is varied, the values of variables dependent on that independent variable will vary as well, while if a value of a dependent variable is varied, the values of independent variables will remain the same. Usually the role of independent variables is played by spatiotemporal variables and while the other variables are taken to be dependent on them.

Secondly, there is a distinction between *boundary variations* and *interior variations* of an action (or of a similar quantity). This comes from the fact that the action is usually an integral of some quantity over a domain of at least two dimensions, so that this domain has some boundary, provided by the initial and the final points over which the integral spreads, and an interior region, consisting of what is confined between the boundary points. So a variation is called a boundary variation when it concerns values on the boundaries and an interior variation when it concerns values within the region confined by the boundaries. In general these values can belong as much to dependent as to independent variables. As the action is usually an integral (or a sum) of a Lagrangian density over theoretical spacetime (or of a Lagrangian over an axis in the theoretical space), boundary points are usually theoretical spatiotemporal (or spatial) points and the region confined between them is likewise spatiotemporal (or spatial). As in addition spatiotemporal variables are usually independent variables, varying the values of independent variables amounts to varying the boundary and/or the region of integration.

Thirdly, there is a distinction between global and local groups of transformations defined as for the gauge principle above.

A symmetry of an action amounts to stating that its specific variations vanish (i.e. that specific transformations of values of some variables on which the action depends leave invariant the value of the action). Hamilton's principle starts by stating more precisely that interior variations of dependent variables vanish. One then derives the Euler-Lagrange equations by noticing that they are the equations whose satisfaction is a sufficient condition for the symmetry in question to obtain. Whose dynamics can be described by these equations depends on which of the variables present in the Lagrangian density were supposed to be varied. For instance, if the action is an integral of a total Lagrangian density generated by the gauge principle, this Lagrangian density will feature both a wavefunction and a gauge potential. So one will obtain a description of the evolution of a matter field or of a gauge field depending on whether the action is assumed to be invariant under the variation of the wavefunction or of the gauge potential values respectively. In the former case, for example, the simplest Euler-Lagrange equations for the matter field ψ will have the form $\partial L/\partial\psi - \partial_\mu(\partial L/\partial(\partial_\mu\psi)) = 0$, where L is a total Lagrangian density.

Noether's theorems, on the other hand, are obtained by stating that both boundary and interior variations of both dependent and independent variables vanish. Thus the variations used in Hamilton's principle make part of the variations used for Noether's theorems. Because of that one can impose the satisfaction of the Euler-Lagrange equations to isolate the remaining variations. It is also possible to reformulate variations in dependent and independent variables so that they only concern dependent variables. The condition stating that the resulting interior variations of dependent variables vanish has then the form of a continuity equation. *Noether's first theorem* says that if the initial variations of the action constituted a global group of transformations, the quantity whose derivative vanishes according to this continuity equation can be further simplified. The resulting continuity equation can then be used to derive, under appropriate boundary conditions, an integral conservation law. For instance, starting with the global variation in the wavefunction and using the matter Euler-Lagrange equations given above (or those based on the free matter Lagrangian density) one gets the conservation of the electric current (according to a simplified continuity equation) and of the electric charge (according to an integral conservation law), while starting with any theory whose action is invariant under global groups of time translations, spatial translations, spatial rotations and *boosts* (additions of constant uniform velocity) one gets the conservation of energy, linear momentum, angular momentum and centre of mass motion respectively. Meanwhile, *Noether's second theorem* says that if one starts instead with a local symmetry group, then before imposing the satisfaction of the Euler-Lagrange equations one already gets different continuity equations which say that a derivative of a derivative of some quantity vanishes, where the quantity in question is usually a gauge field strength. As Brading and Brown explain in their [2000], in interaction theories these different continuity equations can then be used to derive those usual continuity equations and conservation laws that follow from Noether's first theorem.

In addition to Hamilton's principle and to Noether's theorems there are other ways of deriving equations of motion and conservation laws starting from different symmetries of actions. Besides, there are other variational principles which allow to derive from different symmetries of actions other results of potential relevance to the empirical success, such as the relationships between matter and gauge fields. Such derivations are discussed e.g. in Brading and Brown's text [ibid.] on the basis of texts by Utiyama [1959], Trautman [1962] and Al-Kuwari and Taha [1991], among others.

Subsection I. 2. 5. 2. Ontology of symmetries of actions

By contrast to the gauge principle variational principles usually are not criticised for insufficient motivation of various stages in the derivations, and the potential relevance of variational principles to the ontology is usually not contested. Yet, there are again many reasons why the attribution of an independent ontological significance to symmetries of actions on the basis of variational principles is not straightforward.

Firstly, it does not necessarily follow from the ontological significance of observational consequences that the theoretical underpinnings which are used to derive these observational consequences have an independent ontological significance. In particular, this would not necessarily follow even if such theoretical underpinnings were unique. But they are not unique in the case we are discussing, because there are many ways of deriving conservation laws, and even the derivation of equations of motion is not unique (for instance, to build the Euler-Lagrange equations of a field theory it suffices to know the general form of these equations and to know or postulate how much dynamical fields there are in a Lagrangian density and which derivatives of them are to be taken into account). Hence one has to both prove that the independent ontological significance is to be attributed in the kind of cases we are discussing and tell to the theoretical elements of which of the derivations it should be attributed more precisely.

Secondly, to derive observational consequences from symmetries of actions further theoretical elements should be interposed between the latter and the former such as equations of motion with specific initial conditions or the passage to an integral form of conservation laws under appropriate boundary conditions. So even if some theoretical elements of a given derivation are to receive an independent ontological significance, one can argue that these intermediary theoretical elements should be considered as having more rights to receive it than symmetries of actions, for example because the latter are more remote from observational consequences than these intermediary theoretical elements are.

Thirdly, to the extent that the attribution of the ontological significance to symmetries of actions via Hamilton's principle, Noether's theorems and their likes hinges on the satisfaction of the Euler-Lagrange equations of motion, it depends in particular on the ontological interpretation of these equations of motion. This interpretation, however, is far from being settled.

Indeed, one important topic in philosophy of science is the ontological significance of *(theoretical) laws*, of which the Euler-Lagrange equations of motion are usually supposed to make part. The main positions say that the laws of empirically successful theories are: empirical generalisations (David Hume; a close position was later defended by Eugene Wigner); theoretical systems that achieve a fairly good balance between the predictive strength and simplicity (David Lewis; earlier the usefulness of science for the systematisation of phenomena was highlighted by Pierre Duhem); theoretical statements that have a greater counterfactual resilience than those statements that bear on particular phenomena (Marc Lange); theoretical statements that match with necessities in the world (David Armstrong and Michael Tooley); theoretical statements that match with dispositional properties in the world (Stephen Mumford and Alexander Bird); theoretical statements that match with some primitives in the world (Tim Maudlin). Here the last three positions ascribe to laws an independent ontological significance by matching them with unobservable entities in the world, while at least the first two positions are compatible with the reduction of the ontological significance of laws to that of their observational consequences. So one has plenty of reductionist and non-reductionist positions to choose from concerning the ontology of laws in general and of the Euler-Lagrange equations in particular.

One's choice may then influence one's ontology of symmetries of actions. For one thing, this is because theoretical symmetries may have the same properties as theoretical laws do but to a greater extent. For instance, Wigner (e.g. in his [1967]) thought that (geometric) symmetries provide an even more condensed account of phenomena than laws do, while Lange [2007] developed this into an account by which symmetries have a greater counterfactual resilience than laws do. Similarly, the reasons that one may take to support a reductionist position about the ontological significance of the Euler-Lagrange equations, such as the remoteness of these equations from observational consequences, may to a greater extent apply to symmetries of actions. On the other hand, the reasons for holding a non-reductionist position about the ontological significance of the Euler-Lagrange equations may on the contrary prevent one from attributing an independent ontological significance to symmetries of actions, because as said one has the possibility to argue that such an ontological significance is to be attributed to the intermediary theoretical elements such as the Euler-Lagrange equations alone. This can be done even more readily if one takes seriously those derivations of the Euler-Lagrange equations that are not based on symmetries of actions.

Fourthly, the attribution of symmetries to actions may be contested in the cases where the same symmetries are shared by actions, Lagrangian densities and the Euler-Lagrange equations of motion. For in those cases the invariance of actions and of the Euler-Lagrange equations under a transformation seems to be the consequence of the invariance of a Lagrangian density under the same transformation and of the fact that these actions and Euler-Lagrange equations are based on that Lagrangian density (an action being essentially an integral of a Lagrangian density and the Euler-Lagrange equations being essentially a sum of derivatives of a Lagrangian density). So in such cases one may argue that the relevant symmetries are primarily symmetries of Lagrangian densities. At the same time, one may argue that the independent ontological significance is primarily to be attributed to actions and/or to the Euler-Lagrange equations instead, because as said Lagrangian densities seem to only lead to observational consequences when they make part of one or the other of the latter theoretical elements. In this way the empirical success may lead to the attribution of an independent ontological significance without this significance being attributed to symmetries of actions.

Fifthly, one may argue that if symmetries are to matter for observational consequences, which kind a symmetry belongs to should likewise matter for them. Noether's theorems seem to confirm this, because they lead to different continuity equations for global and for local symmetry groups. However, as said the continuity equations resulting from local symmetries can be used to derive the same conservation laws as in the case of global symmetries. So perhaps whether a symmetry is global or local distinction does not matter for obtaining observational consequences in the case of conservation laws. One may object to this that some derivations of the usual conservation laws from local symmetry groups actually rely on them having a global subgroup, so that what matters in any case is the global character of a symmetry group or of its subgroup. However, as Brading and Brown [2000] show on the basis of the analysis of Utiyama's [1959] derivation, local symmetries may lead to the usual conservation laws also when they are not explicitly presupposed to have a global subgroup. To clarify this issue it would remain to determine whether the global subgroup is not implicitly presupposed in these derivations. One reason why this may be the case is because the derivations in question concern interaction theories. If it happens that one can get the same conservation laws from global symmetries and from local symmetries that do not have a non-trivial global subgroup, it will mean that some observational consequences are insensitive to whether a symmetry group is global or local. This may be considered as a weakening of the link between symmetries and observational consequences unfavourable to the idea of attributing an independent ontological significance to the former on the basis of the empirical success of the latter.

Sixthly, there are other reasons to think that the link between symmetries and observational consequences is not very tight. These reasons are reminiscent of the situation with symmetry breaking in the case of the gauge principle. In that previous case observational consequences seem to be successful because of the absence rather than a presence of a symmetry. Similarly, in the case of variational principles observational consequences can be insensitive to the absence of a strict symmetry or even dependent on such an absence, as explained by Brown and Holland [2004]. The first of these situations happens with quasi-symmetries of actions (and of Lagrangian densities) when Hamilton's principle is used, because it yields the same Euler-Lagrange equations, and hence the same observational consequences associated to these equations, for those Lagrangian densities that differ by a divergence. Meanwhile, the other situation happens with quasi-symmetries of actions (and of Lagrangian densities) when Noether's theorems are applied, because then the divergence term arising from the quasi-invariance contributes to the quantities preserved in continuity equations and conservation laws, so that these quantities take a different value because of that term.

Moreover, a similar situation happens in the case of Feynman's path integral approach used to generate predictions of quantum versions of non-gravitational gauge theories. Indeed, observational consequences in that approach are derived by taking into account all possible paths linking the boundaries of a domain of integration on which an action is defined, while in the classical case only that unique path is taken into account which extremises the value of the action. The latter path is singled out by imposing a symmetry on the action, so to get to Feyman's approach one has to abandon that symmetry. Thus in the case of Feyman's approach again observational consequences happen to arise thanks to the absence of a symmetry rather than to its presence. On the other hand, symmetries of actions can still be said to retain some bearing on observational consequences in that case, because contributions of different paths in terms of probabilities to have a given observation are assumed in Feynman's approach to be proportional to the similarity of these paths to a classical path singled out by a symmetry of an action. However, this does not necessarily lead to symmetries of actions retaining an independent ontological significance in the quantum case, even supposing that such an ontological significance is to be assigned to them in the classical case. For it is possible to argue that while a theoretical classical path matches with a physical path followed by a classical (e.g. macroscopic) physical system in the world, a quantum physical system follows all the paths envisaged in Feynman's approach at once or chooses a definite path to follow that may be different from the classical one (for the latter position see [Kent 2013]). Or worse, one may argue that the situation with physical systems taken to belong to the quantum realm also extends to what is taken to be the classical realm, which may result in a claim that the classical paths are never physically distinguished, for instance because any physical system follows all the paths at once (for the latter position see [Terekhovich 2018]).

Finally, assuming that one manages to prove that an independent ontological significance has to be ascribed to symmetries of actions, one still needs to explain more precisely which constituents of these symmetries should get an independent ontological significance and on which grounds. For this one should first distinguish between various constituents within such symmetries, for instance by taking a preserved value of an action to be the invariant part, the transformations of values of variables on which a Lagrangian density depends to be the symmetry transformations and the differing values of variables to be the differing parts. One should then explain which of the constituents so distinguished should be taken to have specific counterparts in the world and why.

Chapter I. 3. A brief presentation of direct empirical status (DES)

Direct empirical status (hereafter *DES*) is a status that a theoretical symmetry has if it is matched with an empirical symmetry. DES will be our main topic for the rest of this work, so to begin with I will briefly present in this section the main literature on this topic that will be analysed in more detail in Part II, the reasons for studying DES and some of the questions that will be addressed by my own analysis of DES in Part III.

Understood in a wide sense, the literature on DES could range on several centuries. What makes it especially old is that what are now taken to be the usual examples of empirical symmetries are actual or thought experiments described a long time ago. Indeed, the recognised empirical symmetries are usually traced back in particular to Galileo Galilei [1632/1967, pp. 186-188], Michael Faraday (see [Maxwell 1881, §67]), Einstein (from 1907 on, see [Norton 1985]) and Gerard 't Hooft ([1980]). Because of that DES can be thought of as an old topic.

Understood in a narrower sense, however, the literature on DES is rather scarce and all recent. This literature can be divided into three groups. The first group are what I will call *precursor texts* not as much with respect to time as with respect to their treatment of the topic. These include in particular the articles by Harvey Brown and Roland Sypel [1995], Tim Budden [1997], and Tim Budden with Michel Ghins [Ghins and Budden 2001], as well as some parts of the book by Richard Healey [2007]. I will not be analysing such texts in detail, because although they do discuss something relevant to the topic of DES, they neither make DES their central topic nor even consider it explicitly as a separate topic. The second group is *the main literature on DES*. I will take it to consist so far of just four articles written by Peter Kosso [2000], Katherine Brading and Harvey Brown [2004], Richard Healey [2009], as well as Hilary Greaves and David Wallace [2014]. As seen from how I call them, I take these texts to be crucial for the study of DES, so I will devote Part II of my work to presenting and analysing them. The third group of texts constitute what I will call *the follow-up literature on DES*. It can be taken to include the articles by Simon Friederich ([2015], [2017]) and Nicolas Teh [2016], as well as a manuscript by James Ladyman and Stuart Presnell [2016/01/08][2]. Although these texts cannot be said to be unimportant, I will not be considering them in detail because the first three texts are conceived as reactions to Greaves and Wallace's article while Ladyman and Presnell's manuscript is unpublished and besides resembles Healey's [2009] article in some significant respects. When I will be referring to *the literature on DES*, I will mostly mean the main literature on DES, or else it and the follow-up literature on DES taken together.

There are several reasons for concentrating specifically on the four articles that I called above the main literature on DES. Firstly, these four texts are to my knowledge the earliest ones to treat DES explicitly. Therefore, examining them has a historical interest of understanding how a new topic had been developing. Secondly, besides the follow-up literature on DES these four texts are to my knowledge the only publications so far that posit DES as their central topic. So it is from them, rather than from any other text that can be said to treat DES implicitly and/or as a side topic, that we can expect to learn the most about DES. Thirdly, by contrast to the follow-up literature on DES the four articles in question aim at providing four independent frameworks for analysing DES. So it is by considering these texts that we will get most varied points of view as to how DES can be analysed. Fourthly, the four texts are highly interconnected (without being conceptually repetitive) in that the authors of later texts in our list build their analyses on the acceptance or critique of what was proposed in the earlier texts of the same list. Arguably this helps the early analyses to be better assessed, the later analyses to be more profound, and so both the earlier and the later analyses to be more worthy of study than if all these texts had no direct bearing on each other. Fifthly, the four articles are separated by rather long periods of time. So their authors had a chance to reflect more on the previous literature on DES and make their own position more mature. Finally, four is not a great number, and so concentrating on such a narrow body of texts will allow us to have a more detailed and profound discussion of the ideas there contained and hence of our topic than if we had plenty of texts to review. By these reasons it is only the articles by Kosso [2000], Brading and Brown [2004], Healey [2009] as well as Greaves and Wallace [2014] that I will be analysing in more detail in Part II.

Before getting to the details, however, it is useful to get some basic idea of the main aspects of DES especially as presented in these texts. To this purpose I will now summarise their answers (and my answers where theirs lack) on such questions as what are empirical symmetries, which theoretical symmetries are matched with them, how empirical and theoretical symmetries get matched and what this implies for the ontology of the latter.

The answer to the first question could take a form of a definition of empirical symmetry. As an empirical symmetry is a kind of physical symmetry, we could begin with a definition of physical symmetry and add to it some characteristics specific to empirical symmetries. This work however has not been carried out properly so far, so this is one of the tasks I will be addressing later on in the present work. Instead the literature on DES usually specifies the notion of empirical symmetry

[2] I thank Steven French for sending me this manuscript and James Ladyman for agreeing that it be sent to me.

through some or all of the four *recognised empirical symmetries*, namely Galileo's ship, Einstein's elevator, Faraday's cage and 't Hooft's beam-splitter empirical symmetries. So to understand what an empirical symmetry is in general we should firstly get acquainted with these four particular empirical symmetries.

Galileo's ship empirical symmetry is usually described as consisting in that experiments (e.g. mechanical, electromagnetic and so on) within a physical system (e.g. a ship) have observably the same results whether the system is at rest or boosted (i.e. moves with a constant rectilinear velocity) with respect to a reference (e.g. the shore). *Einstein's elevator empirical symmetry* is usually said to involve observably the same results of experiments within a physical system (e.g. an elevator or a rocket) in the following two contexts: firstly when we compare a system is uniformly accelerated in the absence of a gravitational field and a system at rest within a gravitational field, and secondly when we compare a system at rest (or uniformly moving) in the absence of a gravitational field and a system in free fall within a gravitational field. *Faraday's cage empirical symmetry* consists in that experiments within a physical system (e.g. a cage covered with conducting materials and insulated from the ground) have observably the same results whether the system is uncharged or charged (as manifested respectively by the absence or presence of sparkles on the outer boundaries of the cage). *'t Hooft's beam-splitter empirical symmetry* involves setups where (usually charged) particles are emitted from a source, pass through a screen with two slits and arrive at the second screen where an observable interference pattern is formed. These setups are usually completed with “the electron-optical equivalent of a half-wave plate” ['t Hooft 1980, p. 115], or a *half-wave plate* for short (i.e. a device that is supposed to shift particles' phase by 180 degrees), and/or with a magnet, usually in the form of a *solenoid* (a wire wound in coils around a long core and connected by a switch to a source of electricity). A half-wave plate is usually placed behind one or both slits, while a magnet is usually placed behind the first screen in between the slits and is shielded from the particles. There is a disagreement as to what should be invariant in this empirical symmetry and under the change of what. The option I will be defending later on is that observable interference patterns should be the same whether (a) half-wave plate(s) and/or a switched-on solenoid are present or not.

In the literature on DES each recognised empirical symmetry is usually matched with one or several theoretical symmetries. Thus Galileo's ship empirical symmetry is usually matched with global theoretical symmetries constituted by theoretical boosts (i.e. additions of constant velocities); Einstein's elevator empirical symmetry is matched in particular with local theoretical symmetries constituted by certain transformations of gravitational potential; Faraday's cage empirical symmetry is matched with global theoretical symmetries constituted by electrostatic potential shifts (i.e. additions of constant electrostatic potentials) and local theoretical symmetries constituted by some electromagnetic potential transformations; and 't Hooft's beam-splitter empirical symmetry is matched with global theoretical symmetries constituted by phase shifts (i.e. additions of a phase factor constant across a domain) and with local theoretical symmetries constituted by some phase and electromagnetic potential transformations.

The grounds for these matchings are usually rather obscure, so spelling them out and checking on this basis whether the usual matchings are correct will also be some of the tasks that I will address below. What can be noted for now is that as seen from the formulation of the matchings just stated they are thought to depend on the transformations that theoretical symmetries feature. More precisely, firstly it is thought to matter for these matchings which kind of variable a transformation acts on, or in my terms which *kind of transformation* a theoretical symmetry features, where I say that a transformation is of a given kind if it acts on a given variable (thus a boost is a kind of transformations acting on velocity, etc.). And secondly, it is thought to matter for these matchings whether a theoretical symmetry is global or local in some sense, where this is actually determined by whether a transformation is respectively global or local in some sense. There are some disagreements as to which global/local distinction is of relevance here, and I will also have more to say on this later on. But at least the matchings above show us that symmetries usually classified as global involve transformations that consist of additions of constant quantities.

These matchings also show us that some empirical symmetries are matched with both global and local theoretical symmetries while for other empirical symmetries this is not the case. In fact the main claim in the debate about DES had firstly been that only global and not local symmetries can be matched with empirical symmetries and so can have DES. Afterwards it has become recognised that local symmetries can also be matched with empirical symmetries, and moreover that sometimes both global and local symmetries can be matched with the same empirical symmetry. This led to the sophistication of the previous idea by which only global symmetries have DES, namely its development into an idea by which while both global and local symmetries can have a weaker DES, only global symmetries can have a stronger DES. This is one of the main ideas that I will be assessing (and in fact refuting) in my work.

Finally, the general relationship between DES and the ontology seems rather straightforward, although it is rarely made explicit. My explanation of this relationship goes as follows. An empirical symmetry is a kind of physical symmetry, i.e. a kind of symmetry in the world. So DES arises from a matching between a theoretical symmetry and something in the world. Hence DES is a status that involves attributing some ontological significance to a theoretical symmetry. What ontological significance it is more precisely depends on how matchings are done, so this is another thing that will be clarified after the basis for matchings will be clarified. The resulting kind of ontological significance will anyway be the one that theoretical symmetries with DES will have and theoretical symmetries without DES will lack. So the study of DES allows us to divide theoretical symmetries into those that have this kind of ontological significance and those that do not by determining which theoretical symmetries have DES and which do not. If DES is to be divided in turn on a weaker DES and a stronger DES or in some other way, we will have to examine whether the ontological significance likewise has to be divided into multiple kinds and which theoretical symmetries should have which kind of ontological significance by virtue of having which kind of DES.

From this it already becomes clear why someone interested in the ontology has to study DES, while this is just the general level where the link of our topic with the ontology is specified. If we go next to the level where such a link is to be spelled out in more detail, the importance of studying DES actually becomes even more evident. What I take this more detailed level to consist in can be seen from my presentation above of different discussions and arguments within the ontology of theoretical symmetries. Indeed, besides being interested there in a more general question whether given theoretical symmetries have the ontological significance according to a given discussion or argument, I was interested in a more specific question the ontological significance of which of the different constituents that I have distinguished within a theoretical symmetry, namely the identical part of the states, the differing parts of the states and the transformations between the states, follows from this or that discussion or argument. Now what we see from my presentation above is that some discussions and arguments (such as the gauge argument or the discussion of symmetries of actions) do not seem to be advanced enough to clearly answer this more specific question; other discussions and arguments (such as Leibniz's discussion with Clarke and Newton or the hole argument) do answer the specific question but only for some of the constituents; yet other discussions and arguments (such as the discussion of 'indistinguishable' objects) provide a too deflationary or a spuriously strong ontology for theoretical symmetries (by interpreting theoretical symmetry transformations respectively as identities or as changes of haecceities or essences); and yet others (such as the discussions of objects with external or internal differences) do answer the specific question for all the constituents but by proposing for them only possible and not obligatory interpretations. This situation makes us wonder whether any topic within the study of the ontology of theoretical symmetries exists which allows us to answer the specific question by unconditionally attributing the ontological significance to all the constituents a theoretical symmetry has. More specifically, we may ask whether DES is precisely the topic of this sort. Answering this last question requires going through the analysis of DES that I will be carrying out in what follows, so at that point we are not in a position to answer it yet, but we may consider this as an additional motivation for the analysis in question. However, as I have an idea of what this analysis is going to be, let me also announce you that it will lead us to answering our question positively and even to going beyond it. It is perhaps this advantage of DES with respect to many other more traditional topics within the ontology of theoretical symmetries presented above that actually makes DES most worthy of studying.

Part II. Presentation and analysis of the main literature on direct empirical status

The purpose of this part is to present the main literature on DES that I take to consist of texts by Kosso [2000], Brading and Brown [2004], Healey [2009], as well as Greaves and Wallace [2014]. These texts will be considered in the chronological order. For each text a short summary and an extended summary will be provided, as well as a detailed critical assessment and a summary of the latter (except for the last text which will be treated more briefly). It will become clear from this how the understanding of DES has been developing and which proposals and claims have been made, as well as in which respects the existing analyses of DES are unsatisfactory and how my own account deals with some of the issues. Especially when discussing Brading and Brown's and Healey's texts the applications of my account to particular cases will be explained because as we will see these authors use the empirical approach as I do.

It should be noted that there is a lot of variation in vocabulary across these texts, which is in part a reflection of underlying differences in meaning and in part a matter of personal preferences. In my summaries and critique I will keep such lexical differences whenever the former seems to be the reason but replace the author's vocabulary by my own whenever the latter seems to be at stake. This will allow the discussion to be more uniform and will help us to only concentrate on conceptual disagreements[3].

Chapter II. 1. Kosso [2000]

Section II. 1. 1. Brief summary of the text

In Section 1 Kosso introduces DES and IES [pp. 81-83]. In Section 2 he divides symmetries into external and internal ones, as well as into global and local ones [pp. 83-85]. In Section 3 he gives two conditions under which a physical symmetry is observable [pp. 85-88]. In Section 4 he discusses some specific matchings between theoretical symmetries and observable physical symmetries and argues on that basis that external global and internal global theoretical symmetries have DES while external local and internal local theoretical symmetries do not [pp. 88-96]. In Section 5 he concludes that theoretical symmetries do or do not have DES depending on whether they are global or local [pp. 96-97].

Section II. 1. 2. Extended summary of the text

Kosso's article consists of five sections of which Section 1 is an introduction, Section 2 deals with kinds of symmetries, Section 3 with observability of physical symmetries, Section 4 with instances of DES and Section 5 is a conclusion.

In Section 1 Kosso envisages two empirical statuses for theoretical symmetries (which he calls 'analytic symmetries'). *IES* of a theoretical symmetry *obtains for Kosso* when consequences of a matching physical symmetry are observed, while *DES* obtains for him if a matching physical symmetry itself is observed [p. 82]. He promises to find out which empirical status different theoretical symmetries have. In particular, he wishes to clarify how to observe physical symmetries and promises to tell us which theoretical symmetries are matched with observable physical symmetries [pp. 82-83].

In Section 2 *Kosso defines a symmetry* as an invariance under a transformation [p. 83]. His examples of what can be invariant in a symmetry are an object such as a snowflake (or its appearance more precisely) or else a law of nature [ibid.].

In the same section Kosso also presents two distinctions for symmetries: the

[3] Note also that in this part a reference to a page or a (sub)section without the indication of the text's author(s) and date concerns the text mentioned in the title of the section in which the reference occurs.

external/internal [p. 84] and the global/local [pp. 84-85]. Kosso does not say whether these distinctions apply to theoretical symmetries, physical symmetries or both. He says though that he considers these distinctions in particular because they are often made for symmetries in physics [pp. 83-84]. This can make us expect that these distinctions primarily apply to theoretical symmetries, but Kosso's examples in this section show that the global/local distinction happens to apply for him to physical symmetries as well, while his examples in Section 4 will show that the same can also be said of his external/internal distinction. For Kosso the two distinctions have in common the fact of being a function of properties of transformations constituting symmetries [p. 84]. Because of that we can use the terms 'global', 'local', 'external' and 'internal' not only for the relevant kinds of symmetries but also for the kinds of transformations featuring in these kinds of symmetries.

A symmetry for Kosso is external if its transformation affects a spatial or temporal variable and *internal* otherwise. Examples of external symmetries for him are those constituted by mirror reflection, time reversal and spatial rotations, translations and boosts; examples of internal symmetries are those constituted by permutations of protons with neutrons and those constituted by phase transformations.

Kosso contrasts the external/internal distinction with Wigner's [1967, p. 15] geometrical/dynamical distinction. *For Wigner symmetries are geometrical* if they concern properties of events and *dynamical* if they concern properties of laws of nature. The main extensional difference between the two distinctions consists in whether symmetries of general relativity belong to the same category as special-relativistic symmetries or to the same category as the other usual symmetries. The former option obtains for the external/internal distinction where special-relativistic and general-relativistic symmetries are both counted as external. The latter option obtains for Wigner's geometrical/dynamical distinction where general-relativistic symmetries and other symmetries such as permutations of protons with neutrons are both counted as dynamical. Kosso says that he will stick to the external/internal distinction, though we will actually see him saying more about Wigner's distinction in his Section 5.

To describe the global/local distinction Kosso uses several pairs of expressions. *For Kosso a symmetry is global* if the transformation “is done uniformly through space and time”, “is not a function of space and time”, “is a constant, the same everywhere and for all time” [p. 84], and a symmetry is *local* if the transformation “is a variable function of space and time”, “is variable, different in different locations and times”, “becomes an arbitrary function of space and time” [pp. 84-85]. Examples of global symmetries for him are those obtained by rotating a snowflake by 60 degrees and by performing boosts in special relativity; examples of local symmetries are those obtained by rotations and accelerations in general relativity, as well as *gauge symmetries* which Kosso will later define [pp. 93-94] to be internal local symmetries.

In Section 3 Kosso reminds us to begin with about his distinction between DES and IES. He also says [p. 85] that in particular global theoretical symmetries may have *IES* by virtue of them implying conservation laws via Noether's theorem (namely via the first of her theorems, we may add). He says as well [ibid.] that the converse implication does not hold, but this is erroneous (see [Noether 1918]; [Brading and Brown 2004, p. 648, n. 6]). He does not mention moreover that local symmetries actually also lead to conservation laws via Noether's first and second theorems (see [Brading and Brown 2000]). Anyway, Kosso argues next that DES is superior to IES [p. 85] and proceeds to explaining how to observe a symmetry, as is needed to establish DES. As he deals with observations, we may expect the symmetries he speaks about here to be observable physical symmetries.

Firstly, Kosso says [pp. 85-86] that we may happen to have a theoretical symmetry in our theory prior to observing a physical symmetry, in which case the theoretical symmetry will be our guide to such an observation, but that on the other hand we may also happen not to have a theoretical symmetry in our theory prior to observing a physical symmetry, in which case the results of our observation will guide us as to whether this theoretical symmetry must or may be introduced into our theory. Indeed, he has explained in Section 1 that if we do not observe a physical symmetry, then a theory may have a corresponding theoretical symmetry, but if we do observe a physical symmetry, then a theory must have a corresponding theoretical symmetry [p. 82]. So he concludes that the fact of establishing an observable physical symmetry, as opposed to motivations for its search, should be distinguished from the fact of whether our theory has a given theoretical symmetry [p. 86]. On the other hand, Kosso allows the use of other theoretical assumptions in establishing an observable physical symmetry [p. 85]. Indeed, he will say in Subsection 4.3 that it is admissible that observations be influenced by theoretical assumptions as long as they are not influenced by the particular theoretical claim about the symmetry itself, or, in other words, are independent of the theoretical belief in the symmetry [p. 92].

Secondly, Kosso says that symmetries of laws differ from symmetries of objects in that in the case of an object such as a snowflake we may notice a symmetry without actually transforming this object, while in the case of laws we should always compare two systems, an untransformed one and a transformed one [p. 86]. As an example he takes theoretical external global symmetries of laws under spatial translations, and says that to establish a matching physical symmetry it does not suffice to observe a single event or object but instead we need to observe phenomena both here and there. Still, he says that he will continue recurring to the snowflake example where useful [ibid.].

Thirdly, Kosso distinguishes two conditions for a symmetry to be observed (stated twice: at [p. 85] and at [p. 86]). Namely, we should be able to observe that a certain transformation has taken place and also that an associated invariance held. I will call these respectively *the observable difference condition* and *the observable invariance condition*. About the observable difference condition Kosso says that it requires that an observable physical reference be left invariant so that we be able to say that something changes with respect to it [p. 86]. On this occasion he contrasts being global with being applied on the whole universe, saying that while a rotation of a snowflake is global, it is compatible with preserving a reference [pp. 86-87]. He also says that the observable difference condition can be realised either by observing a system changed in a certain way with respect to an unchanged reference or by observing a reference changed in the opposite way with respect to an unchanged system. *Kosso calls* these realisations respectively *an active* and *a passive (version of a) transformation* and says that this goes back to Joe Rosen [1990, p. 295]. One of Kosso's examples is the active rotation of a snowflake by 60 degrees clockwise with respect to the coordinate system of an observer versus the passive rotation of the coordinate system of the observer by 60 degrees counterclockwise with respect to the snowflake. He also gives a similar example for spatial translations [p. 87]. Passing to the observable invariance condition, Kosso says that in the case of symmetries of objects such as a snowflake their appearance is left invariant, while in the case of symmetries of laws it is rather "*the physics*" that is left invariant, by which he says to mean the forms of the laws of physics and the strengths of the fundamental interactions [ibid.].

Finally, *Kosso distinguishes between observationally incomplete (physical) symmetries* that satisfy both his conditions for being observed and *observationally complete (physical) symmetries* that satisfy the observable invariance condition but not the observable difference condition [pp. 87-88]. He says that in the case of a snowflake an observationally complete symmetry arises for someone who leaves the room while the snowflake is being rotated [p. 88]. The same example was also mentioned by Kosso in Section 1 [p. 83].

In Section 4 Kosso examines which theoretical symmetries have DES. To do so he divides theoretical symmetries into kinds using his distinctions from Section 2. As the external/internal distinction and the global/local distinction are independent from each other, symmetries within each kind of symmetries from one of the distinctions can be of either kind relative to the other distinction. So Kosso gets four kinds of theoretical symmetries: external global, external local, internal global and internal local. He considers each of them in turn in Subsections 4.1, 4.2, 4.3 and 4.4 respectively. To establish whether theoretical symmetries of a given kind have DES he examines whether their physical counterparts exist. These counterparts are also classified by Kosso using the external/internal and the global/local distinctions. So we see that he implicitly extends his external/internal distinction from theoretical to both theoretical and physical symmetries, in parallel to his use of the global/local distinction for both theoretical and physical symmetries.

Firstly Kosso considers external global symmetries, and these get DES on his account, because he takes their physical counterparts to exist. His examples of such counterparts are the rotation symmetry of a snowflake and a physical symmetry under boosts, both of which are also classified by him as external global symmetries [p. 88]. About the physical symmetry under boosts Kosso says that it consists in observing the invariance of phenomena under the change from an original to a boosted physical system (such as a car), so apparently he considers the active version of the transformation here. He says that the condition of observing that the transformation has occurred can be satisfied in the case of a boost by noticing the change in the boosted system's position with respect to an unchanged reference. Meanwhile, the condition of observing the invariance is satisfied in the case of boosts because according to Kosso we observe that the outcomes of relevant experiments, the form of the equations of interactions and even the fundamental quantities such as the speed of light are the same in an original and in a boosted system [pp. 88-89]. So he concludes that theories must feature theoretical symmetries constituted by boosts because matching physical symmetries exist [p. 89].

Next Kosso passes to external local symmetries. His example on the theoretical side is the invariance of laws in general relativity under the transformations that are bestowing an accelerating or a rotating character on a system. On the physical side the observable difference condition according to him can be satisfied by us observing that we pass to an accelerating or a rotating physical system (such as a car or a train) with respect to an unchanged reference (such as an object on the ground). So apparently Kosso also uses the active version of the transformation here. However, the observable invariance condition according to Kosso is not satisfied in the case he is discussing, because the results of mechanical and electromagnetic experiments in the original system and in the accelerated system differ, and so, as he says, "the physics" is different [ibid.]. On the other hand, Kosso says [p. 90] that one can restore the invariance by adding to "the physics" gravity which exactly compensates for the acceleration. According to Kosso, the gravity transformation will have to be local and will thus require "that there be an interaction that can communicate the state of the transformed property from one part of a system to another" [ibid.]. For Kosso there will then be a freedom in specifying the properties of that interaction, but these specifications will have empirically testable consequences which may end up bestowing IES on theoretical external local symmetries. However, there is not going to be a DES for these symmetries by contrast to the DES of theoretical external global symmetries. So Kosso concludes that theoretical external symmetries have or do not have DES according to whether they are global or local respectively [ibid.].

Turning then to internal symmetries, Kosso also concentrates on active versions of transformations for them and reminds us that such versions involve a comparison between original systems and systems with changed properties. He then notes that the latter systems need not be obtained from the former by an actual physical transformation of relevant properties. Instead, the latter systems can be distinct systems exhibiting suitable physical differences in properties with respect to the original systems [p. 91].

Kosso next discusses DES of internal global symmetries on the basis of three examples [pp. 91-93]. One of them is the invariance of strong nuclear force under the theoretical permutations of protons with neutrons (in particular) and under the theoretical global transformations of hadronic isospin (more generally). With these theoretical symmetries Kosso matches a physical symmetry where the observable difference condition is realised by comparing an alpha particle stripped of two neutrons with an alpha particle stripped of a neutron and a proton [p. 92]. The observable invariance condition in this case is established to hold by comparing the measurements of the strong nuclear force for the neutron-neutron, neutron-proton and proton-proton pairs and by abstracting away the electromagnetic component from the latter pair [p. 91]. Another of Kosso's examples of internal global symmetries concerns the invariance of classical electrostatics under the electrostatic potential shifts. With this is matched on the physical side a symmetry where the observable difference condition is satisfied by measuring the difference in potential between a system and a grounded reference [ibid.]. Meanwhile, the observable invariance condition is satisfied in that case by the observable invariance of outcomes of electric experiments. One such experiment Kosso mentions is letting a pigeon to sit on a high-voltage wire as compared with its sitting on the ground, because in this case no electrocution occurs with the pigeon as much on the wire as on the ground [p. 92]. Finally, Kosso's third example concerns the invariance of quantum mechanics under phase shifts. With this he firstly proposes to match a physical symmetry where the observable difference condition is realised by taking systems situated in different locations, because as he notes a phase is generally a function of location [pp. 91-92]. However, to satisfy the observable invariance condition Kosso next proposes to recur to 't Hooft's [1980] thought experiment where electrons emitted by a source pass through a screen with two slits in it and form an interference pattern on the second screen, thereby manifesting their quantum-mechanical properties. On Kosso's proposal the observable invariance condition will be satisfied in this context if we observe that this observable interference pattern is the same despite us establishing that a phase shift was performed [p. 93]. This context actually gives Kosso other ways in which one could realise the observable difference condition. Indeed, Kosso says [ibid.] that the invariance of the observable interference pattern under phase shifts can be established to hold by changing the distance between the source and the slits, or by interacting with the particles before they reach the slits, or, as 't Hooft [1980, p. 115] suggests, by interacting with the particles after they pass the slits by placing a half-wave plate behind each slit. Kosso thus concludes [p. 93] that the situation with internal global symmetries is analogous to that with external global symmetries in that observable physical symmetries of either kind exist and hence corresponding theoretical symmetries of either kind must be there in our theories as well. Kosso notes in addition [p. 92] that internal global symmetries of classical electrostatics also have IES by virtue of implying the conservation of electric charge, but as said this is actually common to all the four kinds of symmetries he considers.

It remains for Kosso to determine whether theoretical internal local symmetries have DES. He explains first of all that this amounts to determining whether gauge symmetries have DES, because according to him in its most common meaning, and hence the one he will adopt, the term 'gauge symmetry' designates precisely internal local symmetries [pp. 93-94]. He has also said in his previous subsection that all his three examples of theoretical internal global symmetries also have localised versions [p. 91], and so he now mentions these versions as examples of theoretical internal local symmetries [pp. 93-94]. However, of them all Kosso only discusses the localised version of the theoretical phase symmetry in detail [pp. 94-95]. For him it could be matched with a setup of 't Hooft's beam-splitter where the observable difference condition is realised by the addition of a half-wave plate behind just one of the slits, because Kosso following 't Hooft thinks that this amounts to realising a local phase transformation. The observable invariance condition would then have to be realised again by the invariance of the observable interference pattern under such an addition, but this invariance happens not to hold. (Kosso refers to this setup as being described in ['t Hooft 1980, p. 116]. In fact the correct reference is [ibid., p. 115], while in [ibid., p. 116] one learns that the effect of making a phase shift with a half-wave plate can be mimicked by the application of the electromagnetic field using a shielded magnet. This seems to be one of Kosso's sources of inspiration, along with the analogy with general relativity, for what he says next.) Kosso says then that one can again restore the invariance by adding an interaction, in the case of local phase transformations the electromagnetic one, and then one can test its consequences to see whether IES obtains. In the case of quantum gauge theories such consequences for Kosso are predictions concerning gauge bosons, so he takes IES to obtain in the electromagnetic case because he takes predictions concerning electromagnetic bosons, i.e. photons, to be confirmed. So Kosso concludes that the situation with internal local symmetries in this case is entirely analogous to that with external local symmetries [p. 95]. In the case of other gauge theories he is more reluctant to attribute IES because their predictions concerning massless bosons are not confirmed, but he mentions the possibility of such bosons' acquiring a mass via the Higgs mechanism [pp. 95-96]. This remains just a possibility for him because Kosso writes in 2000 while the Higgs boson was only declared to be detected at CERN in 2012. After that he would probably take this mechanism to be confirmed and so would attribute IES to symmetries of the other gauge theories too. As with external symmetries, Kosso ends up concluding that internal symmetries do or do not have DES according to whether they are global or local respectively [p. 96].

Unsurprisingly, Kosso's general conclusion stated ***in Section 5*** is then that the distinction between theoretical symmetries that have and those that do not have DES coincides not with the external/internal distinction but with the global/local distinction [pp. 96-97]. He also makes a couple of additional conclusions. One is that in the case of global symmetries the existence of observable physical symmetries makes us introduce corresponding theoretical symmetries, while in the case of local symmetries it happens the other way round, namely it is the existence of theoretical symmetries that makes us check their predictions by observation [p. 97]. Another is that given observable physical global symmetries he has discussed there must be corresponding theoretical global symmetries in our theories, while given the absence of observable physical local symmetries he has discussed there can but need not be corresponding theoretical local symmetries in our theories [ibid.]. Thus theoretical local symmetries come out to be less close to observation than theoretical global symmetries. This is where Kosso finds a link with Wigner's geometrical/dynamical distinction, because Kosso takes Wigner [1967, p. 15] to say that dynamical symmetries are much less close to observation than geometrical symmetries, and Kosso identifies local symmetries with Wigner's dynamical symmetries, so for Kosso both happen to say the same [p. 97]. (In fact what Wigner qualifies as being less close to observation is very unclear in his [1967, p. 15], but nevertheless he should have thought that dynamical symmetries are more remote from the empirical level, given that as said Wigner understood geometrical symmetries as symmetries of events and dynamical symmetries as symmetries of laws that summarise events.) Kosso likewise finds his own conclusions to be in agreement with the views of 't Hooft.

Section II. 1. 3. A critical analysis

Subsection II. 1. 3. 1. Positive aspects and importance

Let us state to begin with what is so good and important about Kosso's text.

One thing is that he is the first to my knowledge to explicitly introduce and discuss DES. This is to be contrasted for instance with such earlier articles as [Brown and Sypel 1995] as well as [Budden 1997]. They discuss in particular what can or cannot be matched on the theoretical level with what will be later called Galileo's ship empirical symmetry, and this is actually the topic that will later be thought of as a paradigmatic part of the discussion on DES. However, in these articles this topic is considered without such themes typical for DES as its implications for the ontology of theoretical symmetries and its generalisation beyond the relativistic case. Instead, these articles consider this topic through its links with the principle of relativity, which is itself a traditional topic within the study of spatiotemporal and gravitational theories. Only from Kosso's [2000] article on do we find a discussion of matchings between theoretical and physical symmetries constituted by boosts as an instance of a more general relationship that involves some external and internal theoretical symmetries only and results in bestowing on them DES with its specific ontological significance. Brown has only written on DES in this sense after Kosso, namely in his article with Brading [Brading and Brown 2004]. This will be the text we will consider in the next chapter.

Secondly, Kosso introduced some vocabulary and some ideas that have remained accepted in all or most of the subsequent literature on DES. This is sometimes to the good and sometimes to the bad, but anyway shows the importance of his work. Among examples of what persisted since his article are the term “direct empirical status” (although 'status' became usually replaced by 'consequences'); the distinction between DES and IES; the idea that IES primarily arises by virtue of theoretical symmetries' implying conservation laws via Noether's theorem(s); the idea that to observe an observable physical symmetry one has to observe both that a transformation has occurred and that an invariance obtains; the discussion of physical symmetries involving boosts, those involving acceleration and gravity, those involving electrostatic phenomena and those involving 't Hooft's beam-splitter as four sorts of observable physical symmetries of relevance to DES; the idea that gauge symmetries do not have DES; the idea that whether theoretical symmetries have DES or not depends primarily on the properties of their transformations; the idea that the external/internal distinction does not coincide with the distinction between theoretical symmetries that have DES and those that do not; and finally the question whether the latter distinction coincides with the distinction between theoretical symmetries with global transformations and theoretical symmetries with local transformations. Thus the subsequent discussions of DES are built to a considerable extent on Kosso's legacy.

Thirdly, the way Kosso's discussion is organised has some merits. For one thing, it is systematic at a large scale in that the first three sections lay down some elements of Kosso's general framework and then the fourth section combines them to obtain specific results. In particular, the use of two distinctions from Section 2 in Section 4 structures the latter and even allows Kosso to dismiss one of these distinctions as irrelevant for DES. For another thing, as much the general sections as the application section are full of ordinary (snowflake) and scientific examples, so that we are constantly getting a concrete understanding of what Kosso's discussion bears on.

Subsection II. 1. 3. 2. The physical level: problems with the active/passive distinction

Despite the positive aspects mentioned above, there are also many serious problems with Kosso's discussion which turn out to invalidate most of his analysis.

To begin with, several problems concern Kosso's use of the active/passive distinction. To remind, in Section 3 when discussing how to observe a physical symmetry Kosso introduces following Rosen the distinction between active and passive (versions of) transformations. There Kosso says that an active transformation involves changing a system with respect to an unchanged reference while a passive transformation involves changing a reference with respect to an unchanged system [p. 87]. He then says that realising either transformation amounts to satisfying the observable difference condition and hence allows us to establish a physical symmetry provided the observable invariance condition is satisfied too. One problem with this is that this is incompatible with what Kosso says a page earlier, namely that to observe a physical symmetry of interest to physics one always has to observe two systems, a changed one and an unchanged one, his example being that for observing a physical symmetry under translations one has to observe phenomena both here and there [p. 86]. This last means of observing physical symmetries is incompatible both with the passive version of transformations, because no change of reference is envisaged in this last means contrary to that version, and with the active version of transformation, because that version deals with the same system in two different states while this last means admits or consists in taking distinct systems in different states. A further problem is that Kosso actually needs this last means of observing physical symmetries, because as I mentioned above he only uses this means when discussing internal theoretical symmetries in his Section 4. On the other hand, as also mentioned when discussing external theoretical symmetries in the same section Kosso only uses active versions of transformations instead. So more precisely he uses either the last means or active versions of transformations but not passive versions of transformations. In other words, he either considers the same system in an original and a transformed state together with an unchanged reference, or two distinct systems one of which is in the former and another in the latter state together with an unchanged reference, but not the same reference in an original and a transformed state together with an unchanged system, nor, we can add, two distinct references one of which is in the former and another in the latter state together with an unchanged system.

Concerning the reconciliation between the two means he uses, Kosso's solution later on happens to count the option of considering two distinct systems in different states as another way of realising active transformations along with the way where one takes the same system in two different states [p. 91]. This solution will also be adopted later on by Brading and Brown [2004], but in their case this is done from the outset [ibid., p. 646], while Kosso adopts it after having admitted an incoherency earlier in his text [pp. 86-87]. To avoid it he would have to introduce this solution at that earlier point instead, claiming there that one should always consider either two distinct systems in different states or the same system in different states rather than claiming first that one should always consider two systems in different states and then turning a page later to the case where the same system in different states is considered.

That is not all, however, because Kosso also needs to explain why discard passive versions of transformations, i.e. changes of references alone. Kosso only seems to be concerned with that when discussing internal symmetries, where he claims that in their case "the active interpretation of the transformation is easier to visualize than the passive interpretation" [p. 91]. At the end of this phrase he makes reference to Redhead's [1988, p. 16] stronger claim by which internal symmetries have only active interpretations. After that Kosso proceeds to reminding us what the active version of a transformation is and continues by distinguishing between the two ways of realising it, thereby implementing the solution that I have just explained. So apparently Kosso takes Redhead's and his own distinction between active and passive interpretations of a transformation in the claims just mentioned to be synonymous with Kosso's and Rosen's active and passive versions of a transformation and their amendment via Kosso's solution. But most probably Redhead's active/passive distinction from [1988, p. 16] is different from Kosso's, because although Redhead does not define his own distinction in [ibid.], we saw that he does so in his [1982, p. 82]. And there, to remind, Redhead takes a theoretical transformation to be interpreted actively when it is matched with a transformation of a physical system that changes the identity of that system, while he takes a theoretical transformation to be interpreted passively when it is matched with the fact that merely a description of a physical system changes. So firstly, Redhead's distinction is about matchings between theoretical symmetries and physical symmetries while Kosso's distinction is about the physical level alone; and secondly, in so far as these distinctions can be compared Redhead's active interpretation is stronger (less deflationary) than Kosso's active version, because Redhead excludes the option of taking distinct systems in two physical states, while Redhead's passive interpretation is weaker (more deflationary) than Kosso's passive interpretation, because Kosso excludes changes of conventions that do not bring about a physical change in a reference. So the two distinctions are different, and in addition, as I argued above, Redhead's claim in [1988, p. 16] about internal symmetries' only having the active interpretation is wrong, so the appeal to Redhead hardly constitutes any support for Kosso.

It may seem that passive versions of transformations can be discarded simply by saying that active versions are of more interest to us (or to Kosso). Or perhaps one can propose a bit more sophisticated argument along the same lines saying that if we define a system as that part of the world which (besides being dynamically closed etc.) is of interest to us and a reference as that part of the world which (while helping us in some ways) is not of interest to us per se, then whatever the thing whose transformations or changes we are considering, it will be a system and not a reference (or it will become a system by definition despite us taking it to be a reference previously), so that passive versions of transformations are automatically discarded. That seems to be a good argument in that it relies on a pragmatic difference between the notion of system and the notion of reference which I think is indeed warranted by the ordinary use of these notions in physics.

Still, this argument does not actually support Kosso's point. For it only helps us to discard passive versions where keeping references unchanged is admissible. However, empirical symmetries seem special in that they do require changing what we usually count as references. To see this, note that to establish the invariance of experimental results within a car or a Galileo's ship before and after it is boosted we need to be boosted with it if previously we were at rest with respect to it. Indeed, otherwise for instance a ball released from a mast of the ship will look to us as falling following a straight line before the transformation and as falling following a curve after it. This is the point that no literature on DES addresses properly, so we will not have an occasion to discuss it in more detail until we get to my own account of DES in Part III. In the meantime, note that if it is right that establishing an observable empirical symmetry requires changing what we usually take to be a reference besides changing a system, then discarding passive versions of transformations and keeping active versions alone in the context of establishing observable physical symmetries leading to DES does not make any sense, because no pure active version or pure passive version will ever lead to such a symmetry, but only their mix will lead to it. So Kosso's and the others' mistake is not even to privilege one version over another but to think that they can be separated in our context while in fact they cannot.

Subsection II. 1. 3. 3. The theoretical level: problems with the global/local and the internal/external distinctions

The next problem with Kosso's account concerns distinctions on the theoretical side. This one is brief to explain and consists in that these distinctions are not well defined. The global/local distinction, in particular, is defined through several expressions. Namely, to remind, a symmetry for Kosso is global if the transformation "is done uniformly through space and time", "is not a function of space and time", "is a constant, the same everywhere and for all time" [p. 84], and a symmetry is local if the transformation "is a variable function of space and time", "is variable, different in different locations and times", "becomes an arbitrary function of space and time" [pp. 84-85]. These expressions are similar but not identical in intension, so they may and in fact do have different extensions as well. Moreover, these expressions are such that they even allow the same transformation and hence the same symmetry to be both global and local. For instance, we may have a transformation of which we can say that it "is done uniformly through space and time" because of being smooth and yet "is a variable function of space and time" because of attributing different changes in value of some variable at different spacetime points. That is a serious problem, because the central outcome of Kosso's analysis is supposed to be that only global but not local symmetries have DES. And now we see that with Kosso's definitions one cannot actually say for sure which symmetries are global and which are local to begin with. Meanwhile, the external/internal distinction, although at the end not favoured by Kosso, is not the less problematic in his approach. For, to remind, he takes a symmetry to be external if its transformation affects a spatial or temporal variable [p. 84]. And so it is unclear how an external transformation, which changes something about space and time, can be global or local on Kosso's definitions of the global/local distinction while these involve evaluating how a transformation behaves across space and time apparently thought of as fixed. To be sure, this problem can be solved e.g. if one defines a transformation as a collection of prescriptions of change on points of the original spacetime (compare with [Rosen 1990, p. 296]), but without doing something of this kind Kosso's claim that external global symmetries have DES has one more way to fail to hold.

Subsection II. 1. 3. 4. DES: unwarranted generalisations, mixed levels and missing conditions

More problems are found in the matchings between theoretical symmetries and observable physical symmetries from Section 4. The first problem is that even if we suppose them to be correct, they will be unable to warrant the more general claims that Kosso makes on their basis, namely that global symmetries have DES while local symmetries do not, as well as any other general claims as to theoretical symmetries of which kind have or do not have DES. For these matchings only concern specific theoretical symmetries and specific observable physical symmetries, and no reason is given by which their analysis has to be generalisable to other cases featuring theoretical symmetries of the same kind. So at best Kosso is right that a given theoretical symmetry has DES by virtue of being matched with a given observable physical symmetry, but he need not be right as to which kinds of theoretical symmetries have DES and which do not even supposing that he has classified all the theoretical symmetries into some well-defined kinds. To do so he would have to demonstrate that there exists a general link between say being global in some well-defined sense and having DES, and a couple of examples satisfying that supposition is not enough to warrant it.

Moreover, Kosso need not even be right as to which among the theoretical symmetries he has considered do not have DES, because those theoretical symmetries that do not match with the observable physical symmetries he has considered may happen to be matched with other observable physical symmetries he has not considered. This is indeed what happens with external local and internal local theoretical symmetries to which Kosso did not attribute any DES, for actually observable physical symmetries can be found with which these theoretical symmetries can be matched. What can be matched with some external local symmetries is actually Einstein's elevator empirical symmetry. We will see an example of such a matching in [Greaves and Wallace 2014, Section 6]. As to local internal symmetries, on my view they can be matched with different versions of 't Hooft's beam-splitter empirical symmetry that originate in ['t Hooft 1980]. In particular, as said Kosso incorrectly quotes [ibid., p. 115] as [ibid., p. 116], but does seem to have read the latter page as well. On that page, among other things, is actually described a version of 't Hooft's beam-splitter empirical symmetry consisting of a state where the beam-splitter features one half-wave plate alone and of another state where it features a shielded magnet alone (where, we may add, the role of this magnet can be played by a switched-on solenoid). As 't Hooft explains, the magnet can be so chosen (e.g. through the choice of a suitable electric current in the solenoid) that both states yield the same interference pattern as illustrated in [ibid., p. 110, bottom row]. So Kosso's observable invariance condition is satisfied in a setup consisting of these two states. Meanwhile, the observable difference condition is satisfied in such a setup too, because whether a state features a half-wave plate or a shielded magnet (e.g. in form of a solenoid) is an observable difference. Hence Kosso would have to count the whole as an observable physical symmetry. It will then yield DES to internal local symmetries if these are to be matched with it. On my account this does happen as we will see.

Kosso does consider something like the setup in question, but when doing so he seems to mix the physical level with the theoretical level in a way that compromises his analysis. Indeed, recall that when discussing internal local symmetries Kosso asks whether we can make a symmetry using the first state above, i.e. the one featuring one half-wave plate alone. He notes correctly that this is not the case if we combine it with a state without any half-wave plates or magnets, for such a state yields an interference pattern different from the one obtained in the state with one half-wave plate alone (for an illustration see [ibid., p. 110, left column]). So what Kosso proposes then is that "[t]he theoretical account [be] amended with the description of an interaction that can cause the change in the experimental outcome" [p. 95]. In other words, it looks like he is proposing to compensate a physical and observable change in the state with one half-wave plate with respect to the state without any half-wave plates by a theoretical change! That is problematic, because no-one to my knowledge accepts compensations going from the theoretical level to the physical level. Understood literally, this would presuppose the ability of theoretical things to causally affect physical things, and that is usually denied.

Can we understand Kosso differently? It seems that we may alternatively understand him as saying that we need to postulate a physical entity corresponding to the theoretical description he mentions. In the case he is discussing such an entity apparently has to be the electromagnetic field (see [ibid.]). This understanding would make him speak of a compensation among physical entities, thus avoiding the critique just mentioned. In that case, however, we could seek a physical context in which this compensation is actually realised. One way of providing such a context is by arranging the setup I took from ['t Hooft 1980, p. 116] and described above. There 't Hooft apparently takes both states to be equally physical and the second state to involve the presence of an electromagnetic field. So if Kosso followed this route, he would have to assign DES to internal local symmetries on that basis. He does not assign it, however, because apparently he takes the compensation 't Hooft speaks about as being inter-level. So even in case Kosso was aware of the second state featuring the shielded magnet (whose illustration Kosso should have seen even if he only read up to [ibid., p. 115], because it can be found already at [ibid., p. 110]), apparently he did not think of considering that state as a physical state rather than a theoretical state. Thus although Kosso's proposal about the compensation looks ambiguous, what he concludes from it seems to go against the less problematic understanding of that proposal as concerning the physical level alone and seems to confirm instead that he does mix the theoretical level with the physical level in a problematic way.

Moreover, this is not something we find in Kosso's discussion of internal local symmetries alone, but actually something that we find in his text all the time. The closest example of the same flaw can be found in Kosso's discussion of external local symmetries. There he also finds an observable change and proposes that it be compensated by adding “to the physics a claim about a specific force that restores the invariance” [p. 90], the force this time being gravity. Recall that by “the physics” he means the forms of the laws of physics and the strengths of the fundamental interactions [p. 87]. So this notion is sufficiently theoretical to understand his proposal as that of adding theoretical rather than physical gravity to the picture, and it is this understanding that seems next confirmed by his conclusion that external local symmetries do not have DES.

Brading and Brown [2004, Section 4] also interpret Kosso's proposals in this way. For their reproach to Kosso's compensating proposals in his discussion of external local symmetries and of internal local symmetries is that “[m]ere coordinate transformations cannot be used to bring real physical forces in and out of existence” ([ibid., p. 661]; see also [ibid., p. 662]). Thus they also take Kosso's compensating transformations not to be physical in the sense of obtaining in the world. So this understanding of Kosso's text seems the most obvious to different readers. One problem with Brading and Brown's critique, on the other hand, is that as seen from this quote they seem to take Kosso's compensating transformations to be passive interpretations in Redhead's sense, because they characterise these transformations as coordinate transformations. I doubt however that Kosso himself understands his compensating transformations in Redhead's deflationary sense, e.g. because otherwise they would hardly lead to IES for him, but this does not invalidate the idea that Kosso's understanding of these transformations as theoretical seems plausible.

Some more examples where Kosso seems to mix the theoretical level with the physical level are found in his discussion of external global symmetries. For instance, he says that to establish physical symmetries under boosts we need to compare “experimental outcomes in two distinct reference systems that differ by a constant relative velocity” [p. 88]. This can be interpreted charitably as an instance of Kosso's passive version of transformation (if “reference systems” refer to references) or of Kosso's active version of transformation (if “reference systems” refer to physical systems of interest), because a couple of lines below Kosso speaks of apparently the same case as amounting to “observing a changing space-time location of the physical system with respect to physical objects of reference" [ibid.], which is an instance of the active version. However, in general "reference systems" most often refer to systems of conventions that may involve physical reference objects but are primarily theoretical themselves, and so Kosso's use of this expression quoted above may also suggest that he proposes to appeal to theoretical changes in conventions when establishing physical symmetries. The mixing of the two levels becomes even more flagrant when Kosso goes on saying that "it is possible to observe that the physics is invariant" under physical boosts [p. 89], where he has just defined "the physics" as "[t]he forms of the equations of interactions" and "the fundamental quantities such as the speed of light" [ibid.]. But the forms of equations are theoretical entities that nobody has ever observed because of them being theoretical, and nor could one observe their physical counterparts if we supposed these to exist, because these would have to be unobservable anyway. Likewise observing how physical boosts leave forms of equations invariant is impossible, while claiming that physical boosts do leave them invariant is trivial unless one admits that physical things such as boosts can have a causal influence on theoretical or abstract things such as forms, which is a presupposition usually denied as much as the presupposition mentioned above by which theoretical things causally affect physical things.

With the last quote one can hardly believe that Kosso's mixing of the two levels is simply due to a carelessness towards how he formulates his thoughts in a language. Rather, quotes like this suggest that he mixes the two levels in his thoughts in a way that makes his analysis highly questionable. In particular, the mixings just mentioned in the quotes from Kosso's discussion of external global symmetries give some reasons to think that even his positive matchings between specific theoretical symmetries and specific physical symmetries may not work like he probably thought them to. However, as we will now see there are more general reasons why Kosso's specific matchings are all invalidated. They consist in that he does not give us any explicit conditions under which theoretical symmetries are to be assigned DES. In other words, Kosso just does not tell us in general when a situation qualifies as a situation where a theoretical symmetry has DES and when it does not.

Wait, how can this be? First of all, did not Kosso tell us in his Section 1 that DES depends on whether we observe physical symmetries, and did not he gave us two conditions under which to observe them in his Section 3? Indeed, but in fact these conditions are neither necessary nor sufficient to get DES. To see that they are not necessary, recall Kosso speaking of observationally complete physical symmetries, i.e. those where the observable invariance condition holds but the observable difference condition does not hold. His example was the rotation of a snowflake during one's, say ours, absence from a room where the snowflake is placed: here the snowflake looks the same before and after the transformation, so the observable invariance condition holds, and we have not seen its changing, so observable difference condition does not hold. Now if we do not know that the snowflake was rotated, we may think of it as being untransformed, hence we will not identify the whole as a physical symmetry involving any actual transformations and so will not attribute any DES on its basis. But nothing seems to prevent such a symmetry from giving rise to DES of theoretical symmetries constituted by theoretical spatial rotations without us knowing that. So while we cannot establish DES without the observable invariance condition being satisfied, it seems that it need not necessarily be satisfied for DES to actually obtain, and a similar argument can be made about the observable difference condition. On the other hand, neither are the two conditions sufficient for DES to obtain. To see this note that these conditions are about physical symmetries only, or more precisely about them, a reference and their observer only, while DES is about physical symmetries together with theoretical symmetries. So no conjunction of conditions that does not concern theoretical symmetries at all can be sufficient for telling us when DES obtains, and thus in particular the conjunction of Kosso's two conditions above cannot be sufficient for this either.

On the other hand, did not Kosso provide some condition involving theoretical symmetries through his use of the external/internal and the global/local distinctions? Perhaps he can be blamed for being implicit, but implicitly at least it seems that he did have a criterion for matchings based on these distinctions. Indeed, in Section 4 he always proceeds by seeking for a theoretical symmetry of a given kind (e.g. external global) a physical symmetry of the same kind (e.g. also external global). So perhaps his implicit criterion for a theoretical symmetry of some kind to get DES is that there has to exist an observable physical symmetry of the same kind. But if that is his criterion, as we saw it is problematic firstly because his definitions of the two distinctions do not allow to clearly classify all the theoretical symmetries. However, in addition there happens to be a second problem brought about, along with many others mentioned above, by Kosso's mixing of the theoretical level with the physical level. The problem consists in short in that the criterion in question presupposes that we can classify physical symmetries using the external/internal and the global/local distinctions, while in fact we cannot do so because these distinctions are too theoretical themselves. Indeed, for example as said Kosso defines 'external' as featuring a transformation that "is a change of a spatial or temporal variable" [p. 84], but 'variable' cannot be but a theoretical notion. Similarly, one of his definitions of the global/local distinction is based on whether the transformation is or is not a function of space and time [pp. 84-85], where 'function' can likewise be understood theoretically. One could think that this means just that to apply such distinctions to physical symmetries one needs to introduce similar definitions where such theoretical terms are replaced by their physical analogues. But the problem is actually deeper, for even supposing that such amendments are done we still cannot classify a physical symmetry according to the external/internal distinction without making theoretical assumptions about which physical changes are spatiotemporal and which are not. And likewise, given that the global/local distinction for Kosso has to do with the uniformity or non-uniformity of a transformation across space and time, we cannot classify a physical symmetry according to the global/local distinction without making theoretical assumptions about which changes are uniform across space and time and which are not. Now as we have to recall Kosso does admit the use of theoretical assumptions as long as these do not condition the attribution of DES. But if Kosso's criterion for attribution of DES is as above, then the theoretical assumptions just mentioned are of precisely this kind. Hence we must either not use them or not take them to condition the attribution of DES, where the second option amounts to not using the criterion above. Either way Kosso's framework thus cannot actually tell us in which case theoretical symmetries have the right to be assigned DES. So all his conclusions about which specific theoretical symmetries have DES are disqualified in that we cannot take the matchings they are based on to be cases yielding DES.

Subsection II. 1. 3. 5. The two approaches

More generally, some of the basic questions about DES are not only unanswered by Kosso's text but even not asked in it. In particular, it is not asked what is the nature of the relationship giving rise to DES. But there are things to say on that question. For instance, this relationship has to be a relationship between theoretical symmetries and physical symmetries, so it should belong to a kind of relationships that is able to link something theoretical and something physical. Thus for example causal relationships, as we saw above, would usually be discarded by that reason. Moreover, it is not asked whether a relationship giving rise to DES is symmetric, i.e. going as much from the theoretical level to the physical level as the other way round, or asymmetric, i.e. going only in one of these directions, and in the latter case in which of these directions it goes. Answering this question is also important, because it may determine whether one's analysis of DES should proceed in one or the other direction. In this way we get to the question, likewise not asked by Kosso, whether when studying DES one has to follow the theoretical approach (that I define as a procedure where one moves from the theoretical level to the physical, particularly empirical (observational), level) or the empirical approach (that I define as a procedure where one moves from the physical, particularly empirical (observational), level to the theoretical level).

As Kosso always goes from the theoretical level to the physical level in his analysis in Section 4, he looks as favouring the former option, but no explicit motivation for this is given in that section. In his Section 1, on the other hand, Kosso does give an epistemic motivation for going from the theoretical level to the physical level rather than the other way round, namely that identifying physical symmetries seems less evident than identifying theoretical symmetries. However, after he has given the two conditions for observing physical symmetries in his Section 3, both theoretical symmetries and physical symmetries become identifiable and proceeding from the theoretical level to the physical level rather than the other way round ceases to be the only option available. In this situation one may still try to motivate Kosso's way of proceeding pragmatically by his interest in whether specific theoretical symmetries have DES. But if the relationship leading to DES happened to be symmetric, or if there are two asymmetric relationships leading to DES, one directed from the theoretical level to the physical level and the second the other way round, that are suitably linked (as happens in fact in my account), then proceeding from the physical level to the theoretical level could address Kosso's interest too.

What actually undermines the use of the empirical approach in Kosso's framework is not the absence of motivation from Kosso's side but the poor results this approach would yield him. That this would happen follows from the fact that Kosso's notion of observable physical symmetry, as defined by his observable difference and observable invariance conditions, is very weak. Indeed, it can be satisfied for instance as much by such dynamic symmetries as Galileo's ship empirical symmetry as by static symmetries of a snowflake, because this notion places no restrictions on whether a physical symmetry should be dynamic or static. Similarly, Kosso's notion is indifferent as to whether a physical symmetry exhibits a relational nature, which is a feature that will be later taken as characteristic of empirical symmetries. In fact this generality, or in other words weakness, of the notion of observable physical symmetry provides a reason why it has been replaced by a narrower notion of empirical symmetry in the later literature on DES. But it is also a reason why the empirical approach is quite useless for Kosso. For in that approach it is the symmetries on the physical level that provide a starting point for establishing DES, and the weaker this starting point is, the weaker are the results. This suggests of course that the empirical approach is saved once the narrower notion of empirical symmetry is introduced, but this idea happens not to be satisfactorily explored in the literature on DES. So we will have to wait until my Part III to thoroughly address it and its relationships with some other basic questions about DES, such as whether it is right or not that theoretical symmetries of laws have DES and that whether theoretical symmetries have DES is determined by the kinds of transformations they have, as Kosso thought.

Section II. 1. 4. Brief summary of the analysis

Kosso's text is important in that it is the first to explicitly introduce and discuss DES. It is also the source of the main question of all the literature on DES, which consists in asking whether theoretical symmetries' having DES depends on the properties of their transformations and more precisely on whether these transformations are global. Besides, Kosso's discussion has the merits of being systematic at a large scale and well illustrated by examples.

However, Kosso's analysis fails to hold because on the physical side he overlooks the need of changing physical references together with physical systems while on the theoretical side he does not define the external/internal and the global/local distinctions satisfactorily enough. In addition, contrary to what Kosso suggests his specific matchings between theoretical symmetries and physical symmetries do not warrant neither the general claims as to which kinds of theoretical symmetries do or do not have DES nor the specific claims as to which specific theoretical symmetries do not have DES. Furthermore, his specific matchings cannot even be said to warrant specific claims as to which specific theoretical symmetries do have DES, because Kosso does not provide us with any non-question-begging and necessary or sufficient criteria under which a theoretical symmetry is entitled to be assigned DES. More generally, Kosso does not address such basic questions as what is the nature of the relationship giving rise to DES, whether it is symmetric or asymmetric and what is the best way of establishing DES. In any case, he is forced to follow the theoretical approach for establishing DES because his notion of observable physical symmetry is too weak to make the empirical approach fruitful.

Chapter II. 2. Brading and Brown [2004][4]

Section II. 2. 1. Brief summary of the text

In Section 1 [pp. 645-648] Brading and Brown present DES, observable physical symmetries in general and Galileo's ship empirical symmetry in particular. In Section 2 [pp. 648-650] they discuss different ways of dividing theoretical symmetries into global and local ones. In Section 3 [pp. 650-658] they examine whether internal transformations lead to DES on the basis of 't Hooft's beam-splitter case. Subsection 3.1 [pp. 651-657] deals with local phase transformations considered separately in part 3.1.1 [pp. 651-654] and together with the electromagnetic potential transformations in part 3.1.2 [pp. 654-657], while Subsection 3.2 [pp. 657-658] deals with global phase transformations. Brading and Brown claim that firstly, local phase transformations alone cannot lead to DES because they do not constitute theoretical symmetries and cannot be matched with 't Hooft's beam-splitter empirical symmetry [pp. 651-654]; secondly, joint local phase and electromagnetic potential transformations constitute theoretical symmetries but do not lead to DES because they cannot be matched with this empirical symmetry [pp. 654-657]; and thirdly, what they call relative phase transformations can matched with this empirical symmetry while not qualifying as global phase transformations [pp. 657-658]. In Section 4 [pp. 658-662] Brading and Brown state briefly that external global symmetries have DES. They also argue that the situation with coordinate transformations considered as applied to matter alone and as applied to matter together with the metric is analogous to the situation obtaining with local phase transformations considered alone and together with the electromagnetic potential transformations respectively. In Section 5 [pp. 662-663] Brading and Brown claim that global symmetries whether external or internal and local symmetries whether external or internal may all have IES. In Section 6 [p. 663] they conclude that only external global symmetries have DES while internal global symmetries and local symmetries, whether external or internal, do not have DES.

Section II. 2. 2. Extended summary of the text

Brading and Brown's article consists of six sections. From these Section 1 deals with DES and observable physical symmetries, Section 2 with the definitions of the global/local distinction for theoretical symmetries, Section 3 with whether internal transformations lead to DES, Section 4 with whether external transformations lead to DES, Section 5 with IES and Section 6 is a conclusion.

In Section 1 [pp. 645-648] the authors begin by declaring their interest in whether symmetries of laws have DES [p. 645]. They define transformations of such symmetries as the transformations of variables in terms of which laws are specified such that they preserve the explicit form of the laws [ibid.]. They do not say what 'laws' are more precisely, but at the very least these have to be theoretical things because they are to be specified in terms of variables and variable is usually understood as a theoretical notion, as well as because Brading and Brown are asking whether their symmetries have DES while only theoretical symmetries can have DES.

Next Brading and Brown mention Galileo's ship empirical symmetry (without calling it neither an empirical symmetry nor a physical symmetry) as an example of a case leading to DES of

[4] Note that Brading and Brown [2004] as well as Greaves and Wallace [2014] do not use the pagination of 't Hooft's original article [1980], although it is to this original publication that they refer.

corresponding theoretical symmetries (which they call 'physical symmetries' apparently because these belong to physics; however, for uniformity in what follows I will continue to reserve the latter expression for symmetries in the world instead). They also mention three characteristic traits that are present in Galileo's ship case and so, as we may say, define a class of Galileo's ship-like physical symmetries. These traits are all mixed up in Brading and Brown's presentation [pp. 645-647], but it is useful to consider them separately so we will do so.

One trait is the fact of featuring a subsystem (e.g. a ship) transformed with respect to something else. On the one hand, Brading and Brown say that the transformation is done with respect to a reference [p. 646]. This seems to imply that a reference itself should not be transformed, which excludes Kosso's idea that a physical symmetry can be realised by transforming a reference without transforming a (sub)system. On the other hand, Brading and Brown say that the transformation is done with respect to all the things in the universe [ibid.]. This and the previous implication by which the reference should be untransformed imply that the reference must be located in that untransformed remainder of the universe and not in the subsystem. For Brading and Brown [ibid.], like for Kosso (see [Kosso 2000, p. 88, n. 1]), the reference should be observable.

The second trait present in Galileo's ship empirical symmetry is its observability. Here Brading and Brown adopt Kosso's [2000] observable difference and observable invariance conditions. They phrase them in terms of empirical distinguishability and empirical indistinguishability. They also take into account the previous trait concerning subsystems. As a result, they require more precisely that the transformation of a subsystem with respect to a reference yields a physical state that is overall empirically distinguishable from the original state but empirically indistinguishable as far as the internal evolution of a subsystem is concerned [p. 646]. They also say that when we are establishing their version of the observable invariance condition, a subsystem must be effectively isolated in the sense that its interactions with the rest of the universe are either negligible or irrelevant [p. 647, n. 5]. On the other hand, they allow this requirement not to hold when we are establishing their version of the observable difference condition, illustrating this by saying that they admit a shipwreck as an evidence that a ship is in a boosted state [ibid.]. As they note, this involves admitting that the observable invariance condition and the observable difference condition can be checked to be realised at different times [ibid.].

A third trait concerns how Galileo's ship empirical symmetry can be realised. In the case of a ship a physical transformation of boosting is physically implementable, i.e. we can actually perform it [p. 645]. By contrast Brading and Brown note that we can hardly boost a planet [p. 646]. To accommodate both kinds of cases they admit as instances of physical symmetries as much the cases where we obtain two different physical states of the same physical system by a physical transformation as the cases where we just compare two distinct systems in different physical states [p. 647, n. 5]. We may call these kinds of cases respectively *the first* and *the second way of realising physical symmetries*. We can also recognise in them Kosso's two ways of realising active versions of transformations and can see now that as announced Brading and Brown accept them from the outset after Kosso's coming to accepting them together towards the end of his analysis. We may also recall that Kosso's motivation for accepting the second way was its usefulness in the case of internal symmetries, but as we see with Brading and Brown's examples it is actually useful already for such more traditional symmetries as boosts. Indeed, the second way allows us in particular to get instances of Galileo's ship empirical symmetry for large objects such as planets by comparing a large object in some state and a distinct large object boosted with respect to it instead of trying to physically boost the same large object.

Besides the characterisation of Galileo's ship empirical symmetry (and of any physical symmetries that resemble them) in terms of the three traits Brading and Brown tell us in their Section 1 under which conditions on their opinion a theoretical symmetry is entitled to be assigned DES. This is an essential component of an account of DES that as I argued above was missing in Kosso's [2000] article. *Brading and Brown's conditions for attributing DES to a theoretical* *symmetry* given at [p. 645, p. 647, pp. 658-659, p. 663] are as follows: firstly, we should be able to interpret the theoretical symmetry as involving an observable transformation of an effectively isolated subsystem with respect to the rest of the universe, and secondly, we should be able to actually implement a physical realisation of this interpretation. Thus we may say that from Brading and Brown's conditions the first is *a theoretical condition on the interpretation* while the second is *a physical condition on the existence*. More precisely, the second condition for attributing DES requires that there exist effectively isolated subsystems satisfying Brading and Brown's observable difference condition, and Galileo's ship-like physical symmetries satisfy this by virtue of them having the three traits above, so they can serve as a basis for DES. Meanwhile, what is needed to satisfy the first condition for attributing DES is not said explicitly by Brading and Brown, although we may guess that this condition presupposes that one be able to apply a theoretical transformation on a restricted domain and that this transformation brings about new observational consequences.

As Brading and Brown have specified their conditions for attributing DES, they can determine which theoretical symmetries have DES by checking in which cases their conditions hold. Brading and Brown claim that their conditions are satisfied in particular for matchings of theoretical symmetries constituted by boosts, spatial translations and spatial rotations with respectively Galileo's ship empirical symmetry, physical symmetries constituted by physical spatial translations and physical symmetries constituted by physical spatial rotations [p. 647, pp. 658-659, p. 663]. So it follows from their account that these matchings yield DES to the theoretical symmetries just mentioned.

On the other hand, Brading and Brown claim that observationally complete theoretical symmetries do not have DES [p. 648, n. 7]. Hence in the case of these symmetries at least one of Brading and Brown's conditions for attributing DES must fail to hold. For support Brading and Brown refer to [Kosso 2000, p. 88] where observationally complete symmetries are discussed. However, recall that Kosso used this term there to designate physical symmetries that do not satisfy his observable difference condition. Apparently Brading and Brown use his term in a different though related sense, namely *they seem to call observationally complete symmetries* those theoretical symmetries whose transformations do not bring about any new observational consequences (which is the sense that I used to define one of the senses of the term 'gauge symmetry' in Part I). That such symmetries do not have DES on their account is unsurprising, for because of their transformations' not bringing about any new observational consequences these symmetries will fail to satisfy Brading and Brown's first condition for attributing DES which seems to only hold for theoretical symmetries whose transformations do bring about new observational consequences.

Just after their mention of observationally complete theoretical symmetries Brading and Brown say that DES fails to obtain for theoretical symmetries whose transformations act on the domain of the whole universe [p. 648]. *I will call transformations universal* if they act on a whole domain available and *restricted* if they act on a restriction of a domain available. So in these terms Brading and Brown say that theoretical symmetries constituted by universal transformations do not have DES. With a hindsight about some discussions of the ontology of theoretical symmetries in physics (such as Leibniz and Clarke's debate) one may think that Brading and Brown say so because they take theoretical symmetries constituted by universal transformations to belong to their category of observationally complete theoretical symmetries. However, note that theoretical symmetries constituted by universal transformations can also fail to satisfy Brading and Brown's first condition for attributing DES even in case they are not observationally complete in Brading and Brown's sense. Indeed, this condition seems to require not only that theoretical symmetry transformations bring about new observational consequences but also that they be applicable on restricted domains, i.e. be restricted in my terms. The latter requirement obviously fails to be satisfied for theoretical symmetries constituted by universal transformations even if they happen to satisfy the former requirement. Example here would be theoretical universal spatial reflections: they satisfy the former requirement because they lead to a difference in observational consequences for experiments involving weak interactions, but obviously fail nevertheless to satisfy the latter requirement.

Although Brading and Brown have thus been able to determine about many theoretical symmetries whether they have DES already in their Section 1, they have not yet told us much about which of the theoretical symmetries considered by Kosso [2000] have or do not have DES on their account, so this is what they will do in their Section 3 and their Section 4. Presumably to prepare this discussion Brading and Brown present ***in Section 2*** the external/internal and the global/local distinctions [pp. 648-650]. This is done for continuous symmetries only [p. 648], thus presumably excluding e.g. spatial reflections because of them usually being considered discrete. There is also a specific focus on gauge symmetries in this section in line with the focus on gauge symmetries announced by the title of Brading and Brown's article and most clearly implemented by their Section 3.

For Brading and Brown external symmetries involve transformations of spacetime coordinates while *internal symmetries* do not [ibid.]. Given the mention of coordinates this distinction apparently applies for them to theoretical symmetries. They do not say much more on that distinction and instead refer to Kosso [2000, p. 84] for details. To remind, Kosso defines this distinction in terms of variables rather than coordinates, but associates 'external' with 'spatiotemporal' just as Brading and Brown do (which is the usual practice for this distinction in physics). So apparently Brading and Brown more or less accept Kosso's definition of the external/internal distinction.

By contrast Brading and Brown find unsatisfactory Kosso's discussion of the global/local distinction [p. 648]. Indeed, to remind, Kosso used several rather vague ways of defining that distinction in his [2000, pp. 84-85]. So Brading and Brown devote the remainder of their Section 2 to the discussion of various definitions of the global/local distinction. We see from their definitions and examples that this distinction too applies for them to theoretical symmetries.

Firstly Brading and Brown tell us which ways of defining the global/local distinction they are going to discard. Among these is a distinction which amounts to my universal/restricted distinction introduced above; distinctions based on the notions of locality and non-locality as employed in quantum theory; and distinctions based on the notion of locality pertinent to relativity theory [p. 648].

After that they say that in the context of continuous symmetries and in particular *in the context of gauge symmetries the global/local distinction in their opinion* is none of the above but rather the distinction where *symmetries are called global* if they depend on constant parameters and *local* if they depend on arbitrary smooth functions of space and time [pp. 648-649]. On their view global in this sense are in particular Galilean spacetime symmetries constituted by spatial translations, temporal translations, spatial rotations and boosts, as well as internal symmetries constituted by phase shifts, while local in this sense are in particular the internal symmetry of electromagnetism (constituted by electromagnetic potential transformations, we may add) and the external symmetry of general relativity constituted by diffeomorphisms [p. 649].

Then, while apparently trying to explicate this 'right' global/local distinction for gauge symmetries, Brading and Brown present it as *a distinction that they attribute to Weyl* [*1918*], according to which *a length scale is global* if when fixed at one spacetime location it is fixed at all spacetime locations, while it is *local* if when fixed at one spacetime location it is not fixed at all spacetime locations because the lengths of two vectors are only directly comparable at the same spacetime location but not at separated spacetime locations [p. 649]. Next Brading and Brown say that the same global/local distinction is used by Yang and Mills [1954, p. 192] and quote from the page they refer to. On their opinion the quote shows that Yang and Mills wish to have an *isospin specification as to what counts as a proton* that would be *local* in the same sense as the sense in which a length scale is local in the distinction they attribute to Weyl [1918]. Namely, on Brading and Brown's opinion Yang and Mills wish to have an isospin specification as to what counts as a proton which when fixed at one spacetime location is not fixed at all spacetime locations. They note that while such a specification for some original location would thus not be valid in general for some other location, it would be valid for any nucleon at the original location [p. 649].

Next, while apparently still discussing the global/local distinction they attribute to Weyl [1918] and find instantiated by Yang and Mills [1954], Brading and Brown present it as *a third distinction* where *a transformation of a length scale is global* if it makes the same change at any point and *local* if the changes it makes vary smoothly but arbitrarily from point to point [p. 650]. They next declare that this third distinction is the one which actually applies to gauge transformations [ibid.], so that they seem to prompt us to identify it also with their first 'right' global/local distinction for gauge symmetries above.

To conclude Brading and Brown wish to contrast the global/local distinction they attribute to Weyl and to Yang and Mills with what I called the universal/restricted distinction above. To do so Brading and Brown introduce *a fourth distinction* by which *a Weyl-type local scale transformation applied to a system consisting of two subsystems* is a transformation which allows one subsystem to be freely rescaled relative to the other subsystem only when these are spatially separated but not when they are at the same spacetime location, while what Brading and Brown simply call *a scale transformation of one subsystem with respect to another* is a transformation which allows one subsystem to be rescaled relative to the other subsystem independently of whether they are at different spacetime locations or at the same spacetime location [ibid.].

We will return to Brading and Brown's distinctions in the analysis of their text below, but the first impression that their Section 2 gives is that they hardly do better than Kosso in solving the task of defining the global/local distinction uniquely. In fact they even manage to accept at least four distinct definitions of the global/local distinction where Kosso was satisfied by accepting three.

After that Brading and Brown pass ***to their Section 3*** where they say they intend to examine whether gauge symmetries have DES [pp. 650-658]. Given its length this can be easily identified as the central section of their article. Its discussion is restricted to DES which could or could not arise on the basis of different setups of 't Hooft's beam-splitter empirical symmetry. In Subsections 3.1 and 3.2 Brading and Brown examine whether respectively local internal and global internal theoretical transformations lead to DES. ***Subsection 3.1*** is further divided into two parts numbered 3.1.1 and 3.1.2 dealing with phase transformations alone and with joint transformations of phase and electromagnetic potential respectively.

In part 3.1.1 [pp. 651-654] Brading and Brown firstly consider the phase transformations expressed by their equation (1.2). These are the phase transformations of the form $\psi \rightarrow \psi' = \psi e^{iq\theta(\mathbf{x},t)}$, where ψ is a wavefunction, $e^{iq\theta(\mathbf{x},t)}$ is a phase factor, q is an electric charge and $\theta(\mathbf{x},t)$ is an arbitrary function of theoretical space points $\boldsymbol{x}$ and of theoretical moments of time t. As we may recall, transformations of this form are precisely *the local phase transformations* from my discussion of the gauge principle above, except that there I combined x and t into theoretical spacetime points x_μ with the help of the relativistic notation. When discussing these transformations above I classified them as local using the distinction common in the discussion of the gauge principle, namely a distinction by which *groups of transformations* are said to be *global* and *local* according to whether they essentially depend on one or more parameters or on one or more functions respectively. Brading and Brown also classify such phase transformations as local [p. 651], although they do not say which global/local distinction they are using.

Brading and Brown's question about these local phase transformations is whether the usual claim by which when considered alone these transformations do not constitute theoretical symmetries is correct. When answering this question they concentrate on the Schrödinger equation from quantum mechanics which they give as their equation (1.1) [p. 651]. This equation features a wavefunction ψ and hence is subject to phase transformations. Brading and Brown's answer is then that local transformations of ψ fail to constitute theoretical symmetries because the Schrödinger equation is not invariant under such transformations [ibid.]. Indeed, recall that for Brading and Brown theoretical symmetries are primarily theoretical symmetries of 'laws' and that 'laws' are usually taken to comprise equations of motion such as the Schrödinger equation. So, given that the local phase transformations in general do not preserve the form of the Schrödinger equation, they can be said not to constitute theoretical symmetries in Brading and Brown's sense. However, we may add that there could have been another equation which could also be subject to phase transformations and whose form could happen to be preserved under local phase transformations. Besides, even the form of the Schrödinger equation is actually preserved in the special case where the function $\theta(\mathbf{x},t)$ reduces to a parameter θ. In that case the phase transformations acquire the form $\psi \rightarrow \psi' = \psi e^{iq\theta}$, i.e. they become *the global phase transformations* from my discussion of the gauge principle above, classified as global according to the global/local distinction for groups employed there. So the form of the Schrödinger equation is invariant under the global phase transformations which make part (more precisely, constitute a subgroup) of the local phase transformations.

Note that when answering their question Brading and Brown interpret the wavefunction ψ as a wavefunction of a charged particle, namely an electron [ibid.], but at the end of their part 3.1.2 and at the end of their Section 4 they speak of matter fields instead (in particular, in the latter section an analogy is considered which if developed fully would imply that ψ concerns matter fields also in the Schrödinger equation). Besides, Brading and Brown say of their analysis from part 3.1.2 that it cannot go through without assuming that the electron beam approximates a plane wave rather than wavepackets [p. 657, n. 9], but at the same time they do not say clearly whether this assumption should be extended to the other parts of their analysis. So there is some disparity and vagueness in their text concerning the interpretation of ψ which seems to come from the fact that ψ gets interpreted in whatever way suits best a given part of Brading and Brown's analysis. Still, this does not affect Brading and Brown's argument we are now discussing in so far as local phase transformations when taken alone will in general not preserve the form of the Schrödinger equation however ψ is interpreted.

Most of Brading and Brown's part 3.1.1 happens to be devoted to a second question which asks whether there is an observable failure of invariance in the world that has to be matched with a theoretical failure of invariance under local phase transformations. Brading and Brown attribute the claim that this is the case to 't Hooft [1980] and Kosso [2000], as well as to Auyang [1995] and Mainzer [1996] and say at least of the first three authors that they take the relevant observable failure to be exemplified by the setup of 't Hooft's beam-splitter case where a half-wave plate is inserted behind one of the slits [pp. 651-652]. *I will number this setup (1)* for the purpose of my analysis below (illustrations to all the numbered setups can be found in the Appendix at the end of my work). To remind, in such a setup a physical state without a half-wave plate and a physical state with a half-wave plate exhibit observably different interference patterns, so we can see why the authors Brading and Brown mention took such a setup to be an instance of an observable failure of invariance in the world. Brading and Brown do not say whether they agree with this interpretation. Instead, they say that they deny that such a setup matches with local phase transformations. This is because according to them this setup is rather to be matched with what they call relative phase transformations.

To better understand the kinds of transformations Brading and Brown discuss in relation to the 't Hooft's beam-splitter case we need to know that one of their criteria for distinguishing between different kinds of theoretical transformations in this discussion will later [p. 658] turn out to be whether they act on properties of subsystems or of their parts. In the case of 't Hooft's beam-splitter the relevant property is the phase of a wavefunction, and as we learn later on [ibid.] Brading and Brown happen to denote a wavefunction as Ψ when characterises a subsystem and as ψ when it characterises a part of a subsystem. For most of their Section 3 they presuppose that the whole beam is a subsystem while its halves of which one goes through the upper slit and the other through the lower slit are parts of a subsystem. So they denote their wavefunctions respectively by Ψ, ψ_{I} and ψ_{II}.

Now when discussing the gauge principle and the invariance properties of the Schrödinger equation above I was qualifying the phase transformations of the form $\psi \rightarrow \psi' = \psi e^{iq\theta(\mathbf{x},t)}$ as local phase transformations and the phase transformations of the form $\psi \rightarrow \psi' = \psi e^{iq\theta}$ as global phase transformations without saying anything about whether ψ characterises a part of a subsystem or a subsystem or something else, because the global/local distinction for groups that I was employing there classifies these transformations as respectively local and global independently of what it is that ψ characterises and of whether it characterises anything to begin with. However, *for Brading and Brown* in their discussion of 't Hooft's beam-splitter *local phase transformations* turn out to only be those phase transformations of the form $\psi \rightarrow \psi' = \psi e^{iq\theta(\mathbf{x},t)}$ which act on Ψ, where they say to take such transformations to be local according to the global/local distinction they attribute [p. 652] to Weyl. Likewise, *for Brading and Brown* in their discussion of 't Hooft's beam-splitter *global phase transformations* turn out to be only those phase transformations of the form $\psi \rightarrow \psi' = \psi e^{iq\theta}$ which act on Ψ [pp. 657-658]. By contrast, *for Brading and Brown* in their discussion of 't Hooft's beam-splitter other phase transformations of the form $\psi \rightarrow \psi' = \psi e^{iq\theta}$ (in Brading and Brown's notation with θ replaced by Λ) that act on ψ_{I} or ψ_{II} alone are not counted as global phase transformations. Instead it is such transformations that Brading and Brown call *relative phase transformations* [p. 652].

As said, Brading and Brown's aim is to show that it is their relative phase transformations and not their local phase transformations that are to be matched with the setup of 't Hooft's beam-splitter case where a half-wave plate is added behind one of the slits. According to their argument this should be done because it is their relative and not their local phase transformations that yield predictions matching with what we observe in this setup. To formulate this argument they firstly name the interference pattern A an interference pattern exhibited by the initial physical state of the setup they are discussing, and they name the interference pattern B different from A an interference pattern exhibited by the transformed physical state of the same setup (the one where, as Brading and Brown suppose, a half-wave plate was added behind the upper slit) [p. 651]. They also suppose [ibid.] that the interference pattern A is correctly described by some wavefunction Ψ which can be presented in terms of its components as $\Psi = 1/\surd 2\ (\psi_{\mathrm{I}} + \psi_{\mathrm{II}})$ according to Brading and Brown's equation (1.3). So their task is next to determine which theoretical transformations of Ψ yield the interference pattern B. They say then [p. 652] that their relative phase transformations acting on ψ_{I} alone yield $\Psi'' = 1/\surd 2\ (\psi_{\mathrm{I}} e^{iq\theta} + \psi_{\mathrm{II}})$ according to their equations (1.4) and (1.5), while their local phase transformations acting on the whole Ψ yield $\Psi''' = 1/\surd 2\ (\psi_{\mathrm{I}} + \psi_{\mathrm{II}}) e^{iq\theta(\mathbf{x},t)} = \Psi e^{iq\theta(\mathbf{x},t)}$ according to their equation (1.6). They next claim that Ψ''' predicts the interference pattern A just like Ψ, while Ψ'' (erroneously also mentioned as Ψ''') predicts the interference pattern B [p. 653]. So their conclusion is that the transformation between the two physical states, which, we may add, can be expressed in terms of transformations of interference patterns as A → B, should be matched with their relative phase transformations $\Psi = 1/\surd 2\ (\psi_{\mathrm{I}} + \psi_{\mathrm{II}}) \rightarrow \Psi'' = 1/\surd 2\ (\psi_{\mathrm{I}} e^{iq\theta} + \psi_{\mathrm{II}})$ and not with their local phase transformations $\Psi = 1/\surd 2\ (\psi_{\mathrm{I}} + \psi_{\mathrm{II}}) \rightarrow \Psi''' = 1/\surd 2\ (\psi_{\mathrm{I}} + \psi_{\mathrm{II}}) e^{iq\theta(\mathbf{x},t)} = \Psi e^{iq\theta(\mathbf{x},t)}$ [ibid.].

Brading and Brown's explanation of why their argument holds is that predictions depend on the relative phase between ψ_{I} and ψ_{II}, and this phase is only affected by their relative phase transformations that change ψ_{I} to $\psi_{\mathrm{I}} e^{iq\theta}$, while their local phase transformations only affect the overall phase by changing Ψ into $\Psi e^{iq\theta(\mathbf{x},t)}$ [ibid.]. This in turn they take to come from the fact that relative phase transformations can to change the phase of just one of the component wavefunctions located at the same point while local phase transformations can only change the phase of each of the component wavefunctions located at the same point by the same amount [ibid.]. So, we may add, on Brading and Brown's opinion local phase transformations are at best able to change the relation between phases of component wavefunctions at distinct points while relative phase transformations are able to change the relation between phases of component wavefunctions at the same point.

Brading and Brown next note that in their local phase transformations we could choose $\theta(\mathbf{x},t) = 0$ where only ψ_{II} is present and $\theta(\mathbf{x},t) = \theta$ where only ψ_{I} is present, so that in the regions where ψ_{I}

and ψ_{II} do not overlap we would get that the actions of their local phase transformations and of their relative phase transformations coincide [ibid.]. However, they say that the interference pattern (or rather its prediction, we may say) only arises thanks to the region where the component wavefunctions overlap, so they take these particular local phase transformations to be of no help for someone arguing that they are to be matched with the setup they discuss [ibid.]. This, we may add, is because for Brading and Brown only their relative but not their local transformations can make so that the wavefunctions in the overlap region yield the desired prediction by which the interference pattern changes.

Finally, Brading and Brown say [ibid.] that although the original wavefunction Ψ and the wavefunction Ψ'' resulting from the application to the original wavefunction of their relative phase transformations predict different interference patterns, both wavefunctions evolve according their equation (1.1), i.e. the Schrödinger equation for free charged particles. By contrast, they say that although, as just mentioned, their relative phase transformations and their local phase transformations can be made to coincide in the regions where the component wavefunctions do not overlap, the wavefunction Ψ''' resulting from the application to the original wavefunction of their local phase transformations evolves according to another equation they number (1.7), and this is what allows to that wavefunction to predict the same interference pattern as that predicted by the original wavefunction Ψ [pp. 653-654]. With this Brading and Brown conclude their discussion of whether local phase transformations lead to DES. We may summarise their claims on that question as being firstly that the local phase transformations taken alone do not constitute theoretical symmetries (of the Schrödinger equation) and secondly that Brading and Brown's local phase transformations are not to be matched with the setup of 't Hooft's beam-splitter case featuring a half-wave plate behind one slit because this setup is to be matched with their relative phase transformations instead.

As a preamble to the discussion of their next topic Brading and Brown end up their part 3.1.1 with some details about their equation (1.7) which on their opinion describes the evolution of the wavefunction Ψ'''. This equation is a modification of the Schrödinger equation according to which charged particles are no longer free but interact with an electromagnetic field instead. So while the Schrödinger equation for free charged particles only features a wavefunction ψ (which accounts for particles), the interaction equation features not only a wavefunction ψ but also the magnetic vector potential $\mathbf{A}$ together with the electric scalar potential φ also named the electrostatic potential (which is erroneously denoted as $\theta(\mathbf{x},t)$ just below the equation (1.7) [p. 654]). These two potentials can actually be combined with the help of the relativistic notation into the electromagnetic potential A_μ, so I will sometimes designate them together in this way. Now as I mentioned above the free Schrödinger equation is invariant under the global phase transformations from my discussion of the gauge principle, but, as Brading and Brown note, this equation is not invariant under the local phase transformations (which are not their specific local phase transformations of Ψ that figure in their discussion of 't Hooft's beam-splitter, but those general local phase transformations which also figure in my discussion of the gauge principle). Meanwhile, the interaction equation (1.7) is invariant under the transformations of ψ, $\mathbf{A}$ and φ expressed by Brading and Brown's three equations numbered (1.8) when these transformations are applied together. The transformations of ψ given there are just the local phase transformations which, when taken alone and not reduced to the global phase transformations, failed to constitute symmetries of the free Schrödinger equation (but Brading and Brown now give them in the notation where $\theta(\mathbf{x},t)$ is replaced by $\chi(\mathbf{x},t)$ and where the constants ħ (the Planck constant) and c (the speed of light) are inserted so that the charge q get divided by them). As to the transformations of $\mathbf{A}$ and φ given in (1.8), they actually amount to the transformations of A_μ which in the relativistic notation have the form $A_\mu \to A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$, where $\theta(x_\mu)$ is the same function as in the local phase transformations. And the latter electromagnetic potential transformations are actually *the local electromagnetic potential transformations* that we have also encountered in my discussion of the gauge principle above. So the three transformations from (1.8) are thus a combination of the local electromagnetic potential transformations and of the local phase transformations that we have encountered in the discussion of the gauge principle.

Perhaps you wonder why there is so much links between my discussion of the gauge principle and the material from Brading and Brown's Section 3. So I think it will be useful to make here *a digression about the relationship of the gauge principle to the invariance properties of the Schrödinger equation and of its interaction modification*. The relationship consists in that actually these invariance properties can be thought of as a consequence of the application of the gauge principle. Indeed, recall that the gauge principle as applied to the global group of phase transformations of the form $\psi \rightarrow \psi' = \psi e^{iq\theta}$ consists in that one firstly localises it, i.e. extends it into a local group of phase transformations via the replacement of a parameter θ by a function $\theta(x_\mu)$ (which is the relativistic notation for $\theta(\mathbf{x},t)$), and then introduces the electromagnetic potential A_μ which transforms usually as $A_\mu \rightarrow A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$, where $\theta(x_\mu)$ is the same as in the local phase transformations and the electromagnetic potential transformation is such that it yields a factor which compensates for the extra factor arising from the localisation just mentioned. This allows to pass from a free matter Lagrangian invariant under the global phase transformations but not the local phase transformations to the sum of the free matter Lagrangian and of an interaction Lagrangian such that this sum is invariant under the joint local phase and electromagnetic potential transformations. Now the free matter Lagrangian can serve to obtain the Schrödinger equation (see [Kobe 2007, Section 3][5]) via the Hamilton's principle that I have also discussed in Part I above, which means that the Schrödinger equation can be thought of as a Euler-Lagrange equation built of derivatives of that free matter Lagrangian. Because of that we should not be surprised that the Schrödinger equation has the same invariance properties as the free matter Lagrangian in question, namely this equation is likewise invariant under the global phase transformations but not the local phase transformations. Analogously, therefore, we should expect the interaction equation (1.7) to be invariant under the joint local phase and electromagnetic potential transformations under which the sum of the free matter Lagrangian and of the interaction Lagrangian is invariant.

In addition, thanks to the gauge principle there is one more thing we should expect with regard to the invariance under joint local transformations. Namely, recall that in the gauge principle one usually adds to the sum of the free matter Lagrangian and of the interaction Lagrangian a free gauge Lagrangian without this destroying the invariance of the total Lagrangian under the joint local matter and gauge potential transformations. So in particular in our case one can add to the sum of the free matter Lagrangian and of the interaction Lagrangian a free electromagnetic Lagrangian without this destroying the invariance of the total Lagrangian under the joint local phase and electromagnetic potential transformations. In our case this is enabled by the fact that the free electromagnetic Lagrangian is itself insensitive to the local phase transformations and invariant under the local electromagnetic potential transformations. Similarly, to the interaction equation (1.7) a free electromagnetic term can be added and the resulting equation will still be invariant under the joint local phase and electromagnetic potential transformations. So it follows that the sum of the free matter Lagrangian and of the interaction Lagrangian is invariant under the joint local phase and electromagnetic transformations independently of whether the free electromagnetic Lagrangian is present, and similarly that the equation combining the free matter part and the interaction part is invariant under the same transformations independently of whether the free electromagnetic term is present. This independence is going to play some role in the next part of Brading and Brown's analysis.

In part 3.1.2 [pp. 654-657] Brading and Brown discuss two questions similar to those from the part 3.1.1 but this time pertaining to local phase transformations when considered together with local electromagnetic potential transformations. The first question consists in whether the claim that joint local phase and electromagnetic potential transformations constitute theoretical symmetries is

5 This article is dated 2007 at its *arXiv* page, but in the article's text the date 2013 figures instead.

right, and here Brading and Brown's response is that it is because such transformations leave their equation (1.7) invariant. The second question which is longer to consider is whether the claim that theoretical symmetries constituted by joint local phase and electromagnetic potential transformations have no DES but only IES is correct. Brading and Brown wish to show that the affirmative answer to this question is right but that it should be supported by another evidence than that brought in its support by " 't Hooft and co." [p. 654], where the latter expression apparently designates 't Hooft together with the other authors Brading and Brown were disagreeing with in part 3.1.1, namely Kosso, Sunny Auyang and Klaus Mainzer.

To do so Brading and Brown firstly present the position of these authors in more detail. They say in particular that 't Hooft's [1980] and Auyang's evidence involves setups where a switched-on solenoid is added in between the slits and adjusted so as to compensate for the action of a half-wave plate behind one of the silts [pp. 654-655]. They also say that when such a solenoid is added instead of a half-wave plate, it yields the interference pattern B, while when added together with a half-wave plate, it yields the interference pattern A [p. 655]. So, we may add, if one takes the physical state with a half-wave plate behind one slit together with the physical state with the suitable solenoid in between the slits, one gets a setup where both states exhibit the interference pattern B, while if one takes the physical state with neither half-wave plates nor solenoids and the physical state with both a half-wave plate and a suitable solenoid, one gets a setup where both states exhibit the interference pattern A. *I will number these setups respectively (2b) and (2a)* for the purpose of my analysis below. Next Brading and Brown quote [ibid.] from Mainzer [1996, p. 423] and Auyang [1995, p. 56] who apparently discuss respectively the former and the latter of the setups I just mentioned. In either case the setup discussed seems to be matched with a theoretical invariance under joint local phase and electromagnetic potential transformations. A bit later Brading and Brown say that Mainzer and Auyang as well as Kosso and 't Hooft take such matchings to lead not to DES but only to IES [p. 656].

Brading and Brown's view is however that the denial of DES and the attribution of IES on the basis of such matchings is wrong because the matchings themselves are wrong. They give two arguments to support this. One of the arguments is given in a concise form. It consists in saying that the action of the suitable solenoid should also be matched with Brading and Brown's relative phase transformations [pp. 655-656]. Another of their arguments begins by stating that "the Lagrangian associated with the … equation (1.7) is locally gauge-invariant even when the electromagnetic fields happen to vanish … [So] [l]ocal gauge invariance alone does not require non-zero electromagnetic fields" [p. 655]. Here I guess Brading and Brown appeal to the fact I have explained a couple of paragraphs above, namely that the relevant total Lagrangian is invariant under the joint local phase and electromagnetic potential transformations independently of whether the free electromagnetic Lagrangian is present. The remainder of this argument goes as follows: "Therefore, the presence of a non-zero electromagnetic field cannot be necessary for the phenomena to exhibit local gauge symmetry. To put the point another way, the result of inserting a phase-shifter into one path in the above experiment can be described by the locally gauge-invariant equation (1.7), just as well as when the … solenoid is added. Local gauge symmetry is independent of whether there are electromagnetic fields present" [ibid]. I must confess that I do not see how to make sense of this remainder.

Finally, Brading and Brown refer us to their Section 5 for their explanation of why symmetries constituted by joint local phase and electromagnetic potential transformations have IES and meanwhile give us their explanation of why these symmetries have no DES [pp. 656-657]. Somewhat spelled out their argument consists in that to get DES such theoretical symmetries would have to be matched with an isolated material and electromagnetic physical system observably transformed with respect to another system, and to be able to be so matched such symmetries would have to be able to account for the observable differences resulting from the transformation of that physical system, but for symmetries to be able to account for such observable differences the theoretical transformations constituting these symmetries would have to bring about suitable differences in observational consequences, and this can only be done if these theoretical transformations induce changes in the electromagnetic field strengths and in the relative phase of the system's components; but joint local phase and electromagnetic potential transformations cannot induce changes of either quantities but can only change the overall phase of some system relative to another system as well as the electromagnetic potential in a way that does not affect observational consequences, therefore symmetries constituted by these theoretical transformations cannot account for the relevant observable differences and hence cannot be matched with the phenomenon in question [p. 656]. As a possible way out Brading and Brown next propose to consider a joint local phase and electromagnetic potential transformation restricted to one of two subregions of a region in which the whole system is located. They say however that this still does not yield the desired result, because if the electromagnetic potential is discontinuous on the boundary of these subregions and hence on the boundary of the subsystems located in these subregions, then the relative phase is not defined, while if it is continuous, then we get a special case of the local transformation on the whole region and hence no change in observational consequences as explained above [ibid.]. For them this last outcome is brought about by the fact that both the part of the system not affected by the original local phase transformations, which they present as a matter field, and the part affected by the original local phase transformations, which they present as another matter field, are embedded in the same electromagnetic potential field, and so, when the electromagnetic potential field also gets locally transformed in the same subregion as the transformed matter field but is supposed to go smoothly to the identity outside this subregion, this supposition induces a further phase transformation on the originally untransformed matter field located outside this subregion [p. 657]. This argument about the denial of DES for restricted joint local phase and electromagnetic potential transformations will be criticised by Greaves and Wallace [2014, pp. 82-83].

In Subsection 3.2 [pp. 657-658] Brading and Brown briefly discuss whether global phase transformations lead to DES. To do so they consider a setup of 't Hooft's beam-splitter case where there are two half-wave plates each of which is located behind a distinct one of the two slits [p. 657]. In this case the initial physical state where no half-wave plates are present and the final physical state where two half-wave plates are present yield the same interference pattern (namely, we may add, the interference pattern A). *I will number this setup (3)* for the purpose of my analysis below. Brading and Brown attribute to 't Hooft, Auyang, Mainzer and Kosso the claim that this setup should be matched with a theoretical symmetry constituted by global phase transformations and quote from Kosso to illustrate (it is said that they quote from [Kosso 2000, p. 83], but the correct reference is [ibid., p. 93], this is in Kosso's Subsection 4.3 where he argues in favour of DES of internal global symmetries). Again, Brading and Brown wish to show that something about the claim in question is wrong.

Here Brading and Brown do not contest the matching itself, for they match the setup in question with their global phase transformations [p. 657]. To remind, in the context of their discussion of 't Hooft's beam-splitter these are those phase transformations of the form $\psi \rightarrow \psi' = \psi e^{iq\theta}$ that act on Ψ. So what they match with the setup they are discussing are more precisely the phase transformations $\Psi = 1/\sqrt{2}\ (\psi_{\mathrm{I}} + \psi_{\mathrm{II}}) \rightarrow \Psi' = 1/\sqrt{2}\ (\psi_{\mathrm{I}} + \psi_{\mathrm{II}})e^{iq\theta} = \Psi e^{iq\theta}$ as expressed by their equation (1.9) [ibid.]. It is worth noting that as mentioned in the equation (1.9) this transformation passes by the stage $\Psi = 1/\sqrt{2}\ (\psi_{\mathrm{I}} + \psi_{\mathrm{II}}) \rightarrow \Psi' = 1/\sqrt{2}\ (\psi_{\mathrm{I}}e^{iq\theta} + \psi_{\mathrm{II}}e^{iq\theta})$ [ibid.]. What Brading and Brown contest in the position they attribute to 't Hooft, Auyang, Mainzer and Kosso is thus not the matching of the setup with two half-wave plates with global phase transformations but the idea that it leads to DES of symmetries constituted by these transformations. This Brading and Brown contest because on their opinion the setup involved is not an observable physical symmetry given that it yields the same interference pattern in both physical states and hence does not satisfy Kosso's observable difference condition (i.e. does not exhibit an observable difference in one physical state with respect to the other) [pp. 657-658]. Brading and Brown then answer to some possible objections to their claim. One objection consists in arguing that the physical phase transformation does occur in the setup involved when we pass from one of its physical states to the other on the grounds that the latter physical state features two half-wave plates. However, Brading and Brown renounce to take the presence of two half-wave plates as a sign of there occurring a physical phase transformation because they only admit as a suitable sign an observable difference [p. 658]. Another objection consists in arguing counterfactually that if a physical symmetry constituted by physical global phase transformations did not obtain in the world, then inserting two half-wave plates would show that, but as it does not show that, the physical symmetry in question does obtain. To this Brading and Brown respond [ibid.] that if inserting two half-wave plates showed that a physical symmetry did not hold, then we would know at least that this action changes the physical state (or more precisely the physical phase involved in the physical state, we may add), but as we do not know even whether this action makes such a change, we cannot know that it amounts to implementing a physical transformation (of phase, we may add).

As Brading and Brown thus deny that the setup with two half-wave plates in the second physical state and with the same interference pattern A in either physical state can yield any DES, they turn [ibid.] to the setup they discussed in their part 3.1.1 where just one half-wave plate is added in the second physical state and where the interference pattern changes from A to B. After that they proceed like in 3.1.1. Namely, again they do not say of this setup whether it satisfies the observable invariance condition (but do describe it in the middle of [p. 658] as satisfying the observable difference condition). Instead, after having denied in their part 3.1.1 that this setup is to be matched with their local phase transformations rather than with their relative phase transformations, they now likewise deny that this setup is to be matched with their global phase transformations rather than with their relative phase transformations. Recall that they were matching this setup with the phase transformations of the form $\psi \rightarrow \psi' = \psi e^{iq\theta}$ applied to the wavefunction of a half of the beam passing through one of the slits, and that for such transformations to be relative rather than global phase transformations in their framework one had to count that half of the beam as a part of a subsystem rather than as a subsystem. Now Brading and Brown ask [ibid.] whether one can count a half of the beam passing through a given slit as a subsystem instead, in which case the transformations they have matched with the setup in question would become global rather than relative phase transformations in their framework. More precisely, as we now see [ibid.] that Brading and Brown happen to denote wavefunctions of subsystems by Ψ and wavefunctions of parts of subsystems by ψ, the supposition that a half of the beam passing through a given slit counts as a subsystem would amount to matching the setup with a half-wave plate behind a single slit with global phase transformations $1/\sqrt{2}\ (\Psi_{\mathrm{I}} + \Psi_{\mathrm{II}}) \rightarrow 1/\sqrt{2}\ (\Psi_{\mathrm{I}} e^{iq\theta} + \Psi_{\mathrm{II}})$ instead of relative phase transformations $1/\sqrt{2}\ (\psi_{\mathrm{I}} + \psi_{\mathrm{II}}) \rightarrow 1/\sqrt{2}\ (\psi_{\mathrm{I}} e^{iq\theta} + \psi_{\mathrm{II}})$. However, Brading and Brown argue that the supposition in question cannot be adopted, i.e. that a half of the beam passing through a given slit cannot be counted as a subsystem because only the whole beam can be counted as a subsystem [ibid.]. Their arguments are that the decomposition of the beam on halves passing through distinct slits is basis-dependent; that if a half of the beam passing through one of the slits is taken away, then a half of the beam passing through the other slit can be counted as a subsystem but no interference pattern occurs; and that if the beam is composed of a single electron, then we cannot really say that its halves are subsystems but only that the whole electron is a subsystem, and this carries on to cases where the beam is composed of more than a single electron [ibid.]. So, because Brading and Brown only take phase symmetries to be global when their transformations act on subsystems and because they count a half of the beam passing through a given slit not as a subsystem but as a part of a subsystem they cannot match their global phase symmetries with the setup with a half-wave plate behind a single slit. Thus it follows from Brading and Brown's analysis that this setup too cannot yield DES to their global phase symmetries. This analysis will be criticised by Greaves and Wallace [2014, p. 83, n. 24].

In Section 4 [pp. 658-662] Brading and Brown discuss the status of external symmetries.

This is in particular where they claim [pp. 658-659] though without much detail that their conditions for assigning DES are satisfied for external global theoretical symmetries such as those constituted by spatial translations and spatial rotations. Their focus in this section is however on the status of external local theoretical symmetries. More precisely, they say being interested in theoretical symmetries constituted by arbitrary coordinate transformations that preserve the form of equations of motion [p. 659]. They assign the claim that such theoretical symmetries do not have DES but have IES to Rosen [1990] and Kosso [2000] and are going to argue that the arguments of these authors are wrong [p. 659].

To show what they are opposed to Brading and Brown give some quotes from Kosso [2000, pp. 89-90]. To remind, Kosso was claiming that we can observe a difference when passing from a physical system stationary with respect to the ground to a physical system accelerated with respect to the ground but do not observe an invariance within these systems under such a passage. He was then proposing to compensate the observable difference and thus to restore the invariance by an apparently theoretical gravitational force and was claiming that predictions following from the introduction of that force can, if they are confirmed, yield IES to external local theoretical symmetries. Brading and Brown agree with Kosso that the passage from a stationary to an accelerated physical system does not yield an observable physical symmetry [p. 659], but criticise what they take to be Kosso's view of the compensating role of the gravitational force in this situation. On their opinion that view consists in that the gravitational force should help us to describe the passage in question as a symmetry [ibid.]. But, as Brading and Brown argue, no description of that passage as a symmetry can be obtained neither when using Newtonian physics and Maxwell's equations both rewritten in generally-covariant form nor when using general relativity [pp. 660-661]. For according to them in either case a theoretical transformation that constitutes a theoretical symmetry will be a coordinate transformation that recoordinatises both the matter fields and the metric, while a theoretical transformation that matches with the physical passage in question will be a theoretical transformation that acts on the matter fields but not on the metric [ibid.]. So, as they take Kosso's theoretical transformation to be of the former kind, they do not match it or a theoretical symmetry it constitutes with the passage he discusses [ibid.].

Brading and Brown's positive views about Kosso's passage and what matches with it are as follows. Firstly, if there is an invariance associated to the passage it consists in that the failure of the passage to constitute a symmetry obtains in any coordinate system [p. 661]. Secondly, the actual role of the gravitational force is rather to provide an alternative explanation (with respect to the explanation by acceleration) of the failure to observe invariance in the passage in question [ibid.]. Thirdly [pp. 661-662], there are some analogies between theoretical transformations of the theories Brading and Brown considered in their Section 3 when analysing passages between physical states of 't Hooft's beam-splitter case and the theoretical transformations of the theories they considered in their Section 4 when analysing Kosso's passage between stationary and accelerated systems. One analogy Brading and Brown mention is that in the latter theories coordinate transformations acting on both the matter fields and the metric constitute theoretical symmetries and do not bring about any observational consequences, while in the former theories local transformations acting on both the matter fields and the electromagnetic potential fields constitute theoretical symmetries and do not bring about any observational consequences. Another analogy they mention is that in the latter theories coordinate transformations acting on the matter fields alone do not constitute theoretical symmetries and bring about observational consequences, while in the former theories local transformations of the matter fields alone do not constitute theoretical symmetries.

Section 5 [pp. 662-663] is where Brading and Brown treat IES most directly. Beforehand they were only making short remarks on this topic throughout their article. In particular, we saw them claiming in their part 3.1.2 that joint local phase and electromagnetic potential transformations lead to IES but not on the basis of what others take as the evidence for it. We also saw them denying that IES of external local transformations can be established by Kosso's arguments in their

Section 4. Meanwhile, in their Section 1 Brading and Brown actually made a more general claim, namely they were saying there that theoretical symmetries can have IES whether they have DES or not [pp. 647-648]. Now they explain [p. 662] that actually both external global symmetries (to which they assign DES) and internal global symmetries (to which they do not assign DES) have IES by virtue of Noether's [1918] first theorem that links them to conservation laws. In particular, we are told that the internal theoretical symmetry of the Schrödinger equation under the global phase transformations leads by that theorem to the time-independence of the normalisation [ibid.]. The usual formulation of the same consquence is that the symmetry in question leads by that theorem to the conservation of the electric charge. This is for instance what Brading and Brown write themselves in their [2000] and [2003, p. 98]. Anyway, there is usually no denial that global phase transformations lead to IES by virtue of Noether's first theorem, and the same is usually said at least of spatial rotations, spatial translations, temporal translations and boosts. As to local symmetries, Brading and Brown say that they have IES by virtue of Noether's [1918] second theorem and by virtue of a related result that they call the Boundary theorem (presented under this name in their [2003] and in Brading's [2005] and under the name of Theorem 3 in Brading and Brown's [2000]). They also give some details on that topic which we need not discuss. What we need to know about IES from Brading and Brown's works is firstly that as much local internal as local external symmetries can get IES via the ways Brading and Brown mention; secondly that, as Brading and Brown explain in their [2000], local symmetries can get IES in particular by leading to the same conservation laws that global symmetries do; and thirdly that, in agreement with what Brading and Brown seem to suggest e.g. in their [2003, p. 100], there is a way to take local symmetries to have more IES than global symmetries do.

Meanwhile, it remains for Brading and Brown to conclude ***in their Section 6*** [p. 663]. Their conclusion is that while external global symmetries satisfy their criteria for being assigned DES, this does not hold neither for internal global symmetries nor for local symmetries whether external or internal.

Section II. 2. 3. A critical analysis

Let us now rehearse Brading and Brown's discussion to see what holds and what does not.

Subsection II. 2. 3. 1. The physical level: the three traits

On the physical side Brading and Brown propose us three traits of Galileo's ship-like physical symmetries: firstly, it features a subsystem transformed with respect to the rest of the universe, where the latter includes an observable reference; secondly, the transformation of a subsystem brings about some observable differences (which may end up disturbing its isolation) but preserves the observable dynamics within the subsystem (as long as it remains isolated); thirdly, there can be a physical transformation linking two different physical states of the same subsystem or there can be just two different physical states belonging each to a distinct subsystem (that is what I called respectively the first and the second way of realising physical symmetries). These traits are mixed all together in Brading and Brown's presentation [pp. 645-647] and this can be criticised, but as we have distinguished them we can now also evaluate each on its own merit.

The first trait constitutes a restriction with respect to Kosso's position, for he allows an observable physical symmetry to be implemented as much through a transformation of a system with respect to a reference as through a transformation of a reference with respect to a system [Kosso 2000, p. 87], while the first trait only admits the former option. Nevertheless, Brading and Brown's restriction still shares with Kosso's position the underlying idea that transformations of physical systems and transformations of physical references should be put apart, while as I explained this idea has to be abandoned if one speaks of Galileo's ship-like physical symmetries.

This point can be well illustrated by Brading and Brown's own saying according to which phenomena within a Galileo's ship stay observably invariant relative to the cabin of the ship [p. 646]. Here the cabin serves as a reference for establishing invariance, but for this invariance to obtain it has to be boosted itself as much as the things inside the ship that participate in the phenomena there, so the cabin has to make part of what is transformed while staying a reference. A related disagreement I have this time with Brading and Brown alone is that because of their holding that a reference should not be transformed they seem obliged to place it outside the subsystem, while because of my admitting that references can be transformed I have no problem with placing them within subsystems. To see that this is reasonable just notice that the observable invariance of phenomena within a Galileo's ship's cabin also obtains with respect to an observer within the cabin (boosted as much as the other things there). This placement of references within a subsystem is one reason why the distinction between a subsystem and the rest of the universe, or what Greaves and Wallace [2014] call *the subsystem/environment split*, becomes less important for my approach, as will be further explained when analysing their text as well as in Part III.

By contrast, what retains a relatively important role there is the notion of system with its isolation and dynamical character as highlighted by Brading and Brown's second trait. This trait has an explicit predecessor in Kosso's observable difference and observable invariance conditions, and one sign that these conditions are useful is that they have survived to some extent or another for quite long in the literature on DES. But it is Brading and Brown who added to these conditions details as to what should be left observably invariant by the transformation (namely the dynamics interior to a subsystem) and how (namely by preserving a subsystem's observable isolation for some time besides bringing a subsystem into a suitable distinct physical state). This kind of details makes Brading and Brown's notion of Galileo's ship-like physical symmetry richer than Kosso's notion of observable physical symmetry. Hence their notion avoids the problems I mentioned for Kosso's notion, namely the possibility of being satisfied by too many phenomena and thus the impossibility to yield an interesting DES when proceeding via the empirical approach. On the other hand, there are also many other ways in which Kosso's notion could be enriched, so we may ask whether Brading and Brown's enrichment is in need of some motivation that would make us to prefer it to other options. However, in Brading and Brown's time perhaps any option could be tried, because the topic was not yet shaped. In any case, their choice was to incorporate the traits present in Galileo's ship case into Kosso's notion of observable physical symmetry. This was then taken up by Healey whose article [2009] will be the next for us to discuss, for he treated Galileo's ship case as paradigmatic when introducing his own modification of Kosso's notion under the name of empirical symmetry. Other recognised cases of empirical symmetries have then been added, so I think the current task is to formalise the notion of empirical symmetry on their basis, and as this has not been done yet, this is what I will do when presenting my own approach below. The ultimate motivation for such a definition of the notion of empirical symmetry, besides its accommodating the current use of the term, is that it does yield an interesting DES for instance in the sense of helping us to clarify the ontology of theoretical symmetries.

Whether Brading and Brown's third trait, i.e. the realisation of physical symmetries via the two ways, also comes from from Kosso's article or not, it can be found in the latter as I explained above. However, as I also mentioned Brading and Brown treat this trait in a more satisfactorily manner because of accepting it from their Section 1 while Kosso [2000] waits until his Subsection 4.3 to do so. On the other hand, both Kosso's and Brading and Brown's examples relevant to the third trait are useful. Brading and Brown's examples [p. 646] show that either way is realisable for Galileo's ship empirical symmetry, while Kosso's examples [2000, Subsection 4.3] show that at least the second way is realisable for other observable physical symmetries. This suggests that perhaps not all physical symmetries are realisable (in principle or in practice) via either way. So we may ask physical symmetries realisable via which way lead to DES. This depends on how DES is defined, and to some extent this is again a matter of choice, for one may choose to build into one's definition of DES whether it is to arise on the basis of just one of the two ways taken alone, on the basis of either way taken alone or on the basis of both ways taken together. A further question when both ways are admitted to give rise to DES is whether they both should be treated as equivalent bases for DES, particularly when an empirical symmetry is realisable via both of them. My answer is affirmative and I will give some arguments for this when describing my own views on DES.

When the three traits are considered together one easily notes that the formulations of the first and of the second trait are incompatible with the third trait because they are made in terms of transformations of a physical state of the same subsystem and so take only the first way of realising physical symmetries into account. Correcting this may lead us as far as abandoning the mention of the identity of subsystems and perhaps the mention of transformations as well. Indeed, to obtain the formulations of the two first traits for the second way we need to replace in the current formulations given above the mentions of different states of the same subsystem linked by a transformation with the mentions of different states of distinct subsystems. The first trait will then consist in that a physical symmetry features two distinct subsystems each in a different physical state with respect to the rest of the universe (and we will then have to make precise whether the latter expression for a given subsystem comprises all the rest including the other subsystem or excluding the other subsystem). Meanwhile, the second trait will then consist in that the two physical states of distinct physical subsystems have some observable differences (which may end up disturbing the isolation of their subsystems) but are observably identical as far as the dynamics within their distinct subsystems is concerned (as long as these subsystems remain isolated). From there different roads are available for obtaining formulations of the two first traits which would likewise suit for either way of realising physical symmetries. If we proceed by only leaving what is common to the two ways, it seems that we should anyway omit from the formulations we have so far the mentions of whether the subsystems are the same or different, but as to the mentions of transformations we may either omit them too or keep them but admit that they may involve changing a subsystem's identity. In the latter case however our description of physical symmetries becomes somewhat unnatural (e.g. it involves saying that not only two different states of the same ship are linked by a transformation (of changing the state) but also that two ships in different states are linked by a transformation (of changing the state and the ship's identity)). So the first option seems preferable. On this road the formulations are as follows: the first trait says that a physical symmetry consists of two physical states different with respect to the rest of the universe (where the latter notion is again to be made precise) while the second trait says that the two physical states have some observable differences (where an observable difference of a state may end up disturbing the isolation of a subsystem in that state) but yield the same observable dynamics within subsystems in these states (as long as such subsystems remain isolated). As we see, such formulations are made mainly in terms of physical states, which illustrates the usefulness of the notion of *physical state* in our context. The whole starts resembling a precise account of Galileo's ship-like physical symmetries. So Brading and Brown's description of Galileo's ship-like physical symmetries is good in that it provides grounds for developments of that kind, but as we see Brading and Brown do not carry on any such developments themselves (and then I do not agree with some of these developments as mentioned).

Subsection II. 2. 3. 2. DES and the two approaches

Similarly, as already said Brading and Brown should be praised for explicitly stating their conception of DES at all by contrast to Kosso, but as we will now discuss their statement is unsatisfactory at least because of being elliptic. Indeed, Brading and Brown's conditions for assigning DES to a theoretical symmetry given at [p. 645, p. 647, pp. 658-659, p. 663] are firstly that we should be able to interpret the theoretical symmetry as involving an observable transformation of an effectively isolated subsystem with respect to the rest of the universe and secondly that we should be able to actually implement a physical realisation of this interpretation. Here the first condition determines the satisfaction of which features the second condition should require from phenomena. And in the form stated the first condition implies that the second condition should require the existence of phenomena satisfying the first trait and some aspects of the two others, but does not require from these phenomena that they be realisable through the second way from the third trait nor that these phenomena satisfy such aspects of the second trait as the observable invariance of subsystems's interior dynamics and the possibility for the isolation to end up being destroyed as a result of the transformation of a physical state of the same subsystem or of our passage to considering a different physical state of a distinct subsystem. To some extent the omission of these details can be a matter of Brading and Brown's willingness to summarise their conditions for assigning DES shortly. Indeed, the missing details I just named are all mentioned in a note [p. 647, n. 5] whose purpose is precisely to enhance the short formulation of the two conditions from [p. 647]. Yet it gives the impression that Brading and Brown take the missing conditions to be unimportant and even dispensable. And if they do, it follows that the class of physical symmetries giving rise to DES is considerably larger than the class of Galileo's ship-like physical symmetries, because the latter satisfy the three traits while the former only need to satisfy around a half of them. This annihilates the value of much of the richness that as I have just shown Brading and Brown's description of Galileo's ship-like physical symmetries can be used to bring into Kosso's notion of observable physical symmetry, because as far as what gives rise to DES is concerned we are then back to a notion weaker than the notion of Galileo's ship-like physical symmetry. Moreover, one could think that perhaps the resulting notion is at least still stronger than Kosso's notion of observable physical symmetry (except in that the resulting notion does not admit transformations of references), but actually while stronger in some aspects the resulting notion is weaker in a crucial respect. Indeed, this notion is not just Kosso's notion with more details; instead, the resulting notion, the one satisfying the first trait concerning transformations of subsystems, the first way from the third trait and, from the second trait, the observability of differences resulting from the transformations and the isolation during the dynamics interior to the system, trades off for such details as subsystems, their dynamics, their isolation and their transformations an essential ingredient of Kosso's notion that was the presence of an observable invariance. Arguably, there is no symmetry without invariance, so the trade-off destroys the essence of the notion of physical symmetry and hence of DES following from it. In other words, it allows to get DES on the basis of phenomena that may turn out not to be physical symmetries at all. That is a dangerous implication which arguably must be precluded by treating the occurrence of an invariance as necessary. Brading and Brown do not make that explicitly because they have not noticed the danger.

There is also a converse issue with the class of Galileo's ship-like physical symmetries: while as just shown not all phenomena yielding DES have to be Galileo's ship-like physical symmetries in Brading and Brown's approach, it may also happen there that not all Galileo's ship-like physical symmetries are phenomena yielding DES. Indeed, Brading and Brown's formulation of their conditions for assigning DES presupposes that we only check whether the second physical condition is satisfied after having checked whether the first theoretical condition holds. So which phenomena we will be taking into account depends on which theoretical symmetries we consider to begin with. And which theoretical symmetries we consider depends on which theoretical symmetries we have and on which of them are of interest to us. So it may happen that some Galileo's ship-like physical symmetries are not taken into account because no theoretical symmetries we can and/or wish to consider happen to be such that the second condition requires the implementation of these Galileo's ship-like physical symmetries. Of course, all the same can be said of any other non-Galileo's ship-like phenomena that happen to be neglected in our enquiry about DES because of being irrelevant to whether the specific theoretical symmetries we consider have DES. Thus we get that because of a technical or pragmatic impossibility to consider some theoretical symmetries the number of phenomena capable of yielding DES turns out to be much lower in Brading and Brown's approach than it could be if we proceeded starting with a physical condition rather than a theoretical one. In other words, this is a situation which arises in the theoretical approach to DES (which starts with a theoretical condition), of which Brading and Brown's criteria for assigning DES are an instance, when compared with the empirical approach to DES (which starts with a physical condition). Here at least that restriction of the number of phenomena capable of yielding DES which has to do with our not willing to consider specific theoretical symmetries seems unsatisfactory. So this can be one reason in favour of adopting the empirical approach instead of the theoretical approach.

In fact Brading and Brown do adopt the empirical approach at least in their Section 3. Indeed, recall that in their Subsection 3.2 and in either of their parts 3.1.1 and 3.1.2 of their Subsection 3.1 they always start by considering some setup of 't Hooft's beam-splitter case and next ask which theoretical transformations it should be matched with. So they do move from the physical level to the theoretical level in that part of their analysis, and this may be praised by the partisans of the empirical approach. But of course this also raises a fatal reproach: how could Brading and Brown propose as their criteria for assigning DES something that is in blatant contradiction with their own analysis? The answer is that they did not go as far as making the distinction between the two approaches and determining that these are not equivalent. But although it should be noted in their defence that all the other literature on DES has been doing the same, this does nothing to cancel the fact that Brading and Brown's analysis is just incoherent.

Still, we may wish to overcome that incoherence in order to see whether there is something right in Brading and Brown's claims from their Section 3. For this we have to adapt their criteria for assigning DES, formulated within the theoretical approach, to the empirical approach they use in that section. This adaptation involves changing the order of these two conditions so that the physical condition now precedes the theoretical condition. But the details of what the physical condition says can be left the same: it is just that we should no longer consider them as dictated by the theoretical condition. We thus get again that according to the physical condition we should be able to actually implement a physical situation which satisfies the first trait and some aspects of the two other traits, namely a physical situation which consists in that a subsystem is transformed with respect to the rest of the world including an observable reference (the first trait), where the transformation yields an observable difference and a subsystem remains isolated for some time (aspects of the second trait) and where the transformation can be such that it preserves a subsystem's identity (the first way from the third trait). Taking into account my critique above, we may further enhance this physical condition for instance by requiring that in addition an extra aspect from the second trait holds by which the transformation also preserves some features, in which case we get a physical symmetry, or by requiring that all the three traits be fully satisfied, in which case we get Brading and Brown's version of Galileo's ship-like physical symmetry. Next the theoretical condition should come, and, as we are in the empirical approach, this time it is this condition that should be determined by the physical condition rather than the other way round. So we should make the theoretical condition a function of the physical condition in a way similar to that in which the physical condition previously depended on the theoretical condition. The original theoretical condition was saying that we should be able to interpret the theoretical symmetry in some way which the physical condition next required to be physically implementable. So the previous dependence was a matter of the asymmetric relation of physical implementability of a theoretical interpretation (asymmetric because it only goes in one direction, here from the theoretical level to the physical level but not vice versa). Therefore, the new dependence relation should be something like a theoretical implementability of a physical situation. Arguably, the dependence relation that suits is then the asymmetric relation (this time going from the physical level to the theoretical level) of theoretical representation of a physical situation. Or at least that is what Brading and Brown should agree with, because in their analysis of the setup (1) of 't Hooft's beam-splitter case their criterion for matching setups with theoretical transformations turns out to be whether they are able to represent what happens with the interference pattern there [p. 653]. The last detail to add is that as DES should be assigned to theoretical symmetries, the theoretical condition should require more precisely that the representation of the physical situation satisfying the physical conditions should be made by a theoretical symmetry. To sum up, *the reformulation of Brading and Brown's conditions for assigning DES to the empirical approach,* particularly in the context of 't Hooft's beam-splitter case, thus consists in that firstly the physical condition should hold by which we should be able to actually implement a physical situation that satisfies some or all of the three traits above, and secondly the theoretical condition should hold by which we should be able to represent that physical situation by a theoretical symmetry, and in particular we should be able to represent (and predict) by a theoretical symmetry transformation what happens with the interference patterns in a setup of 't Hooft's beam-splitter physical symmetry.

Subsection II. 2. 3. 3. 't Hooft's beam-splitter case: specific claims

Now we can start checking whether Brading and Brown's specific claims about DES from their central case study, namely the study of 't Hooft's beam-splitter case from their Section 3, are correct. As we recall, these claims there are as follows:

(1) the setup with neither half-wave plates nor switched-on solenoids in the initial state exhibiting the interference pattern A and with only a half-wave plate behind a single slit in the final state exhibiting the interference pattern B is to be matched with neither local phase transformations nor global phase transformations but only with relative phase transformations;

(2a) the setup with neither half-wave plates nor switched-on solenoids in the initial state exhibiting the interference pattern A and with both a half-wave plate behind a single slit and a switched-on solenoid in the final state exhibiting the interference pattern A is to be matched not with joint local phase and electromagnetic potential transformations but only with relative phase transformations applied twice, and this does not yield DES as the observable difference condition does not hold because of the interference pattern being the same in both states;

(2b) the setup with only a half-wave plate behind a single slit in the initial state exhibiting the interference pattern B and with only a switched-on solenoid in the final state exhibiting the interference pattern B is to be matched not with joint local phase and electromagnetic potential transformations but only with relative phase transformations applied twice, and this does not yield DES as the observable difference condition does not hold because of the interference pattern being the same in both states;

(3) the setup with neither half-wave plates nor switched-on solenoids in the initial state exhibiting the interference pattern A and with a half-wave plate behind either slit in the final state exhibiting the interference pattern A is only to be matched with global phase transformations, and this does not yield DES as the observable difference condition does not hold because of the interference pattern being the same in both states.

The numbering of the claims corresponds to that of the setups they mention.

Subsection II. 2. 3. 4. 't Hooft's beam-splitter case: some global/local distinctions

The first thing to say is that if the specific claims just listed held, this would shake Brading and Brown's own conclusions [p. 663]. For firstly these conclusions would then turn out to omit a crucial result, and secondly the presupposition they are based on would then be undermined. The presupposition is that the external/internal and the global/local distinctions have any relevance to DES. This presupposition is implied by the conclusions because they are formulated in terms of these two distinctions and because these distinctions are essential there in so far as DES is there attributed to some theoretical symmetries qua external global symmetries. What the specific claims above say, however, is that the setups (2a), (2b), (3) all fail to satisfy the observable difference condition because of featuring the same interference pattern (be that A or B) in both their states. It follows then that the setup (1) does satisfy the observable difference condition because of featuring different interference patterns in the two states. And arguably it also satisfies the observable invariance condition because all the other relevant observable features of the physical states (such as the properties of the source of the beam, the size of the slits, the distance between the screen with the slits and the second screen) except as to whether a half-wave plate behind a single slit is present seem to be presumed not to change. Moreover, the setup in question can be thought of as a change in a physical state of an isolated subsystem (consisting e.g. of the beam or of the beam plus the source and the screens) with respect to the rest of the universe. By these reasons the reformulated physical condition for assigning DES seems to be satisfied. Meanwhile, the reformulated theoretical condition is satisfied as well at least with respect to theoretical transformations. For firstly, Brading and Brown match their relative phase transformations with the setup (1) precisely because on their opinion they are able to represent (and predict) what happens with the interference pattern in that setup [p. 653]. And secondly, as I explained above, because of being of the form $\psi \to \psi' = \psi e^{iq\theta(\mathbf{x},t)}$ Brading and Brown's relative phase transformations are a particular case of the phase transformations that can be classified as global according to the global/local distinction for groups and that leave invariant the form of the Schrödinger equation, hence these relative phase transformations constitute theoretical symmetries in Brading and Brown's sense because of preserving the form of the Schrödinger equation. Therefore, Brading and Brown have to admit that their relative phase transformations in their framework should lead to DES by virtue of the fact that the setup (1) is matched with them. So firstly, where is this important statement in their conclusions? And secondly, it now follows contrary to the conclusions that at least the global/local distinction is irrelevant to DES at least in the context of 't Hooft's beam-splitter case, because what matters there is whether transformations are relative. And also all that is said on the global/local distinction in Section 2 is thus irrelevant to a DES arising from Section 3.

What to do? One way to rescue the relevance of the global/local distinction and of some part of Brading and Brown's Section 2 is to claim that the phase transformations with which Brading and Brown match the setup (1) (and hence also those with which they match the setups (2a) and (2b)) are actually a kind of global phase transformations despite Brading and Brown having classified them differently. That is not difficult to defend because Brading and Brown's relative phase transformations and what they classify themselves as global phase transformations (those with which they match the setup (3)) have the same form $\psi \to \psi' = \psi e^{iq\theta}$. But for Brading and Brown their global phase transformations differ from their relative phase transformations in that the former act on subsystems and the latter on their parts. So to defend the rescuing claim it would remain to overcome this difference. One possibility is to argue that a half of the beam passing through a single slit qualifies as a subsystem. But Brading and Brown's arguments to the contrary may seem convincing. So instead one can treat their global phase transformations and their relative phase transformations as distinct subcategories of a more general category comprising any phase transformations of the form $\psi \to \psi' = \psi e^{iq\theta}$ irrespective to what they act on. How should this category be named? Brading and Brown oppose to the phase transformations of the form $\psi \to \psi' = \psi e^{iq\theta}$ the phase transformations of the form $\psi \to \psi' = \psi e^{iq\theta(\mathbf{x},t)}$, and this distinction as we know corresponds to the specialisation to phase transformations of the distinction between global and local groups of transformations. Besides, it also corresponds to the specialisation to phase transformations of another global/local distinction that I will name *the global/local distinction in the broad sense*. I will define it by saying that *a theoretical transformation (and hence a theoretical symmetry constituted by it) is global in the broad sense* if it features one or more parameter(s) and *local in the broad sense* if it features one or more function(s). On the first distinction the phase transformations of the form $\psi \to \psi' = \psi e^{iq\theta}$ are a global group of phase transformations and on the second distinction they are global in the broad sense, thus either way they are global. So likewise

Brading and Brown's relative phase transformations as well as their global phase transformations (which we will now rename *global phase transformations in the narrow sense*) are global on either distinction because of being subsets of the phase transformations of the form $\psi \rightarrow \psi' = \psi e^{iq\theta}$. The relevance of the global/local distinction to Brading and Brown's relative phase transformations is thus restored. This in turn happens to save the relevance of some part of Brading and Brown's Section 2, because *the first global/local distinction Brading and Brown accept in their Section 2* consists in that *symmetries are called global* if they depend on constant parameters and *local* if they depend on arbitrary smooth functions of space and time [pp. 648-649]. This is just a specification (with respect to properties of functions) of my global/local distinction in the broad sense. So the overarching category of any phase transformations of the form $\psi \rightarrow \psi' = \psi e^{iq\theta}$ and hence its subcategory of Brading and Brown's relative phase transformations turns out to also be global in the sense of Brading and Brown's first global/local distinction. It is a pity they have not told us before! We would not then wonder whether any of the global and local transformations they describe in their Section 2 is of any relevance to the relative phase transformations. At the same time as restoring the relevance of the global/local distinction and of some content of Brading and Brown's Section 2 for their relative phase transformations, however, the move just described happens to definitely invalidate Brading and Brown's conclusions [p. 663]. For it now follows that some global phase transformations in the sense of at least any of the three global/local distinctions we have considered lead to DES, i.e. that some internal global transformations lead to DES, while that is what the conclusions were denying.

The three global/local distinctions for which the move just described works are inequivalent in both intension and extension, so to assess the specific claims (1), (2a), (2b) and (3) we now should say which of these distinctions (and of any other suitable global/local distinctions) we take these claims to employ. I am going to assume that all these claims employ my global/local distinction in the broad sense, because it allows us not to bother with properties of functions while remaining close to Brading and Brown's first global/local distinction. So let us consider whether under the assumption just mentioned the specific claims are right about which global and local theoretical transformations are and are not to be matched with which setup.

The first question is whether the positive matchings are right. So, is it right that the setup with a single half-wave plate in the final physical state should be matched with the relative phase transformations as the claim (1) says, that the setups with a half-wave plate in one state and a switched-on solenoid in the same or in another state should be matched with the relative phase transformations applied twice as the claims (2a) and (2b) say respectively, and that the setup with two half-wave plates in the final physical state should be matched with the global phase transformations in the narrow sense as the claim (3) says? This seems right indeed, because it is right for each of these claims that the passage between the interference patterns in the setup mentioned in a given claim can indeed be represented by the observational consequences of those phase transformations with which according to this claim this setup should be matched. So in particular the passage A → B between the interference patterns of the two physical states constituting the setup (1) can indeed be represented by Brading and Brown's relative phase transformations, the passages A → A and B → B between the interference patterns from the setups (2a) and (2b) respectively can indeed be represented by their relative transformations applied twice and the passage A → A between the interference patterns from the setup (3) can indeed be represented by the global transformations in the narrow sense.

However, as the matchings of setups with theoretical transformations are made by Brading and Brown on the basis of whether the observational consequences of these transformations are able to account for what happens with the interference pattern in a given setup, if any other theoretical transformations happen to have the same observational consequences as those implied by the theoretical transformations with which we have matched a given setup, then the same setup will have to be matched with these other theoretical transformations as well. And so if the original transformations will lead to DES given their observational consequences and the setup, then the other transformations will lead to DES as well. This *proliferation of matchings* and hence the *proliferation of assignments of DES* is typical of the empirical approach when based on weak notions of physical symmetry and matching. And the proliferation of matchings due to Brading and Brown's requirement of being able to represent what happens observably with the interference pattern is just a special case of this, for because of being a requirement to account for a single observable feature this matching requirement is very weak indeed. Thus when used in this way the empirical approach can be very permissive when compared with the theoretical approach not only as to the number of physical situations giving rise to DES, as we saw above, but also in particular as to how much theoretical theoretical transformations lead to DES.

Now Brading and Brown do not deny that more than one theoretical transformation can be matched with a given setup (and possibly lead to DES), for they propose to match a given setup with a collection of transformations rather than a single transformation. But they it is not any theoretical transformation that they match with a given setup either.

More precisely, the setups (1), (2a), (2b) are matched by Brading and Brown with their relative phase transformations while the setup (3) is matched by them with what they take to be the global phase transformations in the narrow sense in our terms. However, as said the transformations with which Brading and Brown match the setup (3), although reducing to $\Psi = 1/\sqrt{2}\ (\psi_{\mathrm{I}} + \psi_{\mathrm{II}}) \rightarrow \Psi' = 1/\sqrt{2}\ (\psi_{\mathrm{I}} + \psi_{\mathrm{II}})e^{iq\theta} = \Psi e^{iq\theta}$ as expressed by Brading and Brown's equation (1.9) [p. 657], pass by the stage $\Psi = 1/\sqrt{2}\ (\psi_{\mathrm{I}} + \psi_{\mathrm{II}}) \rightarrow \Psi' = 1/\sqrt{2}\ (\psi_{\mathrm{I}}e^{iq\theta} + \psi_{\mathrm{II}}e^{iq\theta})$ as also mentioned in the same equation (1.9). In other words, although one may take the transformations with which Brading and Brown match the setup (3) to act on the whole beam, one may also take them to act on either half of the beam. And so, although one may take these transformations to be the global phase transformations in the narrow sense, one may also take them to be their relative phase transformations applied twice like those with which Brading and Brown match the setup (2a) (where the interference pattern is also A in both states) and perhaps also like those with which they match the setup (2b) (where the interference pattern in also the same in both states). Indeed one could even argue along the same lines that all the global phase transformations in the broad sense are actually (combinations of) Brading and Brown's relative phase transformations. Thus it is actually unclear whether the setups (1), (2a), (2b) are matched with the relative phase transformations while the setup (3) is matched with the global phase transformations in the narrow sense or whether any of the setups (1), (2a), (2b), (3) is matched with the relative phase transformations. But it is anyway clear that for Brading and Brown either of the setups (1), (2a), (2b) and (3) is matched with some subset of the global phase transformations in the broad sense, because both the relative phase transformations and the global phase transformations in the narrow sense are subsets of the global phase transformations in the broad sense. And besides it is also clear that for Brading and Brown each setup will be matched with a subset of the global phase transformations in the broad sense such that it is a proper subset of the set of all the global phase transformations in the broad sense, i.e. such that it comprises less than all the global phase transformations in the broad sense. So Brading and Brown do accept that the same setup can be matched with more than one phase transformation, but they do not accept that it can be matched with something else than a proper subset of the global phase transformations in the broad sense as opposed to any global phase transformations in the broad sense and as opposed to local transformations in the broad sense. Is that the right position to adopt?

Here the idea that a given setup cannot be matched with all the global phase transformations in the broad sense is correct. Indeed, even if all the global phase transformations in the broad sense are relative phase transformations, for instance in particular because the global phase transformations in the narrow sense are (combinations of) relative phase transformations, we may note that a relative phase transformation with which the setup (1) can be matched should bring new observational consequences about the interference pattern while relative phase transformations with which the setups (2a) and/or (2b) and/or (3) can be matched should not do so. Supposing then that the same relative phase transformation with a given domain cannot have different observational consequences, we get that a relative phase with which the setup (1) can be matched is distinct from that with the setup (2a) and/or (2b) and/or (3) can be matched. Hence for any of the setups (1), (2a), (2b), (3) not all the global phase transformations in the broad sense will have to be matched with that setup even if all the global phase transformations in the broad sense are relative phase transformations. Meanwhile, if instead the global phase transformations in the narrow sense are genuinely different from Brading and Brown's relative phase transformations, then the global phase transformations in the broad sense have at least two distinct subcategories, and as the setup (3) is to be matched with the transformation of the former of these subcategories while the setups (1), (2a), (2b) are to be matched with the transformations of the latter of these subcategories, we also get that for any of the setups (1), (2a), (2b), (3) not all the global phase transformations in the broad sense will have to be matched with that setup. So that much of Brading and Brown's position is right.

By contrast, that part of Brading and Brown's position by which the same setup can only be matched with a proper subset of the global phase transformations in the broad sense but not with the local transformations in the broad sense is wrong. This follows from the proof that I will give when we get to my own views on DES in Part III. According to that proof, if a global transformation brings about some observational consequences, then a local transformation bringing about the same observational consequences can always be constructed (provided a few conditions are satisfied). To see what this implies for our case we need to know some facts about this proof.

Firstly, it uses a global/local distinction different from the global/local distinction in the broad sense. In fact *the global/local distinction used in the proof* can be obtained by making precise Kosso's idea of a global/local distinction that has to do with effective uniformity or non-uniformity of transformations, or else by generalising beyond length scale transformations *the third global/local distinction from Brading and Brown's Section 2* by which *a transformation of a length scale is global* if it makes the same change at any point and *local* if the changes it makes vary smoothly but arbitrarily from point to point [p. 650]. To remind, Brading and Brown seem to accept as much as four definitions of the global/local distinction in their Section 2 and apparently take them all to be equivalent, but as we will see no couples of these distinctions are equivalent even extensionally. So in particular their first distinction that we have recently encountered when discussing the relevance problem above and their third distinction just mentioned are not equivalent (for one thing, only the latter speaks of length scale transformations), and their generalisations to the global/local distinction in the broad sense and to the global/local distinction used in the proof respectively are actually not equivalent either. However, if a transformation is global in the sense of the global/local distinction in the broad sense (i.e. features parameters), then usually it is also global in the sense of the distinction used in the proof (i.e. effectively uniform), while if a transformation is local in the sense of the distinction used in the proof (i.e. effectively non-uniform), then usually it is also local in the sense of the global/local distinction in the broad sense (i.e. features functions). Because of this it follows from the proof that if a transformation global in the broad sense has some observational consequences, then a transformation local in the broad sense will also have the same observational consequences.

Secondly, it can be demonstrated that only some but not all transformations local in the broad sense will have given observational consequences. To do so we may proceed in the way analogous to the one I used to demonstrate above that only some but not all global phase transformations in the broad sense are matched with a given setup. Namely, we conceive two incompatible observational consequences and note that if for either observational consequence there is a local transformation in the broad sense having that observational consequence by virtue of my proof, as well as if the same transformation cannot have incompatible observational consequences (which we can usually arrange by fine-graining what counts as the same transformation), then the local transformations in the broad sense having incompatible observational consequences are distinct, and hence not all local transformations in the broad sense have given observational consequences.

Thirdly, the local transformations in the broad sense with given observational consequences obtainable via my proof may act on a different set of variables than that on which the original global transformation in the broad sense acts, but in the resulting theoretical construction there will anyway be a transformation acting on the original variables, and it can be made local in the broad sense (so that it becomes part of those local transformations with suitable observational consequences that we were willing to obtain via the proof). This means that in our case instead of the wavefunction as in the global phase transformations in the broad sense the local transformations in the broad sense obtainable via the proof may act for instance on both the wavefunction and the electromagnetic potential as in the joint local phase and electromagnetic potential transformations.

Thus given the facts about the proof just mentioned *the crucial result following from my proof* is that wherever the proof holds there will never be a situation in which only some global transformations in the broad sense have given observational consequences (and lead to DES on their basis in case these observational consequences lead to DES) but no local transformations in the broad sense have the same observational consequences (and lead to DES on their basis in case these observational consequences lead to DES). Instead, wherever the proof holds it will always be the case that when some global transformations in the broad sense have given observational consequences (and lead to DES on their basis in case these observational consequences lead to DES), then some local transformations in the broad sense have the same observational consequences (and lead to DES on their basis in case these observational consequences lead to DES). And finally what we get for our case thanks to the proof and the facts about it is thus that if some global phase transformations in the broad sense have given observational consequences (and lead to DES on their basis in case these observational consequences lead to DES), then the same will hold of some local transformations in the broad sense which act on wavefunction and/or on other variables.

Now any of Brading and Brown's claims (1), (2a), (2b), (3) says or implies that a given setup is to be matched with certain global phase transformations in the broad sense but not with any local transformations in the broad sense. So it follows that if a matching of a setup with certain global phase transformations in the broad sense postulated by a given claim is right, then this claim is wrong because of the crucial result just mentioned (and of its specification to our case). But as said above actually the positive matchings from all of Brading and Brown's claims are right, i.e. it is right for each claim that what happens with the interference pattern when passing from one to the other of the physical states constituting a setup mentioned in that claim can be represented by the observational consequences of those global phase transformations in the broad sense with which according to that claim the setup in question should be matched. Therefore, all of Brading and Brown's claims are wrong because they imply that matchings with other than some global phase transformations in the broad sense are impossible while the crucial result just mentioned shows that other matchings with local transformations in the broad sense, which may moreover include local phase transformations in the broad sense, will always be possible.

Subsection II. 2. 3. 5. 't Hooft's beam-splitter case: the distinction between subsystems and their parts and the universal/restricted distinction

As different setups of 't Hooft's beam-splitter case can thus be matched with both global and local transformations in the sense of the generalised Brading and Brown's third global/local distinction and in the sense of my global/local distinction in the broad sense, and as Brading and Brown's first global/local distinction is a specification of the latter global/local distinction, neither the generalised Brading and Brown's third global/local distinction nor their first global/local distinction turn out not to coincide with *the distinction between theoretical transformations with* *which different setups of 't Hooft's beam-splitter case are matched and theoretical transformations with which these setups are not matched*. Moreover, given the reasons just mentioned and the fact that the theoretical transformations leading to DES on the basis of 't Hooft's beam-splitter case are a subset of the theoretical transformations with which different setups of 't Hooft's beam-splitter case are matched it follows as well that neither the generalised Brading and Brown's third global/local distinction nor their first global/local distinction coincides with *the distinction between theoretical transformations leading to DES on the basis of 't Hooft's beam-splitter case and theoretical transformations not leading to DES on the basis of that case*. Let us see then what else in Brading and Brown's framework could coincide with this last distinction.

To do so recall that in their Section 3 *Brading and Brown call local phase transformations* those phase transformations of the form $\psi \rightarrow \psi' = \psi e^{iq\theta(\mathbf{x},t)}$ and *global phase transformations* those phase transformations of the form $\psi \rightarrow \psi' = \psi e^{iq\theta}$ that act on a subsystem's wavefunction Ψ constituted for them by the wavefunction of the whole beam, while they call *relative phase transformations* those phase transformations of the form $\psi \rightarrow \psi' = \psi e^{iq\theta}$ that act on a subsystem's part's wavefunction ψ_{I} or ψ_{II} constituted for them by the wavefunction of a half of the beam passing through the upper or the lower slit alone respectively. As we know, they next deny that their local and global phase transformations (as well as local electromagnetic potential transformations) lead to DES but have to admit that their relative phase transformations lead to DES. So the distinction between theoretical transformations that lead to DES on the basis of 't Hooft's beam-splitter case and theoretical transformations that do not lead to DES on the basis of that case should correspond for Brading and Brown to a distinction which puts their relative phase transformations on one side and their local and global phase transformations (as well as local electromagnetic potential transformations) on the other side. Looking at the definitions of these transformations just given, we see that the distinction we seek can be *the distinction between theoretical transformations acting on parts of subsystems and theoretical transformations acting on subsystems*. For this distinction does put Brading and Brown's relative phase transformations on one side because of them acting on parts of subsystems and Brading and Brown's local and global phase transformations on the other side because of them acting on subsystems. So the distinction between theoretical transformations that lead to DES on the basis of 't Hooft's beam-splitter case and theoretical transformations that do not lead to DES on the basis of that case corresponds in Brading and Brown's framework to the distinction between theoretical transformations acting on parts of subsystems and theoretical transformations acting on subsystems.

This actually rules out the correspondence of the distinction between theoretical transformations that lead to DES on the basis of 't Hooft's beam-splitter case and theoretical transformations that do not lead to DES on the basis of that case with *the fourth global/local distinction Brading and Brown accept in their Section 2* (even if adapted to 't Hooft's beam-splitter case). The latter distinction is defined by saying that *a Weyl-type local scale transformation applied to a system consisting of two subsystems* is a transformation which allows one subsystem to be freely rescaled relative to the other subsystem only when these are spatially separated but not when they are at the same spacetime location, while what Brading and Brown simply call *a scale transformation of one subsystem with respect to another* is a transformation which allows one subsystem to be rescaled relative to the other subsystem independently of whether they are at different spacetime locations or at the same spacetime location [p. 650]. This fourth global/local distinction thus contrasts transformations acting on subsystems in different ways, so the transformations global on that distinction and the transformations local on that distinction are both transformations acting on subsystems, while as just explained transformations acting on subsystems are transformations not leading to DES on the basis if 't Hooft's beam-splitter case in Brading and Brown's framework. So in that framework the fourth global/local distinction cannot correspond to the distinction between theoretical transformations that lead to DES on the basis of that case and theoretical transformations that do not lead to DES on the basis of that case because the fourth global/local distinction contrasts different kinds of transformations within transformations that do not lead to DES on the basis of the case in question. So already three of Brading and Brown's four global/local distinctions they accept in their Section 2 happen not to capture the distinction between theoretical transformations that lead to DES and those that do not lead to DES from Brading and Brown's Section 3. The remaining *second global/local distinction Brading and Brown accept in their Section 2* will be of no help either: as we will see below, it is not even about transformations at all. So after all their whole Section 2 is completely irrelevant to their Section 3.

Meanwhile, Brading and Brown's Section 3 has its problems as well. Firstly, if we compare Brading and Brown's use of the distinction about subsystems and their parts with the global/local distinction in the broad sense (together with their first global/local distinction), we see that Brading and Brown miss an option. This option consists in having transformations local in the broad sense and acting on parts of subsystem. For instance, in the case of phase transformations Brading and Brown distinguish as we know between the phase transformations of the form $\psi \to \psi' = \psi e^{iq\theta(\mathbf{x},t)}$ that act on a subsystem's wavefunction Ψ, the phase transformations of the form $\psi \to \psi' = \psi e^{iq\theta}$ that act on a subsystem's wavefunction Ψ and the phase transformations of the form $\psi \to \psi' = \psi e^{iq\theta}$ that act on a subsystem's part's wavefunction ψ_{I} or ψ_{II}. So an option is missing where the wavefunction transformations of the form $\psi \to \psi' = \psi e^{iq\theta(\mathbf{x},t)}$ would act on a subsystem's part's wavefunction ψ_{I} or ψ_{II}, and it is not explained why this option would be impossible. Moreover, if in Brading and Brown's framework theoretical transformations that lead to DES on the basis of 't Hooft's beam-splitter case are theoretical transformations acting on parts of subsystems, then these phase transformations of the form $\psi \to \psi' = \psi e^{iq\theta(\mathbf{x},t)}$ that act on a subsystem's part's wavefunction ψ_{I} or ψ_{II} would also have lead to DES in their framework. And more generally, if in Brading and Brown's framework any theoretical transformations that lead to DES are theoretical transformations acting on parts of subsystems, then any theoretical transformations local in the broad sense (and possibly in the sense of Brading and Brown's first global/local distinction) and acting on parts of subsystems lead to DES in that framework. Thus Brading and Brown's framework turns out to allow transformations local in a relevant sense to be such that they lead to DES even though Brading and Brown were supposed to show that local transformations do not lead to DES.

Secondly, this framework turns out to also be incoherent with Brading and Brown's own views on assigning DES. Indeed, on the one hand the explicit matching criterion they give [p. 653] is that a theoretical transformation be able to represent (and predict) what happens with an interference pattern in a given setup when we pass from one to the other of its states. But for this it is not necessary that a theoretical transformation acts on a part of a subsystem rather than on a subsystem. What is necessary is just that it has the right observational consequences as to whether the interference pattern is to remain the same or to change. And there does not seem to be any necessary link between having such observational consequences and acting on what counts as a part of a subsystem rather than a subsystem. So while according to Brading and Brown's framework only theoretical transformations acting on parts of subsystems lead to DES, according to their own matching criterion this is not going to be the case.

To save Brading and Brown's framework one may try to appeal to the physical condition on assigning DES reformulated above for the empirical approach. This condition, as we recall, involved accounting for the fact that we deal with a physical subsystem. But firstly, what we need to save Brading and Brown's framework is a condition that involves a part of a subsystem and not a subsystem, and modifying the reformulated physical condition in that way seems both ad hoc and inadequate e.g. with respect to Galileo's ship observable physical symmetry where a ship is recognised to be a subsystem. Secondly, even if we did have a condition of that kind, why would it have to influence theoretical transformations rather than theoretical states? After all being a subsystem or a part of a subsystem is a property of something before and after the transformation rather than during the transformation. Thirdly, actually neither theoretical transformations nor theoretical states would not need to deal with what theoretically counts as a part of a subsystem even if the physical condition required to represent something observable as a part of subsystem. For to satisfy the latter requirement it suffices that we get in our theory something that observationally behaves as a part of a subsystem. So whatever the theoretical underpinnings, if their observational consequences are such that what they describe will look as a part of a subsystem, that is enough. In other words, arguably for a theory to represent something as a part of a subsystem it is not necessary that a theory recognises it as a part of a subsystem but only that it makes it look as such. Indeed, recall how many theories tell us that what we see is not what it looks like: we are told by theories that tables are collections of particles in the void (to take Arthur Eddington's and Bertrand Russel's example) and so on. But such theories do their work of representing (and predicting) phenomena. And the physical condition in question needs no more than that. Hence satisfying it by acting on something that theoretically is not a part of a subsystem is fine. Likewise for satisfying it by acting on something that theoretically is not a subsystem, or is a subsystem, and so on. It just does not matter what theoretical thing the theoretical transformation acts on as long as the requirement to represent something so that it looks in some way is satisfied by it. Fourthly, in 't Hooft's beam-splitter case we actually cannot even impose a requirement of the kind just mentioned, i.e. the requirement to represent a half of the beam passing through a given slit as a part of a subsystem because it looks so. For we cannot actually say that a half of the beam passing through a single slit looks like a part of a subsystem rather than a subsystem: we do not see it at all. And so we cannot say whether it is a part of a subsystem or a subsystem without turning to theories. That is what Brading and Brown do when discussing the question what of these a half of the beam passing through a slit is [p. 658]. But for the empirical approach this is simply irrelevant. For there what is to be accounted for on the theoretical level is derived from the physical level and not from the theoretical level. So in 't Hooft's beam-splitter case it is even harder for the physical condition to impose something about parts of subsystems on the theoretical level, perhaps unless the parts are separate particles; but that is not the parts Brading and Brown are interested in, for their interest lies instead in the parts constituted by the halves of the beam passing through given slits.

Besides, the distinction between theoretical transformations acting on subsystems and those acting on their parts also has problems concerning the notion of subsystem. The issue Greaves and Wallace note [2014, p. 83, n. 24] is that one can redefine the notion of subsystem so that the two halves of the beam count as subsystems. For Greaves and Wallace this seems to show just that there may exist a definition on which the halves of the beam turn out to be misclassified by Brading and Brown as parts of subsystems instead of subsystems. But there is also a more serious issue with Brading and Brown's distinction which may occur without us changing the definition of what counts as a subsystem. Namely, if our notion of subsystem is such that all parts of subsystems are themselves subsystems, then Brading and Brown's distinction between theoretical transformations acting on subsystems and those acting on their parts collapses because all theoretical transformations acting on parts of subsystems turn out to also be theoretical transformations acting on subsystems. But actually even a weaker problem where a single theoretical transformation acts on a part of a subsystem that is itself a subsystem is already fatal for Brading and Brown's framework. For there theoretical transformations acting on parts of subsystems are supposed to lead to DES while theoretical transformations acting on subsystems are supposed not to lead to DES, and so the weaker problem implies that a theoretical transformations supposed to lead to DES now is also supposed not to lead to DES.

In sum, Brading and Brown would better not to link the question of whether a theoretical transformation acts on subsystems or their parts with the question of which theoretical transformations lead to DES.

Can we then eliminate the mention of subsystems and their parts from Brading and Brown's account of that question and keep the rest of it? To do so we have to find another distinction that is extensionally like the one Brading and Brown favour but does not mention subsystems and their parts explicitly. Some other distinction based on the domains of application may suit. One possibility is to use my *universal/restricted distinction*. In the context of 't Hooft's beam-splitter transformations acting on the whole beam will be universal and transformations acting on a half of the beam passing through a given slit will be restricted with respect to the domain of the whole beam, because the former transformations act on the whole of that domain and the latter on a restriction of it. So *Brading and Brown's idea* by which the theoretical transformations (e.g. phase and/or electromagnetic potential transformations) acting on a subsystem cannot account for changing interference patterns (and hence do not lead to DES) while the phase transformations acting on a part of a subsystem can account for changing interference patterns (and hence do lead to DES) can now be *reformulated* as the idea by which the theoretical transformations universal with respect to the domain of the whole beam cannot account for changing interference patterns (and hence do not lead to DES) while the phase transformations restricted with respect to the same domain can account for changing interference patterns (and hence do lead to DES). This amounts to putting the distinction between the transformations that cannot account for changing interference patterns (and hence do not lead to DES) and the phase transformations that can account for that (and hence do lead to DES) in correspondence not with the distinction between the theoretical transformations acting on subsystems and the phase transformations acting on parts of subsystems but with the distinction between the universal theoretical transformations and the restricted phase transformations drawn with respect to the domain of the whole beam. So in this way the universal/restricted distinction allows us to eliminate the mention of subsystems and their parts. But there is also another advantage in using it, namely that it allows us to extend the reformulated Brading and Brown's idea we have obtained above beyond 't Hooft's beam-splitter case. For that it suffices to omit in the reformulation just obtained the mentions of phase, as well as to replace there the whole beam by some relevant domain and accounting for changing interference patterns by accounting for some relevant observable change. We may also go a bit further and replace changing interference patterns by any observable change. The theoretical transformations that cannot account for any observable change will be *observationally complete theoretical transformations* in the sense I attributed above to Brading and Brown's [p. 648, n. 7], i.e. theoretical transformations that do not bring about any new observational consequences. Meanwhile, the theoretical transformations that can account for some observable change will be *observationally incomplete theoretical transformations* in the sense of theoretical transformations that bring about some new observational consequences. *The generalised reformulated Brading and Brown's idea* will then consist in saying that the theoretical transformations universal with respect to a relevant domain are observationally complete (and hence do not lead to DES) while the theoretical transformations restricted with respect to the same domain are observationally incomplete (and hence do lead to DES). This amounts to putting the distinction between observationally complete transformations (that do not lead to DES) and the observationally incomplete transformations (that do lead to DES) in correspondence with the distinction between the universal theoretical transformations and the restricted theoretical transformations drawn with respect to a relevant domain. If we go as far as assigning DES on the basis of whether theoretical transformations are observationally incomplete, then the generalisation just formulated squares well with Brading and Brown's claims that observationally incomplete transformations do not have DES [ibid.] and that universal transformations taken with respect to the domain of the whole universe do not have DES [p. 648].

Let us leave the assessment of whether observationally complete or observationally incomplete transformations lead to DES for later and check for now whether it is right that universal theoretical transformations are observationally complete while restricted theoretical transformations are not. This half of the generalised statement above is actually correct for many transformations classified as global on various distinctions, at least when the domain with respect to which the universal/restricted distinction is drawn is taken to be the whole universe. For instance, such external global transformations as boosts, spatial rotations, spatial translations and temporal translations are indeed observationally complete when universal with respect to that domain and observationally incomplete when restricted with respect to it. This has important consequences for the ontology of theoretical symmetries. For one thing, that external transformations such as those just mentioned are observationally complete when universal with respect to the domain of the whole universe is as we recall the basis of Leibniz's response to Clarke by which these spatiotemporal transformations do not have any ontological significance. For another thing, that boosts are observationally incomplete only when restricted with respect to the domain of the whole universe tells us that the observable changes arising from the fact of physically boosting a ship can only be accounted for by restricted boosts, and hence that they rather than the boosts universal with respect to the domain of the whole universe will be able to lead to DES on the basis of Galileo's ship case (to the extent that our matching in that case is determined by the requirement to account for the observable changes just mentioned).

Unfortunately for the generalised statement above, however, there are also enough cases where it fails, so that it actually cannot be said to enjoy a general validity. Ironically, one example of such failure is provided by the case where for Leibniz the generalised statement was supposed to hold, namely by the case of universal spatial reflections applied on the whole universe which Leibniz took to be observationally complete too. For it is now believed that the physical universal spatial reflections applied on the whole universe would be *observationally incomplete* in Kosso's [2000, p. 88] sense, i.e. would lead to new observable features, in the domain of electroweak phenomena. This is reflected on the theoretical level by making theoretical universal spatial reflections applied on the whole universe observationally incomplete in the sense of brining about new observational consequences. The same holds for universal charge conjugations and universal temporal reflections when performed separately on the whole universe, for only the combination of universal spatial reflections, universal charge conjugations and universal temporal reflections on the whole universe is now believed to be observationally complete physically and theoretically (this is known as *the CPT symmetry*, where 'C' stands for charge conjugations, 'P' for spatial reflections (also named parity transformations) and 'T' for temporal reflections). So contrary to the generalised statement above not all universal theoretical transformations applied on the whole universe are observationally complete in the sense of not bringing about no new observational consequences.

Note that the CPT transformation and each of its three component transformations is usually classified as *discrete*, while Brading and Brown restrict their analysis to *continuous* symmetries at least in the title of their Section 2 [p. 648]. But they do not provide any serious reason why such a restriction should be made, and there does not seem to be any either. In their abstract Brading and Brown say that Kosso [2000] discussed DES of continuous symmetries, so one could think that they just wish to follow him. But actually Kosso also discussed DES of symmetries that are often classified as discrete, namely the symmetries constituted by permutations of protons with neutrons [ibid., Subsection 4.3]. Brading and Brown also present the external/internal distinction as a distinction for continuous symmetries [p. 648], so that one could think that this distinction applies to continuous symmetries alone and that using it requires discarding discrete symmetries. But that is not the case, because for instance the symmetries constituted by permutations of protons with neutrons are usually classified as internal while the CPT symmetry is usually classified as *mixed* because spatial and temporal reflections are external while charge conjugations are internal. Besides, many transformations initially thought of as discrete can also be thought of as continuous (see [Weingard 1984]). So there is no reason to discard examples such the failure of universal spatial reflections on the whole universe to be observationally complete.

Likewise not all restricted theoretical transformations are observationally incomplete. In the context of 't Hooft's beam-splitter case this can be seen by considering phase shifts by 360 degrees (or by 360 degrees multiplied by an integer) applied on one half of the beam. These do not seem to bring about any new observational consequences and hence are observationally complete despite being restricted. One could think that this example is not legitimate because any phase shifts by 360 degrees (or by 360 degrees multiplied by an integer) are actually the same as the identity transformations. But it is hard to prove this without already presupposing that any phase shifts by 360 degrees (or by 360 degrees multiplied by an integer) are observationally complete. Perhaps one could try to claim that any transformations involving changes by 360 degrees (or by 360 degrees multiplied by an integer) are the same as identity transformations because they change nothing in the theoretical underpinnings. But that is not correct, because such theoretical items as spinors are not invariant under changes by 360 degrees (or by 360 degrees multiplied by an impair integer) but only under changes by twice 360 degrees (or by 360 degrees multiplied by a pair integer). Besides, in the approaches to the formalisation of theoretical symmetries that use intensional means, such as the homotopy type theory which Ladyman and Presnell use to analyse DES in their manuscript [2016/01/08], prescriptions of change by 0 degrees and prescriptions of change by any other number of degrees can be treated as distinct even when their application yields effectively the same result. So even if all theoretical underpinnings were invariant under changes by 360 degrees (or by 360 degrees multiplied by an integer) that would not be by itself a reason to take such changes to be the same as the identity transformation prescribing the change by 0 degrees. So the case of phase shifts by 360 degrees (or by 360 degrees multiplied by an integer) does seem to constitute an evidence against the idea that all restricted transformations are observationally incomplete.

Another evidence with the same effect is actually provided by the diffeomorphisms from Earman and Norton's hole argument: although they are usually defined as transformations between whole manifolds, they are supposed to induce a non-trivial change only on some infinitesimal region of the original manifold and in this sense are restricted transformations. Yet they are observationally complete, and that is why they are usually denied any ontological significance. This and Leibniz's denial, mentioned above, of the ontological significance of observationally complete universal global spacetime transformations on the whole universe suggests that there is *a philosophical preference towards denying ontological significance to observationally complete theoretical transformations*. By contrast, as we saw contrary to the generalised statement we have deduced from Brading and Brown's framework there is no reason to associate in general observational completeness of theoretical transformations with their universal character nor observational incompleteness of theoretical transformations with their restricted character.

Subsection II. 2. 3. 6. 't Hooft's beam-splitter case: the observable difference condition and the observable invariance condition

Theoretical transformations are said to lead to DES in the context of 't Hooft's beam-splitter case if they match with a setup giving rise to DES. As explained above, *Brading and Brown's criterion for saying that a certain setup gives rise to DES* consists in that the setup should be such that the interference pattern is observably different in the two states (besides satisfying to a certain extent the remainder of the three traits above). Meanwhile, *Brading and Brown's matching criterion for theoretical transformations* in the context of 't Hooft's beam-splitter case consists in that the theoretical transformations should be able to represent what happens with the interference pattern when we pass from one to the other state of a given setup [p. 653]. So *for a theoretical transformation to be said to lead to DES in Brading and Brown's framework* this theoretical transformation has to be able to represent what happens with the interference pattern in a setup where the interference pattern is observably different in the two states.

In the last two subsections I was assuming that the account just described is correct and I was exploring under this assumption whether any of the various distinctions relevant to Brading and Brown's framework extensionally coincides with one, either or both of the two other distinctions: the distinction between the theoretical transformations that satisfy Brading and Brown's matching criterion and those that do not, as well as the distinction between those theoretical transformations that for Brading and Brown lead to DES and those that do not. The main conclusion was that the former distinctions in general happen to cut across the two latter distinctions instead of coinciding with them extensionally, and that this is to the disadvantage of Brading and Brown's analysis because it assumes the contrary. So Brading and Brown's analysis does not hold even if we accept their account described above.

In the next two subsections, however, I wish to show that this account need not be accepted either. This is because it turns out that Brading and Brown's criterion for saying that a certain setup gives rise to DES has to be completed and revised while their matching criterion for theoretical transformations also has to be completed. As a result the condition under which theoretical transformations can be said to lead to DES has to be changed as well, and the theoretical transformations satisfying this new condition turn out to be different from those that were supposed by Brading and Brown to satisfy their original condition.

Note to begin with that to say of some setup that it gives rise to DES one should say in particular why this setup counts as a physical symmetry, because by the definition of DES only physical symmetries can give rise to DES. More precisely, Brading and Brown follow Kosso in preferring observable physical symmetries, so they have to say at least what are the observable difference condition and the observable invariance condition which on their account allow a given setup to qualify as an observable physical symmetry. As mentioned above, Brading and Brown's account of which setup gives rise to DES involves a requirement that the interference pattern should observably change, and this as we recall is actually their observable difference condition because they say of the setup (3) where the interference patterns are the same in both states that the observable difference condition is not satisfied there [p. 657]. So *in their account interference patterns contribute to the observable difference condition*. But this is not the only thing they could do. Indeed, *another option is to take interference patterns to contribute to the observable invariance condition instead*. This is actually the option adopted by Kosso [2000, p. 93]. Neither Kosso nor Brading and Brown justify their choices as to what is the condition in which the interference patterns should contribute. However, we need to compare these choices to see which one is better (or whether both of them should be entertained). I am going to argue that it is Brading and Brown's option that has to be abandoned.

Why is that option not good? As the interference patterns there contribute to the observable difference condition, it is something else that should contribute to the observable invariance condition. So something else than an interference pattern should stay observably invariant for the observable invariance condition to obtain and change for that condition to fail to obtain. The problem is then that there does not seem to be any interesting candidates for observably identical features left. Of course, there are plenty of observable features which are apparently supposed to be left invariant in the setups (1), (2a), (2b) and (3) when we pass from one physical state to another, such as the distances between the source, the screen with the slits and the second screen; the distance between the slits, as well as the width of the slits; the kind of particles involved; and even perhaps the time interval from emitting particles to registering an interference pattern on the second screen (on how to ensure the latter invariance see [p. 657, n. 9]). But firstly, all the features just named except the last one are static. And this makes the resulting 't Hooft's beam-splitter observable physical symmetry significantly different from Brading and Brown's notion of Galileo's ship-like physical symmetry where an invariance has to be (or has to be able to be) dynamical. Indeed, in Galileo's ship case itself it is phenomena within a ship that are to be invariant under boosts, as Brading and Brown say [p. 646], and as boosts are additions of a constant velocity, where a velocity is a change of position with time, this means that these phenomena should unfold during more than a single moment of time. Moreover, they should in fact involve observable changes through time, as seen from Galileo's description of the phenomena in question ([Galilei 1632/1967, pp. 186-188], partly quoted in [Healey 2009, p. 698]). For he describes these phenomena as involving experiments such as letting the drops fall into a vessel beneath, and it is the way these changes occur, e.g. the way the drops change their observable position with time, that has to remain invariant under boosts. But in the features I mentioned as Brading and Brown's possible candidates for the invariant observable features of 't Hooft's beam-splitter observable physical symmetry we see no such dynamical character. And secondly, these candidate features are not characteristic enough of the kind of situation constituting 't Hooft's beam-splitter case, because they may likewise be present in many other situations, for instance those where no interference pattern obtains at all (e.g. when we replace half-wave plates by particle detectors). This can be said in particular of the last candidate feature mentioned which was the time interval from emitting particles to registering an interference pattern on the second screen. So even if the latter feature can be said to satisfy the weak version of the first requirement of being dynamical, it does not cope well with this second problem.

By contrast, the second option above says that it is the interference pattern that should stay invariant. And this avoids both problems just mentioned. Indeed, firstly an interference pattern is something that is formed in time. Moreover, although the way it forms may be unique, even in that way some similarities with the way the interference pattern of another state is formed may be found (e.g. the time from the emission of a single particle to its arrival to the screen where the interference pattern is formed; the fact that no particle hits this screen outside a given area). And secondly, that there are interference patterns and that they can be preserved is indeed characteristic of 't Hooft's beam-splitter-type cases as opposed on the one hand to cases where no interference pattern is manifested (e.g. when a beam is replaced by a single particle or when we measure with particle detectors which of the two slits given particles pass) and on the other hand to the cases whose whole point is to show that the interference pattern can change.

The most important example of the latter kind of cases is *the Aharonov-Bohm effect* ([Aharonov and Bohm 1959], [Aharonov and Bohm 1961]). It is very similar to 't Hooft's beam-splitter-type cases in that one typical setup for exhibiting this effect is a setup where the initial physical state is like the initial physical state of the setups (1), (2a) and (3), i.e. with neither half-wave plates nor switched-on solenoids, and the final physical state is like the final physical state of the setup (2b) or the initial physical state of the setup (2a), i.e. with only a switched-on solenoid added; *I will number the resulting setup (4)*. Despite this similarity, it is essential to the Aharonov-Bohm effect that the interference pattern changes, because the point of conceiving theoretically and later testing experimentally this effect was to show that the switching-on of the solenoid influences the interference pattern even though the solenoid is shielded from the particles and hence even though the electromagnetic field is presumed theoretically to be confined inside the shielded zone. There are different versions as to which theoretical thing should be held responsible for predicting this experimentally confirmed effect and hence have an ontological significance on its grounds, with the main competitors being the electromagnetic field (*acting* on particles *at a distance* despite being shielded); the electromagnetic potential (because theoretically its field, contrary to the electromagnetic field, is non-zero where the particles pass when the solenoid is switched on); holonomies, i.e. integrals over closed loops featuring the electromagnetic potential (see [Healey 2007] for and [Nounou 2010] against it; this is another explanation involving *action-at-a-distance*); and topological features (see [Nounou 2003]). Here the fact that the interference pattern changes is essential in particular to the option of assigning the ontological significance to the electromagnetic potential, because before the Aharonov-Bohm effect was discovered such a significance was denied to it precisely on the grounds that transformations of the electromagnetic potential when not accompanied by changes in the electromagnetic field at the same points never lead to changes in observational consequences, and now the Aharonov-Bohm effect has shown that the fact that the theoretical electromagnetic potential field becomes non-zero when the solenoid is switched on at the points where the theoretical electromagnetic field itself is zero can be used to predict the change in the observable interference pattern confirmed by actual observations. By contrast, the purpose of 't Hooft's beam-splitter case at least in the context of the discussion of DES is to show that this case can give rise to an observable physical symmetry, and for this to be shown without transforming 't Hooft's beam-splitter case into the Aharonov-Bohm effect or something similar it must be characteristic of the 't Hooft's beam-splitter case in the context of discussing DES that the interference pattern there can be preserved rather than changed as in the Aharonov-Bohm effect. So *on my view requiring that the interference patterns contribute to the observable invariance condition*, and not to the observable difference condition as for Brading and Brown, *is the most natural thing to do in our context*. And if this other option is adopted, then, contrary to Brading and Brown, we have to assign DES not to the setup (1) (and to the setup (4)) where the interference pattern changes but to the setups (2a), (2b) and (3) where the interference pattern is preserved.

Brading and Brown do envisage this alternative possibility for defining the observable invariance condition as involving the observable invariance of interference patterns, but, as mentioned in the extended summary above, give a couple of arguments when discussing the setup (3) against the idea that the observable difference condition is satisfied when the interference pattern is the same in both physical states. The main idea of these arguments [pp. 657-658] is that we cannot know in such a case that these states are physically different. But of course we can actually know that, because we know that these states differ observably as to the set of *devices* they feature, namely in whether they feature a switched-on solenoid, a half-wave plate, either of them, two half-wave plates or neither of the previous, and such differences occur in the two states of either of the setups (2a), (2b) and (3) (and the same holds for the two states of either of the setups (1) and (4)). So *on my view the observable differences in the set of devices between the two physical states of a setup allow us to realise the observable difference condition*.

Considered more carefully, however, at least the second of Brading and Brown's objections says more precisely that what we do not know of the states featuring the same interference pattern but do know of the states featuring different interference patterns that they implement a given phase transformation (for in their words [p. 658] the question when applied to the setup (3) consists in whether the two half-wave plates implement a global gauge transformation, which in our terms is a global phase transformation in the broad sense). In other words, their observable difference condition by which the interference pattern should change is actually an effective condition whose role is only to indicate whether the underlying change they are really interested in, i.e. the change in phase, occurs, and their objection is that we do not have an effective condition of that kind in setups where the interference pattern is the same in both states.

One attitude we may have towards the latter objection consists in taking it seriously. In that case we may respond in two steps. The first is to accept at least Brading and Brown's effective difference condition and show that it holds. This can be done by arranging the setups where the interference pattern changes, like the setups (1) and (4). The second step is to take that *equipment* (i.e. sources and screens), those devices (i.e. half-wave plates and switched-on solenoids) and even those particles (if we can capture them at the screen where the interference pattern is formed) that we used to check that the effective difference condition is satisfied and use them next to satisfy my observable invariance condition above, i.e. to arrange a setup where the interference pattern does not change, like the setups (2a), (2b) and (3). In this way the same physical things will be shown to satisfy either condition and hence they will be able to yield DES provided the two conditions do describe for us what it takes to yield DES. What may seem strange of this solution is that the two conditions are assessed in different setups rather than in the same setup. But this should not be a predicament for Brading and Brown. Indeed, recall that when discussing Galileo's ship case they propose that one establishes the difference between an original ship and a physically boosted ship by observing how the latter ship gets into a shipwreck. Obviously, this destroys the isolation of what happens in the cabin of that ship, so while establishing the satisfaction of the observable difference condition in that way one cannot establish the satisfaction of the observable invariance condition which on the contrary requires the isolation to hold. So Brading and Brown are led to admit that the observable difference condition and the observable invariance condition can be established at different times [p. 647, n. 5]. To this they should also add different places, because when one checks a boosted ship as to the invariance and when one next observes it to continue its course up to the shipwreck the ship will be at different places because of its being boosted. And this suffices to accept the solution above, because the two steps described there are supposed to involve all the same physical things and will thus only differ as to their position in space and time (besides differing of course as to what constitutes the realisation of the two conditions).

But we may also take another attitude towards Brading and Brown's objection that we do not have an effective difference condition to know that an underlying phase transformation occurs if the interference pattern does not change. This attitude consists in simply dismissing that objection as too theoretical. After all even when the interference patterns changes we cannot actually know whether this is *an observable indicator* of an underlying change in phase or not. It is only the matching of the former change with the observational consequences of a theoretical phase transformation and the assignment (yet to be justified) of the ontological significance to the latter transformation that would allow us to claim that a physical phase transformation is realised when a physical change in an interference pattern occurs. In other words, we need to establish DES to accept Brading and Brown's objection, but the purpose of the objection is precisely to say us how DES is to be established. This cannot work! Hence we should not distinguish between effective and underlying difference conditions, which is what induces the issue with the objection in our context of discussing DES, but instead simply use a difference condition that appeals to the empirical level alone. This squares well with Kosso's vision of the difference condition as an observable difference condition, i.e. as a condition on a change that we should observe and not on any underlying change. One possibility to have an observable difference condition is provided by the observable difference condition I proposed above by which the change is ensured by the observable differences as to which devices are present in which state. Another possibility is provided by Brading and Brown's proposal to appeal to the observable differences in interference patterns in case we no longer regard this as an effective difference condition. As I argued above, however, the observable invariance condition is best ensured by the preservation of interference patterns, so we may wish to have the observable difference condition ensured by something else than interference patterns. Besides, we may also wish to have an observable difference condition that can be realised in the same setup as that observable invariance condition. Both desiderata are satisfied by my proposal for what should count as the observable difference condition and are not satisfied by Brading and Brown's proposal. In addition, my proposal is likewise compatible with making the setups (1) and (4) useful, as we will see soon when discussing my *non-triviality check for empirical symmetries* below. So the combination of my (and Kosso's) observable invariance condition and of my observable difference condition seems the best option to adopt when defining *what counts as 't Hooft's beam-splitter observable physical symmetry*.

Subsection II. 2. 3. 7. 't Hooft's beam-splitter case: the matchings and DES reconsidered

Above we noticed that *Brading and Brown's criterion for saying that a certain setup gives rise to DES*, which involves as the observable difference condition that the setup features different interference patterns in the two states, should be *completed* with an observable invariance condition. We then have established that the observable invariance condition is best ensured by the observable invariance of interference patterns in the two states. This together with the requirement that it be possible to satisfy both the observable difference condition and the observable invariance condition in the same setup has led us to abandon Brading and Brown's version of the observable difference condition in favour of a version where this condition is ensured by the observable difference as to which devices are present in the two states. We thus have to *revise* Brading and Brown's criterion for saying that a certain setup gives rise to DES by incorporating there the new observable invariance and observable difference conditions instead of Brading and Brown's observable transformation condition. As we recall, among the setups Brading and Brown discuss those satisfying our two conditions, i.e. featuring observably the same interference patterns in the two states and observably different sets of devices in the two states, are the setups (2a), (2b) and (3).

As what happens with interference patterns mattered for Brading and Brown's observable difference condition, the ability of theoretical transformations to account for what happens with interference patterns was what Brading and Brown's matching criterion was based on. But now we have different observable difference and observable invariance conditions, so we need to change Brading and Brown's matching criterion as well. On the other hand, given our conditions what happens with interference patterns turns out to matter again, although this matters now for the observable invariance condition rather than for the observable difference condition. Hence we still need to base our matching criterion among other things on the ability of theoretical transformations to account for what happens with interference patterns. Thus what we need to do with Brading and Brown's matching criterion is more precisely not to abandon it but to complete it with something that takes into account our observable difference condition as well. So, as the latter condition consists in that the two states of a setup should feature observably different sets of devices, we simply *complete Brading and Brown's matching criterion* by saying that the theoretical transformations should also be able to account for how the sets of devices change observably when we pass from one state to the other besides being able to account for what happens with the interference patterns when we pass from one state to the other.

Next, as mentioned theoretical transformations are said to lead to DES in the context of 't Hooft's beam-splitter case if they match with a setup giving rise to DES. Thus, as we have revised Brading and Brown's criterion for saying that a certain setup gives rise to DES and as we have completed Brading and Brown's matching criterion for theoretical transformations, we also get a *changed condition as to when a theoretical transformation can be said to lead to DES*. Namely, we get that a theoretical transformation can be said to lead to DES if it is able to represent what happens with interference patterns and devices in a setup where the interference pattern is observably identical in the two states and the sets of devices are observably different in the two states.

When discussing the global/local distinctions above we have already determined which theoretical transformations are able to satisfy *the first of the requirements* just mentioned, i.e. are able to represent that the interference patterns stay invariant when we pass from one to the other state in the setups (2a), (2b) and (3). These transformations were as we recall proper subsets of global phase transformations in the broad sense and of local transformations in the broad sense. So now we should simply retain from these transformations those that are also able to satisfy *the second requirement* just mentioned, i.e. are able to represent that the sets of devices change observably when we pass from one to the other state of the same setup. Curiously, for some setups checking whether the second requirement is satisfied too may involve eliminating global phase transformations in the broad sense to the profit of local transformations in the broad sense. This may seem strange, for were not the latter supposed to have all the same observational consequences as the former given my proof? Well, when we were deriving local transformations in the broad sense via the proof mentioned above, they were only required to preserve the ability of global phase transformations in the broad sense to account for what happens with interference patterns. To that extent, then, the observational consequences of the former and of the latter theoretical transformations had to be the same, but for the rest they had not to coincide. But what are these differing observational consequences of global phase transformations in the broad sense and of local transformations in the broad sense which matter in our case? Roughly, this is the fact that the latter transformations can involve electromagnetism while the former transformations do not involve it. This becomes important when the second requirement is introduced, because it consists in being able to account for the differences as to which devices are present, and one of the devices is a switched-on solenoid whose functioning as we recall is based on testable electromagnetic phenomena. For instance, a measurement operation that theoretically one would interpret as the measurement of the electric current will give a non-zero result, observable on our measurement apparatus, when applied to the wire of a switched-on solenoid. It is the appearance of this kind of phenomena when we pass from a state with no switched-on solenoid to a state where a switched-on solenoid is present, such as in the setups (2a) and (2b) satisfying our observable invariance condition, that can be accounted for if we use for instance the joint local phase and electromagnetic theoretical transformations (provided the electromagnetic potential transformations non-trivial on the region where a switched-on solenoid is added induce changes in the theoretical electromagnetic field in that region) but cannot be accounted for by the global phase transformations in the broad sense. So although I was saying above that Brading and Brown are right in matching the latter transformations with the setups (2a) and (2b), on a fuller account of what is 't Hooft's beam-splitter observable physical symmetry which includes my observable difference condition defended above this ceases to be the case.

What, then, about the other setup satisfying our invariance condition, namely the setup (3)? That one involves adding a half-wave plate, so is accounting for this addition easier for the global phase transformations in the broad sense as opposed to local transformations in the broad sense? In fact this happens to be unclear. For so far I was saying for simplicity that 't Hooft's beam-splitter case involves *half-wave plates* (and will continue to do so later on), but we now need to recall that what 't Hooft mentions is more precisely "the electron-optical equivalent of a half-wave plate" ['t Hooft 1980, p. 115]. Indeed, in fact *half-wave plates properly speaking* are optical devices that shift the linear polarisation of light, such as crystals. But in 't Hooft's beam-splitter case we deal with charged particles (electrons) and the shift in their phase instead. So what 't Hooft speaks about is a thought experiment where a device is envisaged that acts analogously to half-wave plates properly speaking but is suitable to the case he is describing. And so the whole question is how such a device can be implemented in practice. If an actual device turns out not to function electromagnetically, then local transformations in the broad sense involving electromagnetism will have to be excluded, but if on the contrary it turns out to function electromagnetically, then it is the global phase transformations in the broad sense that will have to be excluded.

If the latter option obtains, does this mean that there is no hope for the global phase transformations in the broad sense to lead to DES in the context of 't Hooft's beam-splitter case? Fortunately for them, things may be not so bad. Indeed, recall Kosso saying that theoretically the absolute phase is generally a function of position [2000, pp. 91-92] and also mentioning among the ways of obtaining the same interference pattern the idea to change the distance between the source and the slits when passing to the second state [ibid., p. 93]. Arguably such a passage could then be theoretically legitimately represented with the help of a global phase transformation in the broad sense provided it is coupled to a theoretical spatiotemporal transformation. But it still has to be investigated whether there are suitable passages that are to be matched with global phase transformations in the broad sense when not coupled to theoretical spatiotemporal transformations.

Subsection II. 2. 3. 8. 't Hooft's beam-splitter case: theoretical transformations, symmetries and states

So far we have only seen which theoretical transformations lead to DES in which setups of 't Hooft's beam-splitter observable physical symmetry. Let us now say a few words about the theoretical properties of such theoretical transformations and of the theoretical symmetries they constitute.

Firstly, are the theoretical transformations leading to DES on the basis of 't Hooft's beam-splitter case *gauge transformations* and do they constitute *gauge symmetries*? For it is in whether gauge symmetries have DES that Brading and Brown are supposed to be interested, as manifested by the title of their article ("Are gauge symmetry transformations observable?") and their explicit focus on gauge symmetries in their Sections 2 and 3. What is very unsatisfying about Brading and

Brown discussion, however, is that they do not actually bother with defining gauge transformations despite this being apparently the main object of their article. Besides, as we know from my discussion of the notion of gauge symmetry above this notion is far from being unanimously defined but on the contrary is associated with a set of inequivalent definitions. It should be said to the merit of Kosso that he does comment on that plurality of definitions and does make precise which definition he is using, namely the one by which gauge symmetries are internal local symmetries [ibid., pp. 93-94]. Recall also that with respect to the external/internal and global/local distinctions transformations are usually qualified in the same way as symmetries (so that saying that a symmetry is local internal implies that it is constituted by an internal local transformation). So *to be gauge in Kosso's sense theoretical transformations should be internal local transformations*. Given that sense my question about whether the theoretical transformations leading to DES on the basis of 't Hooft's beam-splitter case are gauge is easy to answer: they are gauge only if local in a relevant sense and not coupled with theoretical spatiotemporal transformations. These conditions are satisfied for instance by the joint local phase and electromagnetic potential transformations above (for they are internal and indeed local on many definitions such as Kosso's definitions of the global/local distinction, the definition of the global/local distinction used in my proof, the global/local distinction in the broad sense, the global/local distinction for groups of transformations and so on). Thus such gauge transformations in Kosso's sense can lead to DES on the basis of 't Hooft's beam-splitter case. But because of vagueness of Kosso's definition of 'local' and because of the unclear situation with the global phase transformations in the broad sense it may also turn out to be possible for other transformations not satisfying Kosso's definition of gauge transformation to lead to DES on the basis of the same case. Similarly if *gauge transformations are understood as local transformations whether internal or external*. But what about a third sense by which gauge transformations are those that do not bring any observational consequences? *Gauge transformations in that sense would constitute observationally complete theoretical symmetries*. The answer is then that if we were interested in our observable invariance condition alone, theoretical transformations had to only account for the observable invariance of interference patterns and for that they would have to be gauge in the sense just mentioned. But given our observable difference condition they also have to account for the observable change as to which devices are present, and so they cannot be gauge in the sense in question anymore. Finally, to answer whether gauge symmetries have DES on the basis of 't Hooft's beam-splitter case we check whether theoretical symmetries that have DES on that basis are constituted by gauge transformations in the various senses mentioned above. On the first and second senses we have got that probably both gauge and non-gauge theoretical transformations lead to DES, hence on these senses probably both gauge and non-gauge theoretical symmetries have DES. Meanwhile, on the third sense only non-gauge theoretical transformations lead to DES, hence on that sense only non-gauge theoretical symmetries have DES. Here the last fact by which *gauge symmetries in the sense of observationally complete theoretical symmetries do not have DES* is shown to hold when DES is established on the basis of 't Hooft's beam-splitter case, but I will show below that this fact also holds more generally provided it involves a specific kind of DES.

Secondly, what are the theoretical symmetries with DES theoretical symmetries of, i.e. what is it that the transformations constituting them preserve? Brading and Brown's wish was that they preserve the form of theoretical 'laws' such as the Schrödinger equation. But what follows from my analysis above is that they should preserve observational consequences pertaining to interference patterns. So the transformations constituting theoretical symmetries with DES may preserve theoretical laws but they need not. And so *the theoretical symmetries with DES in the empirical approach need not be symmetries of laws*. Again, this is shown here to hold for 't Hooft's beam-splitter case alone but in fact is valid beyond it as well.

Does this mean that any reference to laws when discussing theoretical symmetries with DES should be omitted? Not so, at least if 'laws' are equations of motion. For such equations are what could help us to represent the how particles evolve from being emitted at the source to contributing into an interference pattern being formed. If this is added to our requirements as to what is to be accounted for, then laws are useful. In my approach I do add such a requirement. It amounts to paying attention to *theoretical states* besides theoretical transformations, because the evolution just mentioned is a property of physical states rather than of the passage between them. Likewise for example the fact of dealing with subsystems, which is one of Brading and Brown's three traits for Galileo's ship-like physical symmetries, is better thought of, as already mentioned, as a property of physical states to be accounted for by theoretical states rather than by theoretical transformations. Besides, in a sense properties of theoretical transformations are determined by properties of theoretical states, as we will see when discussing how to construct theoretical symmetries in Part III. From this perspective an analysis of theoretical transformations without an analysis of theoretical states just does not make sense. By all these reasons it would be appropriate to be interested in which theoretical states lead to DES besides being interested in which theoretical transformations lead to DES. But Brading and Brown, like Kosso, are not concerned with theoretical states at all, for they only concentrate on theoretical transformations.

Perhaps the only clear case where Brading and Brown speak of theoretical states, although without naming them explicitly, is when they discuss how to define the global/local distinction in their Section 2. Above I have already commented on the differences between the first, the third and the fourth of the four definitions of the global/local distinction that Brading and Brown give there and seem to adopt. Meanwhile, *the second of Brading and Brown's definitions from their Section 2*, a definition that Brading and Brown attribute to Weyl [1918], says that *a length scale is global* if when fixed at one spacetime location it is fixed at all spacetime locations, while it is *local* if when fixed at one spacetime location it is not fixed at all spacetime locations because the lengths of two vectors are only directly comparable at the same spacetime location but not at separated spacetime locations [p. 649]. The same notion according to Brading and Brown is used in Yang and Mills' [1954, p. 192] *local isospin specification as to what counts as a proton* because it has to be such that when fixed at one spacetime location it is not fixed at all spacetime locations. This global/local distinction that Brading and Brown attribute to Weyl together with Yang and Mills is essentially different from Brading and Brown's three other global/local distinctions. For this distinction is actually about theoretical states, because it is based on static properties of length scales and isospin specifications, namely on whether the ways in which they are fixed at given points determine each other, and not on the transformations of length scales and isospin specifications (except in a counterfactual sense). But Brading and Brown do not use explicitly in their analysis of DES neither this specific global/local distinction for theoretical states nor the more general idea of applying the global/local distinction to theoretical states, and in fact no literature on DES does so. Thus we will have to wait until my Part III to see how much a global/local distinction for theoretical states can be useful.

Subsection II. 2. 3. 9. DES of external global symmetries and the non-triviality check for empirical symmetries

While Brading and Brown leave unsaid the DES following from their claim (1) about 't Hooft's beam-splitter case, they do claim overtly that their conditions for assigning DES (in the theoretical approach) are satisfied for matchings of external global theoretical symmetries constituted by boosts, spatial translations and spatial rotations with respectively Galileo's ship empirical symmetry, physical symmetries constituted by physical spatial translations and physical symmetries constituted by physical spatial rotations [p. 647, pp. 658-659, p. 663]. And this is in fact the only positive assignment of DES that they explicitly make. It is very surprising then that Brading and Brown do not bother themselves with giving at least some detail about how their conditions are satisfied in these matchings (particularly those involving spatial translations and spatial rotations). Apparently this is because they find the satisfaction of these conditions in the cases in question straightforward and unproblematic. But it often happens that when checked such apparently clear cases reveal some surprise.

Is there anything to be revealed by the cases Brading and Brown mention? There is at least one circumstance that is worth noting.

When discussing above what is the best observable invariance condition to define 't Hooft's beam-splitter physical symmetry I was stressing the dynamical character of the observable invariance condition in Galileo's ship physical symmetry. The idea was then to find for 't Hooft's beam-splitter physical symmetry the observable invariance condition which would as much as possible resemble that observable invariance condition for Galileo's ship physical symmetry. In other words, the idea was to define 't Hooft's beam-splitter physical symmetry so that it be a Galileo's ship-like physical symmetry at least with respect to the observable invariance condition. This is reasonable for instance in light of the already mentioned subsequent development of the discussion on DES in which Galileo's ship-like physical symmetry has become a paradigmatic example of what is an empirical symmetry, where the latter notion is something stronger than just a physical symmetry. If we really follow this route, however, have to also define 't Hooft's beam-splitter physical symmetry so that it be a Galileo's ship-like physical symmetry with respect to the observable difference condition as well. So, is the observable difference condition I adopted above for 't Hooft's beam-splitter physical symmetry, the one which requires that the set of devices be different in the two states, sufficiently analogous to the observable difference condition for Galileo's ship physical symmetry?

What has to observably change in the latter physical symmetry is whether and/or how a ship moves with respect to a reference. This is also a dynamical feature, so we could say that the observable difference condition for 't Hooft's beam-splitter physical symmetry also has to be dynamical. It is then possible to argue that the requirement that the sets of devices be different in the two states of a setup is also dynamical to some extent, for instance because at least switched-on solenoids involve electromagnetic phenomena and these are of dynamical nature. However, the dynamical character of the observable difference condition for 't Hooft's beam-splitter physical symmetry will probably still be qualified as weaker than that of the observable difference condition for Galileo's ship physical symmetry. But this may be admissible, because the dynamical character of the best observable invariance condition we could find for 't Hooft's beam-splitter physical symmetry is perhaps likewise to be qualified as weaker than that of the observable invariance condition for Galileo's ship physical symmetry.

There is however an important aspect in which the observable difference and the observable invariance conditions for 't Hooft's beam-splitter physical symmetry on the one hand and the observable difference and the observable invariance conditions for Galileo's ship physical symmetry on the other hand are arguably very much similar.

To see what this is let us ask the following: it is always the case that a given observable invariance condition holds in the world? The answer seems to depend on which condition that is. If that is the observable invariance condition for Galileo's ship physical symmetry, for instance, then the answer is no. For we know well that there are cases where this observable invariance condition, consisting in that phenomena within the cabin of a ship should be observably invariant for someone located within the cabin, is not satisfied. These are the cases where a ship instead of being boosted is accelerated (or pulled in an accelerated way by gravity) with respect to a reference. In other words, there happen to exist an observable difference condition which is similar to that observable difference condition which defines Galileo's ship physical symmetry (for both boosts and accelerations (or gravity transformations) amount to acquisitions of velocity) but incompatible with the observable invariance condition which defines Galileo's ship physical symmetry. I take the existence of such an extra observable difference condition to show that Galileo's ship physical symmetry is *non-trivial (or of limited validity),* and so I call *the non-triviality check* for empirical symmetries a check of whether there exists an observable difference condition for a given physical symmetry which is physically incompatible with the realisation of its observable invariance condition despite being similar to the observable difference condition of that physical symmetry to the same extent as accelerations are similar to boosts. This I call the non-triviality check *for empirical symmetries* because empirical symmetries are Galileo's ship-like physical symmetries and hence passing the non-triviality check is presumably necessary for being an empirical symmetry.

My claim is now that 't Hooft's beam-splitter observable physical symmetry as I defined it does pass the non-triviality check. To see this note that given our observable difference and observable invariance conditions for that physical symmetry the non-triviality check amounts to finding a setup sufficiently similar to the setups (2a), (2b) and (3), including as to the kinds of equipment, devices and particles it features, but where the set of devices used is such that the interference patterns are different in the two states. But that is so easy! The suitable setups are just the setups (1) and (4) or some other similar setups. They do feature the same kinds of equipment, devices and particles as the setups (2a), (2b) and (3), and yet exhibit different interference patterns in the two states while the latter setups do not. That is why I said above that on my account the setups (1) and (4) are not useless but ensure the non-triviality check instead.

What, however, about the comparison of the amounts of similarity? The changes in the sets of devices involved in the setups (2a), (2b) and (3) are the addition of a half-wave with a switched-on solenoid, the replacement of a half-wave plate by a switched-on solenoid and the addition of two half-wave plates respectively, while the changes in the sets of devices involved in the setups (1) and (4) are the addition of a half-wave plate and the addition of a switched-on solenoid respectively. So are the changes in the sets of devices involved in the setups (2a), (2b) and (3) as similar to the changes in the sets of devices involved in the setups (1) and (4) as boosts are similar to accelerations (or gravity transformations)? One way to answer yes is to note that the changes to compare belong to a single overarching category in either case. That is, boosts as well as accelerations belong to the single overarching category of changes in velocity, and similarly the changes in the sets of devices involved in the setups (2a), (2b) and (3) as well as the changes in the sets of devices involved in the setups (1) and (4) belong to the single overarching category of changes in the sets of devices which are half-wave plates and switched-on solenoids. One may still wonder whether this sort of similarity is enough. But my claim is now going to be that even it is missing for physical symmetries constituted by spatial translations and spatial rotations.

Indeed, given Brading and Brown's interest to Galileo's ship-like physical symmetries in their Section 1 Brading and Brown should not contest the idea to check whether physical symmetries constituted by spatial translations and spatial rotations are Galileo's ship-like physical symmetries. So in particular we have to check whether they satisfy the non-triviality check for empirical symmetries. The question is then whether we can find a physical change that would be as similar to spatial translations and spatial rotations as accelerations are to boosts and yet incompatible with the observable invariance of any phenomena (for it seems that any phenomena are invariant under spatial translations and spatial rotations). And that is where the problem lies, for I could not think of any change that would satisfy the requirements just described by involving the amount of similarity greater than or at least equal to that involved in the non-triviality check for 't Hooft's beam-splitter physical symmetry.

If no such changes exist, then 't Hooft's beam-splitter physical symmetry as I defined it is a Galileo's ship-like physical symmetry in the way physical symmetries constituted by spatial translations and spatial rotations are not. So if empirical symmetries are Galileo's ship-like physical symmetries and if the non-triviality check is a necessary condition for being a Galileo's ship-like physical symmetry, then 't Hooft's beam-splitter physical symmetry as I defined it can be qualified as *'t Hooft's beam-splitter empirical symmetry* similarly to how Galileo's ship physical symmetry can be qualified as *Galileo's ship empirical symmetry*, while physical symmetries constituted by spatial translations and spatial rotations cannot be qualified as empirical symmetries. And if DES should only arise on the basis of empirical symmetries, as the later literature on DES will assume, then we will only get DES on the basis of *'t Hooft's beam-splitter empirical symmetry* and *Galileo's ship empirical symmetry* but not on the basis of physical symmetries constituted by spatial translations and spatial rotations. Arguably, it was worth exploring the differences between the latter symmetries and Galileo's ship empirical symmetry to find that out. And now Brading and Brown's conclusions [p. 663] by which theoretical symmetries constituted by boosts but also other global external symmetries have DES have to be modified one more time.

Subsection II. 2. 3. 10. DES of external local symmetries and Einstein's elevator empirical symmetry

When discussing whether external local symmetries have DES Brading and Brown concentrate on the same phenomenon as Kosso. This phenomenon is actually what I have qualified as the non-triviality check for Galileo's ship empirical symmetry above, namely the fact that when one passes from a physical system stationary with respect to the ground to a physical system accelerated with respect to the ground this does not yield the observable invariance within a system. Both Kosso and Brading and Brown think that it is external local transformations that are to be matched with this phenomenon, and both deny that this matching makes such transformations lead to DES, but their reasons for denial are different. For Kosso such transformations cannot lead to DES because the invariance when passing to an accelerated system can only be restored by indirectly compensating acceleration by gravity. So he says that external local transformations lead to IES instead. Above I found most plausible to interpret his compensation proposal as involving a physical acceleration and a theoretical gravity, so I criticised this as mixing the physical level with the theoretical level. But Brading and Brown interpret Kosso's compensation differently, namely as involving theoretical things only. On their view the idea is something like that one first makes a theoretical description that matches with the physical passage from a stationary to an accelerated system described by Kosso and this theoretical description turns out not to be a theoretical symmetry, and then Kosso's proposal is to add theoretical gravity to this theoretical description so that to turn it into a theoretical symmetry. Brading and Brown then object that turning this theoretical description into a theoretical symmetry is incompatible with matching it with Kosso's phenomenon, because on their view to be matched with it this description should involve theoretical transformations of the matter fields without theoretical transformations of the metric field while to become a theoretical symmetry it should involve on the contrary theoretical transformations of both the matter fields and the metric field. So for Brading and Brown external local transformations fail to lead to DES because they cannot be matched with the phenomenon in question.

However, actually it does not matter whether external local transformations can be matched with the phenomenon in question. For this phenomenon is not an observable physical symmetry because of not satisfying the observable invariance condition. So it will not give rise to DES anyway. But this changes nothing to the question of whether there exist any other phenomenon which does satisfy this and other conditions in a way that makes external local symmetries to have DES. For the questions of this kind cannot be solved by considering a single phenomenon which does not qualify at least as an observable physical symmetry. I have insisted so much on that this cannot work when discussing Kosso's article above, and now Brading and Brown fall into the same trap. But they should have noticed that even on their own approach not any setup of 't Hooft's beam-splitter case constitutes an observable physical symmetry giving rise to DES. And yet it is possible to find within 't Hooft's beam-splitter case other setups that do qualify as observable physical symmetries giving rise to DES whether one's observable difference and observable invariance conditions and other criteria for giving rise to DES are like Brading and Brown's or not. And then presumably any setup, whether giving rise to DES or not, can matched with some theoretical transformations, and these transformations will be different for setups with incompatible observable features to account for. Thus for example a setup where the interference patterns are the same in the two states will be matched some theoretical transformations while a setup where the interference patterns are different in the two states will be matched with other theoretical transformations. But whatever the setup, among the theoretical transformations matched with it there will presumably always be local transformations in the broad sense (of which moreover it is very probably that at least some are internal), either because some global transformations in the broad sense can be matched with that setup and then my proof applies to generate suitable local transformations, or because we already have suitable local transformations to match them with that setup directly. So while some of internal local transformations will be anyway matched with setups that do not give rise to DES, other different internal local transformations will be anyway matched with setups that do give rise to DES. Hence although the former internal local transformations will not lead to DES, the latter internal local transformations will lead to DES, and hence *there will anyway exist internal local transformations that lead to DES*. And if these transformations preserve some observational consequences, which is ensured by requiring that some setups exhibit an observable invariance and that to be matched with them theoretical transformations should account for that observable invariance by preserving suitable observational consequences, then these transformations will constitute theoretical symmetries whether they preserve 'laws' or not, and these theoretical symmetries will then have DES. So *some internal local symmetries will anyway have DES*. And, in agreement with what both Kosso and Brading and Brown expect, the situation with external local symmetries is going to be analogous to that with internal local symmetries. Indeed, we simply have to run the argument just given again with 'internal' replaced by 'external' to see that *there will anyway exist external local transformations that lead to DES and external local symmetries that have DES*.

What, however, are in that modified argument the analogues of those setups of 't Hooft's beam-splitter case that give rise to DES if the analogue of those setups of 't Hooft's beam-splitter case that do not give rise to DES is the phenomenon constituted by Kosso's passage from a stationary system to an accelerated one? To find this out we may appeal to non-triviality checks. Indeed, the setups of 't Hooft's beam-splitter case that do not give rise to DES are those that ensure the non-triviality check for 't Hooft's beam-splitter empirical symmetry, so the setups of 't Hooft's beam-splitter case that give rise to DES are those that are instances of 't Hooft's beam-splitter empirical symmetry. Therefore, given that Kosso's passage from a stationary system to an accelerated one constitutes a phenomenon that, as I already said, ensures the non-triviality check for Galileo's ship empirical symmetry, the relevant analogues of the setups of 't Hooft's beam-splitter case that give rise to DES will be those passages that constitute phenomena which are instances of Galileo's ship empirical symmetry. So *some external local symmetries are going to have DES on the basis of Galileo's ship empirical symmetry*, surprising as this may be for Kosso and Brading and Brown.

What is perhaps a bit disappointing about the answer just given, however, is that there we do not get DES on the basis of a new empirical symmetry but on the basis of the one we already know. This does not constitute any danger for the answer's correctness, but raises the following question: cannot some external local symmetries get DES on the basis of some other empirical symmetry than Galileo's ship empirical symmetry? In principle why not. So let us now exhibit a different empirical symmetry giving rise to DES of some external local theoretical symmetries.

The first thing we can exploit for this is Kosso's compensation proposal and possible interpretations of it. When discussing this proposal (or more precisely its analogue from his analysis of whether internal local symmetries have DES) I have envisaged a third interpretation of it besides the two recently mentioned above. In this interpretation the compensation was neither between two theoretical changes like in Brading and Brown's interpretation of Kosso's text nor between a physical change and a theoretical change like in my preferred interpretation of Kosso's text but between two physical changes instead. So let us explore the idea that one can try to get an empirical symmetry out of a physical compensation between physical acceleration and physical gravity. I will now show that one can actually construct out of it a version of *Einstein's elevator empirical symmetry* and that some other versions of that empirical symmetry can also be constructed on the way.

The other thing that can help us is some material from Brading and Brown's text and from my analysis of it. It shows that what we should use more precisely to construct this empirical symmetry if we wish that theoretical transformations of acceleration and gravity can be said to lead to DES on its basis is the physical compensation not between physical acceleration and physical gravity but between what we could take to be their *observable indicators*.

To see this recall Brading and Brown's second objection [p. 658] to my observable invariance condition for 't Hooft's beam-splitter empirical symmetry above. There Brading and Brown seemed to say that the observable change in interference patterns is actually an effective difference condition which informs us of an underlying physical change in phase. Presumably it is because they were interested in whether theoretical phase transformations can lead to DES, i.e. in whether their counterpart phase transformations exist in the world, that they were seeking what would be an observable indicator of such unobservable physical phase transformations. I was however replying to their objection that we should not care about whether something is an observable indicator of an underlying physical transformation when defining observable physical symmetries, because doing otherwise would make us presuppose that DES is already established for theoretical counterparts of these putative underlying physical transformations. Because of that I proposed instead to concentrate on such observable indicators themselves and treat them simply as observable changes rather than as observable indicators of certain unobservable changes. For putative underlying physical phase transformations observable indicators could be observable changes in interference patterns or observable changes in the sets of devices present. Of these I chose the latter and ceased to count the difference condition by which the sets of devices present have to change as effective.

The idea is now that while thinking of putative underlying physical transformations is inadmissible when defining observable physical symmetries, it is admissible as a heuristic means when determining whether certain theoretical transformations lead to DES. More precisely, it is admissible to ask, if one is interested in whether certain theoretical transformations lead to DES, the existence of which physical transformations would be implied by such a DES and, in case these physical transformations would be unobservable, which observable changes could be their observable indicators, but this is admissible only if one does not presuppose that the observable changes in question indeed are the observable indicators of the unobservable physical transformations one is interested in. It is against that presupposition that my reply to Brading and Brown's objection was directed. So if one does not make that presupposition, the questions just mentioned are fine.

What is that the answers to these questions give us, i.e. what is the use of knowing what physical counterparts of given theoretical transformations and observable indicators of such physical counterparts could consists in? Arguably, this tells us that if observable changes which one could take for such observable indicators are indeed observed, then the theoretical transformation in question will be able to adequately represent them. This happens provided to determine what an observable indicator of a putative physical counterpart of a theoretical transformation could be one takes into account the observational consequences of the theoretical transformation in question. We can then ask what is it that the ability of a theoretical transformation to adequately represent a certain observable change gives us in turn, but here we already know the answer: this, together with the ability of the same theoretical transformation to preserve some observational consequences able to adequately represent what stays observably invariant under this observable change, is precisely what allows us to say that this theoretical transformation leads to DES on the basis of the observable physical symmetry constituted by the observable change and invariance in question.

Hence as said the appeal to putative observable indicators is a useful heuristics to determine whether a given theoretical transformation leads to DES, provided it does not presuppose in advance that the relevant DES is already established.

So let us use such putative observable indicators to construct an empirical symmetry with respect to which theoretical transformations involving theoretical acceleration and theoretical gravity can be said to lead to DES. Note to begin with that the physical counterparts of these theoretical transformations would have to be physical acceleration transformations and physical gravity transformations respectively. If these are thought of as putative underlying physical transformations, observable indicators for them could be some observable changes that we usually associate with such putative physical transformations. I suggest that a good *observable indicator of a putative physical acceleration transformation is turning on an engine* (when bringing about an observationally non-uniform displacement in time of our system equipped with this engine with respect to a suitable reference). Meanwhile, a good *observable indicator of a putative physical gravity transformation is the introduction of a spherical body with considerable and uniformly distributed mass*, which can be approximated for instance by the Earth. Now our first aim is to define an observable physical symmetry. This we do in the same way as I did this with 't Hooft's beam-splitter empirical symmetry above. Namely, we temporarily forget what the observable indicators are supposed to be indicators of and take them to be simple observable changes instead. This then allows us to use them to define an *observable difference condition for Einstein's elevator empirical symmetry*. It is going to consist in that a suitable engine becomes turned on or off and/or in that a massive body is brought in or removed. We next check what can be observably invariant under such observable changes and take the requirement that such observable invariance holds as our *observable invariance condition for Einstein's elevator empirical symmetry*. Any phenomenon satisfying these two conditions and other requirements making it a Galileo's ship-like physical symmetry will be *Einstein's elevator empirical symmetry*.

To determine more precisely what remains invariant under the observable changes of interest to us we may consider some phenomena where such changes occur in different combinations. We can choose any combinations we like, but to continue the analogy with 't Hooft's beam-splitter empirical symmetry I will list just a few of them which resemble the setups (1), (2a), (2b), (3) and (4). To obtain them we have to replace in these setups the particles and equipment by our system, e.g. a spaceship (originally it was supposed to be an elevator, whence the name of the empirical symmetry we are constructing). We will also need to replace half-wave plates and switched-on solenoids by massive bodies and turned-on engines. Again, the replacements of the latter devices can be made in any way we like, but here I will replace systematically a half-wave plate by a massive body and a switched-on solenoid by a turned-on engine. I will number the resulting phenomenon analogous to a given setup by the same number as that of this setup but with a prime in addition. Here are the phenomena that we get:

The phenomenon (1') amounts to adding a massive body while keeping the engine off. It consists in passing from a spaceship away of massive bodies with the spaceship's engine turned off to a spaceship near a massive body, e.g. the Earth, with the spaceship's engine turned off.

The phenomenon (2a') amounts to adding a massive body while turning the engine on. It consists in passing from a spaceship away of massive bodies with the spaceship's engine turned off to a spaceship near a massive body with the spaceship's engine turned on.

The phenomenon (2b') amounts to removing a massive body while turning the engine on. It consists in passing from a spaceship near a massive body with the spaceship's engine turned off to a spaceship away of massive bodies with the spaceship's engine turned on.

The phenomenon (3') amounts to adding two massive bodies while keeping the engine off. It consists in passing from a spaceship away of massive bodies with the spaceship's engine turned off to a spaceship near two massive bodies, e.g. the Earth and the Moon, with the spaceship's engine turned off.

The phenomenon (4') amounts to keeping massive bodies absent while turning the engine on. It consists in passing from a spaceship away from massive bodies with the spaceship's engine turned off to a spaceship away of massive bodies with the spaceship's engine turned on.

In such phenomena two kinds of observable features can be preserved: firstly the observable phenomena within the spaceship for someone located within it and secondly whether the spaceship is at rest or moving in a certain way with respect to a suitable observable reference (to suit the reference should be effectively unaffected by the introduction of massive bodies, thus either massless or much more massive than such bodies and located sufficiently far away but observable nevertheless). So requiring that one, the other or either of the two kinds of observable features be preserved yields a reasonable observable invariance condition.

The first observable invariance condition will thus consist in saying that the observable phenomena within our system should remain invariant for someone located within it. This is just like the usual observable invariance condition in Galileo's ship empirical symmetry, so with the first observable invariance condition Einstein's elevator empirical symmetry is indeed very Galileo's ship-like. This condition can be made to be satisfied by the phenomenon (1'), because adding a massive body does not change the observable phenomena within the spaceship for someone located within it. This case constitutes one of the recognised realisations of Einstein's elevator empirical symmetry mentioned in the brief presentation of DES from Part I. The other recognised realisation mentioned there consists in placing our system on the surface of an added massive body while turning the engine off. This other realisation likewise satisfies the first observable invariance condition. However, if we combine any of the physical states from one of these two realisations with any of the physical states from the other realisation, this will not yield a third realisation satisfying the same observable invariance condition because the phenomena within the system will look differently in these two states. In other words, the first realisation concerns two ways of realising some phenomena within the system and the second realisation concerns two ways of realising other phenomena within the system.

The second observable invariance condition says that the spaceship should remain either at rest or moving in a certain way with respect to a suitable reference. This condition is perhaps less traditional and harder to implement given the properties of the reference required, but seems admissible nevertheless. This observable invariance condition can be made to be satisfied by the phenomenon (2a') provided the engine pulls the spaceship in the direction opposite from that in which the spaceship is pulled when the massive body is introduced. The same observable invariance condition can also be made to be satisfied by the phenomenon (2b') provided the engine pulls the spaceship in the same direction as that in which the spaceship is pulled when the massive body is introduced. Of these phenomena at least the phenomenon (2a') can be taken to be a realisation of the purely physical interpretation of Kosso's proposal to compensate acceleration by gravity.

The third observable invariance condition combines the first two by requiring that both the phenomena within our system with respect to someone within it and the displacement of our system or the absence of such a displacement with respect to a suitable difference remain observably invariant. Even this condition can be satisfied, and this can be done in particular by the phenomenon (3') provided one takes as its initial physical state the one where a spaceship rotates around a reference, e.g. the Sun, and as its final physical state the one where to the previous state is added a system of two other massive bodies, e.g. the Earth and the Moon, so that the spaceship's original orbit becomes the orbit of one of the Lagrangian points in a system constituted by the spaceship, the Sun and the Earth. Moreover, here the Moon's presence is actually negligible, so excluding it while for the rest keeping the initial and the final physical states as just described yields an instance of the phenomenon (1') where the third condition is also satisfied.

Meanwhile, the phenomenon (4') does not satisfy any of the three observable invariance conditions, so it ensures *the non-triviality check for Einstein's elevator empirical symmetry* defined with any of these three conditions and hence the Galileo's ship-like character of this symmetry to the extent that it depends on this check. Curiously, we can actually recognise in the phenomenon (4') a phenomenon which also ensures the non-triviality check for Galileo's ship empirical symmetry. Whether this suggests that Einstein's elevator empirical symmetry and Galileo's ship empirical symmetry should be unified into a single empirical symmetry is an interesting question.

Our purpose however was to determine whether external local transformations can lead to DES on the basis of something else besides the usual Galileo's ship empirical symmetry. And arguably Einstein's elevator empirical symmetry that I have just constructed suits to answer that question affirmatively. Because firstly, it is different from the usual Galileo's ship empirical symmetry in that it involves the introduction of massive bodies; and secondly, by constructing Einstein's elevator empirical symmetry with putative observable indicators of putative physical transformations of acceleration and gravity we have got an empirical symmetry whose observable changes and possibly observable invariance can be accounted for by theoretical transformations of acceleration and gravity which are or can be made local. So external local transformations can lead to DES on the basis of at least the usual Galileo's ship empirical symmetry and Einstein's elevator empirical symmetry in the same way as internal local transformations can lead to DES on the basis of 't Hooft's beam-splitter empirical symmetry, in particular.

Section II. 2. 4. Summary of the analysis

Brading and Brown's description of Galileo's ship-like physical symmetries brings in three traits that have a potential to make Kosso's notion of observable physical symmetry stronger. One can also find in their text a potentially useful global/local distinction for theoretical states (although not explicitly classified as such). Their account has a merit of stating the conditions for assigning DES explicitly. Their discussion of 't Hooft's beam-splitter case is carried out in much more detail than Kosso's. Overall, Brading and Brown's text provides a rich material to discuss.

On the other hand, most of Brading and Brown's analysis does not hold. For one thing, it is incoherent in that Brading and Brown state their conditions for assigning DES for the theoretical approach but use the empirical approach instead when discussing at length 't Hooft's beam-splitter case. In addition, their neglect of global/local distinctions in their analysis of 't Hooft's beam-splitter case compromises the relevance of their Section 2 and their conclusions [p. 663]. Secondly, despite claiming the contrary Brading and Brown do not find any formal distinction which would coincide at least extensionally with the distinction between theoretical transformations leading to DES and theoretical transformations not leading to DES. Such unsuitable distinctions include all the global/local distinction they define, the distinction between transformations acting on parts of subsystems versus those acting on subsystems and the distinction between restricted and universal transformations. Thirdly, they do not articulate the link between theoretical symmetries with DES and theoretical laws. They also overlook the role of theoretical states of theoretical symmetries in the attribution of DES to the latter. Fourthly, they overlook the role of the observable invariance condition when stating their conditions for assigning DES, defining 't Hooft's beam-splitter observable physical symmetry and addressing the question of whether external local symmetries have DES. This results in the danger of assigning DES on the basis of what does not qualify as a physical symmetry, an unsatisfactory account of what counts as 't Hooft's beam-splitter observable physical symmetry and the discussion of Kosso's acceleration phenomenon instead of physical symmetries more relevant to the question they wanted to answer. Finally, they make assignments of DES to external global symmetries without any analysis, while the denial of DES to internal and external local symmetries that they argue for is actually incorrect.

Chapter II. 3. Healey [2009]

Section II. 3. 1. Brief summary of the text

In Section 1 [pp. 697-698] Healey says that the ability of a theory to exhibit empirical symmetries as perfect with the help of its theoretical symmetries provides a reason to believe in that theory. In Section 2 [pp. 698-701] he asks whether the matching of Faraday's cage empirical symmetry with electrostatic potential shifts bestows the same ontological significance on electromagnetic potential transformations as that bestowed on theoretical Galilean and Lorentz transformations by the matching of Galileo's ship empirical symmetry with theoretical boosts. In Section 3 [pp. 701-705] he defines perfect empirical symmetries as constituted by those transformations between situations that preserve all the intrinsic properties of the situations. In Section 4 [pp. 705-707] Healey defines his theoretical symmetries as constituted by those theoretical transformations between models that preserve the ability of the models to represent a given situation. In Section 5 [pp. 707-712] Healey argues that the answer to the question from his Section 2 is no because while theoretical boosts represent Galileo's ship empirical symmetry as perfect, electrostatic potential shifts do not represent Faraday's cage empirical symmetry as perfect when they make part of the electromagnetic potential transformations. In Section 6 [pp. 712-715] he claims that only restricted theoretical transformations can represent an empirical symmetry as perfect and that the relationship between theories and phenomena enhances our understanding of intrinsic properties. In Acknowledgements [p. 715] Healey reports Greaves' scepticism towards the key argument of his Section 5 by which theoretical boosts represent Galileo's ship empirical symmetry as perfect. So in Appendix A [pp. 715-717] he gives a more technical and more general version of that argument which shows how theoretical symmetries represent empirical symmetries as perfect. Finally, in Appendix B [pp. 717-719] Healey shows that using electromagnetic potential transformations one can obtain from a representation of Faraday's cage empirical symmetry based on electrostatic potential shifts another representation where both the observably uncharged and the observably charged cage are characterised by the same value of the electrostatic potential.

Section II. 3. 2. Extended summary of the text

Healey's article includes six sections and two appendices. In Section 1 he tells which kinds of matchings he will discuss. In Section 2 he presents Galileo's ship empirical symmetry and Faraday's cage empirical symmetry as well as two theoretical transformations they can be matched with. Sections 3 and 4 are devoted to the definitions of empirical and theoretical symmetries respectively. Section 5 explains why the two matchings from Section 2 have a different nature. Section 6 draws some conclusions. After Acknowledgements follow Appendices A and B which provide some complements to Section 5.

Healey's ***Section 1*** is short [pp. 697-698]. He highlights the importance of studying theoretical symmetries, mentions some works on them and notes that only some theoretical symmetries correspond to physical symmetries. He then says that he will discuss that subclass of such correspondences in which an empirical symmetry is exhibited as a consequence of a (certain kind of) theoretical symmetry. More precisely, an empirical symmetry will be exhibited as what he calls a perfect symmetry. He says that exhibiting an empirical symmetry in that way provides an especially satisfying explanation of it and gives reasons to believe in the theory whose theoretical symmetry makes such an explanation possible.

Section 2 [pp. 698-701] opens with a quote from Galileo [Galilei 1632/1967, pp. 186-187] describing *Galileo's ship empirical symmetry*. Are mentioned the physical transformation of a ship from standing still to moving uniformly, as well as the observable invariance under it of some phenomena within the boosted ship's cabin for an observer located within that cabin. The quote is useful in particular because of giving examples of preserved phenomena. These include "how the little animals fly with equal speed to all sides of the cabin" as well as how "[t]he fish [in a bowl] swim indifferently in all directions; the drops fall into the vessel beneath [from a bottle hanged above it]; and, in throwing something to your friend, you need throw it no more strongly in one direction than another, the distances being equal; jumping with your feet together, you pass equal spaces in every direction" ([ibid.], quoted at [p. 698]). Healey notes that for Galileo the transformation involved uniform circular motion more precisely [p. 699, n. 3], but the kind of relativity now associated with Galileo's name involves uniform rectilinear motion instead [p. 699]. He says however that the phenomena described by Galileo are preserved under either transformation [ibid., n. 3].

Afterwards Healey introduces *Faraday's cage empirical symmetry* (it is usually said to involve a cage rather than a cube because it holds even when the walls contain holes). He says that to implement it Michael Faraday "constructed a hollow cube with sides 12 feet long, covered it with good conducting materials but insulated it carefully from the ground, and electrified it to such an extent that sparks flew from its surface" [p. 699]. As we may note, this is actually the description of an observable difference condition (the sparks being observable). The description of an observable invariance condition next follows in Healey's quote of J. C. Maxwell's quote from an entry from Faraday's diary dated May 1836: "I went into this cube and lived in it, but though I used lighted candles, electrometers, and all other tests of electrical states, I could not find the least influence upon them" ([Maxwell 1881, p. 53] quoted at [p. 699]). (This is actually in §67 of Chapter V of [Maxwell 1881] where Maxwell compares electrostatic phenomena with those involving heat conduction. His point is that while the two are similar to some extent, if the cube was heated the result would be very different. He also says that the sparks were appearing constantly while the cube was in the charged state, see [ibid., p. 54].) Thus the charging of the cube resulted in the appearance of the sparks on its outer boundaries but preserved the electrical phenomena within the cage.

Next Healey mentions what he takes to be the most important common traits of Galileo's ship and Faraday's cage empirical symmetries [p. 699, p. 700] that will play a role in his account. These are that either *empirical symmetry* consists of a physical transformation together with two situations it links such that these situations are internally indistinguishable.

Note that Healey's *situations* are actually very similar to my *physical states* in the sense of collections of physical features of physical systems which can be linked by a physical transformation. However, there is an important difference in that for me *physical* means belonging to the actual world and hence my physical states are restricted to this world alone, while Healey's situations range on all possible worlds as he will explain in his Section 3. So I will keep using Healey's term rather than mine throughout most of my analysis of his text.

Then Healey mentions in addition (reminding us very much of Kosso's views) that either empirical symmetry was observed before it was accounted for by any theory and that in either case the observation prompted the elaboration of theories which could account for it. Examples of such theories are for Healey Newton's theory (i.e. classical mechanics with the absolute space, we may add) and special relativity for Galileo's ship empirical symmetry and Maxwell's theory (i.e. classical electrodynamics or classical electromagnetism, we may add) for Faraday's cage empirical symmetry.

After that Healey suggests matching the two empirical symmetries he has presented with some theoretical transformations of the theories he has just mentioned. Firstly, he matches Galileo's ship empirical symmetry with theoretical boosts. He considers these theoretical transformations as making part of theoretical Galilean transformations that constitute theoretical dynamical symmetries of Newton's theory and as making part of theoretical Lorentz transformations that constitute theoretical dynamical symmetries of special relativity. Secondly, Healey matches Faraday's cage empirical symmetry with *theoretical electrostatic potential shifts* of the form $\varphi \rightarrow \varphi' = \varphi + k$, where

φ is the electrostatic potential and k is a constant. He considers these theoretical transformations as making part of what he calls theoretical local gauge transformations that constitute theoretical symmetries of Maxwell's theory. The latter transformations are *theoretical electromagnetic potential transformations* of the form $A_\mu \rightarrow A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$, where A_μ is the electromagnetic potential and $\theta(x_\mu)$ (in Healey's notation $\Lambda(\mathbf{x},t)$) is a function.

As we may recall, these electromagnetic potential transformations are actually the transformations that we have already seen above when discussing the gauge principle as well as Brading and Brown's [2004] article. There we were speaking of these transformations as constituting theoretical symmetries of some Lagrangian densities involving interactions as well as of Brading and Brown's interaction equation (1.7). More precisely, to constitute these symmetries the transformations in question had to be applied jointly with the theoretical phase transformations of the form $\psi \rightarrow \psi' = \psi e^{iq\theta(\mathbf{x},t)}$. However, Healey in his text considers these electromagnetic potential transformations independently of the latter phase transformations (see [p. 700] and [ibid., n. 4]). So considered the transformations $A_\mu \rightarrow A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$ also constitute theoretical symmetries, but this time of the free electromagnetic Lagrangian density and of the equations of motion of free classical electromagnetism.

As also mentioned when discussing Brading and Brown's [2004] article, the electromagnetic potential A_μ can actually be decomposed into the magnetic vector potential **A** and the electric scalar potential φ also named the electrostatic potential. The latter is precisely the electrostatic potential φ figuring in Healey's electrostatic potential shifts $\varphi \rightarrow \varphi' = \varphi + k$ with which he matches Faraday's cage empirical symmetry. As Healey explains, the latter transformations are in fact a specific case of the transformations $A_\mu \rightarrow A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$ when $\theta(x_\mu)$ is chosen to be equal to kt [p. 700]. Indeed, as A_μ can be decomposed into **A** and φ, the transformation $A_\mu \rightarrow A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$ can likewise be decomposed into a transformation of **A** and a transformation of φ. It is in fact in this decomposed form that the transformation $A_\mu \rightarrow A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$ is presented (together with the transformation $\psi \rightarrow \psi' = \psi e^{iq\theta(\mathbf{x},t)}$) in Brading and Brown's [2004] equations (1.8). More precisely, the components of the transformation $A_\mu \rightarrow A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$ given in Brading and Brown's equations (1.8) are (with my and Healey's [p. 718] notational changes) the transformation $\mathbf{A} \rightarrow \mathbf{A}' = \mathbf{A} - \partial^i\theta(x_i)$, where $i = 1, 2, 3$ is a spatial index, and the transformation $\varphi \rightarrow \varphi' = \varphi + \partial^0\theta(x_0)$, where 0 is a temporal index and x_0 is thus the same (in the relativistic notation) as t. As Healey explains, when $\theta(x_\mu)$ is chosen to be equal to kt, the transformation of **A** just given reduces to the identity transformation while the transformation of φ just given reduces to the transformation $\varphi \rightarrow \varphi' = \varphi + k$ [p. 700]. It is with the latter transformation as arising from the transformation $A_\mu \rightarrow A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$ when $\theta(x_\mu)$ is chosen to be equal to kt that Healey matches Faraday's cage empirical symmetry [ibid.].

Healey does not say which distinctions he uses to classify the electromagnetic potential transformations $A_\mu \rightarrow A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$ as local and gauge but instead simply defines *theoretical local gauge transformations* as being the transformations of A_μ of the form $A_\mu \rightarrow A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$ [ibid.]. Nevertheless, as we know the characterisation of these transformations as local and gauge is justified at least on some global/local distinctions (including the global/local distinction for groups of transformations and the global/local distinction in the broad sense) and at least on some definitions of gauge transformations (namely those defining gauge transformations as internal local transformations or as local transformations whether internal or external). What Healey does say is that it is unclear whether his local gauge transformations, i.e. his electromagnetic potential transformations, constitute gauge symmetries in the sense of theoretical symmetries that do not have any ontological significance [p. 701]. This is because on the one hand this interpretation is traditional for those symmetries by contrast to the theoretical symmetries constituted by boosts which are usually interpreted as having some ontological significance, while on the other hand his matchings suggest, contrary to the traditional interpretations and contrary to Brading and Brown [2004], that the theoretical symmetries constituted by electromagnetic potential transformations have some ontological significance in the same sense as the theoretical symmetries constituted by boosts have it [pp. 700-701]. This constitutes *Healey's puzzle* that he promises to resolve in his Section 5 after having explained the physical and the theoretical sides of his framework in the two previous sections.

In Section 3 [pp. 701-705] Healey's aim is to describe a notion of empirical symmetry which would subsume Galileo's ship empirical symmetry as a special case. To do so he starts with some more general notions [p. 701]. Thus firstly *he defines a structure* as consisting of a domain of elements together with relations between these elements. One of Healey's examples of a structure is a structure of a physical object where the elements are parts of the object and the relations are properties and relations among these parts. Another example is a structure of a theoretical object which is the SU(2) symmetry group. *Healey then defines a symmetry of a structure* as an automorphism, i.e. a mapping of the elements of the structure back on to themselves so as to preserve the structure (he says [ibid., n. 5] that this definition is partly due to Fred Muller). Thus a symmetry transformation for Healey preserves the domain (elements) and the extensions of relations constituting the structure.

Healey then discusses *which structure is relevant to his topic*. He firstly says that structures may result from the representation by a theory of physical things with the help of abstraction and idealisation [pp. 701-702]. He also says that what can be (represented as) symmetric is a system, its situation or a class of situations [p. 702]. In this last case the elements of the structure are situations and symmetry transformations preserve the extension of relations between these situations. This Healey illustrates with the example of renderings of a tune. There the flawless renderings of the tune are elements of a subclass S belonging to a larger class C of all the renderings of the tune, the key transpositions are the symmetry transformations f when performed on the subclass S and the tune is a property P that these transformations preserve, so this property defines the subclass S [ibid.]. (Note that Healey uses K and D instead of my S and C but does so inconsistently at [p. 702], namely first with K denoting a subclass and D denoting a larger class and then the other way round, so instead I denote all his mentions of subclasses by S and all his mentions of larger classes by C.) Healey's idea is then that Galileo's ship empirical symmetry is to be formalised analogously to this last example with the symmetry transformations f being boosts, P being the dynamic structure of processes occurring in the cabin of a ship, C being the class of kinematically possible motions (i.e. those motions of the objects in the ship's cabin relative to the cabin in which every object has some continuous trajectory) and S being the class of dynamically possible motions (i.e. those kinematically possible motions that are compatible with the forces acting on the objects) [ibid.].

Next Healey wishes to characterise the invariance in Galileo's ship-like cases more precisely. To do so he distinguishes between *three kinds of invariance*: with respect to any measurements (observations) of a certain type [p. 703], with respect to any measurements (observations) of intrinsic properties of a situation [ibid.] and with respect to any intrinsic properties (whether observable or unobservable) of a situation [p. 705]. He calls the cases satisfying these kinds of invariance respectively *empirical symmetries with respect to a certain type of measurements*, *strong empirical symmetries* and *perfect empirical symmetries*.

Healey also gives examples of observations that on his opinion do not belong to intrinsic properties of a situation of a system because of rather concerning the relations of that situation of a system with the external environment [p. 703]. For a boosted Galileo's ship such observations include hearing the ship's wake, viewing it through a porthole and consulting a GPS device in the cabin, while for a charged Faraday's cage these include dangerously stepping out of the cage or more safely observing the sparks at the outer boundaries of the cage. Healey's point is that while such observable features are not preserved in Galileo's ship and Faraday's cage empirical symmetries [ibid.], what he classifies as observable intrinsic properties are to some level of approximation preserved in Galileo's ship case [p. 704] and apparently in Faraday's cage case too. So he classifies Galileo's ship empirical symmetry as an approximation to a strong empirical symmetry [ibid.] and apparently the same should also hold for him with respect to Faraday's cage empirical symmetry.

Another of Healey's ideas is that the situations constituting empirical symmetries may belong to the actual world or to other possible worlds [pp. 703-704]. In this we see one difference between Healey's notion of situation and my notion of physical state, because the qualification of a state as physical means for me that it pertains to the actual world in agreement with my more general practice of calling '*physical*' whatever pertains to the actual world, be that state, transformation, symmetry or something else. I make this restriction to the actual world because the present work is supposed to contribute to clarifying the ontology of the actual world. Healey's extension of his notion of situation to possible worlds, on the other hand, allows him to do several things.

Firstly, Healey's extension to possible worlds serves to give examples of strong empirical symmetries properly speaking rather than of approximate strong empirical symmetries. Thus Healey says that in a Euclidean space spatial translations and spatial rotations can constitute strong empirical symmetries properly speaking of actual or possible situations of geometrical figures [pp. 703-704]. Likewise, in a Newtonian world (by which Healey means apparently a possible world where Newton's theory holds) Galileo's ship case is a strong empirical symmetry properly speaking when confined to mechanical phenomena, while in a special-relativistic world (by which Healey means apparently a possible world where special relativity holds) it is a strong empirical symmetry properly speaking of also electromagnetic and all other phenomena [p. 704].

Another way in which the extension to possible worlds is apparently useful for Healey is that it allows him to admit that Kosso's observable difference and observable invariance conditions be satisfied counterfactually [ibid.]. It may be asked however whether this was not encoded in Kosso's conditions anyway because of the notion of observability being to some extent a counterfactual notion. In any case, Healey says that actual observations happen to be enough to establish Galileo's ship and Faraday's cage empirical symmetries [ibid.].

Finally, Healey uses the extension to possible worlds to contrast strong empirical symmetries and perfect empirical symmetries. As said above, the difference between the two for him consists that in the former empirical symmetries only observable intrinsic properties are preserved while in the latter empirical symmetries both observable and unobservable intrinsic properties are preserved. Healey says that situations sharing all intrinsic properties (and hence involved in perfect empirical symmetries) are called *(intrinsic) duplicates* and that all perfect empirical symmetries are strong empirical symmetries while the converse does not hold [p. 705]. He illustrates this by saying that in a Newtonian world Galileo's ship case is a strong empirical symmetry but not a perfect empirical symmetry because in such a world observable intrinsic properties are preserved but unobservable absolute velocities are not, while in a special-relativistic relativistic world Galileo's ship case is both a strong empirical symmetry and a perfect empirical symmetry because there are no unobservable absolute velocities in such a world [pp. 704-705].

In Section 4 [pp. 705-707] Healey discusses theoretical symmetries in a way analogous to his discussion of physical symmetries in his Section 3. Namely, firstly he says that theoretical symmetries in general can be those of equations of motion or those of a class of models picked by these equations of motion [p. 705]. He then concentrates on the latter option without endorsing the semantic approach [ibid.] (by which a theory just reduces to a class of models, we may add).

Healey says that *models* for him are theoretical structures that can be used to represent situations [ibid.]. So, similarly to how Healey's situations are close to my physical states, Healey's models are close to my *theoretical states* in the sense of theoretical constructions able to represent physical states. (This is not the only sense in which I use the latter term, for alternatively I call theoretical states whatever is linked by a theoretical transformation, particularly by a theoretical symmetry transformation. But in the context of my approach to DES the two senses coincide extensionally.) Moreover, there does not seem to be any significant difference between Healey's models and my theoretical states contrary to the significant difference in scope that I noted above between Healey's situations that range on all possible worlds and my physical states that range on the actual world alone. However, as because of the latter difference I will be keeping Healey's notion of situation in most of my analysis of his text, I will likewise keep his term of model throughout most of this analysis.

In agreement with his general definition of symmetry *a theoretical symmetry of interest to Healey* is a mapping among models. It then remains for him to say what is it that this mapping should preserve. Among the options he discards are the preservation of the class of models, some proposals inspired by [Ismael and van Fraassen 2003] (namely the preservation of qualitative features of models and of theory-laden measurable features) and the preservation of theoretical spacetime structure [pp. 705-706]. Healey's own proposal that he qualifies as narrower than those just discarded is to concentrate on those theoretical symmetries among models that are constituted by *theoretical transformations that preserve the ability of a model to represent a given situation* [p. 706]. Healey says that such theoretical transformations do not reduce to identity (i.e. that more than one model can represent a given situation) in gauge theories, in general relativity including in its coordinate-free formulation (where models are linked by diffeomorphisms) and in analytic Euclidean geometry (where models representing a given spatial configuration differ by choices of spatial origin, types of coordinates, choices of coordinate axes and orientations of axes) [pp. 706-707].

It is unfortunate that Healey calls these specific theoretical symmetries of interest to him *theoretical symmetries*, because this unjustifiably deprives the other kinds of theoretical symmetries of this name and at the same time makes a reader mix Healey's and other theoretical symmetries together. So instead I will call the specific theoretical symmetries just described *Healey's theoretical symmetries* (or *his theoretical symmetries* to avoid repetitions), which is arguably a more appropriate name.

Section 5 [pp. 707-712] is central to Healey's article and contains a resolution of the puzzle from his Section 2 as to whether the two matchings discussed there likewise bestow the ontological significance.

First Healey says that if an empirical symmetry is entailed by a theory, this fact can be used to explain an approximate empirical symmetry we observe [p. 707]. So in particular as Newton's theory entails a strong empirical symmetry constituted by boosts, Galileo's ship empirical symmetry that we observe can be explained by noting that the actual world is approximately a Newtonian world [ibid.]. However, Healey says, in the example just given this explanation works even though theoretical boosts do not constitute his theoretical symmetries in Newton's theory [ibid.]. Indeed, on Healey's opinion theoretical boosts do not constitute his theoretical symmetries in Newton's theory because on his opinion two models linked by theoretical boosts cannot both represent the same situation in Newton's theory [ibid.]. The latter idea in turn follows for Healey from the fact that in Newton's theory a theoretical boost changes absolute velocity [ibid.]. This absolute velocity in turn comes from the notion of absolute rest implied by Newton's postulation of the absolute space [p. 708]. So, Healey says, this illustrates Earman's [1989] idea by which the excess of dynamical theoretical symmetries over spacetime theoretical symmetries goes together with an excess in spatiotemporal structure, because in the case Healey discusses theoretical symmetries constituted by theoretical boosts are dynamical but not spacetime symmetries and this goes together with the postulation of the absolute space [p. 708]. Still, says Healey [ibid.], it is possible to eliminate the absolute space from the models of Newton's theory to obtain an empirically equivalent theory set in what Robert Geroch [1978] calls Galilean spacetime. In that theory theoretical boosts will constitute Healey's theoretical symmetries and so one will be able to explain Galileo's ship empirical symmetry by appealing to these theoretical symmetries [p. 708]. This revised Newtonian theory exhibits empirical symmetries constituted by physical boosts as not merely strong empirical symmetries but also perfect empirical symmetries [ibid.].

Healey next argues that the same has also happened with Maxwell's theory. Namely, firstly it treated theoretically differently the same phenomena depending on whether these involved bodies at rest or in motion, and then in 1905 Einstein formulated “a new understanding of Maxwell's theory” which eliminated this theoretical asymmetry at the price of revising our notions of space and time [ibid.] (and making them special-relativistic, we may add). In this way the previous explanation of these phenomena was replaced by an explanation based on a theoretical symmetry. Again, this explanation exhibited the relevant phenomena as not merely strong empirical symmetries but also perfect empirical symmetries [ibid.].

The common pattern of the two cases on Healey's view is as follows: one begins with an empirical symmetry; one explains it as a consequence of a theory where the explanation entails that the empirical symmetry is strong but not perfect; this theory is perceived as defective because its models attribute unobservable differences (namely differences in uniform absolute velocity in either case) to what looks like instances of the same situation; the theory is reformulated by removing the extra structure responsible for the defect in question; as a result, the theory features a new theoretical symmetry; this theoretical symmetry serves to provide another explanation of the empirical symmetry on which this empirical symmetry is not merely strong but perfect [pp. 708-709]. The differences between the two cases consist for Healey in that Einstein's theoretical revisions are more radical and concern dynamics in addition to spacetime, as well as in that they imply that Galileo's ship empirical symmetry should hold not only for mechanical phenomena but also for all others including electrodynamic ones [p. 709, n. 10].

Next Healey comes to explaining how it is that a theory exhibits an empirical symmetry as perfect when it has his theoretical symmetry. Indeed, Healey's theoretical symmetry only consists in that a theoretical transformation preserves the ability of a model to represent a given situation, so his theoretical symmetry only makes that we have at least two different models able to represent the same situation. However, exhibiting an empirical symmetry as perfect consists for Healey in representing two situations as (intrinsic) duplicates, i.e. as sharing all their intrinsic properties. So he owes us an explanation of how to pass from the ability of two models to represent the same situation to the representation of two situations as (intrinsic) duplicates. Healey describes this passage for special relativity (although he could have also done this for the revised Newton's theory or for the revised special-relativistic Maxwell's theory, we may add). His explanation of how the passage occurs takes the form of an argument that he describes in his Acknowledgements [p. 715] as “the key argument of [his] Section 5”. I will quote this argument in full:

“How exactly does the theory of special relativity imply that Lorentz boosts are perfect symmetries of situations represented by its models? Here is a sketch of the argument: further details are spelled out in Appendix A. Consider a localized situation s represented by model m of the special theory of relativity. s is localized because it occupies a compact region of spacetime. Because Lorentz boosts are theoretical symmetries of the theory, s is equally represented by a Lorentz-boosted model $m' = \Lambda_{-\boldsymbol{v}}(m)$: one can think of m' as representing s from the perspective of a frame F' moving at velocity $\boldsymbol{v}$ with respect to a frame F from whose perspective s is represented by m. Now special relativity itself implies that m also represents a distinct situation $s' = \Lambda_{\boldsymbol{v}}(s)$ in exactly the same way that m' represents s. Since m and m' represent s as having exactly the same intrinsic properties, it follows that m represents s and s' as having exactly the same intrinsic properties. Hence special relativity implies that Lorentz boosts are perfect empirical symmetries of all localized situations to which the theory applies” [p. 709].

Healey's question is next whether the situation with Maxwell's theory is analogous. Above he was already speaking of a revision of Maxwell's theory analogous to the revision of Newton's theory, but there the question was whether either theory is able to exhibit Galileo's ship empirical symmetry as perfect, and the answer was that it is. Now, however, Healey asks whether Maxwell's theory is able to exhibit Faraday's cage empirical symmetry as perfect. Answering this question would resolve *Healey's puzzle* which consisted in asking whether the two theoretical transformations that he proposed in his Section 2 to match with the two empirical symmetries he considers match with them in the same way and hence bestow the same kind of ontological significance on the relevant theoretical symmetries. Answering this question positively would go against the common opinion by which the theoretical symmetries constituted by the electromagnetic potential transformations $A_\mu \rightarrow A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$, of which the electrostatic potential shifts $\varphi \rightarrow \varphi' = \varphi + k$ are a special case, do not have any ontological significance while the theoretical symmetries constituted by the theoretical Galilean transformations or by the theoretical Lorentz transformations, of which theoretical boosts make part, do have ontological significance.

However, Healey's view is that the traditional opinion should not be changed. Indeed, he says that the two matchings he considers are different and that the argument he has just presented as applying to theoretical boosts does not work for the electrostatic potential shifts [pp. 709-710]. According to him, this is because "[s]pecial relativity itself implies that situation s' differs from situation s precisely by application of a Lorentz boost, thereby justifying the equation $s' = \Lambda_v(s)$. It implies that these situations differ in just this way, because every joint model in the theory of the combined situation $s \oplus s'$ represents s, s' as related by a Lorentz boost" [p. 710]. As we saw, the equation $s' = \Lambda_v(s)$ figures in Healey's argument quoted above. So Healey's idea is apparently that the use of this equation in his argument is only justified because this equation holds in special relativity for any joint model of the combined situation $s \oplus s'$. In other words, *Healey's additional requirement is that any joint model of a theory exhibits the two situations as linked by the same kind of transformations besides exhibiting them as (intrinsic) duplicates*. By contrast, says Healey [ibid.], in Maxwell's theory it is not the case that any joint model of the combined situation $C_0 \oplus C_+$ featuring the uncharged Faraday's cage together with the charged Faraday's cage respectively represents C_0, C_+ as related by an electrostatic potential shift, because some of them instead represent the interiors of both C_0 and C_+ as linked by an identity transformation as Healey will illustrate in his Appendix B. Therefore, the analogue of Healey's argument cannot go through because the relevant analogue of the equation $s' = \Lambda_v(s)$, namely the equation $C_+ = C_0 + k$, that he seems to need for this is on his view not justified.

More precisely, Healey says that because of the problem in question the analogous argument run with the electromagnetic potential transformations only establishes the existence of a situation C_+ different from the situation C_0 but not that these are involved in any empirical symmetry, whether prefect or not [ibid.]. On the other hand, Healey also says that the analogous argument does go through if run with spatiotemporal translations, because C_0 and C_+ are linked by a spatiotemporal translation in any model of Maxwell's theory. So for Healey C_0 and C_+ are exhibited by Maxwell's theory as not only distinct but also involved in a perfect empirical symmetry constituted by physical spatiotemporal translations [ibid.].

Next Healey asks however whether the analogous argument could not go through also in the case whose discussion according to Healey was suggested to him by David Wallace [ibid., n. 12]. This case consists in that in any model C_0 and C_+ are linked by the same value of the line integral L of the electric field along a curve joining a point on the interior of C_0 with a point in the interior of C_+ (where the value of L is independent of which points the curve consists in provided it starts and ends in the interior of the relevant cages) [p. 710]. As Healey says, the value of line integral L is invariant under theoretical electromagnetic potential transformations [p. 711]. Likewise, in special relativity frame transformations leave the difference in the four-velocity between s and s' invariant [ibid., n. 13]. Thus apparently *one more of Healey's requirements is that the difference between the two situations should be represented by an invariant value of a quantity*, and this requirement is satisfied in Wallace's case.

However, says Healey, the value of L represents a relation between C_0 and C_+ which "comes about only by virtue of their relations to the electromagnetic situation outside" the cages [p. 710] in the same way as the relation of two things consisting in that they have the same owner comes about by virtue of their relations to the owner [pp. 710-711]. So Healey classifies [ibid.] the former relation in the same way as the latter, namely as neither an *internal relation* nor an *external relation* in David Lewis's terms [1986, p. 62] (i.e. as not supervening on intrinsic properties of relata neither when the relata are taken separately nor when they are taken together). Healey calls that he follows the usual practice of calling such relations *extrinsic relations* [p. 711]. The upshot of this for Healey seems to consist in that because the value of L represents the relation between C_0 and C_+ as an extrinsic relation, it represents "the electromagnetic condition" inside the cage as not affected by charging it [ibid.]. So, we may add, this resembles Healey's objection against those electrostatic potential transformations (to be exhibited in his Appendix B) that are identity on the interior of the cage. Healey notes however that exhibiting the cages as linked by a given value of L remains possible in Maxwell's theory also when we exclude electromagnetic potentials from it, but in this case there are no electromagnetic potential transformations which leave the value of L invariant [ibid.]. Meanwhile, we may add, given what Healey says in Maxwell's theory with electromagnetic potentials there are the electromagnetic potential transformations which leave the value of L invariant, but then the problem is that the value of L represents the relation between C_0 and C_+ as an extrinsic relation. So apparently *Healey's extra requirement is that the relations between the two situations should be exhibited as supervening on the intrinsic properties of these situations, i.e. that these relations should be exhibited as either internal or external but not extrinsic.*

Now it happens that Healey does find a theory which on his opinion exhibits C_0 and C_+ as involved in a perfect empirical symmetry, and this theory is classical electrostatics [ibid.] There the electrostatic potential shifts $\varphi \rightarrow \varphi' = \varphi + k$ constitute Healey's theoretical symmetry [p. 712]. Moreover, Healey says that in this theory any joint model of C_0 and C_+ represents these situations as linked by an electrostatic potential shift $\varphi \rightarrow \varphi' = \varphi + k$, that all these electrostatic potential shifts associated with $C_0 \oplus C_+$ leave the value of k invariant and that they all represent the relation between C_0 and C_+ as external rather than extrinsic [pp. 711-712]. In other words, classical electrostatics satisfies all the three Healey's extra requirements that I have deduced above. If there is anything that Healey finds unsatisfactory about classical electrostatics, this is firstly that it only deals with cages at rest with respect to each other [p. 711] and secondly that it is only true in the absence of magnetic phenomena while out world does have magnetic phenomena [p. 712]. For the rest Healey finds that classical electrostatics exhibits Faraday's cage empirical symmetry as perfect in the same way as special relativity (together with revised Newton's theory and revised special-relativistic Maxwell's theory, we may add) exhibits Galileo's ship empirical symmetry as perfect [ibid.]. So Healey concludes that if only classical electrostatics was true in our world, the fact that his theoretical symmetry in that theory implies Faraday's cage empirical symmetry would give us an indirect empirical evidence that differences in the electrostatic potential are real in the same way as in our world we have indirect empirical evidence, in addition to direct one, to believe that differences in velocity are real [ibid.]

In Section 6 [pp. 712-715] Healey draws some conclusions.

His first moral [pp. 712-713] is that theoretical transformations can only represent empirical symmetries if these theoretical transformations act on less than the domain of the whole spacetime. Healey's argument [p. 713] is that this happens because theoretical transformations acting on the domain of the whole spacetime cannot make his argument from [p. 709] go through because it begins by requiring that a model be able to represent a situation localised in the sense of occupying a compact region of spacetime. Thus his conclusion is that in particular active diffeomorphisms on the whole spacetime featuring in the hole argument cannot represent empirical symmetries while restricted diffeomorphisms can represent what is now called Einstein's elevator empirical symmetry [p. 713].

Healey's second moral [pp. 713-714] is that the understanding of empirical symmetries as perfect comes from theories. In part this is because it is theories that often yield the best representations of the situations involved in empirical symmetries [p. 713], while in part, and more deeply, this is because our world seems to only exhibit approximate empirical symmetries and so we often use theories to abstract away some features of such phenomena as inessential [p. 714] (and, perhaps we may add here, to turn approximate empirical symmetries into strong empirical symmetries, in particular). Thus what models of a theory represent as situations involved in perfect empirical symmetries, as well as what they can be linked with by an isomorphism, are situations idealised with the help of the theory rather than crude phenomena [ibid.].

Healey then applies this second moral to the notion of intrinsic properties and duplicates [pp. 714-715]. Firstly, according to Healey when exhibiting an empirical symmetry as perfect a theory needs to say whether the relevant situations share all their intrinsic properties, hence it needs to say which properties are intrinsic and this allows us to learn what intrinsic properties can be [p. 714]. Secondly, according to him duplicate situations only share all intrinsic properties if the one-to-one correspondence between the situations induces an isomorphism between their intrinsic relations, and a theory can somehow clarify this by saying that duplicate situations are those related by a perfect empirical symmetry according to that theory [ibid.]. Finally, Healey says that when David Lewis was trying to articulate the link between theoretical and intrinsic properties, he was passing by the idea that theoretical properties coming from physics are natural in the sense of “carving at the joints” (as Plato was saying) a possible world, presumably ours, but that on Healey's opinion we need not assume that the physics' possible world is ours to link theoretical properties coming from physics with intrinsic properties in the way Healey proposed [pp. 714-715].

Next follow ***Acknowledgements*** [p. 715] where among other things Healey reports Hilary Greaves' persistent scepticism towards his argument at [p. 709] that I quoted above and says that this scepticism motivated him to formulate another version of the same argument (that he qualifies as more detailed, see [ibid.]) in his ***Appendix A*** [pp. 715-717].

In this Appendix Healey first of all gives a general argument about how one passes from one situation with two models to two situations for one model [pp. 715-716]. This argument goes as follows. Firstly Healey considers a model α as a structure with a domain, says that because α is a model, it represents a situation a, and then says that because there is his theoretical symmetry linking α with another model β, β also represents a by virtue of β's symmetry link with α (which preserves the ability to represent situations by definition of his theoretical symmetry) and by virtue of α's being able to represent a. Secondly Healey assumes that there exist an embedding of α and β into a joint model $\alpha \oplus \beta$ such that the domains of α and β as embedded in the joint model do not intersect, and says that because $\alpha \oplus \beta$ is a model, it represents a joint situation $a \oplus b$, and this while α as embedded in the joint model retains its ability to represent a. So Healey concludes that β as embedded in the joint model represents b and that correspondingly β as not embedded in the joint model also represents b, where b is distinct from a by virtue of the assumption that the domains of α and β as embedded in the joint model do not intersect.

Next Healey illustrates this general argument by specialising it to a specific kind of models and by concentrating on spacetime transformations [pp. 716-717]. Namely, he firstly assumes that a model is composed of a restricted region of a manifold, a restriction on that region of absolute objects defining spacetime properties and a restriction on that region of dynamical objects defining other properties such as electromagnetic ones. He then supposes that a representation function sends in particular a (possible or actual) region of spacetime where the situation a is located to the (theoretical) region of the manifold making part of a given model, as well as that this model is linked by his symmetry transformation with another model, where this transformation comprises in particular a diffeomorphism between the manifold regions of the two models such that this diffeomorphism is “representing a spacetime translation” [p. 716]. He next runs the argument again and concludes [p. 717] that the other model represents both the original situation a and a situation b such that b is distinct from a because of a and b being represented as occupying distinct manifold regions in the model of the joint situation $a \oplus b$.

Afterwards Healey claims more generally that when the embedding assumption holds (and so his argument goes through), theoretical symmetries involved imply perfect empirical symmetries because they allow the same model to represent two situations [ibid.].

Finally Healey says that if one model of some joint situation $a \oplus b$ represents a and b as linked by a transformation not reducing to a spacetime translation, but another model obtained for instance by applying his symmetry transformation to the first model represents them as linked by a transformation reducing to a spacetime translation, then the situations in question do not constitute a new perfect dynamical empirical symmetry [ibid.]. Meanwhile, says Healey, if all models of the joint situation $a \oplus b$ represent a and b as linked by a transformation not reducing to a spacetime translation and if moreover in all these models a theoretical transformation representing this link induces a certain difference between the dynamical objects associated with a and those associated with b, then the situations in question do constitute a new perfect dynamical empirical symmetry [ibid.].

Appendix B [pp. 717-719] is on another topic. There Healey describes the promised theoretical representation of $C_0 \oplus C_+$ where the electrostatic potential φ has the same value within both C_0 and C_+.

To understand how Healey does this it is useful to make *a digression about the relationship between potentials and field strengths in classical electromagnetism*. In the pre-special-relativistic formulation of Maxwell's theory the main variables are the magnetic field **B** and the electric field **E** as well as the magnetic vector potential **A** and the electric scalar potential φ also known as the electrostatic potential. Meanwhile, in the special-relativistic reformulation of Maxwell's theory the main variables are the electromagnetic field strength $F_{\mu\nu}$ as well as the electromagnetic potential A_μ. But **B** and **E** are actually the components of $F_{\mu\nu}$ in the same way as **A** and φ are the components of A_μ. So many things we can say of the relationship between the combination of **B** and **E** and the combination of **A** and φ also hold of the relationship between $F_{\mu\nu}$ and A_μ, and vice versa. So we will first discuss the relationship between $F_{\mu\nu}$ and A_μ because it is shorter to express. The relationship between A_μ and $F_{\mu\nu}$ is many-to-one, i.e. to many (but not all) values of A_μ corresponds the same value of $F_{\mu\nu}$. This is important because, unless we are in the context of the Aharonov-Bohm effect, passing from one value of A_μ at a given point to another value of A_μ at the same point such that both correspond to same value of $F_{\mu\nu}$ at this point seems to induce no change in observational consequences. In other words, the values of $F_{\mu\nu}$ define *equivalence classes* of values of A_μ with respect to observational consequences. Now such values of A_μ which correspond to the same value of $F_{\mu\nu}$ can actually be obtained by the very familiar to us electromagnetic potential transformations $A_\mu \rightarrow A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$, which *in this context*, because of the relationship between the observational consequences of different values of A_μ which correspond to the same value of $F_{\mu\nu}$, *are gauge transformations in the sense of observationally complete theoretical transformations*. Healey's idea is then to generate from an original representation of $C_0 \oplus C_+$ featuring some value of A_μ a new representation of $C_0 \oplus C_+$ featuring another value of A_μ by the gauge transformation in question so that to preserve the observational consequences by preserving the value of $F_{\mu\nu}$. The only thing is that he rather works in pre-special-relativistic Maxwell's theory in his Appendix B, so more precisely he is going to generate a new combination of values of **A** and φ from an original one by the gauge transformations on which the gauge transformation $A_\mu \rightarrow A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$ can be decomposed so that to preserve the observational consequences by preserving the combination of values of **B** and **E**.

Here is how Healey does this [pp. 717-719]. Firstly he assumes that the cages C_0 are C_+ are very big and located side by side with a small gap in between. This allows him to concentrate on a single dimension x defined by an axis such that it is directed from C_0 to C_+, its origin $x = 0$ is located on the wall of C_0 closest to C_+ and its point $x = a$ is located where the axis x crosses the closest wall of C_+. Besides, Healey assumes that the cages are at rest, which allows him to eliminate the magnetic field **B** from the theoretical representations of $C_0 \oplus C_+$, and that there is no electric field inside the cages (which is not question-begging if formulated in empirical terms, i.e. as a requirement on the observation and measurement results within the cages), which allows him to eliminate the electric field **E** from the theoretical representations of the interior of the two cages.

Under these assumptions the interior of C_0 can be represented in terms of potentials by $\varphi(x) = 0$, the interior of C_+ by $\varphi(x) = \varphi_0$ and the region in between by $\varphi(x) = \varphi_0 \cdot (x/a)$, i.e. by the value of $\varphi(x)$ which grows uniformly along the positive direction of the x axis in between $x = 0$ and $x = a$ (the points $x = 0$ and $x = a$ being excluded), while **A** in this representation is zero everywhere. This *original theoretical representation* of $C_0 \oplus C_+$ is encoded in Healey's equations (1). The point of choosing this particular representation consists apparently in that in this representation the interiors of the cages C_0 and C_+ are linked by the electrostatic shift $\varphi(C_0) = 0 \rightarrow \varphi(C_+) = \varphi(C_0) + \varphi_0 = \varphi_0$ which has the form $\varphi \rightarrow \varphi' = \varphi + k$. In terms of fields this representation corresponds to **B** being zero everywhere and to **E** being zero everywhere except in the region between the cages (the points $x = 0$ and $x = a$ being excluded) where $\mathbf{E} = -(\varphi_0/a)$. This is encoded in Healey's equations (7).

Next Healey applies to his original representation defined in terms of the potentials **A** and φ the gauge transformation $A_\mu \rightarrow A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$ decomposed into the gauge transformations for **A** and φ. The decomposition is the one I mentioned when discussing Healey's Section 2 above, namely (with some notational changes) into $\mathbf{A} \rightarrow \mathbf{A}' = \mathbf{A} - \partial^i\theta(x_i)$ together with $\varphi \rightarrow \varphi' = \varphi + \partial^0\theta(x_0)$. As said these transformations (with some notational changes) also make part of Brading and Brown's [2004] equations (1.8). As to Healey, he encodes them in his equations (2).

The function $\theta(x_\mu)$ in these transformations is arbitrary, so one may choose it as one likes. Healey chooses it to be $\theta(x_\mu) = -\varphi(x)$ and encodes this in his equations (3). When this choice is applied to the transformation $\varphi \rightarrow \varphi' = \varphi + \partial^0\theta(x_0)$ where φ is the $\varphi(x)$ from Healey's original theoretical representation, this has the effect of setting $\varphi(x) = 0$ everywhere along the x axis except for the points $x = 0$ and $x = a$. However, when the same choice $\theta(x_\mu) = -\varphi(x)$ is applied to the transformation $\mathbf{A} \rightarrow \mathbf{A}' = \mathbf{A} - \partial^i\theta(x_i)$, this results in that while **A** remains 0 elsewhere, it acquires the value $\mathbf{A} = t\cdot(\varphi_0/a)$ in between $x = 0$ and $x = a$ (these two points excluded). These results constitute *the obtained theoretical representation* of $C_0 \oplus C_+$ and are encoded in Healey's equations (4) and (5), where the equations (5) encode what happens with **A** on the axis x and the equations (5) encode what happens with **A** on the axes y and z as well as with φ (namely they all remain or turn to 0).

Healey's equations (6) encode the relationship between **A** and φ on the one hand and **B** and **E** on the other. I said that this relationship is many-to-one by have not said what it consists in. According to Healey's equations (with some notational changes), the relationship is more precisely as follows: $\mathbf{B} = \partial^\mu \times \mathbf{A}$ and $\mathbf{E} = -\partial^i\varphi - \partial^0\mathbf{A}$. As Healey says, applying this relationship to the obtained representation of $C_0 \oplus C_+$ leads to the same values of **B** and **E** as those to which the original representation leads, namely to the values of **B** and **E** encoded in his equations (7). Hence, we may say, it also leads to the two representations of $C_0 \oplus C_+$ being observationally equivalent.

However, while the original representation represents the interior of C_0 as characterised by $\varphi(x) = 0$ and the interior of C_+ as characterised by $\varphi(x) = \varphi_0$, the obtained representation represents the interior of either cage as characterised by $\varphi = 0$. So, Healey concludes [p. 719], he has obtained a promised representation of $C_0 \oplus C_+$ where the interior of either cage is represented as untransformed in terms of the electrostatic potential φ. He could have added that in this representation the interior of either of C_0 and C_+ is also represented, besides $\varphi = 0$, by $\mathbf{A} = 0$, $\mathbf{B} = 0$ and $\mathbf{E} = 0$, as well as consequently by $A_\mu = 0$ and $F_{\mu\nu} = 0$, and that in this representation the two situations are related by an identity transformation in terms of any of **A**, φ, **B** and **E**, as well as consequently in terms of either of A_μ and $F_{\mu\nu}$.

Note on the other hand that if Healey used Maxwell's theory without electromagnetic potentials instead, then the same thing happened there with a combination of values of **B** and **E** as what happened in this theory with a value of Wallace's line integral L. Namely, there would not then be different combinations of values of the potentials **A** and φ of which a given combination of values of **B** and **E** would be an invariant but just a single combination of values of **B** and **E** per observationally different joint situation $C_0 \oplus C_+$.

Section II. 3. 3. Analysis of the text

Subsection II. 3. 3. 1. Book versus article

Healey's [2009] article is not his first text on DES. Beforehand he published a book [2007] on the ontology of gauge theories where many topics later treated in his article are also briefly discussed. In particular, one finds in this book a description of Faraday's cage empirical symmetry [ibid., p. 4] and an analogy between gauge potentials and absolute velocities aimed at showing that neither should be granted an ontological significance [ibid., pp. 112-123]. In addition, the book contains an analysis [ibid., pp. 150-157] comprising most of the content of Sections 2, 3, 4, 5 from Healey's [2009] article and a mention of the result that we find in Appendix B of that article. This analysis also employs most of the notions and names for them to be found in the article, such as those of strong empirical symmetry, (intrinsic) duplicate, joint situation, (Healey's) theoretical symmetry and joint model, although the notion of perfect empirical symmetry does not receive any specific name yet. DES is I think nowhere named in this way explicitly, but Healey does speak of "empirical consequences" from [2007, p. 149] on.

The article and the analysis from the book thus mostly coincide in content, but the article was published later and a number of points are treated there in more detail, so I almost exclusively concentrate on the article in my work. There are however a few exceptions where what is said in the book completes or diverges with what is said in the article in the way important for my discussion, and I will mention some such points below. One complement that I will not discuss is also worth mentioning: this is Healey's analysis in the book [ibid., pp. 157-159] of whether local symmetries involving potential transformations have DES on the basis of the Aharonov-Bohm effect and of 't Hooft's beam-splitter case. There are several reasons why I will not discuss it: firstly, Healey's analysis relies on setups different from those we have discussed; secondly, as Healey says [ibid., p. 157, n. 1] the basic setup he discusses is very hard to implement; thirdly, I find his discussion too condensed to be evaluated properly; fourthly, Healey's discussion of the Aharonov-Bohm effect here is analogous to his discussion of the representation of Faraday's cage empirical symmetry in Maxwell's theory, while his discussion of 't Hooft's beam-splitter case is analogous to Brading and Brown's discussion of that case; fifthly, I believe that essentially the same analysis as the one I have already proposed for Brading and Brown's views and will propose for Healey's views on the representation of Faraday's cage empirical symmetry in Maxwell's theory could also apply to Healey's views on whether local symmetries involving potential transformations have DES on the basis of the Aharonov-Bohm effect and of 't Hooft's beam-splitter case.

While the description of the analysis from Healey's book [ibid., pp. 150-157] which will later develop into his article is somewhat hasty and unclear, the way the article is organised deserves a special mention. For one thing, as we know from the empirical cases Healey only discusses Galileo's ship and Faraday's cage empirical symmetries in his article. And that is arguably a good decision, because concentrating on less cases allows him to treat them in more detail and makes his analysis more transparent. Besides, Healey's article is conceptually well-structured: we do not find there problems proper to Kosso's text such as frequent confusions between the physical level and the theoretical level as well as contradictory statements [2000, pp. 86, 87, 91] about whether both ways of realising physical symmetries should be admitted. Also all the content of Healey's article is relevant to his analysis: there is no apparently idle material like Brading and Brown's [2004] Section 2 on the global/local distinctions (although on my view omitting the presentation on the alternative definitions of theoretical symmetries from Healey's Section 4 would not make any harm to the remainder of his analysis either).

Subsection II. 3. 3. 2. Kinds of DES

Healey's article also does not mention DES explicitly, and yet it is DES that it discusses. Indeed, all the familiar elements are there: an empirical symmetry, a theoretical symmetry and a matching between the two. Moreover, Healey inherits some important features from Brading and Brown's treatment of DES. One feature is the use of the empirical approach, i.e. the move from the empirical to the theoretical level. It is seen in that Healey firstly describes in his Section 2 how the two empirical symmetries are established independently of theories and then considers in his Sections 2 and 5 different matchings of them with theoretical symmetries. Another feature is the understanding of the matching between empirical symmetries and theoretical symmetries in the empirical approach as based on the ability of the latter to represent the former. This is seen in particular from the fact that Healey concentrates on theoretical symmetries whose transformations preserve the ability of models to represent situations.

However, there are also differences between Brading and Brown's account and Healey's, and a particularly important difference is that *Healey's approach implies that there are different kinds of DES*. Indeed, Healey concentrates on two kinds of matchings: those exhibiting an empirical symmetry as strong and those exhibiting it as perfect. So these different kinds of matchings can be said to lead to different kinds of DES. Meanwhile, Brading and Brown were also close to the idea of introducing different varieties of DES when they were willing to match the setups (2a) and (2b) of 't Hooft's beam-splitter case with their relative phase transformations despite the fact that these setups can also be represented using joint local phase and electromagnetic potential transformations [ibid., pp. 655-656]. However, the idea in question was neither spelled out by Brading and Brown nor used in any essential way by them by contrast to Healey.

Next, Healey's second matching based on exhibiting an empirical symmetry as perfect is *stronger* than his first matching based on exhibiting an empirical symmetry as strong in that the second matching comprises the first *plus an extra requirement*. Indeed, the second matching requires that two situations be exhibited as intrinsic duplicates not only with respect to all their observable features (as is needed to exhibit an empirical symmetry as strong) but also with respect to all their unobservable features. Hence we may say that Healey implicitly distinguishes between a weaker DES based on representing an empirical symmetry as strong and a stronger DES based on representing an empirical symmetry as perfect. Thus *Healey's approach can be said to also lead to a gradation of kinds of DES*, for which I will account by calling the two kinds of DES just mentioned respectively *Healey's weaker DES* and *Healey's stronger DES*. These names may seem however confusing in that the kind of DES based on exhibiting empirical symmetries as strong is here named Healey's weaker DES. But firstly, this DES is indeed weaker with respect to Healey's stronger DES, and secondly, it is actually also weak in another way.

Indeed, I already insisted when analysing Brading and Brown's text above that the requirement to be able to represent a given observable feature is not strong enough to preclude *the proliferation of matchings*. In fact even the requirement to represent all observable features there can be is most probably not enough to preclude the proliferation of matchings, because it seems that there will always be more than one theoretical construction able to accommodate any given set of observable features (recall e.g. Quine's arguments on *the underdetermination of theories by experience* in this respect). By that reason I will call any DES only based on the requirement to represent all observable features *a weak DES in the empirical approach*. A similar notion of *a weak DES in the theoretical approach* can be defined as comprising any DES only based on the requirement to instantiate some or all observational consequences. It also leads to the proliferation of matchings by a similar reason, namely by the fact that for any set of instantiated observational consequences there seem to always be more than one theoretical construction able to imply these observational consequences. Given this similarity I will later on speak of *a weak DES* simpliciter when referring to one or both of the varieties of DES just defined.

Now Healey's weaker DES is weak because it is a version of a weak DES in the empirical approach, and the latter DES is so qualified because it leads to a proliferation of matchings. Moreover, Healey's weaker DES is weaker than *the strongest weak DES in the empirical approach* based on the requirement to represent all observable features, because Healey's weaker DES is only based on the requirement to represent some observable features, namely all observable intrinsic features of some 'localised' situation [p. 709]. So if even the requirement to represent all observable features is not enough to preclude the proliferation of matchings, a fortiori any weaker requirement, and in particular Healey's requirement on which his weaker DES is based, is not enough to preclude the proliferation of matchings (and can be even expected to enhance it). And indeed, as we saw Healey finds a lot of theoretical symmetries which allow to represent a given empirical symmetry as strong.

A way to *reduce the proliferation of matchings* enabled by some DES is to introduce a stronger DES. This can be done *by strengthening a DES* we already have. The strengthening consists in that we add extra requirements that have to be satisfied to get a DES. To strengthen a DES in a way that reduces the proliferation of matchings we have to add those requirements whose satisfaction is independent from the satisfaction of the requirements needed to get the DES we started with. In that case it will not necessarily happen that all the theoretical symmetries having the original DES will also be able to have the strengthened DES. Instead, it may happen that we reduce the number of matchings as desired.

For Healey's weaker DES at least two kinds of such strengthenings are available. Firstly, we can require more observable features to be represented. I will call this *a weak strengthening* because a strengthened DES in that case will still be a weak DES in the sense above. On this road we can obtain in particular my notion of weak DES that I call *the observational DES in the empirical approach*, and we will see how to do so in this chapter. Secondly, we can add a requirement concerning the representation of unobservable features. On this road we encounter in particular Healey's stronger DES, because as said above it is a strengthening of his weaker DES such that it amounts to the satisfaction of his weaker DES plus an extra requirement concerning the representation of unobservable features. We can also combine both routes for instance by strengthening my observational DES with a requirement concerning the representation of unobservable features. In this case we can obtain in particular my stronger DES that I call *the ontological DES in the empirical approach*, and I will have more to say on it at the end of my analysis of Healey's text as well as in Part III.

Subsection II. 3. 3. 3. Healey's weaker DES: observable invariance

To begin with let us discuss Healey's weaker DES. The first question we can ask about any DES is why we should be interested in it at all. Healey's answer for his weaker DES would most probably be that it says us which theoretical symmetries can account for the characteristic observable traits of at least Galileo's ship and Faraday's cage empirical symmetries, for it is when seeking characteristic observable traits of these empirical symmetries in his Sections 2 and 3 that Healey deduces his notion of strong empirical symmetry which this DES requires to be able to account for. So, if it is indeed characteristic of these two empirical symmetries (and perhaps of empirical symmetries in general, e.g. because of them being defined as Galileo's ship-like and Faraday's cage-like physical symmetries) to be (approximately) strong empirical symmetries, then arguably it is worth knowing which theoretical symmetries can represent this fact adequately.

At least in his Section 2 Healey concentrates among the constituents of theoretical symmetries on theoretical transformations more precisely, which as we know is in line with Kosso's and Brading and Brown's way of analysing DES. So we have to determine firstly in which case for Healey theoretical transformations can be said to lead to his weaker DES. As *a theoretical* *symmetry has Healey's weaker DES* when it is able to represent the situations involved in an empirical symmetry as sharing all their observable intrinsic properties, *to lead to Healey's weaker DES a theoretical transformation* should preserve the representation of a situation as having given observable intrinsic properties without adding the ability to represent a situation as having extra observable intrinsic properties. Given that it is observational consequences that can account for any observable properties, this amounts to requiring that the theoretical transformation preserves observational consequences pertaining to the representation of all the observable intrinsic properties of a relevant situation without adding new observational consequences of that sort.

Therefore, to assess next whether a given theoretical transformation leads to Healey's weaker DES we need to determine what are all the observable intrinsic properties of an initial situation and what the theoretical transformation does with the observational consequences able to account for them. One issue is however that, as Healey explains in his Sections 3 and 6, in part it is theories that determine which exact theoretical transformations should be matched with which inexact physical transformations. So from this perspective the value of the assessment just described is to some extent questionable. Another issue is that to implement this assessment we would have to understand what counts as intrinsic. But it happens that Healey does not give any definition of this term. Fortunately, we can temporarily circumvent this problem by recalling that Healey's observable intrinsic properties in our context are just those phenomena within a relevant system (i.e. the ship or the cage) observed by someone located within it that are preserved when we pass from one to the other of the system's physical states (or actual and possible situations) constituting a given empirical symmetry. For it is when Healey wanted to describe these phenomena more precisely in his Section 3 that he identified them as being observable intrinsic properties. So we can replace the original requirement above by which to lead to Healey's weaker DES a theoretical transformation should preserve observational consequences pertaining to the representation of all the observable intrinsic properties of a situation involved in a given empirical symmetry (without adding new observational consequences of the same sort) by *a reformulated requirement* by which to lead to Healey's weaker DES a theoretical transformation should preserve observational consequences pertaining to the representation of all the phenomena within a system whose invariance is characteristic of a given empirical symmetry (without adding new observational consequences of the same sort).

Now Healey does not say in general which kinds of theoretical transformations are going to lead to his weaker DES, but makes instead some *specific positive claims about his weaker DES* particularly in his Sections 2 and 5. According to them theoretical boosts can be said to lead to his weaker DES on the basis of Galileo's ship empirical symmetry whether they belong to a theory without the absolute space, like special relativity and reformulated Newton's theory, or to a theory with the absolute space, like the original Newton's theory. Besides, electrostatic potential shifts for Healey can likewise be said to lead to his weaker DES on the basis of Faraday's cage empirical symmetry whether they belong to a theory where they are the only relevant transformations, like classical electrostatics, or to a theory where there are also other relevant transformations, such as the electromagnetic potential transformations not reducing to electrostatic potential shifts in Maxwell's theory.

If we then assess whether the theoretical transformations of the kinds just mentioned (i.e. theoretical boosts and electrostatic potential shifts) satisfy the reformulated requirement for leading to DES with respect to the empirical symmetries Healey associates with them, then we will see that transformations of these kinds can indeed satisfy that requirement. Hence Healey's view by which transformations of these kinds can be said to have his weaker DES is right. However, it must be noted that this holds for many but not all of the theoretical transformations of the kinds mentioned. For *those transformations of these kinds that represent a part of the relevant system as observably transformed with respect to the rest of it will not preserve observational consequences relevant to the representation of the phenomena within the system*. Hence such transformations will not qualify as leading to Healey's weaker DES by our reformulated requirement for leading to Healey's weaker DES, and neither will they qualify for this by Healey's original requirement given that both formulations have extensionally the same observational consequences to preserve. So although Healey claims in his Section 6 that only theoretical transformations restricted with respect to the whole universe can lead to DES, we see that more precisely not all theoretical transformations so restricted lead to DES at least if we deal with Healey's weaker DES on the basis of matchings between theoretical boosts and electrostatic potential shifts on the one hand and the two empirical symmetries Healey discusses on the other hand.

Healey's next proposal from his Section 2 Is to take into account that theoretical boosts pertain to theoretical symmetries constituted by theoretical Galilean or Lorentz transformations while electrostatic potential shifts pertain to theoretical symmetries constituted by the electromagnetic potential transformations. His idea is then that his matchings involving theoretical boosts and electrostatic potential shifts have an impact on the theoretical symmetries constituted by the theoretical transformations just mentioned of which the former transformations make part. Perhaps it would not be stretching too far to suppose that the impact on these theoretical symmetries for Healey consists precisely in that such theoretical symmetries receive his weaker DES by virtue of the matchings in question.

So interpreted Healey's position may well be held, because one may define conditions for assigning DES as one likes. However, as to myself I would not assign DES in that way. This is in particular because in the theoretical symmetries Healey associates with theoretical boosts there are other kinds transformations that are simply irrelevant to the matching he discusses. Indeed, as said for theoretical boosts the theoretical symmetries Healey has in view are those constituted by Galilean or Lorentz transformations. But at least Lorentz transformations are more than theoretical boosts, for they include theoretical spatial rotations in addition. But theoretical spatial rotations are irrelevant to the passage between the physical states (or actual and possible situations) constituting Galileo's ship empirical symmetry, because as just mentioned Healey only matches theoretical boosts with this passage. So the best is to take Healey's matching involving Galileo's ship empirical symmetry to only bestow his weaker DES on theoretical symmetries constituted by theoretical boosts but not on any larger groups of theoretical symmetries constituted by various kinds of theoretical transformations of which theoretical boosts make part. And more generally, *the best is to only match an empirical symmetry with those kinds of theoretical transformations which satisfy our requirements on representing it* but not with any larger groups of theoretical transformations and symmetries of which these kinds of theoretical transformations make part. As to how *a kind of theoretical transformations* can be defined in this context, my view is that we can take it to be determined by the variable(s) a transformation acts on. Thus transformations acting on φ, A_μ, velocity v and space x_i are all different kinds of theoretical transformations for me.

It must also be noted that the kinds of transformations Healey considers, namely theoretical boosts and electrostatic potential shifts, are both global in the sense of various global/local distinctions such as the global/local distinction in the broad sense and the global/local distinction for groups of transformations. So Healey's attributions of his weaker DES (and hence also of his stronger DES because of its being a strengthening of his weaker DES) fit the tradition initiated by Kosso and continued by Brading and Brown of attributing DES to theoretical symmetries constituted by global theoretical transformations. However, as the transformations Healey considers are also global in the sense relevant to *my proof to be presented in Part III*, the proof applies to them and therefore implies that there will also be other transformations local in the relevant sense that have the same observational consequences and hence will likewise lead to DES on the basis of the same empirical symmetries. So in particular, analogously to how both global and local theoretical transformations lead to a weak DES on the basis of 't Hooft's beam-splitter, Galileo's ship and Einstein's elevator empirical symmetries as I discussed when analysing Brading and Brown's [2004] text above, there are also going to be both global and local theoretical transformations leading to a weak DES on the basis of Faraday's cage empirical symmetry.

It is curious that it is actually Healey's text that contains *the method for obtaining local theoretical transformations with given observational consequences* for Faraday's cage empirical symmetry. This method can be found in his Appendix B and consists in that one starts with an original theoretical representation of an instance of Faraday's cage empirical symmetry built using an electrostatic potential shift, applies to that theoretical representation an observationally complete electromagnetic potential transformation $A_\mu \rightarrow A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$ and obtains an observationally equivalent theoretical representation of the same instance of that empirical symmetry. Healey employs this method to obtain a theoretical representation where both cages are represented as untransformed while the region between them is represented as transformed in a way different from that described in the original theoretical representation. However, he could have used the same method to obtain instead a theoretical representation where one or both of the cages become transformed in a way different from that described in the original theoretical representation and such that the two cages become represented as linked by an electromagnetic potential transformation local in a relevant sense. This theoretical transformation linking the representations of the cages would then be a local theoretical transformation that leads to a weak DES, and in particular Healey's weaker DES, on the basis of Faraday's cage empirical symmetry. My proof from Part III actually presents a generalisation of this second use of the specific method just mentioned and shows that this general method is widely applicable. I also explain in that part which important consequences this fact has. As to Healey, although he can thus be credited for having used the specific method just mentioned, firstly he has used it in a different way as just explained, secondly he has not generalised it in any way, thirdly he has not explained what this generalisation implies, and fourthly he used his method within the context of his weaker DES while this DES is actually unsatisfactory.

Subsection II. 3. 3. 4. A weak strengthening of Healey's weaker DES: observable differences

Indeed, above I have already highlighted that any matching relationship, and hence any DES arising from it, is unsatisfactory when based on requirements related to observable features alone, because this makes it too permissive and leads to the proliferation of matchings. However, there can also be reasons to say that among the matching relationships and among the kinds of DES based on requirements related to observable features alone some are less satisfactory than others, and now I wish to show that *Healey's weaker DES is unsatisfactory* in the latter sense too. Note that *exactly the same critique applies to Ladyman and Presnell's manuscript* [*2016/01/08*] *where exactly the same error to be now explained is made*. But that several persons do the same thing is surely not the reason to keep it doing.

To see where the problems come from note that the original or the reformulated requirement for leading to Healey's weaker DES only requires that a theoretical transformation preserves some observational consequences (whether related to the representation of observable intrinsic properties as in the former requirement or related to the representation of observable invariant properties confined to systems as in the latter requirement). So there is no requirement in Healey's framework by which a theoretical transformation has to change some observational consequences. Hence *Healey's framework actually allows all gauge transformations in the sense of observationally complete theoretical transformations to lead to his weaker DES and all gauge symmetries in the sense of observationally complete theoretical symmetries to have his weaker DES*. Thus for example it allows the observationally complete electromagnetic potential transformations $A_\mu \rightarrow A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$ to lead to his weaker DES, it allows observationally complete diffeomorphisms featuring in the hole argument to lead to his weaker DES, it allows observationally complete global spacetime transformations featuring in Leibniz and Clarke's debate to lead to his weaker DES, and so on. But that is a huge *proliferation of theoretical transformations and symmetries with DES* that is hardly admissible!

Besides, this is actually *incoherent with Healey's own views on the ontology of theoretical symmetries*. For Healey refuses to assign any substantial ontological significance to observationally complete theoretical transformations and symmetries, as can be seen from his denial to say that any electromagnetic potential transformations lead to his stronger DES and from his critique of the substantivalist position about spacetime [p. 713]. Moreover, this actually goes against Healey's claim from his Section 6 by which theoretical transformations universal with respect to the whole universe cannot lead to DES, because both global and local theoretical transformations are able to be observationally complete when they are universal in that sense.

Furthermore, *this just mixes up the topic of DES with the topic of whether observationally complete theoretical transformations and symmetries have any ontological significance*. I find these topics anyway related, as will be clear in particular from my Part III, but I do not think we need to mix the two. For what was novel about the topic of DES is precisely that it discussed the ontological significance of observationally incomplete theoretical transformations and symmetries while almost all the previous discussions presented in Part I (such as Leibniz and Clarke's debate, the hole argument, permutations of 'indistinguishable' objects, the gauge principle, Hamilton's principle and Noether's theorems) were apparently concentrating on the ontological significance of observationally complete theoretical symmetries instead. So the ontological significance of observationally incomplete theoretical transformations and symmetries has not been studied enough, but this does not mean that it is not worth studying, and thanks to the topic of DES we have an opportunity to do so. Consequently, my view is that we should keep understanding the topic of DES as primarily dealing with the ontological significance of observationally incomplete theoretical symmetries and DES itself as primarily a property of observationally incomplete theoretical symmetries. We should then concentrate on analysing DES so understood and should see where this leads us. And if this leads us to also addressing the question of the ontological significance of observationally complete theoretical symmetries, as I think it does, then we will have to return to the latter question, but on the way we will have discovered many things about the ontological significance of observationally incomplete theoretical symmetries, and that it why this road is worth pursuing.

But that is not all, for *requiring that a theoretical transformation accounts for observable differences is actually reasonable from different perspectives*.

First, on the one hand as I was saying myself there is no symmetry without invariance, but on the other hand arguably there is no non-trivial symmetry without a difference. And arguably it is better if DES involves non-trivial physical symmetries. Therefore, it should rather involve physical symmetries with physical differences.

Second, taking a physical difference in addition to a physical invariance to be constitutive of the kind of physical symmetries that give rise to DES allows to diminish the proliferation of matchings in the empirical approach adopted by Healey as well as Brading and Brown. Hence in this way we obtain a stronger and thus potentially more useful notion of DES in that approach.

Third, without physical differences being observable we just cannot *identify a phenomenon as a physical symmetry* because we cannot establish that a change has occurred. This point was made by Kosso when discussing observationally complete physical symmetries [Kosso 2000, p. 88] and will be further defended in my Part III.

Fourth, as already mentioned Healey's requirement by which theoretical transformations should account for the observable invariance for them to lead to his weaker DES is motivated in his Sections 2 and 3 by this invariance being characteristic of the empirical symmetries he considers. And indeed, if we wish to account for a specific kind of phenomena, it is specific features characteristic of this kind of phenomena that have to be primarily accounted for. But as we know, observable differences have also been presented as being characteristic of Galileo's ship-like physical symmetries, for Brading and Brown have described these physical symmetries as satisfying Kosso's observable difference condition. Moreover, Healey seems to accept Kosso's observable difference condition with respect to empirical symmetries as well [p. 704]. Furthermore, mentioning observable differences has become traditional for what are now taken to be the usual examples of empirical symmetries, as can be seen from Brading and Brown's as well as Healey's own descriptions of Galileo's ship case, 't Hooft's beam-splitter case and Faraday's cage case. Therefore, any DES based on accounting for characteristic observable features of empirical symmetries is incomplete when not requiring to account for observable differences proper to empirical symmetries.

Fifth, not only are observable differences not less characteristic of the usual empirical symmetries than are observable invariances, but the former are actually more characteristic than the latter when we deal with *macroscopic empirical symmetries*, i.e. with empirical symmetries typically involving macroscopic systems, such as Galileo's ship, Einstein's elevator and Faraday's cage empirical symmetries. In particular, as Healey insists [p. 708; p. 709, n. 10], as much mechanical as electromagnetic phenomena are invariant in Galileo's ship empirical symmetry, and as we may add, as much mechanical as electromagnetic phenomena are also invariant in Faraday's cage empirical symmetry, but as we may also notice, it is primarily mechanical (linked to boosts) observable differences that define Galileo's ship empirical symmetry, while electromagnetic (linked to charging) observable differences primarily define Faraday's cage empirical symmetry instead. So if we are to account for characteristic traits of the usual empirical symmetries, including the usual macroscopic empirical symmetries, we actually have more reasons to require that a theoretical transformation changes observational consequences relevant to the representation of the observable differences than we have to require that it preserves observational consequences relevant to the representation of an observable invariance.

Sixth, there is a way to argue that the former requirement is not in conflict with the original requirement by which to lead to Healey's weaker DES a theoretical transformation should preserve observational consequences pertaining to the representation of all the observable intrinsic properties of a relevant situation without adding new observational consequences of that sort. For one may argue that *the observational consequences that are required to change* do not concern the representation of observable intrinsic properties but *rather concern the representation of observable extrinsic relations*. This is because observable differences of macroscopic empirical symmetries are like Healey's and Lewis's example of the relation of having the same owner which Healey classifies as extrinsic. Namely, in the same way as in the latter case two things are only linked by the relation of having the same owner (or different owners) because of their relationships to the owner(s), in the former case two systems or situations only exhibit an observable difference because of their relationships to a reference. So the requirement to change observational consequences relevant to the representation of observable differences characteristic of macroscopic empirical symmetries is independent of the requirement to preserve observational consequences relevant to the representation of observable intrinsic properties because the former requirement is about observable extrinsic relations instead.

What happens if, convinced by the reasons as above, we *add the requirement to change observational consequences relevant to the representation of characteristic observable differences* to Healey's original or reformulated requirement by which to lead to his weaker DES theoretical transformations should preserve relevant observational consequences? As we are adding an independent requirement, we are likely to be able to eliminate some of theoretical transformations satisfying one or the other of our initial requirements because of them not satisfying the new requirement. More precisely, our specific added requirement allows us to we eliminate in particular all observationally complete theoretical transformations, but also all observationally incomplete theoretical transformations that do not change the observational consequences in the way we need.

To determine which observationally incomplete theoretical transformations are to eliminate note that *the realisation of observable differences for macroscopic empirical symmetries* usually consists in that in the first way we observably change a system with respect to an unchanged reference while in the second way we consider two systems observably different with respect to an unchanged reference. (If you recall me saying when discussing Kosso's article above that a reference also has to change, note that I was speaking of another reference there, namely the one which allows *to realise the observable invariance for macroscopic empirical symmetries* instead. For, as we will see in Part III, my position consists somewhat more precisely in that we need different references to establish the observable differences and to establish the observable invariance when we are realising a macroscopic empirical symmetry.) Therefore, as far as the first way is concerned we have to eliminate those observationally incomplete theoretical transformations that represent both our system and a reference as observably changed and leave those observationally incomplete theoretical transformations that represent the system but not the reference as observably changed. As to the second way, the use of theoretical transformations there is more contestable in general, and I will have more to say on this in Part III. But Healey finds such a use acceptable, as we see from his use of theoretical transformations to represent the relationship between two situations in a joint situation (where the latter is arguably just a realisation of an empirical symmetry via the second way). Given this we should eliminate for the second way those observationally incomplete theoretical transformations that represent the second system and the reference as observably changed while we can leave those observationally incomplete theoretical transformations that represent the second system but not the reference as observably changed.

In short, while Healey's initial requirement (whether in its original or its reformulated form) of being able to preserve some observational consequences only makes us exclude some observationally incomplete theoretical transformations and hence the theoretical symmetries constituted by these transformations, adding the requirement of being able to bring about a suitable change in observational consequences allows us to exclude all observationally complete theoretical transformations and some more observationally incomplete theoretical transformations as well as all the theoretical symmetries constituted by these transformations. So as expected, and as is desirable, the independent requirement allows to strengthen Healey's weaker DES into we may call *a strengthened Healey's weaker DES*.

More precisely, it is *a weakly strengthened Healey's weaker DES*, because like the initial requirement the added requirement also concerns the observable features and hence *the resulting DES is still a weak DES* in the sense above. Moreover, if the initial requirement is used in the reformulated form, then the DES we have obtained is actually what I call *the observational DES in the empirical approach*, because I define the latter as a DES that a theoretical symmetry has if it is able to adequately represent the observable invariant features and the observable differing features constituting an empirical symmetry. By *the features constituting an empirical symmetry (or the features constitutive of an empirical symmetry, or simply the constitutive features)* I mean the features necessary and sufficient for a physical symmetry to be counted as an empirical symmetry. For Galileo's ship and Faraday's cage empirical symmetries I take the constitutive features to be in particular those features that are usually taken to be characteristic of these empirical symmetries, namely the observable differences in uniform rectilinear movement of a system for the former of these empirical symmetries or the observable differences concerning sparkles on the outer boundaries of a system for the latter empirical symmetry together with the observable features invariant within a system for either empirical symmetry.

Note finally that despite the strengthening brought about by our additional requirement we still have a lot of theoretical transformations leading to the DES we have obtained, as expected from the fact of its being a weak DES. In particular, *my proof still applies* and therefore implies that not only some global but also some local theoretical transformations are going to be matched with any given empirical symmetry. Thus in particular Healey's matchings of Galileo's ship and Faraday's cage empirical symmetries with global theoretical transformations alone remain incomplete even after the addition of our requirement. Moreover, Healey's claim in his Section 6 by which theoretical transformations universal with respect to the whole universe cannot lead to DES remains wrong because local theoretical symmetries seem to be able to be observationally incomplete in the sense needed to satisfy our added requirement as much when they are restricted as when they are universal. Meanwhile, Healey's claim from the same section by which only theoretical transformations restricted with respect to the whole universe can lead to DES now needs extra corrections, for one now has to exclude not only those restricted transformations mentioned above that fail to satisfy the initial requirement for leading to DES (whether in its original or its reformulated form), but also those restricted transformations that fail to satisfy the requirement we have added.

Subsection II. 3. 3. 5. Healey's stronger DES:
unobservable intrinsic properties

Returning now to Healey's weaker DES in its original form, as said there is another way to strengthen it which consists in adding requirements concerning unobservable features. This is the way Healey takes by introducing his stronger DES. To remind, Healey's weaker DES is my term to designate Healey's case where a theoretical symmetry is able to represent an empirical symmetry as strong, while Healey's stronger DES is my term to designate Healey's case where a theoretical symmetry is able to represent an empirical symmetry as perfect. While representing an empirical symmetry as strong consists for Healey in representing the situations involved in that empirical symmetry as sharing all their observable intrinsic properties, representing it as perfect consists for him in representing this situations as intrinsic duplicates, i.e. as sharing all their observable and unobservable intrinsic properties.

As before we need to know in which case *a theoretical transformation can be said to lead to Healey's stronger DES*, and what we can infer from the definition of the latter is that this can happen when this theoretical transformation preserves the representation of a situation as having given observable and unobservable intrinsic properties without adding the ability to represent a situation as having extra observable or unobservable intrinsic properties. Given that it is observational consequences that can account for any observable properties and that it is theoretical underpinnings that can account for any unobservable properties, this amounts to requiring that the theoretical transformation preserves observational consequences and theoretical underpinnings pertaining to the representation of all the intrinsic properties of a relevant situation without adding new observational consequences and theoretical underpinnings of that sort.

To assess which theoretical transformations satisfy this requirement we next have to know in particular *what are the intrinsic properties* in question. However, as you may recall I claimed that it is not clear which properties should be counted as intrinsic in Healey's framework already when we needed to understand this above for determining which theoretical transformations lead to Healey's weaker DES. But I then circumvented this problem for observable intrinsic properties by proposing an alternative way of capturing the relevant properties that does not rely on the qualification of them as intrinsic. Namely, I described these properties as observable properties confined to the system and observably invariant for someone located within it. However, for unobservable intrinsic properties it does not seem possible to recur to a similar move, because we do not seem to have another means of capturing the properties in question. To be sure, we have indexical means of referring to them like when we speak of “those unobservable intrinsic properties that are involved in Galileo's ship empirical symmetry” or “those unobservable properties underlying the observable intrinsic properties (or my extensional analogue of the latter) involved in Galileo's ship empirical symmetry”, but such means are probably not of much help when we wish to check whether the representation of a situation as having certain unobservable intrinsic properties is preserved. Therefore, in the case of unobservable properties we cannot proceed to evaluate Healey's claims without understanding what counts as intrinsic.

Why is that unclear in Healey's framework? As Healey suggests in his Section 6, this understanding comes in part from theories, but more precisely, it is supposed by Healey to come from the theoretical symmetries that have his stronger DES. But clearly this is of no help for those willing to establish which theoretical symmetries have his stronger DES or his weaker DES, because they have to begin by the understanding of what counts as intrinsic rather than to end up with it. Meanwhile, if we search the understanding in question from scientific theories of physics more generally, we are not going to find one there. For it does not look like scientific theories of physics typically come with a classification of properties as intrinsic or extrinsic. Indeed, it is rather when "physical science merges with metaphysics", to take Healey's expression [p. 714], that we get classifications of that sort. As to pure "physical science", it often comes without a metaphysical interpretation, as can be seen from the fact that multiple ontological interpretations have arisen for quantum mechanics or from the (arguably also metaphysical enough) fact that the geometrical interpretation of special relativity was proposed by Hermann Minkowski in 1907 while Einstein formulated special relativity in 1905. So it seems that simply turning to scientific theories of physics is insufficient to determine what counts as intrinsic, although mixing them up with some metaphysics could perhaps help.

Thus we have at least *three options for dealing with Healey's unobservable intrinsic properties*: *first*, search for a metaphysical definition of what counts as intrinsic; *second*, take for granted Healey's claims as to which theoretical transformations preserve the representation of a situation as having some unobservable intrinsic properties and deduce what counts as unobservable intrinsic properties on that basis; *third*, eliminate the mention of the intrinsic character of unobservable properties from the requirement by which to lead to Healey's stronger DES a theoretical transformation should preserve the representation of a situation as having some unobservable intrinsic properties.

Clearly, none of these options is beneficial to the status of Healey's analysis. For the first option makes the validity of this analysis conditional on one's metaphysical views; the second makes the value of this analysis questionable because it does not propose any motivation for Healey's classification of properties as intrinsic; while the third demands to revise Healey's original views on his stronger DES by eliminating the mention of intrinsic character of unobservable properties from it and suggests that we may likewise eliminate the mention of intrinsic properties from Healey's weaker DES e.g. in the way I did it above. So from the point of view of any of these options Healey's framework is or risks to be compromised.

To avoid ending up our own analysis here *let us however suppose that concerning unobservable intrinsic properties the scenario most favourable to Healey occurs*, namely that the second option holds and that moreover a solid motivation for it can be found e.g. via the first option. To be clear, there is no necessity whatsoever for this supposition to hold. I am making it just to investigate whether it saves some of Healey's specific claims about his stronger DES or not.

The supposition just made amounts to accepting *Healey's claims* by which the ability to represent the situations involved in an empirical symmetry as sharing all their unobservable intrinsic properties holds for theoretical boosts in the reformulated Newton's theory, reformulated (special-relativistic) Maxwell's theory and special relativity with respect to Galileo's ship empirical symmetry as well as for electrostatic potential shifts in classical electrostatics with respect to Faraday's cage empirical symmetry. From these claims we can infer some useful things.

First of all, they allow us to infer something about *Healey's views on unobservable intrinsic properties*. Namely, as we know the transformations in question consist in changing values of velocity and of electrostatic potential variables. And if such changes amounted for Healey to representing changes in unobservable intrinsic properties, then Healey's claims just supposed to be right could not be right. Hence what we can infer is that changing the values of velocity and of electrostatic potential variables does not amount for Healey to representing changes in unobservable intrinsic properties. It is moreover reasonable to suggest that what we have just inferred holds because for Healey velocity and electrostatic potential values do not amount to representing unobservable intrinsic properties.

In fact, we should also suggest that by contrast values of absolute velocity do amount to representing unobservable intrinsic properties for Healey, because a difference in such values is for him a reason for not assigning his stronger DES. But this classification of absolute velocity as intrinsic is arguably very implausible, because 'absolute velocity' is actually a velocity relative to the absolute space, and the usual metaphysical account of intrinsic properties says that they do not depend on anything external, while here the absolute space is external to a thing characterised by an absolute velocity. One could object that absolute velocity is not a velocity relative to the absolute space but the velocity in the absolute space, because the absolute space should contain all the things instead of being just compared with them. But even if this was right, I do not see why this would make out of absolute velocity an intrinsic property of a thing characterised by it unless the usual metaphysical account becomes distorted. So *one of Healey's negative specific claims about his stronger DES* by which theoretical boosts do not lead to his stronger DES in theories with the absolute space is probably going not to hold even on the very favourable supposition we have made above.

On the other hand, Healey's claims presented above as being implied by the supposition in question allow us to confirm *Healey's positive specific claims about his stronger DES* by which theoretical boosts from theories without the absolute space and electrostatic potential shifts from classical electrostatics lead to Healey's stronger DES. To see this note that the complement by which leading to Healey's stronger DES differs from leading to Healey's weaker DES is satisfied by these transformations by virtue of the claims in question. Indeed, the complement consists in requiring that theoretical transformations be able to represent the situations involved in an empirical symmetry as sharing all their unobservable intrinsic properties, and the claims consist in saying that the theoretical boosts and electrostatic potential shifts from the theories just mentioned have this ability with respect to some relevant theoretical symmetries. Therefore, it remains to show of the theoretical boosts and electrostatic potential shifts from the theories just mentioned that they also lead to Healey's weaker DES. But if we still agree to eliminate the mention of intrinsic properties from Healey's weaker DES, then all suitable theoretical boosts and electrostatic potential shifts whatever the theory they come from will lead to Healey's weaker DES by my analysis above, and so this will hold in particular of the theoretical boosts from theories without the absolute space and of the electrostatic potential shifts from classical electrostatics. On the other hand, as the revision of Healey's weaker DES is favoured by the third option above while we have supposed that the combination of the first and the second options holds instead, we may also wish to retain the mention of intrinsic properties in Healey's weaker DES. But in this case too we can ensure that the theoretical boosts and electromagnetic potential shifts from the theories just mentioned lead to Healey's weaker DES. This can be done for instance by supposing in addition that the representation of a situation as having some observable intrinsic properties is determined by the representation of a situation as having some unobservable intrinsic properties so that any theoretical transformations which preserve the latter representation will also preserve the former. In any of the cases just described we thus get that theoretical boosts and electrostatic potential shifts from the theories mentioned above lead to Healey's weaker DES and hence (given Healey's claims made true by our supposition above) that they lead to Healey's stronger DES too. More precisely, however, *in any of these cases it is only subsets of theoretical boosts and of electrostatic potential shifts from the theories mentioned above that will lead to Healey's weaker DES and to his stronger DES* because in any case we still have to exclude those theoretical boosts and electromagnetic potential shifts that represent a part of a system as observably transformed with respect to another.

Thus what we get so far is that Healey's positive specific claims about which theoretical transformations lead to his stronger DES can be made correct and that we can also extract some of

Healey's views about the extension of the class of unobservable intrinsic properties. However, this all rests on a rather shaky foundation which was provided by our main supposition about unobservable intrinsic properties above as well as by some more suppositions and restrictions just described. But what I want to show next is that even if the foundation in question somehow turned out to be warranted and Healey's positive specific claims about his stronger DES as well as his views just mentioned somehow turned out to be unconditionally right, this would not help much in saving the remainder of Healey's analysis including in particular his key argument and his other specific claims about his stronger DES on the basis of Faraday's cage empirical symmetry.

Subsection II. 3. 3. 6. Healey's key argument: a failure

To see this let us start by discussing Healey's main argument that he qualifies in his Acknowledgements as "the key argument of [his] Section 5" [p. 715]. The versions of that argument given at [p. 709] and in Healey's Appendix A sometimes complement one another in that some stages of the argument are more clearly described in one of the versions as compared to the other, but Healey intends both to exemplify the same argument and they can be taken to do so indeed. For the rest the difference between the two versions is only in generality, namely at [p. 709] Healey takes as theoretical symmetry transformations theoretical boosts in special relativity and as an empirical symmetry Galileo's ship empirical symmetry, while in his Appendix A all the elements of the argument are first given in a generic form and afterwards theoretical symmetry transformations are supposed to include spatiotemporal translations and also models take a more specific form.

The premiss of Healey's argument is anyway the supposition that Healey's theoretical symmetry obtains. According to Healey's definitions, *his models* are theoretical structures that may be used to represent situations [p. 705] and *his theoretical symmetry* consists in that any model of a theory that may be used to represent a given situation in some possible world is sent by a theoretical transformation to another model of that theory also able to represent that situation [p. 706]. Healey wants his argument *to conclude* that when his theoretical symmetry obtains, the same model represents at least two distinct situations as intrinsic duplicates. He wants to get this conclusion because on his opinion it *amounts to demonstrating* that his theoretical symmetry allows to represent an empirical symmetry as perfect. In fact, however, Healey's argument cannot and need not work like Healey intends it to do.

It cannot work like Healey thinks it does because Healey's premiss is actually neither sufficient nor necessary to demonstrate what Healey wants to. Indeed, firstly Healey's premiss is not sufficient to obtain Healey's conclusion, because the definition of Healey's theoretical symmetry which his premiss postulates to occur only speaks of models as being able to represent situations, while Healey's conclusion speaks of a model as representing at least two situations as intrinsic duplicates. So Healey's conclusion contains several things missing from his premiss, namely that the ability of some model to represent situations is realisable towards at least two situations, involves representing a situation as having some intrinsic properties and is realised towards the two situations in a way that they are represented as intrinsic duplicates. On the other hand, Healey's premiss is also not necessary to obtain Healey's conclusion. To be sure, Healey's premiss is not completely useless, because it says that models are able to represent situations, and this is a pre-requisite for the models' ability to represent situations to be realised in the way we need. However, that models are able to represent situations does not constitute the content of Healey's premiss but rather constitutes its own premiss. Indeed, Healey's premiss does not consist in that models are able to represent situations but rather consists in that when they are able to do so, there is a transformation which preserves this ability. What does consist of saying that models are able to represent situations is just Healey's definition of his models. So it rather this definition that one has to presuppose if one wishes to conclude that a model represents at least two situations as intrinsic duplicates. But for our purpose this presupposition is no stronger than Healey's premiss, and so it is not sufficient to obtain Healey's conclusion either. For *one still has to show firstly*, that the ability of some model to represent situations is realisable towards at least two situations, *secondly*, that this ability involves representing a situation as having some intrinsic properties, and *thirdly*, that this ability is realised towards the two situations in a way that they are represented as intrinsic duplicates. *Besides*, this only entails what Healey wanted his conclusion to demonstrate if one shows additionally that these situations form an empirical symmetry. So Healey needs to join an altogether different argument to his definition of his models to establish these four points.

Can the rest of Healey's framework ensure that these points hold? Hardly so.

Firstly, one could recall that Healey makes appeal to possible worlds and thus suggest that among all possible worlds there will anyway be a possible world where there exist at least two situations such that a model can realise its ability to represent situations towards at least these two situations. However, this need not be the case, because the model may be such that a situation representable by it should be unique in a world. For instance, this may happen when the model depicts a situation as being unique in the world and provided for a model to realise its ability to represent a situation we demand that the situation occurs just as the model says it does. Or this may happen when supposing that there is more than one situation representable by a given model is in contradiction with what is allowed in possible worlds, i.e. induces a metaphysical impossibility. One way to preclude such problems is to allow inter-world couples of situations, and another is to impose suitable restrictions as to which models are admissible, but Healey's framework does nothing of the sort.

Secondly, while a model in Healey's framework has the ability to represent situations, it does not necessarily have the ability to represent situations as having any intrinsic properties. For instance, it is compatible with Healey's definition of his models that a model only assigns to a situation properties that do not qualify as intrinsic. One could think that there is in Healey's framework a restriction of his models to those which represent situations as having intrinsic properties. But there is no mention whatsoever of this restriction in either version of his argument, and I do not think having seen any in the remainder of Healey's text either. What we find in the argument is only Healey saying at [p. 709] that two models he takes from special relativity represent a situation as having the same intrinsic properties, i.e. assign the same intrinsic properties to it, from which we can infer that either model represents a situation as having some intrinsic properties. But firstly, the latter idea is not demonstrated by Healey in any way, i.e. it is not shown that special-relativistic models do assign intrinsic properties to situations, and it would actually be unclear how Healey could show this given that he did not say which properties these models assign to a situation to begin with and given that as explained above Healey does not say either what intrinsic properties are in a way that does not already presuppose exhibiting empirical symmetries as perfect, which is however what it only to be demonstrated by the end of the argument. And secondly, even if special-relativistic models do happen to assign intrinsic properties to situations, there is no mention in the argument of whether one has to require more generally that other models have this ability and of whether and when this requirement can be satisfied. In particular, there is not even a single mention of the term 'intrinsic' in Healey's more general version of his argument from his Appendix A. Note moreover that it is actually not enough for Healey's purposes that a special-relativistic or another model simply has the ability to represent a situation as having some intrinsic properties. For the latter requirement can also be satisfied by only assigning observable intrinsic properties to a situation, in which case the two situations such a model would represent as having the same intrinsic properties would not be represented as intrinsic duplicates in the sense of situations sharing both observable and unobservable intrinsic properties. So Healey would have to require more precisely that a model represents a situation as having some observable and some unobservable intrinsic properties rather than some unspecified intrinsic properties.

Thirdly, as representing is a human activity, a model may fail to realise its representational ability by pragmatic reasons. For instance, a model may fail to realise its ability to represent a situation if we just do not use it to represent a situation. Likewise, a model may fail to realise its ability to represent a situation towards two situations, even if there are two situations representable by that model, in case we are interested in representing a single situation. Also, even if a model has the ability to represent a situation as having some intrinsic properties, it may not realise that ability if we are interested in those properties the model assigns to a situation that are not intrinsic properties. Similarly, a model may happen to realise differently the latter ability if we are interested in applying different parts of a model to different situations, so that a model may happen in particular to assign different intrinsic properties to different situations. One could think that in Healey's case specific interests are salient, consisting in that Healey wishes to represent two situations by the same model so as to exhibit them as intrinsic duplicates. But actually Healey need not have such interests, and we need not have such interests either, as I will explain below respectively in this subsection and in the next one.

Fourthly, even if we suppose that there exist two situations such that they are representable and represented as intrinsic duplicates by a model, it does not follow that they form an empirical symmetry. For one thing, this is because it seems that for Healey situations only form an empirical symmetry if they look like intrinsic duplicates, while our suppositions just made do not guarantee that they look in that way. Indeed, these suppositions can be fulfilled for instance by the existence of two situations that look like differing in some observable intrinsic property but can be and are represented as intrinsic duplicates because the model only accounts for the observable intrinsic properties they share and/or for some unobservable intrinsic properties. To avoid this problem one would have to require that the situations be represented as sharing all the observable intrinsic properties they have and that moreover the situations be indeed such as the model depicts them.

In sum, *to attain what Healey wanted to demonstrate*, namely that the same model represents two situations involved in an empirical symmetry as intrinsic duplicates, we need to add a lot to Healey's definition of models as structures that are able to represent situations. Namely, *we may need to add* that a model we consider is such that it allows a situation it can represent to be not unique in a given world, is able to represent that situation as having some observable and some unobservable intrinsic properties, does realise its ability equally towards at least two situations, does so in a way that depicts the situations as sharing all the observable intrinsic properties they have and that moreover the situations are such as the model depicts them. But what we do not need to add is the supposition that Healey's theoretical symmetry obtains. And this actually undermines the whole purpose of Healey's argument.

Indeed, what Healey intended to show us by his argument is that his theoretical symmetries yield to theories a crucial advantage of allowing to represent empirical symmetries as perfect by virtue of allowing the same model to represent situations involved in these empirical symmetries as intrinsic duplicates. But as follows from what is just explained, what he would have shown if the argument is carried out properly is rather that his theoretical symmetries are completely unnecessary to represent empirical symmetries as perfect in so far as his theoretical symmetries are completely unnecessary to allow the same model to represent situations involved in these empirical symmetries as intrinsic duplicates. In other words, when carried out properly the argument establishes the complete opposite of what Healey wanted to show: his theoretical symmetries come out to be just useless for his purpose.

Why does that happen? The answer is simple: to show that the same model can represent two situations involved in an empirical symmetry as intrinsic duplicates we need not know whether that model forms a theoretical symmetry when combined with a suitable theoretical transformation and with another model. For to show that one model can do this work we need just one model. We do not need other models nor any theoretical transformations.

So, does it follow that theoretical symmetries are completely unnecessary to represent empirical symmetries as perfect? Not at all. What follows, to repeat, is only that they are completely unnecessary to the former purpose in so far as they are completely unnecessary to allow the same model to represent situations involved in these empirical symmetries as intrinsic duplicates. It does not follow that theoretical symmetries are unnecessary to represent empirical symmetries as perfect in some other way. Healey's error was to concentrate on that way of representing empirical symmetries as perfect that makes his theoretical symmetries useless. So the solution is to concentrate on another way of representing empirical symmetries that makes his theoretical symmetries useful – and necessary. That is why I said a few paragraphs above that Healey need not be interested in the case where two situations (involved in an empirical symmetry) are represented by the same model as intrinsic duplicates. For what he should be interested in is rather how to make an empirical symmetry to be represented by his theoretical symmetry.

Before discussing how to do that let us note that the task just formulated clearly belongs to the empirical approach: we start with an empirical symmetry and we ask how to match it with a theoretical symmetry of some kind, where the matching is understood as a representational relationship whose construction likewise proceeds from the empirical level (where the given observable physical thing to be represented is located) to the theoretical level (where something theoretical is to be found or built to represent that physical thing). This placement of the task within the empirical approach is reasonable because the representational understanding of the matching as well as the direction from the empirical level to the theoretical level more generally are as we know dominant in Healey's own analysis in his Sections 2 and 5. However, looking more attentively to his argument from the latter section we notice that it is actually formulated in the theoretical approach instead: the presupposition that Healey's theoretical symmetry obtains comes first and the matching with an empirical symmetry comes second. Thus Healey has carelessly switched to the theoretical approach in the meantime. And this is not without consequences, for the theoretical approach determines the form of the requirements I deduced above under which the four points hold. Indeed, in our particular case we wanted to match the same model with two situations involved in an empirical symmetry so that it represents them as intrinsic duplicates, so we imposed some requirements on the abilities of a Healey's model plus the requirement that a world be such that there exist situations allowing these abilities to be realised. Meanwhile, if we proceeded within the empirical approach instead, we would rather wish to match two situations involved in an empirical symmetry with the same model that represents them as intrinsic duplicates, so we would rather impose some requirements on the properties of the situations (namely that they exhibit observable invariant intrinsic properties) plus the requirement that there exists a model such that it has a suitable representational ability (of representing situations as having observable and unobservable intrinsic properties) and equally realises it towards these two situations. So we see more generally that *the specifying requirements concern the physical (empirical, observational) level in the empirical approach and the theoretical level in the theoretical approach, while the existence requirements concern the theoretical level in the empirical approach and the physical level in the theoretical approach*. When one allows to consider all possible worlds, as Healey does, the existence requirements for the theoretical approach can usually be satisfied easily, but when one restricts the analysis to the actual world, as in my case of discussing the ontology of our world, this no longer happens, and so the existence requirements for the empirical approach come out as easier to satisfy, whence the advantage of using the empirical approach over the theoretical approach in my context. I will develop this point further in Part III. In the context of making theoretical symmetries useful for empirical symmetries, however, the theoretical approach may seem not only more advantageous than the empirical approach but also necessary in that in the theoretical approach we start with something theoretical and so can choose to start with a theoretical symmetry while in the empirical approach we seem not to be guaranteed to have any theoretical symmetry even by the end. Under this impression Healey's switching to the theoretical approach for his argument could seem justified. But that is a wrong impression, for the things we start and end up with (by contrast to which of these things is theoretical and which is empirical) are not a function of an approach but of how we apply it. So as just shown we may use the theoretical approach so that to start with a single model, but we may also use the empirical approach so that to end up with a single model. And likewise we may use the theoretical approach so that to start with theoretical symmetries, but we may also use the empirical approach so that to end up with theoretical symmetries.

Subsection II. 3. 3. 7. Healey's key argument: a replacement

What we wish to do is more precisely to make Healey's theoretical symmetries both useful and necessary to represent empirical symmetries.

To begin with, note that Healey's theoretical symmetry enables two models to represent a single situation, while an empirical symmetry for Healey involves two situations. However, the latter *wider situations*, although typically observably different in some way, look identical when restricted to what happens within a system, so their restriction to what happens with observable properties within the system can be treated as a single *restricted situation*. Therefore, *we can use Healey's theoretical symmetry to represent an empirical symmetry on the condition that the situation that the two models involved in Healey's theoretical symmetry are both able to represent is that restricted situation which both wider situations involved in an empirical symmetry share*. Under that condition we can pass indeed from the fact that two models are involved in Healey's theoretical symmetry to the usefulness of the latter for representing an empirical symmetry.

In that case *one kind of restricted representation of an empirical symmetry by a theoretical symmetry* may consist in that the restriction of one of the wider situations involved in an empirical symmetry is represented by one of the models involved in Healey's theoretical symmetry, the restriction of another of the wider situations involved in the same empirical symmetry is represented by another of the models involved in Healey's theoretical symmetry and the relationship between the instances of the restricted situation within the two wider situations involved in that empirical symmetry is represented by the theoretical symmetry transformation linking the two models in Healey's theoretical symmetry. This kind of representation may look familiar, and indeed we have actually seen Healey making such representations all the time. In his Appendix B, for instance, his original representation is the one where (with slight notational changes) the interior of the observably uncharged Faraday's cage C_0 is represented by $\varphi(x) = 0$, the interior of the observably charged Faraday's cage C_+ is represented by $\varphi(x)' = \varphi_0$ and the relationship between the two is represented by the electrostatic potential shift $\varphi(x) \rightarrow \varphi(x)' = \varphi(x) + k$ with $k = \varphi_0$. Here supposing that Healey's theoretical symmetry obtains for the models $\varphi(x) = 0$ and $\varphi(x)' = \varphi_0$ linked by the latter electrostatic potential shift and that the condition mentioned above is satisfied allows these models to be useful for representing the two situations C_0 and C_+ involved in Faraday's cage empirical symmetry.

A similar passage from the supposition that Healey's theoretical symmetry obtains to the kind of restricted representation just mentioned can actually be found towards the beginning of Healey's argument at [p. 709]. Indeed, Healey starts there by saying that two models m and m' linked by his theoretical symmetry transformation, namely a theoretical boost $m' = \Lambda_{-v}(m)$, can represent the same situation s, and then says that these models can also be used to represent respectively two situations s' and s presumably involved in Galileo's ship empirical symmetry. If he stopped with his argument there, that could make his theoretical symmetries useful to represent his empirical symmetries. Instead he ends up the argument as we know by the idea of representing two situations by the same model, and for this his theoretical symmetries are as explained no use. It is remarkable that in his book [2007, p. 154] Healey actually does drop this last part of the argument. So he starts again by saying that two models linked by a theoretical boost can represent the same situation, and then he says again that these models can also be used to represent two situations involved in Galileo's ship empirical symmetry, and then the argument ends there. So he could have established the usefulness of his theoretical symmetries for representing empirical symmetries in the book, but this of course provided he had explained why *the argument in this shortened form* has to hold. And as I have just shown, such an explanation is indeed available and involves formulating the condition that I have given above. But we do not find anything like this condition in Healey's argument neither in the book nor in the article.

The only apparently relevant thing Healey says at [ibid., p. 154] is that while two models can represent the same situation in both Newton's theory and special relativity, two models can represent two duplicate situations only in special relativity. So we infer that on Healey's view the argument should not work in the shortened form if there is the absolute space in a theory. But in fact when the argument in this form does not work, the reason for this can be the opposite of what Healey takes it to be. Namely, it is not that the absolute space makes a theory unable to represent two duplicate situations by two models, but rather that it can make the theory unable to represent one situation by two models. Indeed, two models in Newton's theory can become unable to represent the same situation for instance if firstly they are embedded in a common absolute space so as to occupy different regions there, secondly the same situation is thought of as a unique situation rather than as a type of situation, thirdly that unique situation is individuated or at least characterised by saying that it occupies a specific unique location in the absolute space, and fourthly that location is to be represented. For then if one of the models represents the location of that situation in the absolute space correctly, the other will be unable to do so. Thus we have an example of a theory with the absolute space for which Healey's argument in the shortened form fails in a certain context because in that context this theory does not give rise to a case where two models can represent the same situation. In other words, the argument fails because in that context the models of this theory do not give rise to Healey's theoretical symmetry. And yet, it is actually possible for these models to represent two situations even under all the four suppositions just mentioned. This shows that Healey's theoretical symmetries are not necessary for representing two situations in that context. Meanwhile, because special-relativistic models are unable to represent any position with respect to the absolute space at all, under the four suppositions just mentioned they will also be unable to represent the same situation, but moreover they will be unable to represent two situations either. So contrary to what Healey envisages there can actually be a context where only Newton's theory and not special relativity can represent two situations by two models.

However, our context is anyway not the one where the four suppositions are satisfied. For our condition above by which a situation with respect to which two models should form Healey's theoretical symmetry is a restricted situation instantiated by two different wider situations cannot hold unless the restricted situation in question is not a unique situation but a type of situation defined by generic features alone. Hence in our context two models from Newton's theory on the contrary can represent the same situation and so they can form Healey's theoretical symmetry because the situation we are interested in being represented by two models is such that it allows this. Therefore, if the wider situations which instantiate the restricted situation we are interested in being represented are the wider situations involved in Galileo's ship empirical symmetry and if the restricted situation which the two models can represent consists of the observable features confined to the ship whose invariance is characteristic of that empirical symmetry, these two models can be used to represent that empirical symmetry by a restricted representation of the kind described above, namely by a representation where one model represents the instantiation of the restricted situation by one wider situation while another model represents the instantiation of the restricted situation by another wider situation. Thus in our context Healey's argument in the shortened form actually works for models of Newton's theory, and in fact it also works in the same way for models of special relativity. Thus Healey's theoretical symmetries from either theory can be useful for representing empirical symmetries in the way described.

But this is not all that we wanted to establish, for we also wanted to show that it is necessary to represent empirical symmetries by Healey's theoretical symmetries. So have we established this at the same time as establishing the usefulness of the latter to represent the former? This does not seem to be the case. For if we are really only interested in representing what happens observably within a system, and if what happens observably within a system can really be taken not to change for the wider situations involved in an empirical symmetry, then we can still be happy with representing two instances of the same restricted situation involved in an empirical symmetry by a single model. In other words, in terms of necessity the argument in the shortened form (as in the book) suggests the same as the argument in the original form (as in the article), namely that we do not need Healey's theoretical symmetries to represent empirical symmetries (as perfect or as strong).

That is however if our interests are as just described. But as I hinted in the previous subsection, they need not be such either. For, as I argued already when discussing Healey's weaker DES above, instead of being only interested in the observable invariance within a system we should be interested in the observable differences characteristic of empirical symmetries too. Moreover, as I argued there, we need to require these differences to be represented not less than we need to require the observable invariance to be represented. And that is where a representation of empirical symmetries using theoretical symmetries can become necessary. Indeed, to exclude a representation using a single model in favour of a representation using different models, as in a representation using a theoretical symmetry, we need a motivation for using different models. And the requirement to represent observable differences involved in empirical symmetries is precisely what provides such a motivation. This happens in case we also *require firstly that each qualitatively different model represents at most one observable difference among those characteristic of an empirical symmetry (without presuming which model represents which observable difference, if any) and secondly that the situations differing by such observable differences be represented as observably different in these observable differences.* Then the wider situations involved in an empirical symmetry will necessarily be represented by different models.

How to do next that these models form Healey's theoretical symmetry? For this we may *require that the models that are to represent observable differences characteristic of an empirical symmetry are also to represent an observable invariance characteristic of the same empirical symmetry*. In that case we will get that the different models representing the observable differences of the wider situations will form Healey's theoretical symmetry because of also being able to represent the restricted situation just as explained in the beginning of this subsection. More precisely, if the models represent the restricted situation like in the kind of restricted representations described above, i.e. with each model representing a distinct one of the instantiations of the restricted situation by the wider situations, then we will get *one kind of unrestricted representation of an empirical symmetry by a theoretical symmetry* where each model represents a distinct one of the observable differences of wider situations plus a distinct one of the instantiations of the restricted situation.

To sum up, here is what we saw in this subsection. Firstly the condition to represent the observable invariance of an empirical symmetry by two models ensures that two models form Healey's theoretical symmetry and that this theoretical symmetry is useful for representing the observable invariance of that empirical symmetry. Secondly the requirement to represent the observable differences of an empirical symmetry plus the two extra requirements recently mentioned ensure that different models are necessary for representing the observable differences of that empirical symmetry. And finally, the last requirement above by which the same models should represent both the observable differences and the observable invariance of the same empirical symmetry ensures that those different models useful and necessary for representing the observable differences of the empirical symmetry are those models forming Healey's theoretical symmetry that are useful for representing the observable invariance of the same empirical symmetry. In this way Healey's theoretical symmetry becomes both useful and necessary for representing the empirical symmetry.

Note however that a theoretical symmetry that becomes necessary is more precisely some Healey's theoretical symmetry and not some particular Healey's theoretical symmetry. Thus *the context we are discussing allows any given empirical symmetry to be represented by more than one Healey's theoretical symmetry*. Note moreover firstly that any such Healey's theoretical symmetry is required to be able to represent the observable invariant features and the observable differing features characteristic of empirical symmetries, secondly that as explained above at least for Galileo's ship and Faraday's cage empirical symmetries such characteristic features can be the same for me as the constitutive features of these empirical symmetries, and thirdly that as also explained above theoretical symmetries able to represent constitutive features of empirical symmetries are said to have my observational DES. It thus follows that *Healey's theoretical symmetries of the kind we are discussing will all have the observational DES with respect to the empirical symmetries whose constitutive features they are able to represent*.

But which theoretical symmetries are they? That is, which theoretical symmetries are able to provide an unrestricted representation of both the observable invariant features and the observable differing features characteristic of say Galileo's ship and Faraday's cage empirical symmetries? As we were mostly considering Galileo's ship empirical symmetry in this subsection, let us end it up by clarifying that question for this empirical symmetry before doing so for Faraday's cage empirical symmetry in one of the following subsections.

How can different models represent observable differences of wider situations involved in Galileo's ship empirical symmetry, i.e. the fact that one Galileo's ship stands still and another moves in a certain way with respect to the shore? As it is with respect to some observable physical reference that observable differences characteristic of macroscopic empirical symmetries arise, models may represent these observable differences by representing the relationships of an observable system with an observable reference. And actually the models Healey speaks about in his argument from [p. 709] are able to do so, because he says that they represent situations with respect to reference frames and because a theoretical reference frame he mentions can be used to represent an observable physical reference object. Hence Healey's models are in principle able to represent the observable differing features characteristic of Galileo's ship empirical symmetry. Moreover, Healey supposed the models from his argument to also be able to represent the observable invariant features characteristic of Galileo's ship empirical symmetry. And finally, as said Healey does envisage in the middle of his argument from [p. 709] that two models that form his theoretical symmetry under theoretical boosts are matched with two situations linked by a physical boost and presumably involved in an instance of Galileo's ship empirical symmetry. Therefore, *Healey's theoretical symmetry constituted by such special-relativistic models, which I will call a first theoretical symmetry able to represent Galileo's ship empirical symmetry, would have to get the observational DES with respect to that empirical symmetry*.

On the other hand, as explained above in this subsection, in our context nothing prevents models from Newton's theory to also form Healey's theoretical symmetry because of them being able to represent the restricted situation that both wide situations involved in Galileo's ship empirical symmetry instantiate. In other words, models from Newton's theory can also form Healey's theoretical symmetry able to represent the identical observable features characteristic of Galileo's ship empirical symmetry. Besides, nothing prevents such models from having provisions for representing the relationships of an observable system with an observable reference. Therefore, *Healey's theoretical symmetry constituted by such models from Newton's theory, which I will call a second theoretical symmetry able to represent Galileo's ship empirical symmetry, would likewise have to get the observational DES with respect to the same empirical symmetry*.

But note that I only say “would have to get the observational DES” in either case. Indeed, there is a reason why in Healey's context neither theoretical symmetry actually gets the observational DES. To see this note that if the first theoretical symmetry would have the observational DES in the way described above, it could not become unnecessary for representing

Galileo's ship empirical symmetry and so Healey could not pass from his argument in the shortened form as in his book to the last stage of his argument from his article where this theoretical symmetry whose two models represent two situations gets replaced by a single model that represents two situations. So why did he become able to make such a passage?

In fact this is not only because Healey overlooks the importance of representing such observable differences, but also because *the choice of relationships between a reference and a system induced by Healey's choice of models conceals observable differences characteristic of Galileo's ship empirical symmetry and makes that we do not really have a full-blown Galileo's ship empirical symmetry in Healey's context.*

Here is how this happens. Firstly, note that here the wider situations in the sense above are Healey's situations from [p. 709] combined with observable references. Next, recall that Healey requires at [p. 709] that the special-relativistic models represent his situations they are matched with in the same way as far as their relationships to reference frames are concerned. *Assuming then firstly* that theoretical reference frames stand for observable references and *secondly* that Healey's situations and their relationships with references are such as the models represent them, we get that the relationships between a system and a reference are the same in the two wider situations. However, observable differences in macroscopic empirical symmetries only arise when the same reference entertains different relationships with two systems. Hence the wider situations Healey chooses cannot feature the observable differences characteristic of the empirical symmetry he discusses.

This is not just a mischance, but an instance of a general rule that will be further explained in Part III. This rule however also tells us that a way out. As just hinted, it consists in taking wider situations where the relationships between a system and a reference are different. Given the two assumptions just mentioned, this obtains provided *we require that the special-relativistic models represent the Healey's situations in different ways as far as their relationships to reference frames are concerned*. In that case we get a full-blown Galileo's ship empirical symmetry featuring observable differences that the models in question do represent. As they are also supposed to be able to represent observable invariant features characteristic of the same empirical symmetry, *Healey's theoretical symmetry formed by such special-relativistic models will now finally indeed have the observational DES with respect to that full-blown Galileo's ship empirical symmetry*. But similarly, as explained above, *Healey's theoretical symmetry constituted by models from Newton's theory will now also have the observational DES with respect to the same full-blown Galileo's ship empirical symmetry*.

But that is not the only DES that either theoretical symmetry will have. For one thing, as we know Healey agrees that both special relativity and Newton's mechanics can represent Galileo's ship empirical symmetry as strong, and this is a condition for having his weaker DES in its original version, therefore *both theoretical symmetries will also have Healey's weaker DES in its original version*. For another thing, the observational DES is a strengthening of the reformulated Healey's weaker DES, therefore because of having the observational DES *both theoretical symmetries will have Healey's weaker DES in the reformulated version too*. And finally, Healey says that special relativity can represent Galileo's ship empirical symmetry as perfect, while as I argued when considering unobservable intrinsic properties above, it may be that despite what Healey says this holds for Newton's mechanics as well. In that case *both theoretical symmetries will have Healey's stronger DES in addition*.

Subsection II. 3. 3. 8. Representing Faraday's cage empirical symmetry: Healey's specific claims

The next topic to discuss are representations of Faraday's cage empirical symmetry, and here again before departing from Healey's position we will examine whether it needs to be amended at all.

Healey considers representations of Faraday's cage empirical symmetry in two theories: in classical electrostatics, where the electrostatic potential shifts $\varphi \rightarrow \varphi' = \varphi + k$ are used, and in Maxwell's theory, where either the electromagnetic potential transformations $A_\mu \rightarrow A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$ are used of which the electrostatic potential shifts $\varphi \rightarrow \varphi' = \varphi + k$ are a special case, or else a value of the line integral L is used. *Healey's specific claims about his stronger DES on the basis of Faraday's cage empirical symmetry* are that in the former theory the theoretical transformations in question lead to his stronger DES but not in the same way as theoretical boosts do, while in the latter theory the theoretical transformations in question do not lead to his stronger DES. When discussing unobservable intrinsic properties above I suggested to accept for the sake of our analysis some suppositions and requirements that enable Healey's positive specific claims about his stronger DES by which theoretical boosts and electrostatic potential shifts lead to his stronger DES to hold. The question is now whether the remainder of Healey's specific claims just mentioned holds in that context.

To defend his specific claims Healey pulls out of nothing some *extra requirements* that I have distinguished when summarising his text above. These consist in that besides exhibiting the situations involved in an empirical symmetry as intrinsic duplicates a theoretical symmetry should be such that *first*, any joint model of the two situations exhibits them as linked by the same kind of transformations; *second,* the difference between the two situations is represented by an invariant value of a quantity; and *third*, the relations between the two situations are exhibited as supervening on the intrinsic properties of these situations, i.e. that these relations should be exhibited as either internal or external but not extrinsic. As said, according to Healey all these requirements are satisfied in classical electrostatics: there firstly any joint model of C_0 and C_+ represents these situations as linked by an electrostatic potential shift $\varphi \rightarrow \varphi' = \varphi + k$, secondly all these electrostatic potential shifts associated with $C_0 \oplus C_+$ leave the value of k invariant and thirdly on Healey's opinion they all represent the relation between C_0 and C_+ as external (i.e. supervening on intrinsic properties of relata when the relata are taken together) rather than extrinsic [pp. 711-712]. Meanwhile, in Maxwell's theory firstly there are joint models of C_0 and C_+ that represent these situations as linked not by an electrostatic potential shift $\varphi \rightarrow \varphi' = \varphi + k$ but by something else, for instance by an identity transformation of the electromagnetic potential as in the representation obtained in Healey's Appendix B; secondly the difference between the two situations is represented not always by the invariant k different from 0 but also by something else, for example by 0 as in the representation obtained in Healey's Appendix B; and thirdly, although the line integral representations can be taken to satisfy the previous extra requirement in Maxwell's theory with electromagnetic potentials, on Healey's opinion the relations between C_0 and C_+ are represented by the joint models featuring the line integral not as external but as extrinsic and so “the electromagnetic condition” inside the cage as not affected by charging it [p. 711]. On these grounds Healey takes theoretical symmetries of classical electrostatics and not those of Maxwell's theory to have his stronger DES.

However, Healey says that classical electrostatics does not hold in our world because it does not take into account moving bodies and magnetic phenomena [pp. 711-712]. So its theoretical symmetries receive his stronger DES rather by virtue of possible worlds from which such bodies and phenomena are absent, while theoretical symmetries of special relativity (and of other theories able to represent Galileo's ship empirical symmetry, we may add) receive Healey's stronger DES by virtue of what happens in our world (see [p. 712]).

What we are to think of all that?

To begin with, personally I do not find any reason for taking Healey's stronger DES to be of essentially different nature for classical electrostatics as compared with special relativity. For both theories are valid of our world in the same way, namely neither theory is a theory of everything and so neither theory holds of all that happens in our world. Perhaps then Healey's point was that the latter theory holds in our world at least of everything to which it is supposed to apply, while for the former theory this is not the case. But I do not see that to follow either, because I do not see why classical electrostatics should be supposed to apply to moving bodies and magnetic phenomena. For one thing, it is called electrostatics, so apparently it is supposed to be about electrical phenomena of static bodies. For another thing, it does not seem to make any sense to count a theory as applying to the phenomena of which this theory is false. To be sure, when testing a theory we do presume that it should apply to certain phenomena, and then it may happen indeed that the theory is false of these phenomena. But my idea is that as soon as it does happen the theory should no longer be supposed to apply to the phenomena in question. Thus in particular even if we happened to suppose that classical electrostatics should apply to moving bodies and magnetic phenomena, we need not suppose that any longer. Pushed forward, such an idea implies that what we need to take a theory to apply to are only the phenomena of which it is correct by our standards of correctness. If so, a theory comes out as automatically correct of all phenomena to which it applies. In this case there cannot be any difference between any theories as to the amount of phenomena of which they are correct among those to which they apply. But whether we accept this development of the idea or my previous considerations alone, the upshot is anyway that to the extent that classical electrostatics is valid of our world in the same way in which special relativity is, this validity cannot ground any difference in Healey's stronger DES. In other words, either theoretical symmetries of both theories should get Healey's stronger DES by virtue of possible worlds or they should both get it by virtue of our world.

One could suggest as another difference that classical electrostatics covers some instances of Faraday's cage empirical symmetry while special relativity covers all instances of Galileo's ship empirical symmetry. But firstly, whether special relativity covers all instances of a relevant empirical symmetry is actually dubious if we note that as I explained above unifying Galileo's ship empirical symmetry with Einstein's elevator empirical symmetry can make sense and that special relativity does not account for the latter. And secondly, even if the difference just mentioned can be taken to obtain indeed, I do not see why this would make Healey's stronger DES somehow defective in the case of classical electrostatics with respect to that of special relativity. For it seems that what matters for assigning that DES is only whether a given theoretical symmetry is able to represent some instance of an empirical symmetry as perfect, not how much instances of some empirical symmetry theoretical symmetries of a given kind are able to represent.

Next, if theoretical symmetries constituted by electrostatic potential shifts are able to represent Faraday's cage empirical symmetry as perfect in classical electrostatics, there is no reason why they should cease to be able to do so in Maxwell's theory (for a similar argument see [Greaves and Wallace 2014, p. 61]). So it cannot be the case that they get Healey's strong DES while being in the former theory but do not get it while being in the latter theory. Now what can be objected to this is that if the basic variable of Maxwell's theory is the electromagnetic potential A_μ rather than the electrostatic potential φ, then there are actually no electrostatic potential shifts in Maxwell's theory but only electromagnetic potential transformations some of which *effectively reduce* to the electrostatic potential shifts in the way Healey mentions in his Section 2. But that can be contested in turn. For firstly, it is not less plausible that the basic variable of Maxwell's theory is not A_μ but $F_{\mu\nu}$, in the same way as the basic variable of classical electrostatics may be not φ but **E**. And secondly, actually it does not matter which variable is basic but just whether a given theoretical transformation is able to represent the relationship between the two Faraday's cages in a way required by a given notion of DES. Thus in particular what matters for Healey's stronger DES is just whether a given theoretical transformation is able to represent the two cages as intrinsically untransformed. And we know that for Healey electrostatic potential shifts are able to do so. Hence any theoretical transformation effectively reducing to the latter will effectively be able to do so as well. Arguably, this is enough to assign Healey's stronger DES to a theoretical symmetry constituted by such a theoretical transformation.

What about those electromagnetic potential transformations from Maxwell's theory that do not reduce effectively to electrostatic potential shifts? If all potential transformations in Maxwell's theory are indeed only electromagnetic potential transformations (some of which reduce effectively to electrostatic potential shifts without being electrostatic potential shifts themselves), then whatever transformations among the potential transformations are used to represent the link between the two cages, they will act on the electromagnetic potential anyway, and so they will satisfy the first of Healey's extra requirements by which any joint model of the two cages should exhibit them as linked by the transformation acting on the same variable. Moreover, contrary to what Healey seems to argue, the limiting case where the linking transformation is identity, as in the representation obtained in Healey's Appendix B, does nothing to affect this. On the other hand, Healey is right in that Maxwell's theory anyway does not satisfy the second of his extra requirements by which the difference between the two situations should be represented by an invariant value of a quantity. However, in our context there does not seem to be any reason for demanding that this second extra requirement be satisfied, in the same way as there does not seem to be any reason for demanding that Healey's first extra requirement be satisfied, because there does not seem to be any link between either of these requirements and the basis for assigning Healey's stronger DES which consists in the ability of theoretical symmetries to represent empirical symmetries as perfect. So the satisfaction or non-satisfaction of either of these extra requirements should not affect in any way the assignment of Healey's stronger DES to any theoretical transformations.

As to Healey's third extra requirement by which the relations between the two situations should be exhibited as supervening on the intrinsic properties of these situations, i.e. as either internal or external but not extrinsic, this requirement may seem at least natural within Healey's framework given his accent on what is intrinsic. But firstly, so far Healey was speaking of intrinsic properties alone, so it is unclear why relations now have to be concerned too. To be sure, although properties are usually understood as monadic properties (involving a single thing), there is also a way to understand *properties* as comprising both monadic properties (involving a single thing) and relational properties (involving more than one thing), in which case *intrinsic properties* comprise both intrinsic monadic properties and intrinsic relational properties. But Healey does not say that he adheres to the latter understanding. And even if he did, we would then just have to understand the case where situations are exhibited as intrinsic duplicates as the case where situations are exhibited as sharing all their intrinsic monadic properties and also all their intrinsic relational properties. But as *relations* apparently only stand for relational properties for Healey, his third extra requirement says only that the situations should be exhibited as linked by an intrinsic relational property. So this requirement cannot ensure that the situations be represented as sharing all their intrinsic relational properties anyway. And even if Healey's third extra requirement was about intrinsic relational and monadic properties alike, it could not necessarily ensure something about all intrinsic relational and monadic properties because it only speaks of a single property. What could ensure that the situations share all their intrinsic relational properties is probably the fact that they share all their intrinsic monadic properties. But then there is no need in being concerned with intrinsic relational properties specifically, because it suffices to check whether the situations share all their intrinsic monadic properties to also know whether they share all their intrinsic relational properties (Lewis [1986, p. 62] says that this is true at least of all internal relational properties, i.e. of all intrinsic relational properties that supervene on intrinsic monadic properties of relata taken separately). And so we can safely return to the understanding of *properties* as primarily monadic properties and of the case where situations are exhibited as intrinsic duplicates as primarily concerning the sameness of their intrinsic monadic properties (so I will now continue calling the latter *intrinsic properties* for short, calling intrinsic relational properties *intrinsic relations* for short).

Secondly, Healey's third extra requirement actually cannot ensure that the situations share all their intrinsic properties. And even worse, this requirement can actually make that the situations do not share some of their intrinsic properties. Indeed, one way of representing situations as linked by an external relation, which is an intrinsic relation supervening on the intrinsic properties of relata taken together, is actually by assigning different intrinsic properties to them. To illustrate, persons' heights are arguably their intrinsic properties, so the relations between these heights are external relations, but while for some couples of persons these external relations are identical, namely they are the relations of having the same height, for other couples these external relations are different, namely they are the relations of being shorter than and being taller than. Thus saying that the heights of two persons are linked by one of the latter external relations will imply that they have different intrinsic properties. So more generally a representation can fail to represent situations as intrinsic duplicates because of representing them as linked by an asymmetric external relation.

Now on Healey's view the representations of Faraday's cage empirical symmetry which use electrostatic potential shifts in classical electrostatics allow to exhibit the cages as intrinsic duplicates and so get his stronger DES. But Healey also says that these representations exhibit the cages as linked by an external relation, and moreover, he says that this is because these representations assign different values of the electrostatic potential to the cages [pp. 711-712]. So Healey looks very much like interpreting different values of the electrostatic potential here as the assignments of different intrinsic properties to the cages which make them involved in an asymmetric external relation. But this difference in intrinsic properties should then preclude the representations from classical electrostatics from getting Healey's stronger DES. To avoid this Healey should rather interpret different values of the electrostatic potential as not standing for different intrinsic properties of the cages, similarly to what he does for changes in values of the electrostatic potential in his book [2007, p. 157], where he claims that classical electrostatics represents the charged cage as changed not intrinsically. But the best way to interpret different values of the electrostatic potential as not standing for different intrinsic properties is by interpreting any values of the electrostatic potential as not standing for any intrinsic properties of the cages. But then the cages should be unable to be linked by any intrinsic relation, whether external or internal, by virtue of being represented by some values of the electrostatic potential, because intrinsic relations only supervene on intrinsic properties of relata. So if the fact of representing the cages using different values of the electrostatic potential represents them as entertaining any relation at all, this can only be an extrinsic relation. But in this case Healey has to renounce from his third extra requirement by which the cages should not be represented as linked by an extrinsic relation if he is to keep his claim that electrostatic potential shifts in classical electrostatics lead to his stronger DES.

Besides, renouncing from this requirement is reasonable for Healey's stronger DES more generally, because representing situations as linked by an extrinsic relation apparently cannot affect the representation of any of their intrinsic properties (and of any of the intrinsic relations supervening on these intrinsic properties), and so representing situations as linked in that way at least does not endanger the representation of these situations as intrinsic duplicates contrary to what representing them as linked by an external relation can do. But if one does allow the representation of situations as linked by an extrinsic relation, then the representations featuring the line integral L will also be able to get Healey's stronger DES, because on Healey's view they represent the cages as linked by an extrinsic relation, and so as just explained they are compatible with representing the cages as intrinsic duplicates.

What about whether the electromagnetic potential transformations that do not effectively reduce to electrostatic potential shifts are compatible with representing the intrinsic properties of the cages (and the intrinsic relations supervening on them) as untransformed? If electrostatic potential shifts are able to represent intrinsic properties as untransformed and if electromagnetic potential transformations that effectively reduce to electrostatic potential shifts are able to represent intrinsic properties as untransformed, then arguably those electromagnetic potential transformations that do not effectively reduce to electrostatic potential shifts are able to do so as well. For if the difference in the electrostatic potential φ does not represent an intrinsic difference and if the difference in the component of the electromagnetic potential A_μ that effectively reduces to φ does not represent an intrinsic difference, then it is unclear why any difference in A_μ would represent an intrinsic difference given that it is unclear why the difference in the other component of A_μ effectively reducing to the magnetic vector potential **A** would represent any intrinsic difference either. So there is no reason why the electromagnetic potential transformations not reducing effectively to the electrostatic potential shifts cannot lead to Healey's stronger DES as well.

Now even if differences in φ, **A** and/or A_μ did happen by some reason to represent intrinsic differences, the representation of Faraday's cage empirical symmetry obtained in Healey's Appendix B and valid in Maxwell's theory does not feature them anyway, because it represents the two cages as untransformed in any of φ, **A** and A_μ, as well as actually in any of **E**, **B** and $F_{\mu\nu}$. So if we are uncertain as to whether differences in the values of these variables stand for differences in intrinsic properties, or if it turns out that they do, the representation obtained in Healey's Appendix B is actually the best representation we could think of, because it represents both cages as not having any differences in these properties. Moreover, this representation actually says that both cages are zero in any of φ, **A**, A_μ, **E**, **B** and $F_{\mu\nu}$. So if we are uncertain as to whether non-zero values of these variables stand for intrinsic properties, or if it turns out that they do, the representation obtained in Healey's Appendix B is also the best representation we could think of, because it represents both cages as not having any of these properties at all to begin with. And this shows the extent to which Healey's insistence on exhibiting situations as intrinsic duplicates is absurd, for the best representation for him turns out to be a representation that represents nothing!

Subsection II. 3. 3. 9. Representing Faraday's cage empirical symmetry: alternatives

But does the representation obtained in Healey's Appendix B really represent nothing significant while it has something non-trivial to say about the region in between the cages because of assigning non-zero **A** and **E** on that region? The answer turns out to be yes if we accept the equivalence between some ways of realising empirical symmetries and require these to be represented in the same manner. To see this note firstly that Healey's joint situation from his Appendix B is an instance of the second way of realising empirical symmetries, i.e. of the way where the two physical states constituting an empirical symmetry are instantiated by distinct systems. But, as I explained when analysing Brading and Brown's text above and as will be further explained in Part III, there can be reasons to take this way of realising empirical symmetries to be equivalent to the first way where the two physical states are instantiated by the same system. Moreover, as I will argue in Part III, it is preferable when assigning DES to require that the two ways be represented in the same manner. Therefore, if this is accepted the representation should be insensitive to the differences between the two ways. Hence a fortiori it should be insensitive to the differences within a given way of realising empirical symmetries. And the electromagnetic phenomena in the region in between the cages which non-zero **A** and **E** could represent constitute precisely a difference of the latter kind, because they are only observed when the cages are placed sufficiently close to each other while not all instances of the second way will respect this condition. Thus in my framework just described or in a weaker framework which says that the representation should abstract away at least the differences among the instances of the second way of realising empirical symmetries these phenomena will not have to be represented, and that is why I discard the possibility of representing them when evaluating whether the representation obtained in Healey's Appendix B really represents anything significant.

This suggests of course that one could counter the result of this evaluation by arguing that we should pay attention to the representation of features proper to a given instance of the second way of realising empirical symmetries. But this is neither easy to defend nor able to ensure a wide applicability to Healey's representation obtained in his Appendix B. So to my mind a better objection would be something else. Namely, one could take the joint situation Healey describes in his Appendix B to involve not a charged cage placed close to an uncharged cage but a charged cage placed close to an *uncharged reference*, which can of course also be some uncharged cage, or else the ground, or say an observer (although staying close to a charged cage may be dangerous). The whole joint model that Healey obtains in his Appendix B would consequently become a representation of a charged cage placed close to an uncharged reference. To obtain *a realisation of Faraday's cage empirical symmetry* we would then have to complement this *final physical state* with an *initial physical state* where an uncharged cage is placed close to an uncharged reference. Likewise to obtain *a theoretical symmetry representing that empirical symmetry* we would have to complement Healey's representation of this final physical state, which is just a *final theoretical state*, by an *initial theoretical state* which represents the initial physical state. This initial theoretical state could be for instance a theoretical state where all of φ, **A**, A_μ , **E**, **B** and $F_{\mu\nu}$ are zero everywhere on the reference, on the cage and on the boundary region between them. Meanwhile, the other theoretical state, which is just the representation Healey obtained in his Appendix B, is as we know also zero in all these variables everywhere except in the boundary region where **A** and **E** are non-zero. So *the theoretical symmetry transformation* linking these theoretical states will be a theoretical transformation sending **A** and therefore **E** on the boundary region from zero to the values indicated in Healey's Appendix B, equations (5) and (7) respectively. In this way Healey's representation obtained in his Appendix B becomes part of a representation applicable to any instance of either way of realising empirical symmetries where there is the same uncharged reference in both physical states at all. So why am I not afraid of this objection?

In fact, this is because what we have obtained is perfectly fine within my framework but not within Healey's. Indeed, in the empirical symmetry we have just obtained the electromagnetic phenomena on the boundary between the cage and the reference are just that characteristic observable difference that a final physical state has to exhibit with respect to the initial physical state for these physical states to constitute Faraday's cage empirical symmetry. In other words, these phenomena are just those sparkles on the outer boundary of the cage which make the difference between an observably uncharged cage and an observably charged cage, and our reference is just what allows this difference to be manifested. Consequently, the theoretical transformation sending **A** and therefore **E** on the boundary region from zero to the values indicated in Healey's Appendix B is simply a transformation capable of representing the change in the observable differences characteristic of Faraday's cage empirical symmetry. And hence the whole representation we have obtained is *an unrestricted representation of Faraday's cage empirical symmetry* of the kind from our discussion of amended Healey's key argument above, i.e. a representation where the theoretical states (designated previously as models) represent not only the characteristic identical observable phenomena within the cage, but also the characteristic observable differences on its boundaries. So the transformation of **A** and **E** on the boundary region can yield precisely what Healey does not care to have, although as we saw he should have to. Thus in the representational context just described Healey's representation does represent something important, but not for Healey.

A similar analysis to that just made for **A** and **E** in the boundary region is actually applicable to the line integral L. One could think instead that the line integral is like the transformations linking the interiors of two cages, because it starts in the interior of an uncharged cage and ends up in the interior of a charged cage. If so, in the representation just constructed the analogue of L would be the identity transformation sending the representation of the interior of the cage from the initial physical state of the empirical symmetry just constructed to the representation of the interior of the cage from the final physical state of the same empirical symmetry. However, transformations of the latter kind only send values ascribed to the interior of one cage to values ascribed to the interior of another cage, while the line integral should necessarily take into account the values at the points in between the two cages [p. 710, n. 12]. So the line integral is more like the values of **A** and

E in between the two cages from the representation obtained in Healey's Appendix B. More precisely, as Healey says [p. 710], the line integral is just a line integral of the electric field. So L is just a continuous sum of the values of **E** at all points along a line joining the interior of two cages. But, as we recall, **E** is zero inside both cages at least in both representations Healey discusses in his Appendix B (see his equation (7)). In these conditions L is just the continuous sum of all the values of **E** in between the cages. So L can play exactly the same two roles as those we have envisaged above for the values of **E** in between the cages in the representation obtained in Healey's Appendix B. Namely, either a non-zero value of L from that representation represents phenomena arising in a limited number of realisations of Faraday's cage empirical symmetry, or a transformation from $L = 0$ to that non-zero value of L represents the observable differences characteristic of Faraday's cage empirical symmetry. More precisely, we envisaged these two roles above as played by **A** and **E** together, but this does not matter, because whether the representational capacities of **E** arise by virtue of those of **A** or vice versa or else both **E** and **A** have the same representational capacities independently of each other, **E** will anyway have the representational capacities in question and therefore so will L.

Returning now to the representations of Faraday's cage empirical symmetry in classical electrostatics using electrostatic potential shifts and to them or their effective analogues in Maxwell's theory, in these representations too there are quantities able to play the two roles. Firstly, these are again **E** and L, which are numerically exactly as in the representations with **A** just discussed because as we recall both **E** and L are invariants of all the representations of a given situation (see [p. 710] and Healey's Appendix B [p. 719]). Secondly, this is again a potential, this time φ, or more precisely its values given by $\varphi(x) = \varphi_0 \cdot (x/a)$ from Healey's equation (1) in his Appendix B. To remind, these are the values which grow uniformly along the positive direction of the x axis in between the point $x = 0$, located at the boundary of the uncharged cage or of the reference whose interior is represented by $\varphi(x) = 0$, and the point $x = a$, located at the boundary of the charged cage whose interior is represented by $\varphi(x) = \varphi_0$. For the first role we take just $\varphi(x) = \varphi_0 \cdot (x/a)$ alone while for the second role we take the transformation from $\varphi(x) = 0$ to $\varphi(x) = \varphi_0 \cdot (x/a)$.

In more detail, a theoretical symmetry that we get when we are interested in the second role with respect to the original representation from Healey's Appendix B is constituted by an *initial theoretical state*, which can again be zero in all of φ, **A**, A_μ, **E** (hence also L), **B** and $F_{\mu\nu}$ everywhere on the reference, on the cage and on the boundary region between them; by a *final theoretical state*, which is the original representation from Healey's Appendix B, i.e. a representation where $\varphi(x) = \varphi_0$ on the interior of the cage, $\varphi(x) = \varphi_0 \cdot (x/a)$ as well as $\mathbf{E} = -(\varphi_0/a)$ on the boundary between the cage and the reference and $L \neq 0$ on a line linking the cage and the reference; and a *theoretical symmetry transformation*, which consists of *component transformations* $\varphi(x) = 0 \rightarrow \varphi(x) = \varphi_0$ on the interior of the cage, $\varphi(x) = 0 \rightarrow \varphi(x) = \varphi_0 \cdot (x/a)$ as well as $\mathbf{E} = 0 \rightarrow \mathbf{E} = -(\varphi_0/a)$ on the boundary between the cage and the reference and $L = 0 \rightarrow L \neq 0$ on a line linking the cage and the reference. I will call this theoretical symmetry *a first theoretical symmetry able to represent Faraday's cage empirical symmetry* because it is based on the original representation from Healey's Appendix B.

Meanwhile, another theoretical symmetry that we constructed above in this subsection when we were interested in the second role with respect to the representation obtained in Healey's Appendix B was as we remember constituted by the same *initial theoretical state*; by the *final theoretical state* which is the representation obtained in Healey's Appendix B, i.e. a representation where $\mathbf{A} = t \cdot (\varphi_0/a)$ as well as $\mathbf{E} = -(\varphi_0/a)$ on the boundary between the cage and the reference and $L \neq 0$ on a line linking the cage and the reference; and a *theoretical symmetry transformation* which consists of *component transformations* $\mathbf{A} = 0 \rightarrow \mathbf{A} = t \cdot (\varphi_0/a)$ as well as $\mathbf{E} = 0 \rightarrow \mathbf{E} = -(\varphi_0/a)$ on the boundary between the cage and the reference and $L = 0 \rightarrow L \neq 0$ on a line linking the cage and the reference. I will call this theoretical symmetry *a second theoretical symmetry able to represent Faraday's cage empirical symmetry* because it is based on the representation obtained in Healey's

Appendix B.

Now the two theoretical symmetries we have constructed surely have some DES by virtue of representing Faraday's cage empirical symmetry. But which DES it is?

Well, we could probably devise a lot of kinds of DES that these theoretical symmetries have, but let us check at least whether they have the kinds of DES we have discussed so far.

The first is Healey's weaker DES, and to get it a theoretical symmetry originally had to represent the two situations involved in an empirical symmetry as having the same observable intrinsic properties. Healey says that this is satisfied when the cages are linked by an electrostatic potential shift, which holds for our first theoretical symmetry, so a fortiori this should be satisfied when the cages are linked by an identity transformation, which holds for our second theoretical symmetry. Hence *both theoretical symmetries have Healey's weaker DES in its original version*.

Next, as argued above the original requirement for leading to Healey's weaker DES can be replaced by a reformulated requirement to represent the two situations as having those observable properties within a system whose invariance is characteristic of a given empirical symmetry. The latter requirement is again satisfied by both theoretical symmetries we have obtained, so *they both have Healey's weaker DES in its reformulated version too*.

Afterwards comes my observational DES, and to get it a theoretical symmetry should adequately represent firstly the observable invariant features and secondly the observable differences constituting a given empirical symmetry. As explained above, at least for Galileo's ship and Faraday's cage empirical symmetries the first requirement to adequately represent the observable invariant constitutive features is satisfied by the theoretical symmetries satisfying the reformulated requirement just mentioned. Meanwhile, the second requirement to adequately represent the observable differing constitutive features is satisfied by the two theoretical symmetries with respect to Faraday's cage empirical symmetry as just argued. Hence *they both have the observational DES as well*.

Finally, there also is Healey's stronger DES to consider, and to get it a theoretical symmetry should represent the two situations involved in an empirical symmetry as having the same observable and unobservable intrinsic properties. As argued above, this ability should hold for any representation where the interiors of two cages are represented as linked by an electrostatic potential shift, hence it should holds in particular for our first theoretical symmetry. A fortiori this ability should then also hold for any representation where the interiors of two cages are represented as linked by an identity transformation in electromagnetic variables, hence it should hold in particular for our second theoretical symmetry. In fact even Healey recognises explicitly in his book, saying [2007, p. 156] that the cages in the representation that he will have obtained in Appendix B of his article, where the two cages are also represented as linked by an identity transformation in all electromagnetic variables, are represented as intrinsic duplicates only differing extrinsically. He then goes on saying that this transformation is not an invariant of representations of the cages in Maxwell's theory, but as I argued above this has nothing to do with whether a theoretical symmetry has Healey's stronger DES. Thus *our two theoretical symmetries should also both have Healey's stronger DES*.

Subsection II. 3. 3. 10. Strengthening DES further and the ontology

Thus representations of Galileo's ship empirical symmetry and those of Faraday's cage empirical symmetry are analogous in that in either case there is more than one representation that gets any of the kinds of DES we have been considering. In fact there are plenty of such representations per any of these kinds of DES and per empirical symmetry and such representations can be easily constructed for instance using *my proof* from Part III. All these representations are *theoretical symmetries* at least in Healey's sense and in mine (of (partially) identical theoretical constructions linked by a transformation), and many of them are also local in different senses such as the sense relevant to my proof (where *local* roughly means effectively non-uniform). This applies for instance to the two theoretical symmetries just constructed that are able to represent Faraday's cage empirical symmetry, because the theoretical transformations constituting them are local in that sense on the boundary region. Thus although as explained towards the beginning of my analysis Healey happens to privilege theoretical symmetries usually classified as global, we see that there is actually no reason to do so from the point of view of any of the kinds of DES we have been considering.

Now, as also explained above, when a proliferation of matchings occurs and this is found unsatisfactory, we may try to strengthen a DES. However, we now see that there is still a considerable proliferation of matchings for both my observational DES, despite its being a strengthening of Healey's weaker DES in its reformulated version, and for Healey's stronger DES, despite its being a strengthening of his weaker DES in its original version. On the other hand, there remains a strengthening option we have not yet tried, namely to combine a strengthening concerning observable features like in my observational DES with a strengthening concerning unobservable features like in Healey's stronger DES. As said, this can be made for instance by introducing my stronger DES which I call the ontological DES.

The ontological DES firstly requires as the observational DES that a theoretical symmetry be able to adequately represent observably different features and observably identical features constitutive of an empirical symmetry, but secondly it requires in addition that this theoretical symmetry also be able to adequately represent unobservable physical underpinnings of these constitutive features. Both these requirements are apparently far from Healey's weaker DES and his stronger DES, so why am I not keeping Healey's kinds of DES? So far I have only explained why we should pass from Healey's weaker DES in its original version to Healey's weaker DES in its reformulated version and from the latter to its strengthening which is my observational DES. So what remains to explain is why I abandon the difference between Healey's weaker DES and his stronger DES, namely the requirement to represent situations involved in an empirical symmetry as having the same unobservable intrinsic properties, and introduce instead of it the requirement to adequately represent unobservable physical underpinnings of the observable constitutive features.

One difference between the last two requirements is that mine is not formulated in terms of intrinsic properties but makes appeal to observable features instead. This is because, as I explained in the subsection on unobservable intrinsic properties above, it seems much easier to capture unobservable properties not via the notion of intrinsic, for which Healey does not provide any satisfactory explicit definition, but by indexical means. In my requirement I am doing precisely this, i.e. I am capturing the unobservable features of interest by saying that they are those unobservable features that provide physical underpinnings to observable constitutive features.

Other differences concern the scope of unobservable features I am interested in. For one thing, these include as said only those unobservable features that provide physical underpinnings to observable constitutive features. My justification for this is that we should be interested primarily in features not contingent to an empirical symmetry but constitutive to it, hence among unobservable features too we should be interested in those that are relevant to the constitutive features, and as the latter are observable features, the unobservable features relevant to them will arguably be those that provide them with unobservable underpinnings. For another thing, I include in the relevant unobservable features not only those that underlie observable invariant constitutive features but also those that underlie observable differing constitutive features, while what Healey does for his stronger DES amounts extensionally to only including unobservable features that underlie observable invariant constitutive features. This is justified by the fact that I adopt the extension of Healey's weaker DES in its reformulated version to my observational DES, because this amounts to admitting observable differing features to be constitutive in addition to invariant features, and so the unobservable underpinnings of both the former and the latter features become relevant.

One more difference is that I am using the vocabulary of features, and these are actually physical features where *physical* as before means for me obtaining in the actual world, while Healey speaks of properties of situations that can be actual or possible. The reason I make the restriction to physical features is that I am interested in the ontology of the actual world, but this can also be easily justified from the perspective of dealing with the proliferation of matchings. For this restriction should amount to drastically restricting the number of combinations of features that can lead to a DES one is defining, because arguably any given world allows much less features to be combined so as to become constitutive of empirical symmetries or provide physical underpinnings for them than all the worlds do when taken together.

Although Healey mostly justifies the passage from his weaker DES to his stronger DES by saying that the latter is more explanatory satisfactory, in fact he also comes to seeking an ontological justification for the latter. This is seen at the end of his Section 1, where he says that the most satisfying explanations of the kind he is interested in may support our belief in a theory, as well as at the end of his Section 5, where he says that what they may support more precisely is our belief in the existence in the world where an empirical symmetry obtains of the counterparts of some constituents of a theoretical symmetry that has his stronger DES with respect to that empirical symmetry. The constituents in question are in my terms the differing theoretical underpinnings of the theoretical states constituting his theoretical symmetries and the theoretical symmetry transformations inducing these differences. However, as we saw above there can be many theoretical symmetries that can have Healey's stronger DES with respect to the same empirical symmetry and that differ in the constituents just mentioned. So if Healey's stronger DES unconditionally warranted the belief in question, that would result in our ontology being very much conflated, because we would have to believe in the existence of counterparts of all these differing constituents at once. But that a single world where some empirical symmetry holds has counterparts to all the theoretical elements in question is highly implausible, so the beliefs in question cannot be admitted. Therefore, if one is to seek an ontological justification for a strong DES, it has to be for a DES which does not work in the way Healey wishes his stronger DES to work.

The ontological DES, in particular, allows to avoid the problem just mentioned not only because it restricts the consideration to a single world, but also because it is not based on whether physical states are represented as having some unobservable features as in Healey's case but on whether the unobservable features physical states have are adequately represented. Thus the ontological DES takes advantage not only of the idea that physical states in a given world generally have less unobservable features than all the features they may be represented as having, but also of the demand to discard all representations except those that assign to a physical state all and only unobservable properties that they indeed have in that world. Under these conditions it may be possible to reduce the number of matchings to just one per instance of empirical symmetry, which would be a paramount achievement for someone interested in strengthening a DES and besides would make out of DES a useful expression of the ontology of physical states of our world.

There are however some reasons why the ontological DES is hard to be established. For one thing, it is uncertain whether a physical state has any definite unobservable physical features at all. For another, it is doubtful that there is a unique right way of adequately representing them. Besides, even if there was such a right way, it is unclear how to determine which representation is the one that employs it. By these reasons the ontological DES is more like a Holy Grail rather than a goal that can really be attained.

What is sure, however, is that the ontological DES is a strengthening of the observational DES and hence *only those theoretical symmetries that have the observational DES can have the ontological DES*. Therefore, a more modest but at least reasonable and useful goal that one may have when interested in the ontological DES is *determining which theoretical symmetries have the observational DES and which do not*, for this *amounts to determining which theoretical symmetries can and which cannot have the ontological DES*. This joins of course to the independent reason for studying the observational DES which consists in that it is arguably useful to know which theoretical symmetries are able to adequately represent the observable constitutive features of empirical symmetries. Given these reasons, I will continue analysing the observational DES in Part III.

Section II. 3. 4. Summary of the analysis

Healey's article is well written and consists of an extended version of his analysis of DES from his book [2007]. Although not mentioning DES explicitly, Healey can be said to introduce a distinction between two kinds of DES which I name his weaker DES and his stronger DES. Either DES demands reformulation because of being defined in terms of intrinsic properties that Healey does not define.

Healey's assignments of DES are at places too narrow (e.g. despite Healey's concentrating on global symmetries alone local symmetries have DES too) and at others too broad (e.g. all transformations of the same kind get DES and all transformations of other kinds forming a symmetry group with a kind of transformations getting DES also get DES despite DES being only ensured by some transformations of the original kind).

Healey's weaker DES based on representing situations as sharing all their observable intrinsic properties has a drawback of allowing all gauge symmetries in the sense of observationally complete theoretical symmetries to have DES. Besides, it overlooks the crucial importance of observational differences to the two empirical symmetries he discusses. This motivates the introduction of my observational DES which requires adequately representing both observable invariant features and observable differing features constitutive of empirical symmetries.

Healey's key argument actually establishes that his theoretical symmetries are useless for representing empirical symmetries, which is the opposite of what Healey wanted to show. Amending this requires shortening the argument as in his book, imposing various conditions and requirements including that to represent the observable differences, and choosing a different realisation of Galileo's ship empirical symmetry. One consequence is that many theoretical symmetries get various kinds of DES with respect to the latter empirical symmetry.

Healey's stronger DES based on representing situations as sharing all their observable and unobservable intrinsic properties is denied to theoretical symmetries of Maxwell's theory on the basis of extra requirements that are either irrelevant to this DES or incoherent with it. These requirements put apart, the assignments of this DES that result are absurd because of being best satisfied by a representation that represents nothing in Healey's framework.

When the amended framework is brought in, many theoretical symmetries capable of representing Faraday's cage empirical symmetry arise, including local ones, that have either of Healey's kinds of DES in agreement with the analogous results for Galileo's ship empirical symmetry. This proliferation of matchings can be reduced by strengthening of my observational DES, which is itself a weak strengthening of a reformulated Healey's weaker DES, into my ontological DES.

Chapter II. 4. Greaves and Wallace [2014]

Section II. 4. 1. Brief summary of the text

In Section 1 [pp. 60-63] Greaves and Wallace associate DES with subsystem symmetries which are not universe symmetries and raise three issues for the traditional view that global but not local theoretical symmetries have DES. In Section 2 [pp. 63-66] they firstly define theoretical and empirical symmetries in a way close to Healey's and then describe the relationship between theoretical symmetries constituted by theoretical boosts and Galileo's ship empirical symmetry which they take to define DES. They next present in Subsections 2.1 and 2.2 what they call

Healey's and Brading and Brown's puzzles, i.e. the matchings of Faraday's cage and 't Hooft's beam-splitter empirical symmetries with local theoretical symmetries which seem to bestow DES on the latter. In Section 3 [pp. 67-68] Greaves and Wallace define subsystem, environment and total system theoretical states, transformations and symmetries. Their main accent is on theoretical states and total systems. In Section 4 [pp. 69-70] they illustrate the application of the notions from their previous section to classical electrostatics. In Section 5 [pp. 70-75] they firstly introduce interior/non-interior and boundary-preserving/non-boundary-preserving distinctions for subsystem theoretical symmetries and say being interested in (non-)boundary-preservingness relative to theoretical states of isolated subsystems. They next claim that interior symmetries do not have DES, non-interior boundary-preserving symmetries have DES by virtue of purely relational empirical symmetries and non-interior non-boundary-preserving symmetries have DES by virtue of what I call intrinsic empirical symmetries. They also illustrate this for classical electrostatics and tell how interior symmetries are related to non-interior ones and to boundary-preserving ones. Section 6 [pp. 75-78], Section 7 [pp. 78-80] and Section 8 [pp. 80-83] illustrate the application of Greaves and Wallace's framework from their Sections 3 and 5 to Newtonian gravity, classical electromagnetism and Klein-Gordon-Maxwell theory respectively. This results in bestowing DES on global and local boundary-preserving and non-boundary-preserving theoretical symmetries. Section 7 also contains a proposal for an alternative definition of the global/local distinction [p. 80, n. 23]. Section 9 [pp. 84-86] summarises with some variations the content of Sections 3 and 5. Section 10 [pp. 86-88] explains why the traditional position by which only global theoretical symmetries have DES is refuted in Greaves and Wallace's framework, tells how the three issues for that position from their Section 1 get dissolved in that framework and summarises Greaves and Wallace's attributions of DES in a table [p. 87]. Acknowledgements [p. 88] contain a reference to fruitful discussions with Brading and Healey.

Section II. 4. 2. Extended summary of the text

Greaves and Wallace's article comprises ten sections. Section 1 is an introduction and contains three issues to be dissolved. Section 2 presents some empirical symmetries. Sections 3 and 5 describe Greaves and Wallace's framework, while Section 9 summarises it. Sections 4, 6, 7, 8 apply this framework to particular cases. Section 10 summarises the applications and explains how to dissolve the three issues. The article ends up with Acknowledgements.

Section 1 [pp. 60-63] begins by opposing two interpretations of symmetries in physics: on the one hand, theoretical states linked by a symmetry transformation are often interpreted as corresponding to the same physical state or at least to observationally indistinguishable physical states, while on the other hand "observable consequences" of symmetries have guided theory construction since Galileo's ship empirical symmetry [p. 60]. Greaves and Wallace then claim that the previous literature on DES escaped this tension by realising that "a transformation can be a symmetry of a subsystem of the world without being a symmetry of the whole world" [ibid.]. They illustrate this with Galileo's ship empirical symmetry, saying that physical boosts on the ship are symmetries on the ship's interior because of the observable invariance of what happens within it but not symmetries of the world because of the observable differences in the relationships of the ship with its exterior [pp. 60-61].

Then Greaves and Wallace say that for them *a theoretical symmetry has DES* when an empirical symmetry exists which is linked to this theoretical symmetry in the same way as Galileo's ship empirical symmetry is linked to theoretical symmetries constituted by theoretical boosts [p. 61, n. 3]. They also oppose DES to IES and say that the latter can be bestowed on theoretical symmetries for instance by Noether's theorems [ibid.]. For them *DES* stands for "direct empirical significance" throughout, but I will keep understanding DES as standing for "direct empirical status" instead in particular for uniformity and to avoid confusion with my notion of ontological significance. However, this of course is not going to change much in my text, as the contractions of the two expressions result in the same letters.

Next Greaves and Wallace introduce a *global/local distinction* described as follows: global symmetry transformations are transformations uniform across space and time like translations and boosts in special relativity and global phase shifts from Klein-Gordon theory (which is a relativistic quantum matter theory), while local symmetry transformations are transformations varying across space and time like in the electromagnetic potential symmetry of classical electromagnetism (which they call "gauge symmetry" in their text) and in the diffeomorphism symmetry of general relativity [p. 61]. Greaves and Wallace then say [ibid.] that one could infer from the above that only global but not local theoretical symmetries have DES, because it seems that a local transformation that is (sic!) a symmetry on each subsystem of a system is also a symmetry of the whole system, while for global transformations this does not seem to be the case. They attribute [ibid.] the view that local symmetries cannot have DES to Kosso [2000], Brading and Brown [2004] and Healey [2007] (for as we recall it is in this book that Healey first presented some of his views later developed in his [2009] article). Greaves and Wallace also note that Brading and Brown go further and deny DES to internal global symmetries as well [p. 61].

Afterwards Greaves and Wallace raise *three issues for the common view* just mentioned [pp. 61-62]. *Firstly*, on their opinion all theories with local symmetry groups retain global symmetry groups as global subgroups, so it is logically impossible to assign DES to global symmetry groups but not to local symmetry groups. Besides, they find mysterious to suppose that a global symmetry group ceases to have DES once it becomes a subgroup of a local symmetry group. (As said, above I used this kind of argument in my analysis of Healey's [2009] article to claim that electrostatic potential shifts should not cease to be able to represent empirical symmetries as perfect once considered as making part of electromagnetic potential transformations.) *Secondly*, Greaves and Wallace say that in the history of physics less accurate theories with global symmetry groups (such as special relativity and (classical) electrostatics) have often been replaced by more accurate theories with local symmetry groups (such as general relativity and (classical) electrodynamics), so it would be odd if the former but not the latter could explain empirical symmetries. Besides, if it was so, then on their opinion the justification for scientific realism would be undermined (perhaps because they take scientific realism to be justified by an increase in explanatory force across the theory change, we may add). *Thirdly*, as they are going to show, for local theoretical symmetries there do exist matchings similar to those between theoretical symmetries constituted by theoretical boosts and Galileo's ship empirical symmetry.

Greaves and Wallace also promise to show that a distinction that determines whether theoretical symmetries have DES is a distinction related to the global/local distinction; that the global/local distinction itself does not determine whether theoretical symmetries have DES; and that local theoretical symmetries have DES [p. 62]. They note in addition that they will present the notions of interior symmetries and boundary-preserving symmetries that as they believe "are crucial to an understanding of the phenomenon of Galileo-ship-type scenarios" [pp. 62-63]. Besides, they report that they wanted to claim that local symmetries of general relativity stand to theoretical symmetries constituted by Poincaré transformations like local electromagnetic symmetries stand to symmetries of classical electrostatics, but renounced to do so because of the interpretation of general relativity being unclear [p. 63, n. 4].

Section 2 [pp. 63-66] starts by descriptions of generic theoretical symmetries and generic empirical symmetries that Greaves and Wallace say to be inspired by Healey's article [2009]. Generic *theoretical symmetries* are defined as automorphisms of the class of models of a theory such that the models linked by a theoretical symmetry transformation represent the same physical state or possible world or observationally indistinguishable physical states or possible worlds [p. 63]. Greaves and Wallace's example of such theoretical symmetries are theoretical symmetries constituted by a universal boost on the whole universe of an inertial frame in Newtonian mechanics [pp. 63-64]. Generic *empirical symmetries* for them are constituted by two physically and empirically (which apparently means observationally, we may add) physical states of the world which cannot be discriminated by means of measurements (thus observations, we may add) confined to some subsystem [p. 64]. Greaves and Wallace say that they do not wish to define the latter notion in terms of Healey's situations [ibid., n. 6]. Their example of an empirical symmetry is Galileo's ship empirical symmetry [p. 64].

Next Greaves and Wallace give the *matching conditions* for their example of an empirical symmetry and their example of a theoretical symmetry: these examples are matched because (i) Newtonian mechanics adequately describes Galileo's ship empirical symmetry, (ii) the theoretical boost symmetry they mentioned above obtains in Newtonian mechanics; (iii) Galileo's ship empirical symmetry obtains (in the world, we may add) in that one cannot distinguish by experiments within the ship whether it is physically boosted [ibid.]. To remind, this specific matching defines for Greaves and Wallace what it is to have DES, because for them a theoretical symmetry has DES if its matching to an empirical symmetry is of the same kind as the matching just described [p. 61, n. 3].

In Subsection 2.1 [pp. 64-65] Greaves and Wallace present Healey's puzzle, which for them consists in that Faraday's cage empirical symmetry seems to be a consequence of local electromagnetic potential symmetries by virtue of them containing electrostatic potential shifts in the same way as Galileo's ship empirical symmetry is a consequence of the theoretical boost symmetry in Newtonian mechanics. They also mention Healey's denial of this analogy and promise to show against him that although there is indeed a dysanalogy between these cases, it is completely unrelated to the global/local distinction and does not make the former theoretical symmetries to have less or less direct DES as compared to the latter theoretical symmetries.

In Subsection 2.2 [pp. 65-66] Greaves and Wallace say that Brading and Brown [2004] consider a similar puzzle of whether 't Hooft's beam-splitter empirical symmetry plays the role of Galileo's ship empirical symmetry for joint local phase and electromagnetic potential symmetries by virtue of them containing phase shifts. They also mention Brading and Brown's denial of this idea and promise to show against them that the idea in question is actually right. Besides Greaves and Wallace make some remarks on their treatment of Faraday's cage empirical symmetry. Firstly, they are going to concentrate on the setup where a single half-wave plate is added into the upper slit in the final physical state [p. 65]. Secondly, they will take the upper half of the beam to be a subsystem analogous to Galileo's ship and the lower half to be the environment analogous to Galileo's shore [p. 66]. Thirdly, they will take the invariance under inserting the half-wave plate to be manifested by experiments involving the upper half of the beam alone and the change to be manifested by experiments involving both halves of the beam [ibid.]. Fourthly, they are claiming that 't Hooft's beam-splitter empirical symmetry and the Aharonov-Bohm effect could also arise for a classical charged field [ibid., n. 10]. Fifthly, they promise to show that 't Hooft's beam-splitter empirical symmetry is more analogous to Galileo's ship empirical symmetry than Faraday's cage empirical symmetry [p. 66].

Section 3 [pp. 67-68] lays out the first part of Greaves and Wallace's framework. Firstly they renounce to define what counts as a subsystem and an environment in general and say that they will only say this for particular cases later on. Next they characterise *theoretical states* as what represents physical states, and then distinguish between theoretical states of a subsystem (that I will call *subsystem theoretical states* for short) representing physical states of that subsystem and theoretical states of an environment (that I will call *environment theoretical states* for short) representing physical states of that environment. They also say that *in the context of field theories theoretical states* can be values of field variables on a subset of manifold points. Besides, for them if a theoretical state of a subsystem concerns given manifold points, then a theoretical state of an environment may concern all the remaining points of the same manifold. Consequently, they define a theoretical state of the total system (where the total system comprises the subsystem and the environment, we may add, and which I will call *a total system theoretical state*) as a pair consisting of a subsystem theoretical state and of an environment theoretical state. They denote a subsystem theoretical state by s, a set of such states by S, an environment theoretical state by e, a set of such states by E, a total system theoretical state by $<s, e>$ and a set of such states by U [p. 67].

Next they list three assumptions [pp. 67-68]. Firstly, they assume that knowing s and e suffices to specify $<s, e>$. Secondly, they assume that a subsystem and an environment can both be treated as distinct and isolated so that $<s, e>$ and $<s', e>$ can both be treated as possible. Thirdly, they assume that some combinations of s and e cannot give rise to a total system theoretical state, e.g. because this is prohibited by equations. So they only denote by $<s, e>$ a combination of s and e that gives rise to a total system theoretical state, while they denote by $s*e$ any combination of s and e whether giving rise to $<s, e>$ or not. They also denote by B_s environment theoretical states that give rise to $<s, e>$ for a given s and by C_e subsystem theoretical states that give rise to $<s, e>$ for a given e. They call B_s and C_e *boundary conditions*.

After this rather long discussion of theoretical states Greaves and Wallace briefly consider theoretical transformations and theoretical symmetries [p. 68]. Either of these can again pertain to subsystems, environments and total systems (although Greaves and Wallace are not much concerned with theoretical symmetries of an environment), so I will name the relevant theoretical transformations and symmetries *subsystem theoretical transformations and symmetries, environment theoretical transformations and symmetries* and *total system theoretical transformations and symmetries* respectively. In fact Greaves and Wallace regularly mix transformations and symmetries together, because they frequently speak of symmetries being transformations (e.g. at [p. 61] as mentioned above and at [p. 68]), but I will continue distinguishing the two and so will take symmetries to be constituted by transformations instead. Also, Greaves and Wallace treat what happens with subsystems and environments as derivative with respect to what happens with total systems. So they firstly consider total system theoretical symmetries constituted by total system theoretical transformations which act on a set of total system theoretical states. These transformations are assumed to be bijective and closed under inverses and composition. A set of such transformations is denoted by Σ and its representative by σ. Greaves and Wallace next assume that these transformations restrict uniquely to subsystem and environment theoretical transformations. Either of the latter theoretical transformations are assumed to be bijective and well-defined. Their sets are denoted by Σ_S and Σ_E respectively and the representatives of these sets are denoted by $\sigma|_S$ (later σ_S) and $\sigma|_E$ respectively. Subsystem and environment theoretical symmetries are constituted by such $\sigma|_S$ and $\sigma|_E$ respectively.

Section 4 [pp. 69-70] illustrates this first part of Greaves and Wallace's framework by applying it to classical electrostatics. Greaves and Wallace are interested more precisely in considering a set of charged particles moving in a static potential field together with some optional background conductors. A subsystem for them is here either any collection of charged particles located far from the other charged particles and the background conductors, or any collection of charged particles located entirely within a background conductor, while an environment is the remainder of the whole universe (and hence the total system is the whole universe). A subsystem or environment theoretical state is obtained by specifying the electrostatic potential and particle trajectories in agreement with the electrostatic equations that Greaves and Wallace list. According to them, if we are interested in theoretical symmetries constituted by electrostatic potential transformations, a subsystem theoretical state can be specified on a restricted spacetime region W presumably occupied by a subsystem and an environment theoretical state can be specified on the remainder of a spacetime manifold, while if we are interested in external theoretical symmetries, the two theoretical states have to be specified on the whole spacetime but only their restrictions to W and to its complement respectively have to be taken into account when defining a total system theoretical state. Boundary conditions consist anyway in that the electrostatic potentials of a subsystem and of an environment should match on the boundary of the subsystem. According to

Greaves and Wallace, theoretical transformations that can yield theoretical symmetries in this context are theoretical spatial and temporal translations, boosts and rotations usually when background conductors are absent, as well as the electrostatic potential transformations $\varphi(\mathbf{x},t) \rightarrow \varphi(\mathbf{x},t) + f(t)$ in any case.

Section 5 [pp. 70-75] contains the description of the other part of Greaves and Wallace's framework. This other part essentially consists of two distinctions for subsystem theoretical symmetries of which as said Greaves and Wallace claimed in their Section 1 that these "are crucial to an understanding of the phenomenon of Galileo-ship-type scenarios" [pp. 62-63]. In either case Greaves and Wallace only define one of the terms of a distinction, treating the other term as designating the negation of what is designated by the first term in relation to the same kind of symmetries. After having given the two definitions, they indicate how their distinctions are linked to DES and use again classical electrostatics to illustrate their discussion (this time within the same section).

The first of Greaves and Wallace's distinctions is between *interior and non-interior subsystem theoretical symmetries*. An interior subsystem theoretical symmetry for them is a subsystem theoretical symmetry such that a subsystem theoretical transformation constituting it, when combined with an environment theoretical transformation which is an identity, constitutes *a total system theoretical symmetry* in the sense that the total system theoretical states linked by that transformation represent the same possible world [p. 71]. Accordingly, we may add that a subsystem theoretical symmetry is non-interior when the transformation constituting it does not satisfy this condition.

Greaves and Wallace say that examples of spatiotemporal (i.e. external) interior subsystem theoretical symmetries are those constituted by diffeomorphisms in the hole argument, because a "hole", i.e. the region on which diffeomorphisms are non-trivial, can be considered as a subsystem [ibid.]. They also say that there are examples of internal (i.e. non-spatiotemporal) interior subsystem theoretical symmetries [ibid.]. They next note a theoretical symmetry is interior when its subsystem transformation coincides with identity on the boundary of a subsystem, and say that whether a theoretical symmetry is interior holds relative to a subsystem [ibid.]. They add that "some universe symmetries" cannot ever be interior, their example being global symmetries [ibid.].

Greaves and Wallace's other distinction is between *boundary-preserving and non-boundary preserving subsystem theoretical symmetries*. A boundary-preserving subsystem theoretical symmetry is for them a subsystem theoretical symmetry such that a subsystem theoretical transformation constituting it preserves the boundary conditions of the initial subsystem theoretical state, i.e. if the same environment theoretical states that give rise to total system theoretical states when matched with the initial subsystem theoretical state also give rise to total system theoretical states when matched with the final subsystem theoretical state [ibid.]. Accordingly, we may add that a subsystem theoretical symmetry is non-boundary-preserving when the transformation constituting it or the relevant theoretical states do not satisfy the conditions just mentioned.

Next Greaves and Wallace introduce a relativisation of their second distinction to sets of theoretical states. They do not spell this out as a nuanced second distinction, but I will do so for convenience. Thus I will distinguish between *subsystem theoretical symmetries boundary-preserving on all theoretical states, those boundary-preserving on some theoretical states, those non-boundary-preserving on some theoretical states and those non-boundary-preserving on any theoretical states.*

Besides, Greaves and Wallace indicate how their two distinctions may be related. Namely, firstly they say that interior transformations make part of boundary-preserving transformations but that the former do not exhaust the latter [ibid.]. Afterwards they make *a cautious suggestion* by which a subsystem symmetry is probably boundary-preserving on all theoretical states just in case it is interior [p. 72]. They motivate it by saying that this suggestion should hold at least for local internal symmetries in field theories, because there subsystem theoretical symmetries are only boundary-preserving on all theoretical states if their transformations coincide with identity on the boundaries of a subsystem, so such subsystem theoretical symmetries can be thought of as restrictions of total system theoretical symmetries when combined with an environment theoretical transformation which is an identity, as is required for such subsystem theoretical symmetries to be interior [ibid.]. Greaves and Wallace do not explain however how the theoretical states of such subsystem theoretical symmetries manage to be matched with the same possible world, as is also needed for these theoretical symmetries to be interior.

Greaves and Wallace's main claim in their article in then that interior subsystem theoretical symmetries (and so subsystem theoretical symmetries boundary-preserving on all theoretical states if their cautious suggestion is right) do not have DES while subsystem theoretical symmetries boundary-preserving on some theoretical states and those non-boundary-preserving on some theoretical states can have DES.

Indeed, first of all Greaves and Wallace claim that interior symmetries do not have DES [pp. 71, 73]. They motivate this firstly by saying that such symmetries cannot be matched with an empirical symmetry because they redescribe the same possible world and hence do not lead to a distinct situation [p. 71]. Later on they say that a subsystem transformation of an interior symmetry cannot lead to DES because its combination with the identity on the environment gives rise to a universe symmetry while to lead to DES a subsystem theoretical transformation should not give rise to a universe symmetry but only to a subsystem symmetry [p. 73]. They also note that interior symmetries make that Kosso's observable difference condition is not satisfied [ibid., n. 16].

Secondly Greaves and Wallace consider theoretical symmetries boundary-preserving on some theoretical states. *They take the relevant theoretical states to be* those subsystem theoretical states that pertain to a subsystem which is isolated from its environment. They say that *the isolation from the rest* functions here as a reason to identify something as a subsystem and also ensures that a symmetry is boundary-preserving on some states. They illustrate this by saying that boosting an isolated collection of particles will often although not always leave its environment unchanged [p. 72]. They next say that in practice we usually deal with *approximate isolation*. On their view this destroys the exact boundary-preservingness but not the *approximate boundary-preservingness* which consists in that while the final subsystem theoretical state is strictly speaking incompatible with a given initial environment theoretical state, a subsystem theoretical state close to this final subsystem theoretical state which is compatible with the same environment theoretical state can be chosen in some way whose details are unspecified but presumed to be irrelevant. They admit such approximate boundary-preservingness as well and say that when it obtains they will denote by $\sigma_S(s)$ in a final total system theoretical state not the final subsystem theoretical state obtained by applying σ_S to s but such a close subsystem theoretical state instead [pp. 72-73].

Of theoretical symmetries boundary-preserving on a subset of subsystem theoretical states Greaves and Wallace claim that these can have DES when their transformations act on subsystem theoretical states from that subset [p. 73]. This for them holds because combining such transformations with identity on an environment preserves the satisfaction of dynamical equations by boundary-preservingness while not constituting a universe symmetry by non-interiority [ibid.]. They claim that such subsystem theoretical symmetries are to be matched with *empirical symmetries that are purely relational* in the sense that the intrinsic properties of a subsystem and of an environment taken separately are entirely unaffected there and only the relations between the two change [ibid.]. They also claim that such empirical symmetries satisfy both Kosso's observable difference condition and his observable invariance condition [ibid., n 16].

Thirdly Greaves and Wallace claim that theoretical symmetries non-boundary-preserving on a subset of subsystem theoretical states can also have DES provided the environment theoretical state is also transformed so that to become compatible with the subsystem final theoretical state and so that the final total system theoretical state is matched with another possible world [p. 73]. They say that the last condition cannot be attained if one uses an environment theoretical transformation which is a restriction of the same total system theoretical transformation of which the subsystem theoretical transformation is a restriction, because for them in that case the initial and the final total system theoretical states are to be matched with the same possible world [ibid.]. However, as in their discussion of local internal symmetries and of interior symmetries in the same section they again say nothing about why the latter matching would hold. Instead they propose to satisfy the last condition by finding a final environment physical state which is observationally distinguishable on the environment from the initial environment physical state [p. 74]. At the same time they maintain that the initial and final subsystem physical states should be observationally indistinguishable on the subsystem [ibid.]. The resulting phenomenon is for them a non-trivial empirical symmetry [ibid.], and we may add that apparently it also satisfies both Kosso's observable difference condition and his observable invariance condition.

Staying within the same section, Greaves and Wallace next illustrate the application of their main claim to classical electrostatics [p. 74]. They say that it has no interior subsystem theoretical symmetries besides identity; that its subsystem theoretical symmetries that are non-interior and boundary-preserving on relevant subsystem theoretical states (namely those where the subsystem is isolated) are subsystem theoretical symmetries constituted by spacetime translations, rotations and boosts and that these have DES by virtue of Galileo-ship-type empirical symmetries; and that subsystem theoretical symmetries of classical electrostatics that are non-interior and non-boundary-preserving on any theoretical states are those constituted by electrostatic potential shifts and that, in agreement with Healey's analysis of the latter symmetries in classical electrostatics, these have DES by virtue of Faraday's cage empirical symmetry.

As Greaves and Wallace match boundary-preserving theoretical symmetries with purely relational empirical symmetries, we see from their analysis of classical electrostatics just mentioned that for them Galileo-ship-type empirical symmetries and so Galileo's ship empirical symmetry in particular are of that kind, i.e. involve no changes in the intrinsic properties of either of a subsystem and an environment but only the change in the relations between them. Meanwhile, of Faraday's cage empirical symmetry which as just said they match with a non-boundary-preserving theoretical symmetry Greaves and Wallace say that they take "the placement of the electric charge on the exterior of the cage" to be an intrinsic change on the environment [ibid.]. From this we can infer that for them empirical symmetries to be matched with non-boundary-preserving theoretical symmetries should on the contrary involve changes in intrinsic properties of an environment, so I will call them not purely relational but *intrinsic empirical symmetries*. Greaves and Wallace say however that although the intrinsic change just mentioned is relevant to their discussion, there are also other intrinsic changes of an environment that are not relevant to it, but that telling which changes of an environment are relevant and which are not lies beyond the scope of their study [ibid., n. 17]. So although they propose that only relevant changes of an environment are to be admitted [ibid.], we may note that their proposal does not have any practical value. Similarly, Greaves and Wallace end up their section by saying that actually many non-interior theoretical symmetries can be matched with the same empirical symmetry and so have DES with respect to it and that these non-interior theoretical symmetries differ from each other by the application of interior theoretical symmetries [pp. 74-75]. However, by contrast to my own account they neither analyse nor use this is any way except when redefining the global/local distinction in their Section 7.

In the next three sections Greaves and Wallace illustrate the application of both parts of their framework to theories other than classical electrostatics. ***Section 6*** [pp. 75-78] does so for Newtonian gravity. Greaves and Wallace say that this theory is similar to classical electrostatics in its equations and in that it only has global theoretical symmetries [p. 75]. So their analysis of Newtonian gravity mostly proceeds in an analogous way. Namely, first of all they take a subsystem to be associated with some spatiotemporal region W; a subsystem theoretical state to consist of trajectories of a subset of particles and of a gravitational potential on the whole spacetime, both specified in agreement with the gravitational equations that they list; and the boundary conditions to consist in that the subsystem particles are confined within W, the environment particles are confined within the complement of W and the gravitational potentials of a subsystem and of an environment match on the boundary of the subsystem, where they note that this matching is going to be approximate as much as in the classical electrostatics case [pp. 75-76]. Next they say that the same spatiotemporal subsystem theoretical symmetries are again going to have DES by virtue of purely relational empirical symmetries [p. 76]. Of theoretical symmetries constituted by gravitational potential shifts Greaves and Wallace say that in principle these are also going to have DES by virtue of non-relational empirical symmetries, but that in practice the latter are hard to implement [ibid.], although they propose to do so by transforming particles far out in space into particles confined within a hollow sphere of massive particles [ibid., n. 21].

Greaves and Wallace claim however that there is an important difference about DES between classical electrostatics and Newtonian mechanics. For them it arises from the fact that charge-to-mass ratio is the same for all particles in the latter as opposed to the former [pp. 75-76]. They say that this leads to an additional theoretical symmetry of equations of Newtonian mechanics which is constituted by a joint transformation of acceleration $\mathbf{k}(t)$ and gravitational potential Φ [p. 76]. Here both transformations are presumed to apply from some time on and the latter transformation has the form $\Phi_S(\mathbf{x},t) \rightarrow \Phi_S(\mathbf{x},t) + \mathbf{k}(t)\cdot\mathbf{x}$ [ibid.]. When applied on a subsystem it is non-boundary-preserving on any theoretical states [ibid.], so Greaves and Wallace propose to complement this transformation by an environment transformation of the form $\Phi_E(\mathbf{x},t) \rightarrow \Phi_E(\mathbf{x},t) + \mathbf{k}\cdot\mathbf{x}$ [p. 77]. They take observational consequences of the resulting theoretical symmetry to be that the internal behaviour of a system (or rather of a subsystem, we may add) is unaffected by whether it is in free float or in free fall and propose to match these observational consequences with Einstein's elevator empirical symmetry [ibid.]. They also say that this empirical symmetry is easy to realise e.g. by jumping from a diving boat [ibid.]. More formally they propose a realisation involving a spacetime tube such that its width is much smaller than the distance from it to any significant massive bodies and a particle within it is in free fall under gravity [ibid.]. They say that in this case the boundary conditions of the tube and of the subsystem (which is apparently the particle, we may add) will differ by an acceleration and so there will be a subsystem symmetry transformation which allows the subsystem to be superimposed into the tube [pp. 77-78].

***Section** 7* [pp. 78-80] shows how Greaves and Wallace's framework applies to a theory they call classical electromagnetism or classical electrodynamics (which is more or less what Healey [2009] called "Maxwell's theory"). They define subsystems and theoretical states there in the same way as in classical electrostatics, except that the role of electrostatic potentials is now played by electromagnetic potentials [pp. 78-79]. External global theoretical symmetries again get DES by virtue of Galileo-ship-type empirical symmetries [p. 78].

What changes is that in place of an internal global symmetry of classical electrostatics constituted by electrostatic potential shifts classical electromagnetism has an internal *local symmetry* in the sense of a theoretical symmetry specified using functions on spacetime rather than parameters [ibid.]. This local symmetry is constituted by the familiar to us electromagnetic potential transformations $A_\mu \rightarrow A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$ [ibid.]. Note that similarly to Healey Greaves and Wallace here consider such transformations independently of the phase transformations $\psi \rightarrow \psi' = \psi e^{iq\theta(x,t)}$. As I explained when summarising Healey's [2009] article above, when so considered the former transformations constitute theoretical symmetries of equations of motion of free classical electromagnetism.

Greaves and Wallace treat theoretical symmetries constituted by the electromagnetic potential transformations of the form just given differently according to what is the value of $\theta(x_\mu)$ on the boundary of a subsystem. If it is zero, they classify these theoretical symmetries as interior and do not assign any DES to them [p. 79]. If it is not zero, they classify these theoretical symmetries as non-boundary-preserving instead [ibid.]. They next assign DES on the basis of Faraday's cage empirical symmetry to those of the latter theoretical symmetries where $\theta(x_\mu)$ is such that the electromagnetic potential transformations coincide with the electrostatic potential shifts on the boundary of a subsystem and where the environment theoretical state is suitably changed [pp. 79-80]. They do not say what happens with the other theoretical symmetries where $\theta(x_\mu)$ is non-zero on the boundary of a subsystem.

Greaves and Wallace's morals are firstly that there is no difference in the kind of DES of the local theoretical symmetries just discussed as compared to DES of global theoretical symmetries and secondly that both global symmetries of classical electrostatics and local symmetries of classical electromagnetism have DES on the basis of Faraday's cage empirical symmetry despite Healey's claims to the contrary [p. 80]. At the same time Greaves and Wallace likewise claim that there is a dysanalogy about Galileo's ship empirical symmetry and Faraday's cage empirical symmetry, but take it to reside in that boundary-preserving theoretical symmetries are matched with the former while non-boundary-preserving theoretical symmetries are matched with the latter [ibid.]. Because of that they rather liken the latter empirical symmetry to Einstein's elevator empirical symmetry with which they also match non-boundary-preserving theoretical symmetries [ibid.]. Similarly, Greaves and Wallace do admit that there is a dysanalogy between global and local theoretical symmetries, but take it to concern not whether given theoretical symmetries have DES but rather how much of them do. Namely, they say that in the case of local symmetries many subsystem transformations yield the same value at a subsystem's boundary and hence get DES, while in the case of global symmetries rather individual subsystem transformations yield a given value at a subsystem's boundary and hence get DES [ibid.]. On their view this happens because in theories with global symmetries there are no interior theoretical symmetries [ibid., n. 23]. Consequently, they propose to *redefine the global/local distinction* for a given subsystem by appealing to whether no or some interior theoretical symmetries are available rather than to whether no or some functions of spacetime are used when specifying a transformation [ibid.].

Section 8 [pp. 80-83] ends up Greaves and Wallace's discussion of particular cases by an analysis of Brading and Brown's main case study which concerns 't Hooft's beam-splitter empirical symmetry. Because of that Greaves and Wallace now pass from considering classical electromagnetism alone to considering its coupling to some quantum matter theory. Greaves and Wallace call the free matter theory they use the Klein-Gordon theory by the names of its founders. As classical electromagnetism is in turn due to Maxwell, Greaves and Wallace call the resulting interaction theory Klein-Gordon-Maxwell theory. The Klein-Gordon theory is a relativistic quantum matter theory (another famous theory of the same sort being Paul Dirac's). Correspondingly, Greaves and Wallace also give classical electromagnetism in relativistic form. This is by contrast to Brading and Brown who used non-relativistic quantum mechanics and non-relativistic formulation of classical electromagnetism. In either case it is however satisfactory that both matter and gauge fields are specified on the same kind of spacetime. Besides, it seems that either of Greaves and Wallace's and Brading and Brown's analyses can carry on from relativistic context to non-relativistic one or vice versa.

On the other hand, as much Brading and Brown's as Greaves and Wallace's choice does not eliminate another discrepancy which consists in that the matter is treated as quantum while the electromagnetic field is treated as classical. Greaves and Wallace say on this that the Klein-Gordon theory can alternatively be interpreted as describing classical matter, but note that when so interpreted it is not empirically adequate [p. 81]. Another response could be to opt for a third choice where both matter and the electromagnetic field are given in a quantised form. This requires passing to empirically adequate quantum electrodynamics and was envisaged already by Kosso [2000, p. 95]. However, this passage induces profound changes in theoretical symmetries as explained by Alexandre Guay [2008, Section 3]. So even if one of Greaves and Wallace's or Brading and Brown's analyses was correct for their theories, its validity for a fully quantum theory would not follow automatically.

Proceeding as before, Greaves and Wallace specify subsystems, theoretical states and boundary conditions of their theory. Subsystems are matter and electromagnetic fields far from other matter and from radiation [p. 81]. A spacetime region W is again associated to a subsystem, and theoretical states of subsystems and environments are given by specifying matter scalar fields ψ and electromagnetic potentials A_μ on the relevant spacetime regions according to the theory's equations of motion [ibid.]. The boundary conditions consist as before in that a subsystem theoretical state and an environment theoretical state agree on the values of the two variables on a subsystem's boundary [ibid.].

Greaves and Wallace also note as usual that external global symmetries get DES by virtue of Galileo-ship-type empirical symmetries, and concentrate on internal local symmetries next [ibid.]. They say [ibid.] that the total Lagrangian of their interaction theory is invariant under the familiar to us joint local phase and electromagnetic potential transformations, which in my notation are $A_\mu \rightarrow A_\mu' = A_\mu + \partial^\mu\theta(x_\mu)$ and $\psi \rightarrow \psi' = \psi\exp(iq\theta(x_\mu))$ with the same function $\theta(x_\mu)$ in both cases. Beforehand I was writing the latter transformation as $\psi \rightarrow \psi' = \psi e^{iq\theta(\mathbf{x},t)}$, but I now replaced $\theta(\mathbf{x},t)$ by $\theta(x_\mu)$ to show that the matter part is also relativistic (as to switching from "$e^{\cdots}$" to "exp(…)", this is a mere notational change). Greaves and Wallace further note that the invariance under the joint local phase and electromagnetic potential transformations carries on from the total Lagrangian to equations of motion derived from it [ibid.]. Indeed, as I explained when summarising Brading and Brown's article above, theoretical symmetries of equations of motions are usually induced by theoretical symmetries of Lagrangians because these equations can usually be expressed as combinations of derivatives of Lagrangians. On the other hand, as I also expained there, the expression just mentioned can be obtained using the Hamilton's principle I presented in Part I, and as this principle is based on symmetries of actions, in a sense it is rather the latter theoretical symmetries that can be said to induce symmetries on equations of motion.

Similarly to their analysis of classical electrodynamics in their Section 7 Greaves and Wallace next divide the theoretical transformations they discuss into those where $\theta(x_\mu)$ vanishes on the boundary of a subsystem and those where it does not and take the theoretical symmetries constituted by the former and the latter transformations to be respectively interior and non-interior [ibid.]. However, while in their previous section all transformations of the latter kind were for them non-boundary-preserving, here they say that these can be boundary-preserving or not according to whether ψ does or does not vanish on the boundary of a subsystem. This for them depends on whether the subsystem is or is not isolated. As they presume that the isolation holds in their case, the subsystem theoretical transformations and symmetries come out as boundary-preserving in that context [ibid.]. They next divide the subsystem theoretical transformations into equivalence classes according to the value of $\theta(x_\mu)$ on the boundary of a subsystem [ibid.] and concentrate on those of these transformations where $\theta(x_\mu)$ is constant and non-zero on the subsystem boundary [p. 82]. Because these are boundary-preserving on the theoretical states of interest, Greaves and Wallace match them with purely relational empirical symmetries [ibid.]. More precisely, as they claimed in their Subsection 2.2 that a setup with one half-wave plate which they take to realise 't Hooft's beam-splitter empirical symmetry can be modelled using a theory with joint local phase and electromagnetic potential symmetries by virtue of them containing phase shifts, Greaves and Wallace now claim that it is this empirical symmetry that is to be matched with the theoretical symmetries they concentrate on [ibid.]. As a result, they say that here local theoretical symmetries have DES on the basis of 't Hooft's beam-splitter empirical symmetry in the same way as theoretical symmetries constituted by theoretical boosts have DES on the basis of Galileo's ship empirical symmetry [ibid.]. A bit earlier Greaves and Wallace claim that the analogy in the case of these two empirical symmetries is closer than if Galileo's ship empirical symmetry is compared with Faraday's cage empirical symmetry [p. 80]. As we now see, this should follow from the fact that they take both 't Hooft's beam-splitter empirical symmetry and Galileo's ship empirical symmetry to be purely relational because of matching boundary-preserving theoretical symmetries with either of them, while Faraday's cage empirical symmetry, as much as Einstein's elevator empirical symmetry, is for them what I called an intrinsic empirical symmetry to be matched with non-boundary-preserving theoretical symmetries.

In the remainder of this section Greaves and Wallace consider objections to the matching they are discussing.

Firstly they quote [p. 82] Brading and Brown's argument [2004, p. 656] which starts by dividing a region where a wavefunction is located into two subregions and applying the joint local phase and electromagnetic potential transformations on one but not the other of these subregions apparently with the aim to get a relative difference in phase between the two parts of the wavefunction. Brading and Brown then argue that if the electromagnetic potential on the boundary between the two subregions is discontinuous, the relative phase is undefined, while if it is continuous, one gets "a local gauge transformation" [ibid.] on the whole wavefunction which does not lead to any new observational consequences. So Brading and Brown's conclusion is that either way the joint local transformations in question cannot lead to DES. Greaves and Wallace say however that if restrictions of a transformation to two subregions are gauge transformations, it does not follow that the transformation on the whole region is a gauge transformation, because if ψ vanishes where the two subregions overlap, it may be impossible to unify the two subregion gauge transformations into a single gauge transformation, and this is precisely what Greaves and Wallace suppose to happen in their case [pp. 82-83].

Next Greaves and Wallace consider two more objections in a lengthy note [p. 83, n. 24]. One of them is also due to Brading and Brown (Greaves and Wallace quote them without giving a page, but the text is from [Brading and Brown 2004, p. 658]). This objection says that two halves of the beam cannot be interpreted as subsystems, because if the beam is composed of a single electron, then we cannot really say that its halves are subsystems but only that the whole electron is a subsystem, and this should carry on to beams composed of more than one particle. Greaves and Wallace reply that firstly, the quantum theory used to model the relevant setup is mathematically equivalent to a classical theory where halves of the beam are counted as subsystems; secondly, one can model the relevant setup using Fock-space methods so that halves of the beam be counted as quantum subsystems; and thirdly, and "more importantly, 'subsystem' is just a word to be used in whatever way is most perspicuous", and in the context in question, as opposed for instance to an analysis of entanglement, the most perspicuous way consist for them in treating two halves of the beam as subsystems [p. 83, n. 24].

The last objection is due to an anonymous referee. This person says that the theory Greaves and Wallace discuss may admit an ontology based on the action-at-a-distance as explained in Healey's book [2007] and so the application of the usual notion of subsystem may be problematic in this context. Greaves and Wallace respond that for one thing, the distinction between systems and subsystems seems to peacefully coexist with the action-at-a-distance involved in entanglement; for another, the regions they speak about are simply connected; but most importantly, they do not wish to opt for "a more ontologically-committed notion" of subsystem but only for a formal one [p. 83, n. 24].

Section 9 [pp. 84-86] contains Greaves and Wallace's summary of their framework.

They now start not with S and E as in their Section 3 but with U. They call the latter either a set of total system theoretical states as before or a set of universe theoretical states. S and E are as usual sets of subsystem and environment theoretical states respectively. All of U, S and E are now supposed from the outset to be compatible with a theory's equations of motion. The elements of these sets are respectively u, s, e and their primed versions [p. 84].

S and E are presumed to be obtainable from U by projections π_S and π_E respectively. It is required that for each s and e there be at most one u such that $\pi_S(u) = s$ and $\pi_E(u) = e$. Greaves and Wallace say that this should allow for subsystem and environment theoretical states to jointly determine total system theoretical states. If there is indeed a u such that $\pi_S(u) = s$ and $\pi_E(u) = e$, s is said to be dynamically compatible with e, which in field context means that their fields approximately agree on the boundary between them. In this case the function * between s and e is said to be defined and more precisely $s*e = u$. Meanwhile, if there is no u such that $\pi_S(u) = s$ and $\pi_E(u) = e$, then $s*e$ is said not to be defined. For a given s its boundary conditions B_s consist of all e such that $s*e$ is defined, and likewise for a given e its boundary conditions C_e consist of all s such that $s*e$ is defined [ibid.]. Contrary to their Section 3 Greaves and Wallace do not use the notation $<s, e>$ to designate those $s*e$ that are defined.

Theoretical symmetries are now simply defined as constituted by some theoretical transformations σ comprised in a set Σ without further details about the character of these transformations [ibid.], while in Greaves and Wallace's Section 3 [p. 68] the transformations in question were said to be bijective and closed under inverses and composition. As before, these transformations are primarily assumed to act on U [p. 84]. It is next required of π_S that if it yields the same result for some u and u', then it should do so for any other total system theoretical states that are linked to u and u' by σ, and likewise for π_E [ibid.]. It is also assumed as before that σ restrict to subsystem and environment theoretical transformations [ibid.]. These are now denoted respectively by σ_S and σ_E instead of $\sigma|_S$ and $\sigma|_E$ as in Greaves and Wallace's Section 3. Their sets are denoted as before respectively by Σ_S and Σ_E [ibid.]. If $\pi_S(u) = s$, $\sigma_S(s)$ is defined as being equal to $\pi_S(\sigma(u))$, and similarly for the environment [ibid.].

Interior subsystem theoretical symmetries are next defined as those σ_S such that $\sigma_S(s)*I$ belongs to Σ [p. 85]. We may guess that I is here identity, but an explanation of how * works for theoretical transformations is anyway missing, for * was only defined for theoretical states a bit earlier. Boundary-preserving subsystem theoretical symmetries for a given s are in turn defined as those σ_S which yield a subsystem theoretical state such that it and s "lie in the same boundary condition, C_e, as one another" [ibid.]. Greaves and Wallace then say that interior theoretical symmetries "clearly" make part of boundary-preserving theoretical symmetries and that "equally clearly" interior theoretical symmetries are just those (subsystem) theoretical symmetries that are boundary-preserving on all s [ibid.]. They thus promote the cautious suggestion from their Section 5 of which they said themselves being unsure and according to which subsystem theoretical symmetries are boundary-preserving on all states just in case they are interior symmetries [p. 72] into the rank of something self-evident.

Afterwards [p. 85] Greaves and Wallace define *intrinsic properties of a subsystem* as those of its properties which depend on $\pi_S(u)$ alone and not on any other features of u. They also require that if the transformation from u to u' does not change which possible world is being represented, which for Greaves and Wallace happens when u and u' are related by a universe theoretical symmetry, then u and u' must agree as to which properties are intrinsic [ibid.]. Besides, they define *an experiment confined to a subsystem* as "an experiment whose outcomes (or probabilities of outcomes) depend on the intrinsic properties of the subsystem alone" [ibid.].

Greaves and Wallace next say that their claims as to which of their kinds of subsystem theoretical symmetries have DES and which do not are "direct consequences of [the preceding] definitions" [ibid.]. These claims are as follows. Firstly, interior symmetries have no DES because although whenever $s*e$ is defined, $\sigma_S(s)*e$ is as well, both represent the same possible world [ibid.]. Secondly, in the case of non-interior boundary-preserving subsystem theoretical symmetries defined total system theoretical states $s*e$ and $\sigma_S(s)*e$ in general represent different possible worlds, but because of being linked by subsystem theoretical symmetry transformation these theoretical states do not differ in intrinsic properties of a subsystem or of an environment nor consequently in experiments confined to a subsystem but only in relations between the two [ibid.]. Thirdly, any environment theoretical states whose boundary conditions are linked by a non-interior subsystem theoretical symmetry transformation (which may now also be non-boundary-preserving) are compatible with the same subsystem theoretical states as one another [p. 86]. Greaves and Wallace think that this should be formalised by saying that if boundary conditions of environment theoretical states are linked by the transformation in question, then whenever s^*e is defined so is $\sigma_S(s)^*e$ [ibid.]. They say in addition that if e and e' differ intrinsically, then passing from s^*e to $\sigma_S(s)^*e$ matches with a not purely relational change in the physical state of the universe and that if there is a "principled connection" between the passage from e to e' and the application of σ_S to s, then this leads to DES of the subsystem theoretical symmetry constituted by σ_S [ibid.]. However, they say that they have not explored "the possibilities for such 'principled connections' " [ibid.].

Section 10 [pp. 86-88] is Greaves and Wallace's conclusion.

They firstly remind us two ideas mentioned in their Section 1 which they now present as "conventional" [p. 86]: one according to which only subsystem theoretical transformations lead to DES and another according to which global but not local theoretical symmetries have DES [ibid.]. They next claim that the framework they have developed in their article accounts adequately for the former idea while exhibiting the latter as erroneous [ibid.]. For on their view a distinction suitable for analysing Galileo-ship-type phenomena is between interior and non-interior subsystem theoretical symmetries rather than between global and local subsystem theoretical symmetries, and the correct view is that it is interior subsystem theoretical symmetries that do not have DES [ibid.]. This does not imply automatically that local subsystem theoretical symmetries do not have DES, because it is not necessarily the case that all of the latter symmetries are interior [ibid.]. Indeed, it is only for identity that such such implication is right [ibid.]. The relationship between the global/local distinction and the interior/exterior distinction (where "exterior" is apparently Greaves and Wallace's unusual way of saying "non-interior") rather consists in that only local symmetry groups contain interior subsystem theoretical symmetries distinct from identity [ibid.].

After that Greaves and Wallace comment on the three issues that they formulated in their Section 1 for the idea that only global symmetries have DES. The first issue was that it is logically impossible to assign DES to global symmetries alone while global symmetry groups form subgroups of local symmetry groups. Greaves and Wallace say that this issue is avoided in their framework firstly because they do assign DES to local symmetries and secondly because they do not assign a property (of not having DES) to a group and an opposite property (of having DES) to its subgroup but assign instead a property to a subgroup and an opposite property to another subgroup [pp. 86-87]. Passing to their third issue next, Greaves and Wallace say having shown in agreement with it that there are indeed matchings between local theoretical symmetries and some empirical symmetries that are analogous to the matching between global theoretical symmetries constituted by theoretical boosts and Galileo's ship empirical symmetry [p. 87]. Moreover, they claim that global symmetries and corresponding equivalence classes of local symmetries will entail exactly the same empirical symmetries [p. 88], but do not provide any demonstration of this contrary to *my proof* from Part III. They also make a more specific claim by which a more close association of Galileo's ship empirical symmetry with global symmetries and of Faraday's cage empirical symmetry with local symmetries is spurious [ibid.]. Finally, Greaves and Wallace's second issue consisted in that not assigning DES to local symmetry groups seemed to entail that localising a symmetry group leads to a loss of explanatory power. Their reply is that given the way matchings are made in their framework no such loss occurs there [ibid.].

On the other hand, Greaves and Wallace divide matchings into those involving boundary-preserving theoretical symmetries and those involving non-boundary-preserving theoretical symmetries [ibid.]. This makes that in their framework 't Hooft's beam-splitter case is more analogous to Galileo's ship case while Faraday's cage case is more analogous to Einstein's elevator case [ibid.]. Their matchings are further summarised in a table [p. 87] as follows. Firstly, Galilean or Poincaré transformations in all flat spacetime theories are classified as global and boundary-preserving and lead to DES on the basis of Galileo's ship empirical symmetry. Secondly, asymptotically constant joint phase and electromagnetic potential transformations from Klein-Gordon-Maxwell theory are classified as local and boundary-preserving and lead to DES on the basis of 't Hooft's beam-splitter empirical symmetry. Thirdly, spatially constant potential shifts from classical electrostatics and Newtonian gravity are classified as global and non-boundary-preserving and lead to DES on the basis of Faraday's cage empirical symmetry and of its gravitational analogue respectively. Fourthly, asymptotically constant electromagnetic potential transformations from classical electromagnetism and their gravitational analogues are classified as local and non-boundary-preserving and lead to DES on the basis of Faraday's cage empirical symmetry and of its gravitational analogue respectively. Fifthly, spatially constant accelerations from Newtonian gravity are classified as global and non-boundary-preserving and lead to DES on the basis of Einstein's elevator empirical symmetry. It is these matchings that show according to Greaves and Wallace that the global/local distinction has no particular importance from the point of view of DES [p. 88]. Apparently it is also these matchings, particularly the third and the fourth ones, that motivate Greaves and Wallace to say as reported above that global symmetries and equivalence classes of local symmetries will match with the same empirical symmetries and that the association of Faraday's cage empirical symmetry with only local theoretical symmetries is spurious [ibid.].

The article ends up with ***Acknowledgements*** where in particular "illuminating and very constructive discussions" with Katherine Brading and Richard Healey are mentioned [ibid.].

Section II. 4. 3. Analysis of the text

Subsection II. 4. 1. 1. Positive aspects and importance

Some positive features of Greaves and Wallace's article are as follows. Firstly, it is systematic in that it describes a general framework and then shows how to apply it in several contexts. In this aspect Greaves and Wallace's analysis is reminiscent of Kosso's. Secondly, Greaves and Wallace's article is the only one among the main literature on DES which provides a detailed discussion of all the four cases that in my vocabulary give rise to the recognised empirical symmetries, namely Galileo's ship, Faraday's cage, Einstein's elevator and 't Hooft's beam-splitter cases. This is explained by the fact that by the time of Kosso's [2000] article the notion of empirical symmetry was not yet formulated, Brading and Brown [2004] were apparently unaware of Faraday's cage empirical symmetry and Healey [2009] decided to concentrate on it and Galileo's ship empirical symmetry alone. In any case, Greaves and Wallace's attempt to elaborate a framework which would accommodate all the four cases is praiseworthy. Thirdly, Greaves and Wallace's article was perceived as important by the research community, as can be seen from the fact that it gave rise to direct responses by Friederich ([2015], [2017]) and Teh [2016] in addition to being taken into account in Ladyman and Presnell's manuscript [2016/01/08]. This article thus significantly contributed in attracting to the topic of DES the attention it deserves and in analysing DES further.

In addition, I find reasonable some aspects of Greaves and Wallace's framework which are absent from the previous literature on DES. One of such aspects is their position by which both global and local theoretical symmetries can have DES. Indeed, I have already argued in favour of this position when analysing the previous articles on DES and will continue to do so in Part III. My defence is somewhat different from Greaves and Wallace's, but the point is in any case that there are rather easy ways to make this position right. This is by contrast to the more traditional and more restrictive position by which global but not local theoretical symmetries can have DES. Although I will also show how to argue in favour of the latter position in Part III, ultimately its chances to be accepted will come out as lower. Briefly, this is because in my framework the former position is right for the observational DES which we are able to establish, while the latter position would only have chances to be shown to hold if we were able to establish the ontological DES.

Another aspect where I agree with Greaves and Wallace concerns attention to theoretical states. In fact theoretical states already matter much for Healey [2009], because theoretical states are what Healey calls models and as we remember models play a prominent role in his definition of theoretical symmetry, in his key argument and in the way one represents empirical symmetries in his framework. Besides, I argued above that Brading and Brown [2004] also deal with theoretical states in one of the global/local distinctions from their Section 2. However, it is in Greaves and Wallace's article that the attention to theoretical states is most explicit and most important for the analysis of DES. And I will argue in Part III that such an attention is right. However, one may accept the importance of theoretical states without accepting Greaves and Wallace's framework. As will be clear from my analysis of their article, that is precisely what happens in my case.

Subsection II. 4. 1. 2. Problems with justifying Greaves and Wallace's assignments of DES

Perhaps the most important claim in Greaves and Wallace's article tells us in general when theoretical symmetries do not have DES and when they do. Namely, Greaves and Wallace claim that theoretical symmetries do not to have DES when they are universe symmetries and do have DES when they are subsystem symmetries which are not universe symmetries [p. 73]. The question is, what is the justification of this claim?

When making it at [p. 73] Greaves and Wallace refer for justification to "the outset", i.e. to the beginning of their article. What we find there is the statement of a tension between the interpretation of theoretical symmetries as redescriptions on the one hand and the use of "observable consequences" of symmetries in theory construction on the other hand [p. 60]. Greaves and Wallace then say that this tension is resolved by distinguishing between universe symmetries on the one hand and subsystem symmetries which are not universe symmetries on the other hand [ibid.]. They next go on to presenting Galileo's ship empirical symmetry as a subsystem symmetry which is not a universe symmetry [pp. 60-61] and suggest that local symmetries do not have DES if for them subsystem symmetries are also universe symmetries [p. 61].

To see whether this yields any justification for Greaves and Wallace's main claim we should understand what the tension and its resolution consist in. The redescription interpretation of theoretical symmetries is rather clear: in my terms what Greaves and Wallace say of it amounts to matching theoretical states constituting theoretical symmetries so interpreted with the same physical state, so that these theoretical states turn out to provide different descriptions of the latter. What is unclear in the formulation of the tension is what are the symmetries relevant to theory construction. The example of Galileo's ship empirical symmetry which comes next and the qualification of consequences of these symmetries as observable suggests that Greaves and Wallace speak of empirical symmetries. But in that case the tension can be resolved not by distinguishing between universe symmetries and subsystem symmetries as Greaves and Wallace propose, but rather by noticing that we have there theoretical symmetries on the one hand and physical symmetries on the other. So the tension becomes unsuitable to justify Greaves and Wallace's main claim, for the latter opposes different kinds of theoretical symmetries while the former comes out as opposing theoretical symmetries to physical symmetries.

This suggests an alternative understanding of the symmetries relevant to theory construction not as physical symmetries but as theoretical symmetries suitably interpreted. More precisely, the suitable interpretation has to be the assignment of DES to these theoretical symmetries, because this is what is able to provide a link with Greaves and Wallace's main claim which concerns assignments of DES and because this agrees rather well with what Greaves and Wallace say of the tension more precisely, namely that "observable consequences of symmetries have guided physicists in their construction of theories ever since Galileo-ship thought experiment" despite the redescription interpretation [p. 60]. The tension then takes the following form: on the one hand we have the interpretation of theoretical symmetries as redescriptions and on the other hand we have the interpretation of theoretical symmetries as having DES. We now add to this Greaves and Wallace's saying [ibid.] that the resolution of the tension comes from distinguishing between universe symmetries on the one hand and subsystem symmetries which are not universe symmetries on the other hand. Given the understanding of the tension just mentioned this yields that theoretical symmetries interpreted as redescriptions should be identified with universe theoretical symmetries while theoretical symmetries with DES should be identified with subsystem theoretical symmetries which are not universe theoretical symmetries. If that is right, then the tension is indeed resolved because theoretical symmetries differently interpreted happen to be different kinds of theoretical symmetries. And the fact that it is more precisely universe theoretical symmetries that get the redescription interpretation and subsystem theoretical symmetries which are not universe theoretical symmetries that get DES then makes right just as desired Greaves and Wallace's main claim by which universe theoretical symmetries do not have DES while subsystem theoretical symmetries which are not universe theoretical symmetries do have DES.

So, can Greaves and Wallace's main claim to be considered as justified? Not yet. What we have got resembles a justification, but in fact it is merely a conditional justification dependent on whether the resolution just explained is right. So the question returns a level higher. Namely, we should ask now the following: what is the justification for identifying theoretical symmetries interpreted as redescriptions with universe theoretical symmetries and for identifying theoretical symmetries with DES with subsystem theoretical symmetries which are not universe theoretical symmetries?

One possible justification would go along the following lines: there are two interpretations of theoretical symmetries to consider and two kinds of theoretical symmetries to consider; interpreting the same kind in different ways would indeed create a tension; so one of the kinds should receive only one of the interpretations and the other should receive the remaining interpretation; and then there must be some reason why the interpretations are to be distributed among the kinds in a given way and not the other way round.

This justification would be admissible if motivated, but again the motivation lacks. For one thing, surely there are more alternatives to DES than just the redescription interpretation. Greaves and Wallace seem to suggest the contrary, for they qualify the redescription interpretation as enjoying “a widespread consensus” [ibid.]. And at first sight this may seem right, for as explained in Part I this interpretation is adopted by or implied by the views of as much persons as Leibniz, Earman and Norton, and Redhead (under the name of passive interpretation of transformations), among others. But as we also saw there, for Leibniz as well as Earman and Norton this interpretation primarily concerns external universe theoretical symmetries, while for Redhead it is reserved for external theoretical symmetries as opposed to internal theoretical symmetries. Thus even those who adopt this interpretation do not necessarily do so for all theoretical symmetries at once nor for the same kinds of theoretical symmetries. Besides, this is not an interpretation that everybody adopts of some theoretical symmetries, as the term “consensus” suggests, but at best a more popular interpretation instead. For again we know from Part I that alternative interpretations different from both DES and the redescription interpretation are also available and sometimes adopted of the same theoretical symmetries. For instance, even if we concentrate on the theoretical symmetries of interest to the authors just mentioned, we see that Newton and Clarke defended of these a view different from Leibniz's, Maudlin objected similarly to Earman and Norton and I myself provided some alternatives to Redhead's views when discussing objects with spatiotemporal differences. And likewise for other theoretical symmetries interpretations other than those discussed by Greaves and Wallace are available. In particular, the gauge argument and IES also presented in Part I show that the theoretical symmetries concerned need not reduce to redescriptions while at the same time they do not receive DES, and these topics are hardly marginal enough to be ignored. In fact Greaves and Wallace say themselves being aware of IES [p. 61, n 3], which is unsurprising given that both Kosso [2000] as well as Brading and Brown [2004] to which Greaves and Wallace refer elsewhere in their article (e.g. at [p. 60] and [p. 61]) mention this topic. As to the gauge argument and the gauge principle, these are known perhaps even more widely than IES and are discussed in particular in the literature on DES by Kosso [2000, p. 94] and Healey [2007, p. 159-

167] (which is just after Healey's discussion of DES at [ibid., pp. 150-159]), where Greaves and Wallace are aware of the latter text too (as they refer to it at [p. 61]). So Greaves and Wallace's presentation according to which everybody interprets all theoretical symmetries as redescriptions is a voluntary oversimplification. Some may find this admissible in introductions, but we should not do the same for our more thorough analysis. Thus we should not limit ourselves to a simple opposition between the interpretation of theoretical symmetries as redescriptions and another interpretation of them as having DES.

Greaves and Wallace's move is then to assimilate some of the alternative interpretations just mentioned to the redescription interpretation. They do so by saying that it does not matter for their analysis whether one interprets a theoretical symmetry as involving a redescription or match it with a passage to a physically different but observationally indistinguishable state of affairs ([p. 60, n. 1]; see also [p. 63, n. 5]). However, firstly such an assimilation cannot be admitted in the context of our ontological study, and secondly which interpretation(s) one takes into account actually matters for the validity of Greaves and Wallace's analysis. For example, note that if we go beyond the binary opposition between theoretical symmetries interpreted as redescriptions and theoretical symmetries interpreted as having DES, then Greaves and Wallace's binary distinction between interior symmetries which are defined as having the former interpretation [p. 71] and non-interior symmetries which are therefore supposed to have the latter interpretation becomes inadequate in the face of the multiplicity of other interpretations of theoretical symmetries, such as the Newtonian interpretation, IES and so on. To accommodate them one either has to consider more than binary distinctions between theoretical symmetries or hold that a given kind of theoretical symmetries has different interpretations in different contexts, but neither is made by Greaves and Wallace. So their framework is simply inadequate once the oversimplified view of interpretations of theoretical symmetries is abandoned.

And something similar occurs on the side of kinds of theoretical symmetries too. Namely, firstly speaking of the kind of subsystem theoretical symmetries which are not universe theoretical symmetries is actually not very useful, because in a sense no subsystem symmetry can ever be a universe symmetry. The best is rather to speak of subsystem theoretical symmetries which do not give rise to universe theoretical symmetries, so that is what I will now be doing. And secondly, there are surely more theoretical symmetries than just universe theoretical symmetries on the one hand and subsystem theoretical symmetries which do not give rise to universe theoretical symmetries on the other hand. For there are also for instance subsystem theoretical symmetries that do give rise to universe theoretical symmetries, as well as environment theoretical symmetries, theoretical symmetries of equations of motion, theoretical symmetries of Lagrangians, theoretical symmetries of actions and so on. So even if we only had two interpretations to choose from, saying which of them applies to universe theoretical symmetries and holding that the other interpretation should apply to some other kind of theoretical symmetries would not automatically imply that the other interpretation should apply to subsystem theoretical symmetries which do not give rise to universe theoretical symmetries, because there are also much more kinds of theoretical symmetries to consider, and so it cannot be ruled out a priori that the second interpretation applies to some other kind of theoretical symmetries than the subsystem symmetries in question while the first interpretation applies to such subsystem symmetries in addition.

Now good news for Greaves and Wallace is that the critique just made is not as dangerous for their main claim as it is for the proposed justification of it. Indeed, the main claim only says that theoretical symmetries of a certain kind do not have DES and that theoretical symmetries of some other kind do have DES, while the justification was also presuming in particular which interpretation the former kind of theoretical symmetries has as well as that there are only two kinds and two interpretations to consider. So even if we drop the latter presuppositions because of the critique just made, the main claim still has chances to hold provided of course one can find some other justification for it. This amounts to justifying the main claim not via the tension and its resolution from the beginning of Greaves and Wallace's article as they wanted to do, but in some other way.

For this it seems that we need not seek far, because Greaves and Wallace also propose another justification for the main claim in the beginning of their article [p. 60, n. 1]. Namely, they present the appeal to subsystem symmetries which do not give rise to universe symmetries as something common to the previous literature on DES, which for them includes [Brown and Sypel 1995] in addition to [Kosso 2000], [Brading and Brown 2004] and [Healey 2009]. So it looks again as if they dealt here with something consensual, something already proved and approved by the previous studies of DES. In fact, however, this is not the case for the literature in question.

For first of all, [Kosso 2000] and [Healey 2009] rarely if ever mention subsystems, preferring to speak of systems instead. This is because actually neither is interested in the distinction between subsystems and the rest of the universe. Indeed, Kosso is only interested in whether a system changes with respect to a reference [2000, p. 87] and in that both do not change at the same time [ibid., pp. 86-87]. And Healey presents the appeal to subsystems of the universe rather than to the universe as a whole as merely typical [2009, p. 701] and does not pay much attention to it in what follows, because as I explained at length he is mostly interested in observable identical features rather than in observable differences and the former obtain for him within a system, so that he does not need to consider what happens outside it. Meanwhile, Brown and Sypel [1995] do describe Galileo's ship empirical symmetry as pertaining to a subsystem of the universe [ibid., p. 236], and this is further extended on what I called Galileo's ship-like physical symmetries in Brading and Brown's text [2004, pp. 646-648]. But this does not seem to be able to justify Greaves and Wallace's main claim either.

Indeed, as I argued when analysing the first trait of the latter empirical symmetries within my discussion of Brading and Brown's text, Brading and Brown's attention to the exterior of a subsystem has to do with their idea that subsystems can be changed while references should not, which makes them obliged to place the latter in the exterior of the former. However, as was promised to be further explained in Part III (and will be done there), some references necessary for Galileo's ship-like physical symmetries actually do need to be changed, and moreover this change is such that it allows them to be placed within subsystems. And likewise other necessary references that do not need to be changed can sometimes be placed within subsystems. So in fact we do not always need to consider any environment, and even less the whole of the rest of the universe, if we are interested in references. And so we do not always need to concentrate on subsystems rather than on systems if we are interested in references. But references as well as phenomena within a system is all we must be interested in, because in fact this suffices to establish both observable invariance and observable differences of Galileo's ship-like physical symmetries, as will also be explained in Part III (and this actually applies to all the recognised empirical symmetries, in particular). So a correct formalisation of these empirical symmetries need not always make appeal to what Greaves and Wallace call *the subsystem/environment split*, and even less to the subsystem/universe distinction.

Moreover, even if one of the latter distinctions was always needed for the formalisation of some empirical symmetries (although as just said this does not seem to be the case), this would not help Greaves and Wallace's main claim either. For this claim is about theoretical symmetries, while the formalisation in question concerns empirical symmetries instead. So this formalisation would only help to persons like Brading and Brown, Healey and myself who use the empirical approach, because they could then ask which theoretical symmetries can be matched with empirical symmetries so formalised. And so Greaves and Wallace's main claim by which theoretical symmetries without DES are universe theoretical symmetries while theoretical symmetries with DES are subsystem theoretical symmetries which do not give rise to universe theoretical symmetries could be motivated in the empirical approach by saying that it should hold because firstly, empirical symmetries are best formalised as subsystem physical symmetries which do not give rise to universe physical symmetries, secondly, to have DES theoretical symmetries must account for the latter fact, and thirdly, being able to account for the latter fact implies being a subsystem theoretical symmetry which does not give rise to a universe theoretical symmetry. But Greaves and Wallace together with Kosso follow the theoretical approach instead, as can be seen from the fact that they systematically start with theoretical symmetries and seek for matching empirical symmetries next. And arguably the theoretical approach should not appeal to any considerations about the empirical level when choosing which theoretical symmetries to start with, because this would amount to applying the empirical approach in disguise (this argument is further spelled out in Part III). Therefore, the theoretical approach should not appeal in particular to whether empirical symmetries necessarily involve subsystem physical symmetries which do not give rise to universe physical symmetries when choosing which theoretical symmetries to start with. So even if the latter necessity obtained indeed, that would not have to change anything for Greaves and Wallace given their theoretical approach.

A response can be made to this that one need not actually choose any theoretical symmetries in the theoretical approach from the beginning. For instead one could consider whatever theoretical symmetries there are and ask of any of them whether these have DES. The formalisation of empirical symmetries would then only intervene on the stage where matchings are checked, and could then single out subsystem theoretical symmetries which do not give rise to universe theoretical symmetries as those having DES because what these theoretical symmetries could be matched with in the world could happen to qualify as empirical symmetries formalisable as subsystem physical symmetries which do not give rise to universe physical symmetries. But this it not how Greaves and Wallace proceed anyway. For they match or not given theoretical symmetries with empirical symmetries not by paying attention to whether what is matched with the former resemble the latter but by presuming that universe theoretical symmetries do not have DES while subsystem theoretical symmetries which do not give rise to universe theoretical symmetries do have DES [p. 73]. But the whole question is why this should be the case. So if we stay with Greaves and Wallace, we are back to asking why one should take the distinction between universe theoretical symmetries and those subsystem theoretical symmetries which do not give rise to universe theoretical symmetries to be relevant to DES to begin with.

Before moving to the discussion of another part of Greaves and Wallace's framework it is worth mentioning Saunders's article [2003] which I presented in Part I as introducing the weak discernibility, in particular. Greaves and Wallace refer to it [p. 60, n. 1] when they try to justify the redescription interpretation of theoretical symmetries as consensual, but in fact this text also contains [Saunders 2003, pp. 299-301] other details very similar to what we have found in Greaves and Wallace's text. Namely, Saunders also highlights the guiding role of theoretical symmetries with DES and separates such theoretical symmetries from local theoretical symmetries, as Greaves and Wallace do in the beginning of their Section 1. Besides, Saunders also makes the opposition between universe symmetries and subsystem symmetries and associates the redescription interpretation with the former and DES with the latter. Finally, Saunders too does not seem to provide any clear motivation for the association in question.

Subsection II. 4. 1. 3. What is unsatisfactory about Greaves and Wallace's framework

Let us now see what is unsatisfactory about the remainder of Greaves and Wallace's framework.

Firstly, we could ask how good is their framework when evaluated within the empirical approach. One motivation for doing this comes from the possibility to justify Greaves and Wallace's main claim about assignments of DES via the empirical approach in the way just explained. Another comes from the fact that Greaves and Wallace (as much as the other literature on DES) do not distinguish between the theoretical approach and the empirical approach explicitly, which means that they do not rule out the use of their analysis within the empirical approach in principle.

Evaluated within the empirical approach, however, Greaves and Wallace's framework is unsatisfactory. For one thing, this is because their treatment of theoretical states is still not general and detailed enough. Indeed, they go as far as distinguishing between subsystem, environment and total system theoretical states, and they even tell that these may consist of assignments of values of some variables on spacetime in the field context. But they still do not tell us for instance how the variables are chosen in the field context and what theoretical states are in the non-field context. But such details must be added, because arguably not all theoretical elements there may be are going to be relevant to the matching between an empirical symmetry and a theoretical symmetry. Hence it must be told which theoretical elements are relevant for this matching, and so Greaves and Wallace's description of theoretical states is at best too general and imprecise to be a satisfactory one.

But things are actually worse, for Greaves and Wallace's framework happens to yield an inadequate DES in the empirical approach. This is because they take the theoretical state of an environment to range on the whole spacetime with the exception of the region occupied by a subsystem in one of the physical states constituting an empirical symmetry, and so they have no means of accounting for instances of two physical states constituting an instance of an empirical symmetry in a way which would place these instances in the same spacetime, unless by treating the subsystem in the second physical state as part of the environment of the subsystem in the first physical state. The latter option would be problematic because it introduces an unjustified asymmetry between the two instances, but Greaves and Wallace's solution is no better, for instead they match the subsystem in the second physical state with another total subsystem-plus-environment theoretical state defined on the whole spacetime (where the environment part of the total system theoretical state either stays invariant or changes insignificantly). But a theoretical state defined on the whole spacetime is usually supposed to represent the world from the beginning to the end. So two instances are represented in Greaves and Wallace's framework as placed in different worlds. Clearly, this is a misrepresentation, for we do not wait till the beginning of another actual world, look into parallel realities or consider possible worlds in order to establish an empirical symmetry in our world. Instead, it suffices to perform actual observations to establish any *observable empirical symmetries*, i.e. empirical symmetries whose constitutive differing and invariant features are observable, and thus to establish all the recognised empirical symmetries, in particular. Moreover, these observations may even coincide in time (when we simultaneously observe the instances of the two physical states in the second way of realising empirical symmetries) or in space (when we observe the instances of the two physical states at the same observable (relative to a reference) spatial location for example in the first way of realising Faraday's cage empirical symmetry).

One could object that such misrepresentations are admissible because Greaves and Wallace are concerned with something like my weaker observational DES rather than something like my stronger ontological DES (for according to Ladyman and Presnell's manuscript [2016/01/08] Wallace has confirmed being interested in the article we are discussing by the weaker 'logical' rather than by the stronger 'physical' consequences of theoretical symmetries, in Ladyman and Presnell's terms). However, if one wishes to establish the observational DES in view of approaching to establishing the ontological DES, one should not admit such misrepresentations already when establishing the observational DES. More generally, one wonders what is the use of assigning something like the observational DES if significant misrepresentations are allowed. For instance, if we admit that a theoretical symmetry may get the observational DES despite misrepresenting the physical states constituting an empirical symmetry as observationally indistinguishable while the constitutive differences of that empirical symmetry are observable, then a gauge symmetry in the sense of an observationally complete theoretical symmetry could get the observational DES by virtue of representing that empirical symmetry; but is it a kind of DES we want to have?

Now as said it seems admissible to check whether some framework fares well in the empirical approach if its authors do not distinguish between the empirical approach and the theoretical approach and so do not rule out the application of their framework to the former approach in principle. But since I do make the distinction between the two approaches, one may next ask whether Greaves and Wallace's framework fares better in the theoretical approach. This is especially natural given that as said it is the latter approach that they seem to implicitly favour, for at least when analysing particular cases of DES they always start with theoretical symmetries first and then seek for empirical symmetries able to instantiate them.

To carry out this second evaluation recall to begin with that the result of Greaves and Wallace's analysis is a list of correspondences between certain theoretical symmetries and the recognised empirical symmetries [p. 87] (to which for completeness global theoretical symmetries matching with 't Hooft's beam-splitter empirical symmetry must be added). As Greaves and Wallace do not seem to be interested in something like the ontological DES, and given that all the recognised empirical symmetries are observable empirical symmetries, i.e. empirical symmetries whose constitutive differing and invariant features are observable, we can take their list to summarise their proposals as to which correspondences lead, in the theoretical approach, to the observational DES of the theoretical symmetries concerned (or at least to some similar kind of DES). Beforehand Greaves and Wallace divide theoretical symmetries into interior and non-interior ones, as well as into into boundary-preserving and non-boundary-preserving ones, and these distinctions are the crucial part of their framework (see e.g. [pp. 62-63]), so one expects their list to follow from these distinctions. If this was so, we could say that Greaves and Wallace's distinctions allow one to determine which theoretical symmetries have the observational DES within the theoretical approach, and thus their framework would have to be recognised as satisfactory at least within that approach.

The problem is however that Greaves and Wallace's distinctions are actually unable to provide any non-trivial and precise answer the question of which theoretical symmetries have the observational DES and which do not, even less to generate Greaves and Wallace's list of matchings [p. 87]. Indeed, classifying a theoretical symmetry as interior only trivially implies the answer to the latter question, for by Greaves and Wallace's definition *interior symmetries* are those where both total system theoretical states describe the same possible world, and so they claim from the outset that an interior symmetry cannot correspond to any empirical symmetry [p. 71]. On the other hand, classifying a theoretical symmetry as non-interior does not generate the answer precisely enough, for some non-interior symmetries may fail to correspond to observable empirical symmetries or more generally to any non-trivial empirical symmetries, and so may fail to have any DES. This last issue seems actually common to any versions of the theoretical approach, as is further explained in Part III, and is a reason to opt for the empirical approach, as is also explained there. Meanwhile, the crucial ingredient is moreover absent from Greaves and Wallace's framework, namely is not explained there how one knows whether a theoretical symmetry is interior or non-interior, i.e. how one knows whether the total system theoretical states of a theoretical symmetry describe the same or different possible worlds. Thus one cannot determine whether a theoretical symmetry cannot or can have a DES by checking whether it is interior or non-interior respectively, because it is unclear how to do the latter.

As to Greaves and Wallace's second distinction, it happens to be orthogonal to the question of which theoretical symmetries have the observational DES, for both boundary-preserving and non-boundary-preserving symmetries figure in their list (this remark is also made in Ladyman and Presnell's manuscript [2016/01/08]). So this distinction cannot be used to determine which theoretical symmetries have the observational DES either. One could think that it could help to reformulate it as a distinction between symmetries boundary-preserving on all environment states and those boundary-preserving on less than all environment states and then to use Greaves and Wallace's cautious suggestion by which interior symmetries may be extensionally the same as symmetries of the first of these two kinds [p. 72]. For in that case symmetries of the second kind would not have any DES because interior symmetries do not have any DES as just explained, while symmetries of the first kind would be able to have some DES because they come out as non-interior, and so the reformulated distinction would become useful for determining which theoretical symmetries are able to have some DES. Moreover, we could take boundary-preserving and non-boundary-preserving symmetries from Greaves and Wallace's list to be different cases within the first kind, namely symmetries boundary-preserving on some but not all environment states and those not boundary-preserving on any environment states respectively, so that they become examples where symmetries of the first kind do have the observational DES. However, the whole move fails because contrary to Greaves and Wallace's suggestion there is no necessary link between being boundary-preserving on all environment states and being interior, and more generally because the interior or non-interior distinction and the distinction between being boundary-preserving on all or less than all environment states are again orthogonal. For by Greaves and Wallace's definition [p. 71] a theoretical symmetry is called *boundary-preserving* if a subsystem state resulting from the application of a symmetry transformation is compatible (i.e. is allowed theoretically to be matched) with the same environment states with which the initial subsystem state was compatible. And clearly, there is no necessary link between the quantity of cases where the initial and the resulting subsystem states are compatible with the same environment states and the fact for different combinations of subsystem states and of environment states to correspond to the same or different possible worlds as is needed for a symmetry to be interior or not respectively. Hence whether a symmetry is boundary-preserving on all or less than all environment states does not determine whether that symmetry does not or does have some DES either.

Besides, in a couple of notes [p. 60, n. 1; p. 63, n. 5] Greaves and Wallace gloss over the idea that describing the same possible world is the same as describing two observationally indistinguishable possible worlds. But firstly, ontologically this is not the same, obviously, and secondly, this does not help either. For if the gloss is taken seriously, then one may hypothesise that non-interior symmetries are those where total system theoretical states are observationally inequivalent, i.e. differ by their observational consequences. And thus one may further hypothesise that the fact for a symmetry transformation to be boundary-preserving on less than all environment states is what ensures that a symmetry constituted by this transformation has observationally inequivalent total system states, and so is non-interior, and so has DES, which could moreover be the observational DES, given that such a theoretical symmetry would be best instantiated by an empirical symmetry with observable constitutive differences. What makes sense here is the link between having observationally inequivalent states, i.e. being non-gauge in the sense of being observationally incomplete, and having the observational DES. However, there is no obvious reason whatsoever by which the boundary-preservingness on less than all environment states could ensure the observational inequivalence of total system states. In other words, clearly the fact that an environment state sometimes needs to be changed to ensure a theoretical compatibility with a subsystem state does not necessarily imply that observational consequences have to change. And neither is the preservation of observational consequences necessarily implied by the fact that the changed subsystem state is compatible with any initial environment state. So boundary-preservingness on all or less than all environment states does not determine whether a symmetry is gauge or non-gauge in the sense just mentioned either.

Section II. 4. 4. Summary of the analysis

Greaves and Wallace's analysis is systematic, deals with all the recognised empirical symmetries and has helped to attract more attention to the topic of DES in the research community. Besides, I agree with Greaves and Wallace's idea of admitting that both global and local theoretical symmetries have DES and with their accent on theoretical states.

However, Greaves and Wallace's main general claim by which theoretical symmetries do not have DES when they are universe symmetries and do have DES when they are subsystem symmetries which are not universe symmetries is not well formulated (as one would better speak of subsystem symmetries which do not give rise to universe symmetries) and moreover is not provided with any justification which would be both clear and admissible. Furthermore, a justification which would be available for this claim in the empirical approach does not actually work, because the accent on the subsystem/environment split which follows from Greaves and Wallace's framework and even more their subsystem/universe distinction are not needed to formalise the recognised empirical symmetries.

In addition, when considered within the empirical approach Greaves and Wallace's treatment of theoretical states is still not detailed enough, and besides their framework singles out theoretical symmetries which admit significant misrepresentations of empirical symmetries and thus do not lead to any adequate DES. Meanwhile, when considered within the theoretical approach their framework does not allow one to determine which theoretical symmetries have the observational DES (or a similar weak DES) and which do not, because the interior/non-interior and the boundary preserving/non-boundary preserving distinctions on which this framework crucially relies are unable to coincide extensionally with the distinction between theoretical symmetries which have the observational DES and those which do not.

Part III. My own account of direct empirical status

I have already had some occasions to present my own views on DES in the previous part, but this was done in passing and mostly concerned their application to particular cases. What I would like to do now is to give a general overlook of my position, explain in more detail some aspects of it, motivate it further and explain what it implies.

Chapter III. 1. Formalising the notion of empirical symmetry

DES consists of three elements: an empirical symmetry, a theoretical symmetry and a matching between them. Hence any respectable account of DES should say something about all of these elements. Besides, as I explained one can establish DES via the empirical approach, where one starts with an empirical symmetry and seeks a matching theoretical symmetry, or via the theoretical approach, were one starts with a theoretical symmetry and seeks a matching empirical symmetry. So any respectable account of DES should also say which approach is to be followed and why. The choice of the approach may determine in which order the notions of empirical symmetry and theoretical symmetry are to be analysed. As I will argue below that the empirical approach is to be preferred, I will analyse these notions in the order natural for that approach, namely by starting with the notion of empirical symmetry.

Although the main literature on DES tries as we saw to make this notion more precise, we do not find there any satisfactory account of what is an empirical symmetry (and in fact neither do we find it in the follow-up literature on DES). Indeed, to remind Kosso's starting notion is that of observable physical symmetry defined by his observable invariance condition and his observable difference condition. As explained, this notion is very weak and can be satisfied by virtually any phenomena. Brading and Brown implicitly define a stronger notion of Galileo's ship-like physical symmetries. However, as I argued above their account of such physical symmetries is defective in several respects, including the wrong understanding of what should happen with references, the insufficient attention to the second way of realising physical symmetries and the absence of the non-triviality check. Healey's formalisation of the notion of empirical symmetry omits important details noticed by Brading and Brown and contains a fatal drawback of overlooking Kosso's observable difference condition. Finally, Greaves and Wallace's account of empirical symmetries is also neither detailed enough nor getting the functioning of references right.

In sum, a more satisfactory account of empirical symmetries has to be provided. But how to do so? *My proposal is to formalise the notion of empirical symmetry by determining what the four recognised empirical symmetries have in common*. This makes sense because it allows firstly to preserve the practice of the literature of DES to call Galileo's ship, Faraday's cage, 't Hooft's beam-splitter and Einstein's elevator cases empirical symmetries and secondly to make the notion of empirical symmetry much more precise because of its being based on a comparison between four very concrete cases. Thus my task in this chapter will be to show how to implement the proposal just mentioned.

Section III. 1. 1. Identifiability

Let us firstly ask the following: should the notion of empirical symmetry be defined in terms of observable features alone, both observable and unobservable features or unobservable features alone? If Kosso's way is to adopt, then the first option has to be chosen. But other positions are possible too. In particular, Ladyman and Presnell's position in their manuscript [2016/01/08] consists in that the invariant features of an empirical symmetry should be observable. This implies that the differing features need not be observable, and that is how Ladyman and Presnell come to ask whether *the Michelson-Morley experiment* is an empirical symmetry. This is an experiment that was supposed to implement with the help of physical spatiotemporal transformations a physical transformation of the orientation of a system in the aether. The outcome of this experiment was that no observable differences were found besides those corresponding to physical spatiotemporal transformations. So one way of interpreting this experiment is that no physical transformation of the orientation of a system in the aether occurred, for instance because the aether does not exist to begin with. But another possible interpretation is that such a physical transformation indeed occurred but constitutes an observationally complete physical symmetry in Kosso's terms, i.e. a theoretical symmetry where Kosso's observable invariance condition is satisfied while his observable difference condition is not. Ladyman and Presnell's preference goes for the former interpretation, because as they say we know that the aether does not exist. But in fact of course we do not know that! So we cannot discard the second interpretation easily, and more generally there does not seem to be any reason for holding that observationally complete physical symmetries are impossible in our world. So the question is whether we should take such possibilities into account when defining the notion of empirical symmetry. Turning to my proposal above could seem to help, because it implies that we should take the four recognised empirical symmetries as a guide and any of these seems to satisfy both Kosso's conditions. But while that is true for how I defined them, as we saw there are other ways of defining 't Hooft's beam-splitter empirical symmetry and the definition of Einstein's elevator empirical symmetry has not yet been fixed. So we should rather make appeal to something else to answer our question.

My solution is then to bring in some pragmatic and epistemological considerations. Namely, what we need actually is not just some notion of empirical symmetry but a notion of empirical symmetry which we could use for establishing DES. And arguably, we cannot establish DES without knowing that an empirical symmetry obtains. Hence what we need for a DES that we can establish, i.e. for *an identifiable DES*, is a notion of an empirical symmetry that we can establish, i.e. of an *identifiable empirical symmetry*. So we should determine which means of identifying empirical symmetries are available to us. More precisely, as I have been saying, any non-trivial symmetry should feature an invariance and some differences. So to identify a non-trivial empirical symmetry we should explore which means we have to establish that a physical invariance and physical differences occur.

Now observation is perhaps the most straightforward way of establishing empirical symmetries and surely the one accepted in the literature on DES. Hence one way of establishing a non-trivial empirical symmetry is by observing both some invariance and some differences. Empirical symmetries for which this is possible satisfy both Kosso's observable invariance condition and his observable difference condition, so I will call them *observable empirical symmetries*. Saying that these can be establish amounts to saying that *identifiable empirical symmetries include observable empirical symmetries*. Thus the answer to our question is for the moment that when we are interested in identifiable empirical symmetries, empirical symmetries defined in terms of observable features alone are admissible.

But are any other empirical symmetries admissible as well? This depends on whether we have other means than observation to determine that a physical invariance and physical differences obtain. More precisely, if observation is admitted as a means to establish a physical invariance and physical differences, then arguably we need not any other means whenever observation is available. Hence what we should examine is whether we have other means to determine that a physical invariance and physical differences obtain in cases where observation is unavailable. But these are actually the cases where a physical invariance and physical differences are unobservable. Hence we should determine whether we have any means to establish that an unobservable physical invariance and unobservable physical differences obtain. Note however that these should be not any just means but only those admissible in the context of establishing DES.

One context where we seem to have such a means is exemplified by Kosso's rotation of a snowflake by 60 degrees and Ladyman and Presnell's rotation of a square tile by 90 degrees. In these examples the initial physical state and the final physical state when compared can be said to feature an observable invariance but no observable differences, i.e. the physical states before and after the rotation transformation happen to be observationally indistinguishable and so form an observationally complete physical symmetry. But during the passage between the two states, i.e. when a rotation is being performed, an observable change arises because the position of a rotated object with respect to some asymmetric reference changes. So observing a change during a transformation may seem to be one suitable means of determining that a physical difference is brought about although this difference ultimately turns out to be unobservable.

However, if the process of performing the physical transformation is observable, this is because intermediate physical states by which the transformation passes are observationally distinguishable from the initial and the final states. But then if we take an empirical symmetry to be constituted by the initial or the final physical state together with an intermediate physical state, this empirical symmetry will no longer feature an observable invariance that we thought it to feature when comparing the initial physical state with the final physical state. Moreover, this will make physical differences of that empirical symmetry observable, while we wanted to consider a case where these are unobservable instead. Meanwhile, if an empirical symmetry is rather taken to be constituted by an intermediate state together with a state obtained from it by the same physical transformation as the one linking the initial and the final states in the original case, then the differences will again be unobservable. So we will have to seek a means of establishing such differences once again, thus arriving to a regress. And finally, if we consider the initial and the final state together with an intermediate state, this may help for the first way of realising empirical symmetries where the two states are realised by the same system, but not for the second way where they are realised by distinct systems because there are no intermediate states in that case, nor for any account where the two ways are held equivalent. Moreover, this will not help in cases like Ladyman and Presnell's example of the Michelson-Morley experiment where the presumed constitutive differences in position with respect to the aether are unobservable for any collection of physical states.

Another means by which one may try to establish unobservable invariance and/or differences is by claiming that some phenomenon is an empirical symmetry (or that an empirical symmetry exists) because a theory says so. But in fact this means does not suit either. For it amounts to presupposing that has something like my ontological DES (more precisely, the analogue of the ontological DES for unobservable empirical symmetries) holds with respect to this phenomenon (this empirical symmetry). And so this will beg the question of which theoretical symmetry has DES. But that is the question we want to answer after having identified what counts as an empirical symmetry, not before. Therefore, not only should it be possible to establish an empirical symmetry without any appeal to theories ([Kosso 2000, p. 85]; [Healey 2009, p. 699]), but it is necessary in our context to limit ourselves to cases where empirical symmetries can be established without theoretical assumptions that presuppose the ontological DES (or its analogue for unobservable empirical symmetries).

We may ask though whether theoretical assumptions exist that are not question-begging in that sense. To try to determine this we may firstly turn to Kosso, who as we saw is also vaguely aware of the issue just explained because he says that we should not use theoretical assumptions dependent on the theoretical belief in a physical symmetry when trying to establish that physical symmetry. Kosso does indeed give examples of theoretical assumptions satisfying his requirement [2000, pp. 91-92]. However, his examples concern empirical symmetries whose constitutive features are observable. And, rather than serving to establish an empirical symmetry, these theoretical assumptions help us to interpret it theoretically, i.e. help us to match it with a theoretical symmetry. So even though there may be non-question-begging theoretical assumptions in our context, there does not seem to be a clear example of them helping us to establish an empirical symmetry whose invariance and/or differences are unobservable. Another option is to turn to

Ladyman and Presnell's manuscript [2016/01/08]. For there we have at least an example of theoretical assumptions that should help us to establish that a phenomenon is not an empirical symmetry whose differences are unobservable. Indeed, actually the reason why they claim that the Michelson-Morley experiment, where the presumed differences in position with respect to the aether would be unobservable, is not an empirical symmetry is because on their view theories say that the aether does not exist. However, even if it was so, this seems to amount to a denial of any ontological DES with respect to that experiment, and hence may likewise be question-begging in our context.

Besides, even if there were non-question-begging theoretical assumptions, it is still unclear whether we would have to appeal to them rather than to observation alone. For while to use observation for establishing empirical symmetries we need to accept reliance on observations alone, to use non-question-begging theoretical assumptions for establishing empirical symmetries we probably have to accept scientific realism about them which includes reliance on observations plus some more assumptions, and it is unclear whether scientific realism has to be accepted.

Finally, could not we somehow establish that an empirical symmetry occurs without establishing that both physical invariance and physical differences occur? In particular, can establishing one or another of the latter be enough? The option with not establishing physical invariance seems rather hopeless, because that a symmetry is defined by featuring an invariance suggests that we cannot be said to have established an empirical symmetry without having established a physical invariance. But the option with not establishing physical differences may look more promising if one agrees to drop the requirement that a non-trivial symmetry should necessarily feature some differences, for in that case it seems that one can establish an empirical symmetry by establishing a physical invariance alone given that there is nothing else besides a physical invariance that an empirical symmetry is to feature in that case. However, actually there is something else to feature even in that case, namely to have an empirical symmetry we still need two physical states in addition to a physical invariance. And arguably we cannot actually know that we have two physical states unless we know that they are physically different. If this is so, we cannot establish even a trivial empirical symmetry by establishing a physical invariance alone.

What follows from the above is that there does not seem to be any other acceptable means of establishing empirical symmetries besides observation. If that is so, *identifiable empirical symmetries are just observable empirical symmetries*. Therefore, *we must define empirical symmetries in terms of observable features alone when interested in identifiable kinds of DES*. Thus empirical symmetries of interest to us must satisfy Kosso's observable invariance condition and his observable difference condition – not just because Kosso proposes to do so, but because this is justified by reasons of identifiability.

From this follows an important result whose instance we saw when analysing Brading and Brown's article above. Namely, I was claiming there that gauge theoretical symmetries in the sense of observationally complete theoretical symmetries do not have DES on the basis of 't Hooft's beam-splitter empirical symmetry when DES is based on the ability of a theoretical symmetry to represent observable differences of that empirical symmetry in addition to its observable invariance. Note now that 't Hooft's beam-splitter empirical symmetry so defined is an observable empirical symmetry, while DES so defined is arguably an identifiable DES because of being based on identifiable empirical symmetries. This suggests that a generalisation of my claim should hold by which gauge theoretical symmetries in the sense of observationally complete theoretical symmetries do not have any identifiable DES at least of the kind relevant to the empirical approach where the matching relationship is that of representation. And indeed, this generalisation can be shown to hold as follows. Firstly, recall that, as I argued when analysing Healey's article above, both observable invariant features and observable differing features that an empirical symmetry has must be represented by a theoretical symmetry for it to have a reasonable kind of DES. Therefore, this should hold in particular of any identifiable DES if it is to be based on adequately representing observable empirical symmetries, given that they do have both observable invariant features and observable differing features by definition. Note then that gauge theoretical symmetries in the sense of observationally complete theoretical symmetries are unable to provide an adequate representation of observable differences because theoretical states of such symmetries are only able to represent physical states as observationally identical while a representation of observable differences cannot be adequate if physical states are not represented as observably different. Consequently, *gauge theoretical symmetries in the sense of observationally complete theoretical symmetries are unable to have any identifiable DES* of the kind we are discussing. In particular, therefore, they are unable to have my kinds of DES, which are the observational DES and its strengthening the ontological DES, given that the observational DES requires that both observable invariant features and observable differing features constitutive of an empirical symmetry be adequately represented. Thus *theoretical symmetries that can have some identifiable DES* including my observational DES *are going to be non-gauge in the sense of not being observationally complete*.

Although these results have not been demonstrated in the main and follow-up literature on DES, doubts are frequently expressed across it as to whether gauge symmetries in different senses have DES, which is in agreement with the traditional view of the wider literature on the ontology of theoretical symmetries in physics by which gauge symmetries do not have any ontological significance. So in particular even those rare authors writing on DES that challenge the traditional idea that gauge symmetries in the sense of local symmetries do not have DES remain traditional in that they advise to exclude from consideration gauge symmetries in the sense of observationally complete theoretical symmetries (or something similar) when analysing DES ([Greaves and Wallace 2014, p. 87]; [Teh 2016, p. 116]). And my result just stated seems to support the latter view, but in fact this is not at all so. For even if gauge symmetries in the sense of observationally complete theoretical symmetries do not have any identifiable DES, they may still be useful for determining which theoretical symmetries do have an identifiable DES. This (together with other things) will actually be illustrated *my* long-promised *proof* to be presented in this part of my work. We will see from it that, as already announced when discussing Healey's article, the topics of which theoretical symmetries have DES and of whether gauge symmetries in the sense of observationally complete theoretical symmetries have an independent ontological significance (not reducing to that of their observational consequences) turn out to be closely related.

Section III. 1. 2. Points of view, observers and references

The discussion just made shows that we need to concentrate on empirical symmetries that can exhibit both an observable invariance and observable differences. However, to establish an empirical symmetry we need not just that it be possible for an empirical symmetry to exhibit both kinds of features but also that we really observe both of them. As already mentioned many times, at least for macroscopic recognised empirical symmetries the latter depends not only on the presence of observers but also on whether observers or observable references have specific properties. However, I did not give much details so far as to how this functions more precisely, so that is what I will do now. Our question will be: *what conditions should observers or references satisfy for us to observe observable invariances and observable differences characteristic of macroscopic recognised empirical symmetries?*

To begin with we consider a simple case where we only have physical observers and observable systems (thus without observable references) and where all different physical states are instantiated by distinct observers and systems. In this context we can present three *points of view* introduced by Luciano Fonda and GianCarlo Ghirardi in their book on symmetry principles in quantum physics [1970, Chapter 1, summarised at pp. 44-45]. These points of view were introduced by them mainly to consider what changes and what stays invariant in theoretical descriptions when we pass from one point of view to another, but I will use these points of view to explain primarily what happens on the physical and observational level.

For Fonda and Ghirardi *the passive point of view* consists in that 2 observers observe 1 system, *the first active point of view* consists in that 2 observers observe 2 systems and *the second active point of view* consists in that 1 observer observes 2 systems. I will denote these points of view the pPOV, the 1aPOV and the 2aPOV respectively. I will also denote distinct observers by O1, O2 and distinct systems by S1, S2.

Now let us suppose firstly that the only system from the pPOV is S1 from the other points of view, that the only observer from the 2aPOV is O1 from the other points of view, as well as that in the 1aPOV O1 observes S1 while O2 observes S2. Denoting by O...(S...) how some observer observes some system, we get under these suppositions pPOV: {O1(S1), O2(S1)}; 1aPOV: {O1(S1), O2(S2)}; 2aPOV: {O1(S1), O1(S2)}. In other words, each point of view then consists of two facts of observation by some observer of some system and these observations differ as to which observers and systems are involved.

Besides, let us suppose that each time we have 2 systems or 2 observers in a point of view they are linked by the same physical transformation in both its kind (where *a physical transformation is of a given kind* if it acts on a given kind of features) and its amount. For instance, they can all be linked by physical spatial translations such that these always make the second system or observer to be located say 5 meters to the right from the first system or observer respectively. Then in the pPOV $x(O2) = x(O1) + 5m$, in the 2aPOV $x(S2) = x(S1) + 5m$ and in the 1aPOV both expressions hold, where x is a position and the positive direction of change in position is to the right of O1 and S1.

Our question is then as follows: in which points of view the two observations constituting it will be the same and in which points of view they will differ? In other words, which observer-system couples constituting a given point of view will have the same observations and which will have different observations?

Well, *features of many kinds will anyway look the same because the physical transformations not acting on features of these kinds will not affect them*. But the question is what happens with the features of a kind affected by the physical transformations.

Given our suppositions, the answer to our question is then that $O1(S1) \neq O2(S1)$ and $O1(S1) \neq O1(S2)$, or, in other words, that in the pPOV and in the 2aPOV the two observer-system couples will yield different observations, while $O1(S1) = O2(S2)$, or, in other words, in the 1aPOV the two observer-system couples will yield the same observation. Indeed, for instance in our example with physical spatial translations we get that if in the 2aPOV S1 is located in front of O1, then O1 will see S2 5m to the right from S1, while if in the pPOV S1 is located in front of O1, then O2 will see S1 5m to the left from the position in front of oneself (assuming the observers are turned towards the same side and look in parallel directions). By contrast, if S1 is located in front of O1, then S2 will also be located in front of O2.

The idea is then that this holds not only for physical spatial translations and other transformations leading to trivial empirical symmetries in my terms, but also for transformations constituting Galileo's ship, Faraday's cage and Einstein's elevator recognised macroscopic empirical symmetries. Thus *the observable differences of macroscopic empirical symmetries can be exhibited using either of the pPOV and the 2aPOV, while to exhibit the observable invariance of macroscopic empirical symmetries we need the 1aPOV*.

A first generalisation we can make of this is to note that both both the pPOV and the 2aPOV can be recovered within the 1aPOV provided we consider the latter as constituted in addition to the two observations O1(S1) and O2(S2) also by the observations O2(S1) and O1(S2). In this way *both the observable differences and the observable invariance of macroscopic empirical symmetries can be realised via this extended 1aPOV. So the extended 1aPOV can be thought of as a paradigmatic realisation of a macroscopic empirical symmetry.*

Another generalisation is to go beyond the simple case we supposed above to obtain where we only have physical observers and observable systems (thus without observable references) and where all different physical states are instantiated by distinct observers and systems. Firstly, we go beyond the latter supposition by also allowing different physical states to be instantiated by the same observer or by the same system. This requires reinterpreting O1, O2 and S1, S2 not as distinct observers and systems respectively but as distinct physical states of an observer and of a system respectively. Secondly, we go beyond the former supposition by admitting that observers can also be replaced by references observed by an observer. This requires reinterpreting O1, O2 as distinct physical states of not always an observer but of sometimes an observer and sometimes a reference observed by an observer. For the points of view to work as above we require that when observers are replaced by references observed by them, the observers consider whether an observable invariance or observable differences obtain not with respect to themselves but with respect to the references observed. In other words, we allow to *separate the task of observing*, which can then be performed by observers in arbitrary physical states compatible with that task, *from the task of ensuring that an observable invariance or observable differences obtain*, which is then to be performed by the observed references which still have to only be in the states O1, O2 of the kind specified above, i.e. differing in the same way as the states S1, S2.

We can now see that we can find a couple of examples of the points of view just presented in my analysis of Healey's article above.

Firstly, recall my discussion of the realisation of Galileo's ship empirical symmetry from Healey's key argument in [Healey 2009, p. 709]. As I explained, he was proposing there something which amounted to requiring that two situations linked by a physical boost have the same relationships with their observable references. In our ontological context his situations are the same as my physical states, so the two situations are two different physical states S1, S2 of the same ship or of distinct ships and the observable references having the same relationships with them are the states O1, O2 of the same or distinct reference(s) or observer(s) such that O2 is physically boosted with respect to O1. The whole thus amounts to realising the 1aPOV but not the extended 1aPOV, whence my claim above that Healey's context allows to observe observable invariance but not observable differences.

Secondly, recall how I proposed to establish observable differences when discussing Faraday's cage empirical symmetry on the occasion of analysing Healey's article. There I took the system to be a cage and a reference to be an observer or the ground. I then required to consider a final physical state where to the cage the physical operation of charging was applied and argued that sparkles will then arise on its boundaries provided no such operation was applied to the observer or the ground. We see now that this amounts to realising the observable differences via the 2aPOV where O1 is the state of the observer or of the reference and S1, S2 are the states of the cage.

In fact Healey envisages realising the pPOV as well. This happens when he passes in his key argument in his article from the stage where two situations are represented by two models to the stage where two situations are represented by the same model. Under the assumptions mentioned towards the end of my discussion of his argument (by which firstly theoretical reference frames making part of models stand for observable references and secondly Healey's situations and their relationships with references are such as the models represent them) and given our ontological context where Healey's situations are the same as my physical states this amounts indeed to realising a case where the same observable reference O1 is in different relationships with two physical states S1, S2 of a system. So this does allow to realise observable differences within Healey's wider situations (where the latter comprise both a physical state of a system and a physical state of a reference), but Healey does not pay attention to them in his argument because he is interested there in a restricted situation (comprising the interior of a system alone) with respect to which the two models form his theoretical symmetry.

Next, Kosso's article and my critique of it can now be further clarified. Indeed, as we recall Kosso's passive and active versions of physical transformations amount respectively to changing the reference but not the system or to changing the system but not the reference. Hence they amount to realising respectively the pPOV and the 2aPOV. Kosso introduces these versions when explaining how to satisfy his observable difference condition [Kosso 2000, p. 87]. We now see that he is right in doing so, because either of these points of view leads to exhibiting observable differences. I would however amend his position in two ways. Firstly, I find observable differences less trivial when it is the system rather than the reference that is transformed, hence I prefer the 2aPOV to the pPOV. Secondly, as already mentioned what is missing in Kosso's analysis is the account of how observable invariance obtains and hence his observable invariance condition becomes satisfied due to a change in both a physical state of a system and a physical state of a reference. This amounts to paying attention to the 1aPOV or, if the realisation of observable differences is required at the same time, to the extended 1aPOV.

Let us finally mention some *features of references* (or of observers if these implement both the task of observing and the task of ensuring that observable invariance and/or observable differences obtain).

Firstly, for the observable invariance to arise physical states of an observable reference should be linked by the same transformation than physical states of an observable system, but these need not be the same states. In other words, the transformations O1 → O2 and S1 → S1 should be the same but O1 = S1 and O2 = S2 need not hold. This arguably makes observable invariance less trivial. This also allows for instance to envisage as the initial physical state of Galileo's ship empirical symmetry the one where a ship moves with respect to the shore rather than standing still. Thus *for exhibiting observable invariance the relationships between a physical state of a system and a physical state of a reference should stay invariant by contrast to exhibiting observable differences where these relationships should differ.* So *macroscopic empirical symmetries have an extra observable invariant feature which consists of a relationship of a system to a reference relevant to establishing the other observable invariant features (in addition to allowing the dynamics within a system to be observable and hence observably invariant because of the system's parts involved in that dynamics being observable).*

Secondly, for an observable object to serve as a reference that ensures observable invariance it should be *asymmetric rather than neutral*, i.e. in a physical state that allows to discriminate between the other physical states of the kind defined by observable differences (where how this kind is formed is to be explained in the next section). For instance, if our observable differences are those of colour, then the reference should also be coloured rather than transparent; if our observable differences are those of length, the reference should be extended; and so on. For movement, whether rectilinear uniform as in Galileo's ship empirical symmetry or accelerated as in Einstein's elevator empirical symmetry, all observable things suit as references because neither is neutral with respect to this kind of features (but then we may wish to only call some of these things references if we wish references to be those things which also define spatial and temporal directions, for instance).

Thirdly, in principle there are no restrictions as to where references are to be located. Thus in particular *all the references we need to establish the observable invariance and the observable differences can be placed within systems within which the observable invariance is to obtain*. To see this suppose that we are in the second way of realising empirical symmetries where physical states of a system are realised by distinct macroscopic systems and that within at least one of these macroscopic systems an observer is located. Then if that observer can verify the state of the other system at all (e.g. by looking at a Galileo's ship or an Einstein's elevator or by touching a Faraday's cage while remaining within the other cage), that observer can establish that the observable differences obtain. And at the same time the observable invariance can be established either by the two observers comparing the descriptions of the experiments they have observed within their systems or by a single observer if that observer can observe experiments on either system while being within one system (e.g. if the walls are transparent or windows are large) and if for at least one of the systems this observer uses some suitably transformed thing as a reference instead of oneself (e.g. for either Galileo's ship a mast of that ship may be used). Thus firstly this allows to account for observations of observable differences within a macroscopic system (such as noticing a difference by looking out of the window and by using a GPS device) by a theory of references instead of relativising observable invariance to kinds of observations like in Healey's framework. And secondly, this shows that Greaves and Wallace's insistence on environments is completely misguided. For there could be no environments at all and yet empirical symmetries could be established!

Section III. 1. 3. Physical features

So far I was using the notions of physical systems, features and states without a precise account of what these notions mean and of how they are related, while they are directly relevant to formalising the notion of empirical symmetry. Therefore, although elaborating a satisfactory account of these notions is not my goal here, I would like at least to make them a bit more precise to use them for the former purpose. For this I will now give some definitions[6].

A physical feature is any monadic or relational generic physical property. *A physical thing* is whatever can instantiate physical features. *A dynamics* is a change of physical features in time. *A dynamics proper to a collection of physical things* is the dynamics of these physical things such that its unfolding is induced by their physical features including relative ones and not by any dynamics which involves something else than these things, although the initiation of the former dynamics may be so induced (this allows to count as proper a dynamics whose initial conditions are set by us). *A physical system* is a physical thing with parts such that these parts have a dynamics proper to them. *A momentary physical state* of a physical thing is a collection of physical features which this thing or its parts possess at a moment of time. *A(n extended (in time)) physical state* of a physical thing or system relative to a period of time is a collection of momentary physical states of the thing or of physical features which the thing or its parts possess at one, some or all moments within this period of time. *An outcome of a dynamics proper to parts of a physical system* is a physical state of the system and/or of its parts having participated that dynamics by which the unfolding of that dynamics ends up.

Some of the definitions just given make more precise what physical features, systems and states stand for. These definitions thus serve in particular to legitimise my talk throughout this work of empirical symmetries as constituted by physical states of physical systems, to which I have recently added physical states of observable references or observers (which are kinds of physical things). This agrees with our aim of formalising the notion of empirical symmetry on the basis of the four recognised empirical symmetries, because they can all be formalised in the way just mentioned. To the same aim also contribute the definitions of a dynamics proper to a collection of physical things and of its outcomes, which are relevant for defining observable invariant features constitutive of respectively macroscopic empirical symmetries and them together with 't Hooft's beam-splitter empirical symmetry (where the presumable dynamics among particles is unobservable and the particular observable dynamics in the sense of points appearing on the second screen is not invariant but its observable outcome in form of an interference pattern is).

On the other hand, we may also have more to say on how to formalise observable differing features constitutive of empirical symmetries. And besides we should say what counts as the same physical state and what does not. To be sure, with the first group of definitions just given we could already do the latter by appealing to the identity of systems having physical states and to lengths of time periods relative to which extended physical states are defined. But it may be better in our context to link the identity of physical states with something more relevant to empirical symmetries,

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[6] If I put a word in parentheses within a defined term, this usually means that logically it must be there but that I am going to omit it in subsequent uses of the term for brevity.

and in fact there is a way to do so. Indeed, I will now introduce another group of definitions which will end up allowing observable differing features to function as criteria of identity of physical states.

Two things are said to be *distinct* if they differ in their identity and *different* if they differ in their features. If difference in some features makes the identity of something that has them to differ, such features are called *essential (to the identity of that thing)* (this is not supposed to necessarily imply that the identity is always constituted by features but only that it may change when some features change). *Incompatible physical features* are those physical features that cannot ever be instantiated by a physical thing simultaneously. *A kind of physical features* is a maximal collection of physical features such that all of them are similar to each other but any two features are incompatible with each other (as are different sorts of movements, different colours and so on). *A kind of physical states defined by a kind of physical features* is a maximal collection of physical states such that each state comprises at most one physical feature of the latter kind and such that any change of a feature belonging to the latter kind necessarily induces a change in the identity of a state belonging to the former kind. In other words, features belonging to the latter kind are such that they are essential to the identity of physical states of the former kind. This does not necessarily imply that these features are also essential to the identity of physical things having these features or states (thus each sort of movement and each colour will bring about a distinct and different physical state defined by movements or colours, but the same thing may move in different ways or have different colours at different times). *A (conservative) physical transformation* of a physical system is a transformation from one physical state to another which preserves the identity of a physical system (thus in particular a physical transformation from one incompatible feature of a system to another and so from one physical state defined by the former feature to another physical state defined by the latter feature can be a conservative physical transformation).

To see how this other group of definitions applies to our topic of formalising the notion of empirical symmetry notice that *observable differences characteristic of any of the recognised empirical symmetries are incompatible physical features* in the sense above. Indeed, we do not ever see a Galileo's ship to move and to be at rest at the same time with respect to the same reference. And similarly, we do not ever see a Faraday's cage to emit some and no sparkles at the same time when placed at the ground. Also, we do not ever see 't Hooft's beam-splitter to exhibit no and some half-wave plates at the same run of the double-slit experiment, and nor do we ever see it to exhibit no and some switched-on solenoids at the same run of the double-slit experiment. Likewise, we do not ever see an Einstein's spaceship with its engine turned on and turned off at the same time, and nor do we ever see it at the same time near a massive body and near no massive bodies. Therefore, for each of the recognised empirical symmetries *we can use the kind of incompatible physical features constituting the characteristic observable differences of our empirical symmetry to define the kind of physical states for which such features are essential* in the way explained above.

There are however two nuances.

Firstly, in agreement with the definition of the kind of physical features above this kind should be maximal, i.e. include all similar incompatible physical features. Therefore, *the kind of features defining the kind of physical states on the basis of the observable differences will include not only the absence or presence of observable indicators but also different ways in which observable indicators can be present*, such as different velocities of Galileo's ships relative to a reference, different intensity of sparkles on the boundaries of Faraday's cages relative to a reference, different quantity of half-wave plates and switched-on solenoids in 't Hooft's beam-splitters, different velocity and character of movement of and within Einstein's spaceships due to differently working engines and different masses of neighbouring bodies. This is a welcome extension in that it allows to naturally generalise the definitions of the recognised empirical symmetries beyond their usual descriptions. This is also reasonable for macroscopic empirical symmetries, because what is an absence of an observable indicator relative to a reference in some state may turn out to be a presence of an observable indicator relative to a reference in another state, and so there is no sense in only distinguishing between the absence and presence while not taking into account different ways of presence. To be sure, perhaps there would be some sense if it mattered with respect to a reference in which physical state an observable indicator is manifested. But what follows from my discussion above is that it only matters whether a reference is in a physical state of a certain kind, namely such that it allows to discriminate between the observable differences, and not whether it is in any specific physical state. Likewise for observable invariant features the kind of physical state but not a specific physical state matters.

Secondly, *sometimes what defines a kind of physical states should be that kind of incompatible features whose representatives are options concerning the combination of relevant observables indicators rather than one of these observable indicators alone*. The most clear case where this applies is perhaps 't Hooft's beam-splitter case. There observable indicators can be taken to define two separate kinds of features: the first is formed by different options about half-wave plates (such as a complete absence, a presence of one half-wave plate, a presence of two half-wave plates) and the second by different options about switched-on solenoids (such as a complete absence and a presence of one switched-on solenoid). In either case we have a kind of features because any options within it cannot be co-instantiated by the same system. But the options from these two kinds do not form a single kind of features, because in principle any option from the first kind can be co-instantiated with any option from the second kind and vice versa. What does form a single kind are options about half-wave plates and switched-on solenoids taken together (such as a complete absence of half-wave plates and solenoids, a presence of one half-wave plate alone, a presence of two half-wave plates alone, a presence of one switched-on solenoid alone and a presence of one half-wave plate together with one switched-on solenoid), because again any such options cannot be co-instantiated by the same system. These options are in general more nuanced than the options from the first two kinds mentioned above, for instance because the option from the first kind by which one half-wave plate is present now divides into the option where one half-wave plate is present and no switched-on solenoids are present, the option where one half-wave plate is present and one switched-on solenoid is present, and so on. And it is this more nuanced classification of options that matters for 't Hooft's beam-splitter empirical symmetry, because for instance whether a switched-on solenoid is present when a half-wave is present is as we know crucial for whether an interference patter exhibits the observable invariance in this run of the double-slit experiment when compared with some other run. Therefore, it is this last kind of features defined by options about combinations of half-wave plates and switched-on solenoids that we should take as defining the kind of physical states relevant to 't Hooft's beam-splitter empirical symmetry. A similar treatment can be made in Einstein's elevator case, taking the relevant kind of features to be that defined by options about combinations of observable indicators of acceleration and observable indicators of gravity. Another possibility in that case is to take the relevant kind to be constituted by features that are observable consequences of the presence or absence of either relevant observable indicator, namely accelerated movements, in which case the treatment will be analogous to that for Galileo's ship case. Perhaps there is a way to find such *common observable consequences* of either observable indicator for 't Hooft's beam-splitter case as well, but it is not evident what would these common observable consequences consist in more precisely in that case.

To use for our purpose the physical states whose kind is defined by the kind of observable differences as just explained we also have to ensure that these states comprise observable invariant features as well. This can be done by *requiring that an observable differing feature be instantiated by a system during the whole time during which an observable invariant feature as well as a dynamics leading to it (if different from the latter) is instantiated by the same system.* Here *an observable invariant feature* is a feature that is to be preserved and *an observable differing feature (or an observable difference for short)* is a feature that is to be changed for us to have an empirical symmetry. So what the requirement just formulated means is of course not that the same observable differing feature should obtain whenever an observable invariant feature obtains, but that each instantiation of an observable invariant feature should be accompanied by an instantiation of an arbitrary one of the observable differing features such that the latter instantiation is not shorter in time than the former instantiation (and similarly for a dynamics leading to an observable invariant feature). This ensures that the whole instantiation of an observable invariant feature (and of a dynamics leading to it) is counted as belonging to the same physical state of the kind defined by observable differences. Besides, this makes the former feature relevant to the latter (contrast this with the case where observable invariant features are manifested at some period of time and observable differences at some other: here *the relevance of observable differences to the observable invariance* may be questioned). And in addition this provides an adequate formalisation of the recognised empirical symmetries, because the requirement just stated holds there. This concerns in particular 't Hooft's beam-splitter empirical symmetry if, as I proposed when analysing Brading and Brown's article above, Kosso's observable difference condition is to be satisfied there at the same time as his observable invariance condition. If so, then it becomes possible to define *the non-triviality check*, also explained on the occasion of that analysis, as the verification of whether incompatible features exist which are sufficiently similar to the incompatible features allowing to satisfy the observable invariance condition at the same time as ensuring the observable difference condition but which fail to satisfy the same observable invariance condition at the same time as ensuring an observable difference condition. Here *the amount of similarity relevant to the non-triviality check for an arbitrary empirical symmetry* should be at most that involved in the non-triviality check for the recognised empirical symmetries.

Section III. 1. 4. Definition of a generic empirical symmetry

Now we are ready to summarise the formalisation proposals I have made so far and some more ideas to be motivated in the next section. These can be expressed in the following (old and new) definitions:

A (non-trivial) physical symmetry is a symmetry in the world constituted by two partially identical physical states of a physical thing or of distinct physical things together with possibly a physical transformation linking these physical states that brings about all the differences there are between these physical states. *Constitutive differing (physical) features (constitutive differences for short) of a (non-trivial) physical symmetry* are features a difference in which is necessary for us to count something as a physical symmetry. *Constitutive invariant (physical) features of a physical symmetry* are features an invariance of which is necessary for us to count something as a physical symmetry. *Constitutive (physical) features of a physical symmetry* are physical features whose instantiation is necessary and sufficient for us to count something as a physical symmetry. *An observable physical symmetry* is a physical symmetry whose constitutive features are all observable. *An empirical symmetry is said to be macroscopic or microscopic* depending on whether a physical system or physical systems that it involves is/are macroscopic or microscopic.

A (generic identifiable) empirical symmetry is a non-trivial observable physical symmetry such that each of its two physical states is a physical state of a physical system; such that these two physical states are either linked by a conservative physical transformation (and thus belong to the same physical system) or belong to distinct physical systems and are a subject of our comparison; such that these two physical states feature as *the constitutive (observable) differences* incompatible observable physical features of the same kind, established for macroscopic empirical symmetries relative to a reference in a suitable physical state; and such that these two physical states feature as *the constitutive (observable) invariant features* observable physical features consisting of the outcomes of a dynamics proper to a physical system's parts together with, for macroscopic empirical symmetries, of the observable dynamics itself and/or, for macroscopic empirical symmetries, of the relationship of a system to a reference in a suitable physical state.

A reference in a physical state suitable for establishing the constitutive differences of a macroscopic empirical symmetry is an observable physical thing in a physical state exhibiting one of the features of the kind to which the constitutive differences to be exhibited belong and not neutral with respect to these constitutive differences (this presupposes that we know which features we wish to exhibit) or to all observable differing features of this kind (this presupposes that we know observable differing features of which kind we wish to exhibit). *A reference or references in physical states suitable for establishing the constitutive invariant features of a macroscopic empirical symmetry* are a couple of observable physical things or a single observable physical thing in two physical states such that these states are linked by the same conservative physical transformation or can yield the same comparison results as the physical states constituting the empirical symmetry in question (this presupposes that both the former and the latter physical states exhibit different features of the kind of features defined by the observable differing features).

Some already mentioned extra properties of the constitutive differences are as follows: any two of them cannot be instantiated a physical system simultaneously (*the incompatibility condition*); an arbitrary one of them should persist throughout the time during which a constitutive invariant feature (and/or a dynamics leading to it) obtains (*the relevance condition*); and there should exist incompatible observable differences which are similar to those constituting an empirical symmetry but which cannot be co-instantiated with the constitutive invariant features of that empirical symmetry (*the non-triviality condition*).

Section III. 1. 5. Microscopic empirical symmetries and Wu-Yang's beam-splitter empirical symmetry

The definition of a generic empirical symmetry given above is indeed general in many respects, but there is at least one respect in which it is specific in a way that may seem unnecessary. Namely, it explicitly relies on the distinction between macroscopic and microscopic empirical symmetries. This was justified above by the claim that macroscopic but not microscopic empirical symmetries have special constitutive differences and constitutive invariant features that are to be exhibited with the help of suitable references. The question is whether this cannot somehow be extended to microscopic empirical symmetries as well, and if not whether this specificity of macroscopic empirical symmetries should be kept in our general definition. My view is that it is surely possible to find a relational component in the microscopic 't Hooft's beam-splitter empirical symmetry even without using theoretical assumptions about how the interference arises, for instance by noticing that an interference pattern is just a certain relative distribution of positions of hits (by particles) of the second screen. One may also find how references can be important in that case. For instance, the usual way 't Hooft's beam-splitter experiment is performed is symmetric in that the source of particles is usually located at the same distance from either slit. In that case the interference pattern obtained, whatever the physical state with respect to phase-shifters and switched-on solenoids, also comes out as *symmetric* with respect to the axis defined by the point at the centre of the source and the point in between the slits equidistant from the slits (for instance in the sense that reflecting the interference pattern along this axis yield the observable invariance of the interference pattern). On the other hand, the interference pattern is generally not symmetric relative to any other axis. Thus to recognise that the interference patterns are the same in two physical states of a suitable setup requires evaluating both interference patterns relative to the axis just mentioned. So the points defining this axis can be considered as references with respect to which the constitutive invariant features of 't Hooft's beam-splitter empirical symmetry are established, thereby making this empirical symmetry similar to macroscopic empirical symmetries as far as their relational nature is concerned. But the dysanalogy with macroscopic empirical symmetries is also apparent here, for as said in the case just mentioned the reference points should be kept unchanged while in the latter empirical symmetries a physical state of a reference must be changed to establish the observable invariance. Thus although I would agree to say that *all the recognised empirical symmetries are dynamical and relational*, I would not say that they are relational in the same sense. And arguably the way macroscopic empirical symmetries are relational is characteristic of them to the extent that it cannot be neglected. Also as already explained we have an interest to make the notion of empirical symmetry more detailed to diminish the proliferation of matchings. Hence my special treatment of macroscopic empirical symmetries in the definition of a generic empirical symmetry above.

But why is my definition much less detailed with respect to microscopic empirical symmetries? This is because for macroscopic empirical symmetries we already have three recognised cases while for microscopic empirical symmetries we have just one. So while in the former case it is easier to determine which features matter for a generic macroscopic empirical symmetry by comparing the three recognised examples to each other, in the latter case there is no other recognised microscopic empirical symmetries with which 't Hooft's beam-splitter empirical symmetry could be compared to determine which features matter for a generic microscopic empirical symmetry. And at the same time it is rather likely that a generic macroscopic empirical symmetry and a generic microscopic empirical symmetry would have to differ in something more than as to whether macroscopic or microscopic systems are involved. For instance, we already know that 't Hooft's beam-splitter empirical symmetry is unlike the recognised macroscopic empirical symmetries in several respects, including the sense in which it is relational and the fact that the unfolding of a dynamics among parts of a system cannot be observably invariant there because of parts of a microscopic system being unobservable. So we would probably have to maintain a distinction between macroscopic and microscopic generic empirical symmetries anyway, but how to define the latter part of this distinction is not clear for the moment. It is also by this reason that there is so much variability in defining 't Hooft's beam-splitter empirical symmetry. For instance, as we know Greaves and Wallace count a half of the beam as a system while Brading and Brown only count the whole beam as a system. Besides, Healey together with Greaves and Wallace follow Brading and Brown in taking the interference patterns to contribute to Kosso's observable difference condition, while 't Hooft, Kosso and myself take it to contribute to Kosso's observable invariance condition instead. I think that although there are other arguments for or against either of such options, some justification for one or another of them may come from considering 't Hooft's beam-splitter together with some other candidate microscopic empirical symmetry.

Let us explore this possibility in a bit more detail. The most obvious place to search for another microscopic empirical symmetry is Kosso's (and Redhead's) example involving protons and neutrons. It has not been discussed in all the other literature on DES. But there seems to be a way to turn it into an empirical symmetry. This can be made using a proposal by Tai Tsun Wu and one of the authors of the Yang-Mills theory Chen Ning Yang [1975, p. 3855][7]. Their proposal consists in realising an isospin analogue of the electromagnetic Aharonov-Bohm effect using as particles "a coherent mixture of neutron and proton in a pure state" so as to obtain at the second screen "not only fluctuations of nucleon intensity, but also fluctuations of the neutron-proton mixing ratio" [ibid.]. The former fluctuations here are those leading to the observable change of the kind familiar to us of the interference pattern with respect to the initial physical state, but Wu and Yang suggest that the latter fluctuations lead to observable changes as well [ibid.]. Thus when the setup is as in the electromagnetic Aharonov-Bohm effect, some observable changes arise at the second screen. This suggests that we may also have other setups as in 't Hooft's beam-splitter case including those where observable changes at the second screen do not arise. As 't Hooft's beam-splitter case leads to 't Hooft's beam-splitter empirical symmetry, I will analogously call an empirical symmetry that would arise from Wu and Yang's proposal *Wu-Yang's beam-splitter empirical symmetry*.

Why did not I rely on that empirical symmetry when discussing how to define 't Hooft's

[7] Note that the relevant section contained at this page comes after Section VII and before Section IX but is erroneously also denoted as Section VII instead of Section VIII.

beam-splitter empirical symmetry, a generic microscopic empirical symmetry and a generic empirical symmetry more generally? Firstly, this is because as Wu and Yang recognise it is unclear whether their proposal is realisable in practice. More precisely, they say that it would surely be so if the gauge isospin particle (similar to photon which is a gauge electromagnetic particle, we may add) was massless, while if it is massive, the proposal is more difficult to realise [ibid.], and it is now believed that the relevant particle should indeed be massive. Secondly, although as just said comparing 't Hooft's beam-splitter empirical symmetry with another candidate microscopic empirical symmetry could be useful, the amount of usefulness may depend on what that other empirical symmetry is. In our case the putative Wu-Yang's beam-splitter empirical symmetry seems too similar to 't Hooft's beam-splitter empirical symmetry to solve such questions as what is to be counted in such contexts as a physical system and whether what happens at the second screen should be counted as contributing to Kosso's observable difference condition or to his observable invariance condition. So while I would not discard the idea of studying further the putative Wu-Yang's beam-splitter empirical symmetry and of seeking other microscopic empirical symmetries, what we have for the moment at this side is not of much use for my purpose.

Section III. 1. 6. Specifications to the recognised empirical symmetries, to their realisations and to their instances

Another specification already present in my definition of a generic empirical symmetry above is the mention of two physical states rather than of an arbitrary number of physical states. But admitting this specification allows to express things more clearly and to discuss them more easily, because the less of them we are to concentrate on, the better. Also, this is hopefully of no detriment to the analysis of our topic, because it does not seem like adding more physical states would improve its understanding in any substantial way. In fact such additions may on the contrary lead to confusions, as shown by the examples with rotation from our discussion of identifiability above. There adding more physical states could make an observationally complete empirical symmetry observationally incomplete. This may be a good news for the empirical symmetry in question, but this leads us away from the question of how to identify observationally complete empirical symmetries. So keeping the number of physical states to the minimum may even be preferable for our analysis.

For the rest my definition is arguably general enough in agreement with what we wanted it to be, but to ensure that it works properly we now have to check that we can recover the recognised empirical symmetries and their particular realisations and instances from it, which we expect to be possible indeed given that this definition was built by the generalisation on the basis of these empirical symmetries. To implement this check we will now be specialising our general definition to different extents and see what we get. These are not the specialisations supposed to modify our definition but just the choices as to how to apply it. For whenever a definition contains some generic element, we may replace it by a more specific element for the purpose of applying the definition to a particular case, and there are such generic elements in our definitions, such as systems, kinds of features and so on. So the idea is to gradually make such generic elements of our definition more special.

The primary specifications will be those that should allow us to obtain definitions of the recognised empirical symmetries. Arguably the first thing to specify for such a purpose are the constitutive features, because as their name implies these are the most important elements of a given empirical symmetry. We may next recall my claiming when analysing Healey's text that for macroscopic empirical symmetries it is more precisely the observable differences that are the most characteristic. To illustrate I was saying there that while the observable invariant features may be say mechanical or electromagnetic for either of Galileo's ship and Faraday's cage empirical symmetry, it is primarily mechanical observable differences (linked to boosts) that define the former and while electromagnetic observable differences (linked to charging) primarily define the latter. If this is right, specifying that we deal with macroscopic physical systems and specialising the constitutive differences in one or the other of the ways just mentioned should be enough to obtain from our generic definition the definitions of the two empirical symmetries in question. This does not mean that the observable invariant features become not constitutive in that case, but rather that they need not be specified if both empirical symmetries indeed preserve any kind of dynamics within their systems.

As to Einstein's elevator and 't Hooft's beam-splitter empirical symmetries, the constitutive differences should be specified in their case as well, but what should count as these is more controversial. The first choice may be to define them by making precise which combinations of observable indicators are to function as constitutive differences, and that is what I was doing so far. The problem with this is however that besides the observable indicators I was using there may in principle also be other observable indicators playing a similar role (e.g. an observable indicator of acceleration could be not only an engine turned on but also persons on a ship rowing). So although it is correct to say that as I define these empirical symmetries the combinations of the observable indicators I use already provide constitutive differences for them, it is also correct to say that these empirical symmetries could also be defined in a more general way than I do by admitting more kinds of observable indicators in. So if we wished to define these empirical symmetries somewhat more generally, we would better use not the specifications to precise observable indicators but something common to them. For Einstein's elevator empirical symmetry this is possible, because there are obvious candidates for common observable features of the relevant observable indicators, namely, as said above these candidates are accelerated movements. But this only functions when such common observable features also differ in the two physical states. So this only functions for the realisations of Einstein's elevator empirical symmetry that satisfy the first of the observable invariance conditions from those mentioned in my discussion of that empirical symmetry within the analysis of Brading and Brown's article, i.e. the condition by which only the observable dynamics within a system should stay invariant. And so this does not function for the realisations of Einstein's elevator empirical symmetry that satisfy the second or the third observable invariance conditions by which the character of movement of a system with respect to a reference should rather or also stay invariant (where the latter character of movement may consist in being accelerated, but it may also consist in being at rest or in a uniform rectilinear movement). Meanwhile, for 't Hooft's beam-splitter empirical symmetry as said I do not see any obvious candidate for common observable features to begin with.

Another question for the last two empirical symmetries is whether we should specify constitutive invariant features to define them. I was suggesting when analysing Healey's article that the observable differences are more characteristic than the observable invariant features for all the recognised macroscopic empirical symmetries, thus including Einstein's elevator empirical symmetry. This implies that we should not specify constitutive invariant features for the latter empirical symmetry similarly to how we need not do that for the other recognised macroscopic empirical symmetries. In fact this is correct when realisations of Einstein's elevator empirical symmetry satisfy the first of the observable invariance conditions, i.e. the condition by which only the observable dynamics within a system should stay invariant. For in that case the invariance in question seems to also range on any kind of dynamics (including mechanical and electromagnetic). Meanwhile, when realisations of Einstein's elevator empirical symmetry satisfy the second or third observable invariance condition by which the character of movement of a system with respect to a reference should rather or also stay invariant, constitutive invariant features will have to be specialised in that way in addition to specialising the constitutive differences. As to 't Hooft's beam-splitter empirical symmetry, its constitutive invariant features need to be specified as well because the invariant outcome of dynamics on my understanding of that empirical symmetry is not just any phenomenon but some interference pattern. Besides, arguably the beam-like character of a system should be specified as well.

To sum up, when starting from my definition of a generic empirical symmetry above we can obtain the definitions of Galileo's ship and Faraday's cage empirical symmetries by specifying that we deal with macroscopic physical systems, by making precise what the constitutive differences should be and by leaving the constitutive invariant features unspecified; the definition of some realisations of Einstein's elevator empirical symmetry in the same way (with the constitutive differences specified via the combinations of the relevant observable indicators or when possible via their common observable consequences); the definition of some other realisations of Einstein's elevator empirical symmetry by specifying the constitutive invariant features in addition; and the definition of 't Hooft's beam-splitter empirical symmetry by specifying that we deal with microscopic physical systems, by specifying the constitutive differences via the combinations of observable indicators (or in some more general way if we find it) and by making precise the constitutive invariant features together with the kind of system as well. These proposals are summarised in a table in the Appendix to the present work and the specifications just mentioned are denoted there by #.

Thus with such specifications we can already obtain some *specific empirical symmetries* (namely Galileo's ship, Faraday's cage and 't Hooft's beam-splitter empirical symmetries) as well as some *specific kinds of realisations of empirical symmetries* (in the case of Einstein's elevator empirical symmetry).

But these are not the only specifications we can make. Indeed, at this stage many general elements of my definition of a generic empirical symmetry above are still unspecified. This concerns not only some of the constitutive features as just mentioned but also for instance which kinds of physical systems are used and which kinds of physical things play the role of references. If we make *secondary specifications* by replacing all the general elements there are in my definition by more specific kinds of features, systems, references and so on, we get *a particular realisation of a specific empirical symmetry*. For instance, a particular realisation of Galileo's ship empirical symmetry can be obtained if we choose a ship as a system and a shore as a reference, take as the original ship that which exhibits no displacement with respect to the shore and as the other ship that which exhibits some displacement with respect to the shore, and choose the interior dynamics to consist of mechanical experiments. In the table in the Appendix to the present work such additional specifications are marked with *.

Finally, we can go even further and specify for all the elements not kinds but specific representatives of kinds. For example, in Galileo's ship case we can choose a specific ship, shore, pace of displacement and so on. If we make such *tertiary specifications* for all the things and features involved except spatiotemporal location, we obtain *an instance of a particular realisation of a specific empirical symmetry*. Indeed, traditionally *instances* are taken to only differ by spatiotemporal location. Thus they do not differ neither by any other (monadic or relational) property (unless those which may supervene on the difference in location) nor by the haecceity of the things involved (in metaphysical accounts with haecceity). Because of that instances provide the most specific level we can get to, for once the differences in spatiotemporal location are eliminated as well, there remains nothing by which to differ. Note that such differences can be observable if they are defined as differences in position with respect to an observable reference.

Note that the instances of empirical symmetries just discussed are each constituted by an instance of some physical state, an instance of a different physical state and possibly an instance of a physical transformation which brings about the difference between these states, where that difference is not (only) a difference in spatiotemporal location. This is not the same as considering something consisting of an instance of some physical state, another instance of the same physical state and possibly an instance of a physical transformation which brings about the difference between these instances, where that difference is a spatiotemporal difference alone. The latter is just *a trivial physical symmetry constituted by physical spatiotemporal translations (sometimes together* *with rotations)* which I was discussing when explaining the non-triviality check within my analysis of Brading and Brown's article, i.e. a physical symmetry that does not satisfy my non-triviality condition. However, this physical symmetry is not as useless as one could think, because as Healey rightly notes (although when discussing spatiotemporal translations without rotations) any empirical symmetry comprises such a physical symmetry in addition. Arguably, this happens because the constitutive differences of an empirical symmetry are incompatible physical features in the sense I described above, i.e. because of them being unable to ever be instantiated by the same system simultaneously. For this implies that any two such features can only be instantiated either non-simultaneously or by distinct systems, and distinct systems are usually supposed to be unable to occupy exactly the same spatiotemporal location. Hence the physical states of the kind defined by such features must always be instantiated at different times and/or at different places, and so an empirical symmetry constituted by such physical states will always comprise the trivial physical symmetry we are discussing in addition. By this reason I will call this physical symmetry as well as its spatiotemporal transformation and the spatiotemporal differences induced by it *concomitant* to a given empirical symmetry.

Section III. 1. 7. The equivalence of the two ways of realising empirical symmetries

It remains to discuss the status of physical transformations in empirical symmetries. We have two options concerning the identity of physical systems that should instantiate two physical states constituting an empirical symmetry: either the same system instantiates both states or distinct systems instantiate different states. I name this respectively *the first* and *the second way of realising empirical symmetries*. As we remember, the two ways were incoherently used in Kosso's article [2000, pp. 86, 87, 91] and then were treated more or less on a par by Brading and Brown [2004, p. 647, n. 5]. Still, their relationship has not been much analysed so far. If we do so, we notice that the difference in the system's identity involved in the two ways leads to a difference concerning the presence or absence of a physical transformation in these ways. For in the first way the same system has to be physically transformed from one of the states constituting an empirical symmetry to another of these states, while in the second way two systems, each of which is in a different state, are compared without any physical transformation being performed. The latter difference implies in turn a further difference. Namely, in the first way physical states are instantiated in the order determined by the physical transformation (so that we can call the state on which the transformation acts *initial* and the state resulting from the action of the transformation *final* with respect to the transformation). But in the second way there are no restrictions on the order of instantiation, and as to the order of comparison of the states, it can be chosen arbitrarily (although one of the states will still be *initial* and the other *final* with respect to the order of comparison chosen; this order is important in that it determines some relational properties, e.g. the same things can be linked by the relationship 'is bigger than' when compared in a given order and by the relationship 'is smaller than' when compared in the opposite order). Suppose now that it does not matter for the generic empirical symmetry and for specific empirical symmetries which of the two ways is used. Then such elements as the order of states and whether a physical transformation is actually present should not be included in the definitions of all these symmetries. The definitions of these empirical symmetries I gave above are then precisely of a suitable kind, for the order of states is not mentioned there while concerning the conservative physical transformation it is only required that it be possible and not that it necessarily occurs.

But why is it that the way of realising empirical symmetries should not matter?

One motivation is that for all the recognised empirical symmetries both ways of realising them seem in principle possible. Hence the definition of a given empirical symmetry should not decide as to which way is used. Such a decision will then correspond to a further specialisation of this definition. It will thus come after the primary specifications in our hierarchy of specifications above. And this decision may then have to be specified further, because we can also choose which physical transformation or which order of comparison of physical states we prefer. Indeed, while in the second way such a choice seems to always be possible, in the first way it is often available as well, for there often exists as much a physical transformation linking given states in some order as an inverse physical transformation linking them in the opposite order (e.g. we can as much boost a Galileo's ship standing still as bring a boosted Galileo's ship to a stop).

A second motivation is that if a way matters, this is for pragmatic reasons. For instance, 't Hooft's beam-splitter empirical symmetry relies in practice on the second way because no-one collects particles at the issue of the first run in order to make them pass through the two slits again, while Galileo's ship empirical symmetry relies in practice on the first way because the default state of ships is on the shore or in a harbour, thus at rest with respect to the shore, and so to get the second way we actually need to physically boost one of the ships, thereby using the first way. Therefore, we should not privilege one way over another in unpragmatic accounts of specific empirical symmetries and in any case in the formulation of the notion of generic empirical symmetry which should be applicable in particular to both empirical symmetries just mentioned.

For a third more speculative motivation, suppose that we are convinced on the one hand that spatial and temporal differences should be treated in the same way, and on the other hand that they determine whether systems instantiating physical states are distinct or identical. For instance, if we adopt Kosso's idea of using theoretical assumptions (given that we do not speak here of establishing empirical symmetries but of realising them), we may find the justification of both claims just mentioned in some kind of classical relativistic metaphysics. The latter claim would be supported by it in particular because two states there will necessarily belong to distinct systems whenever these states are instantiated in different spatial locations at the same time (whenever there is a coordinate system in which these states are instantiated simultaneously) and because for two different states to belong to the same system they necessarily must be instantiated at different times (in any coordinate system). Then, as the difference in spatial location and the difference in time must be treated as equivalent, the facts for two states to belong to distinct systems or to the same system must be treated as equivalent.

Finally, a fourth simple motivation is that distinct physical systems can instantiate an empirical symmetry via the first way, i.e. undergo a physical transformation of their physical state without changing their identity (unless when the constitutive differences are essential for physical systems, i.e. determine their identity, but the constitutive differences of all the recognised empirical symmetries are usually taken not to be of that kind). But then the identity of physical systems is irrelevant to instantiating any of the physical states constituting an empirical symmetry, and thus the second way seems as much acceptable as the first.

In short, at least by all these motivations we should treat the two ways as equivalent when analysing generic and specific empirical symmetries, and hence take the order of states and the occurrence of an actual physical transformation inessential elements of empirical symmetries. This will lead to important consequences as to what has DES in my framework.

Chapter III. 2. The matching relationships

Section III. 2. 1. Ladyman and Presnell's kinds of DES versus my kinds of DES

We have already discussed my kinds of DES to some extent particularly when I was analysing Healey's article above, but I would like to say some more words on them now. To begin with, for Healey's article mattered the definitions for the empirical approach, but I also have mirror definitions for the theoretical approach, so now I give all of them at once in a precise form. For me a theoretical symmetry has *the observational DES in the empirical approach* if its observational consequences are able to adequately represent the observable constitutive features of an identifiable (i.e. observable) empirical symmetry, and it has *the ontological DES in the empirical approach* if it has the observational DES in the empirical approach and moreover the theoretical underpinnings of the former observational consequences are able to adequately represent the unobservable physical underpinnings of the latter observable constitutive features. Likewise, for me a theoretical symmetry has *the observational DES in the theoretical approach* if its observational consequences are instantiated by the observable constitutive features of an identifiable (i.e. observable) empirical symmetry, and it has *the ontological DES in the theoretical approach* if it has the observational DES in the theoretical approach and moreover the theoretical underpinnings of the former observational consequences are instantiated by the unobservable physical underpinnings of the latter observable constitutive features.

Here the difference for the two approaches is essentially just in whether the notion of adequate representation or instantiation is used. These notions are chosen firstly so that the matching relationships go in the direction relevant to the approach in which a DES is defined (for to construct a representation we begin with the physical level like in the empirical approach, while to seek an instantiation we begin with the theoretical (or at least abstract) level like in the theoretical approach). Besides, the aim of these notions is to accommodate some views on DES that we find in the main literature on DES, and indeed they do so particularly for the views appealing to the notion of representation (like Brading and Brown's, as well as Healey's and even Greaves and Wallace's [2014, p. 64]).

When analysing Healey's article above I was explaining why my two notions for the empirical approach are better than his two, and this was primarily because mine were stronger and concerned kinds of features that are better defined and arguably more relevant. The same kind of comparison is less natural for the other articles from the main literature on DES because the notions of DES there are more vague and besides the other authors usually have just one main notion of DES per article. Even the follow-up literature on DES does not focus on the discussion of kinds of DES. An exception is Ladyman and Presnell's manuscript [2016/01/08] where we do find two kinds of DES. So besides Healey's it is with their position that mine should be compared at the first place.

There is at least one way in which Ladyman and Presnell's treatment of DES is more satisfactory than that of Healey. This is simply because even Healey did not really introduce two explicit kinds of DES but just two ways of representing empirical symmetries with the help of theoretical symmetries that I promoted into kinds of DES. Ladyman and Presnell by contrast do introduce two kinds of DES explicitly and are to my knowledge the first to do so.

On the other hand, their kinds of DES are still unsatisfactory in many respects. To see this it suffices to look at their definitions. For Ladyman and Presnell a theoretical symmetry has “logical consequences” when it gives rise to predictions corresponding to observable features of an empirical symmetry, and it has “physical consequences” when it gives us reasons to believe that there exists a physical symmetry in the sense of a symmetry in the world. Here the former notion is what *I call Ladyman and Presnell's weaker DES* and the latter is what *I call their stronger DES*. That these are kinds of DES is rather clear because the mention of empirical consequences of theoretical symmetries became synonymous with DES in later literature on DES particularly after Greaves and Wallace put this expression in the title of their article. It is also clear from Ladyman and Presnell's manuscript that the latter DES is supposed to be a strengthening of the former. On the other hand, they do not present their kinds of DES as an instance of a weaker DES and an instance of a stronger DES respectively: to my knowledge there was no distinction between weaker and stronger kinds of DES in general before my own work.

Why are Ladyman and Presnell's specific kinds of DES unsatisfactory? For one thing, Ladyman and Presnell's distinction seems to only take into account the theoretical approach (where one goes from the theoretical level to the physical level) but not the empirical approach (where one goes in the opposite direction). Also, it is doubtful that the matching between a theoretical symmetry and an empirical symmetry is of logical nature, while this is what the term 'consequences' suggest. Moreover, the difference between the two kinds of consequences is not spelled out (in particular, it is not explained why a theoretical symmetry could not give us reasons to believe that there exists a physical symmetry by predicting its observable features). Besides, the stronger DES does not seem strong because of being formulated in terms of beliefs. By contrast, arguably none of these drawbacks are present in my notions of DES: these are defined for either approach; they involve more precise and more appropriate notions of representation and instantiation; the strengthening by which the ontological DES differs from the observational DES is clearly described; and it is based more satisfactorily on the requirement to account for more physical features or theoretical elements rather than on beliefs. In addition, through the mention of constitutive features the definitions of my kinds of DES take into account the generic notion of empirical symmetry defined above, while Ladyman and Presnell's account of empirical symmetries is less elaborated. So my kinds of DES are more adequate with respect to the empirical symmetries they concern. Furthermore, as already mentioned Ladyman and Presnell follow Healey in completely overlooking the importance of observable differences of empirical symmetries. So while my kinds of DES are identifiable (for the observational DES) or at least partially identifiable and partially possibly identifiable (for the ontological DES), for either of Ladyman and Presnell's kinds of DES this is not necessarily the case.

Ladyman and Presnell also claim that it is their specific kinds of DES that accommodate the views on DES from the main literature on DES. More precisely, they claim that what I call their stronger DES is that of Kosso, Brading and Brown as well as Healey, while what I call their weaker DES is that of Greaves and Wallace, and say that Wallace has confirmed this interpretation for Greaves's and his own [2014] article. It is however implausible that Kosso's, Brading and Brown's as well as Healey's kinds of DES can all be placed into the same category. For Kosso used the theoretical approach while the other authors just mentioned used the empirical approach; Healey had two kinds of DES rather than one; and by the time of Kosso and even Brading and Brown there was no question of holding a stronger DES because firstly the reflection on DES was not yet very elaborated and secondly in general only one kind of theoretical symmetry was thought to be matched with a relevant physical symmetry and hence there was no point in strengthening a DES. As to Greaves and Wallace, a DES from their article is apparently not very strong, and perhaps it is even something like Ladyman and Presnell's weaker DES indeed. But I doubt that it is precisely like Ladyman and Presnell's weaker DES, because there does not seem to be any precise kind of DES in Greaves and Wallace's article to begin with. In particular, as said at some moment they speak of the matching relationship in representational terms [ibid., p. 64], at another they speak of it as an explanation [ibid., 62] and so on. So Ladyman and Presnell's notions do not seem to capture much of the earlier views on DES, and perhaps one actually need not or even will not ever be able to capture all these views because of them being too vague.

It should be said that Ladyman and Presnell's pretension to do the latter was motivated by their idea to oppose Greaves and Wallace to the remaining main literature on DES. This is because Ladyman and Presnell wanted to claim that while both global and local theoretical symmetries can have a weaker DES, only global theoretical symmetries can have a stronger DES. As Greaves and Wallace were assigning some DES to both global and local symmetries and as the other main literature on DES was reserving some DES to global symmetries, the claim just mentioned could be supported indeed if Ladyman and Presnell's pretension was fulfilled. But one could also make the claim Ladyman and Presnell wanted to make without fulfilling it. And on the other hand, even if Ladyman and Presnell's pretension was fulfilled, it would not make their claim right. In fact as I will argue below there are no reasons to hold this claim to be correct.

Section III. 2. 2. The theoretical approach versus the empirical approach

Even if we accept my kinds of DES, we still have to choose whether to proceed via the theoretical or the empirical approach. The choice may matter because the results on the two roads may differ or it may be harder to obtain them on one road as compared to the other. My own view is that for establishing DES the empirical approach is better than the theoretical approach. Let us see why.

As we know, in the theoretical approach to DES one starts with a theoretical symmetry and seeks an empirical symmetry that matches with it. So to get a DES one needs firstly that there exists something that matches with a theoretical symmetry at all, and secondly that this something be an empirical symmetry. In our context of exploring the ontology of our world (as opposed to Healey's talk of all possible worlds) both conditions must be satisfied in our world. But neither is guaranteed. For it may well happen that there is nothing in our world that matches with a given theoretical symmetry, or that for instance only what is invariant under a theoretical symmetry transformation has a counterpart in our world, so that this counterpart itself is not an empirical symmetry. So we are not guaranteed to get any DES in the theoretical approach.

But not so in the empirical approach. For there one starts with an empirical symmetry and seeks a theoretical symmetry corresponding to it. And so to get a DES one needs firstly that there exists something theoretical that matches with an empirical symmetry at all, and secondly that this something be a theoretical symmetry. The first condition here is easier to satisfy, because arguably we always can construct something theoretical corresponding to a given phenomenon. Moreover, it happens to be possible to always satisfy the second condition as well. To get this let us say first of all that *a theoretical thing matches with an empirical symmetry in the empirical approach* if the former is able to adequately represent the latter. This is the representation-based notion of matching used in particular in my definitions of the observational DES and the ontological DES in the empirical approach. Let us add next a reasonable assumption by which for a theoretical thing to be able to adequately represent an empirical symmetry the former should have a *minimal structural similarity* to the latter. Namely, as an empirical symmetry is constituted by two partially identical states and a transformation between them, a theoretical thing able to adequately represent it should likewise be constituted by two partially identical states and a transformation between them. If this is granted, then whenever we have a theoretical thing matching with an empirical symmetry in the empirical approach, that theoretical thing will be *a theoretical symmetry* in the sense of a theoretical construction constituted by two partially identical theoretical states linked by a theoretical transformation. Hence we are now guaranteed to get a DES, i.e. a matching between a theoretical symmetry and an empirical symmetry.

Why could not this last move work in the theoretical approach? To see this let us try to run it there. So we say first of all that *a physical thing matches with a theoretical symmetry in the theoretical approach* if the former adequately instantiates the latter. This is the instantiation-based notion of matching used in particular in my definitions of the observational DES and the ontological DES in the theoretical approach. We add to this *a suitably modified assumption of minimal structural similarity* by which a physical thing adequately instantiating a theoretical symmetry should be constituted by two partially identical states and a transformation between them. We even suppose for the sake of the argument that such a physical thing exist. Do we get a DES then? No, because this physical thing is not necessarily an empirical symmetry – rather, it is a physical symmetry instead. And that is the point. An empirical symmetry is something very much richer than a physical symmetry, as explained at length in the previous chapter. But a theoretical symmetry is not. So to get a theoretical symmetry by the move in question we could start with an empirical symmetry or a physical symmetry alike. But to get an empirical symmetry by that move we would need to start with a much richer notion of theoretical symmetry than usual. But we cannot know what this much richer notion has to be, unless perhaps we find out in advance which theoretical symmetries are able to match with empirical symmetries. But this just amounts to recurring to the empirical approach. Why then not begin with the empirical approach directly? That is exactly what I propose to do.

To illustrate, recall how I was matching gravitational and acceleration theoretical transformations with Einstein's elevator empirical symmetry when analysing Brading and Brown's article above. There the task was precisely to find an empirical symmetry matching with certain theoretical transformations, thus to proceed via the theoretical approach. More precisely, we wanted to match our theoretical transformations with the change in the unobservable constitutive differences of an empirical symmetry such that its unobservable constitutive differences have some observable indicators, which arguably amounts to establishing the ontological DES in the theoretical approach for theoretical symmetries constituted by our theoretical transformations. I went as far as saying what the putative observable indicators of a suitable unobservable empirical symmetry would be, but while the change in some phenomena corresponding to the change in such putative observable indicators existed indeed, there was no certitude that we deal with observable indicators indeed because there was no certitude that the underlying unobservable empirical symmetry exists. So I proposed to interpret these phenomena not as putative observable indicators of some unobservable empirical symmetry, but as observable features on their own, and it then happened that they are compatible with the invariance of some other observable features and so function as constitutive differences of an observable empirical symmetry. I next argued that, because the change in these phenomena is a change in the putative observable indicators of constitutive differences of an unobservable empirical symmetry which would have to be matched with our theoretical transformations, these theoretical transformations should be able to adequately represent the change in the phenomena in question. In other words, our theoretical transformations should be able to adequately represent the change in the constitutive differences of our observable empirical symmetry. The conclusion was that provided the same theoretical transformations also preserve the observational consequences able to adequately represent the constitutive invariant features of the same observable empirical symmetry these theoretical transformations can be said to lead to DES on the basis of this observable empirical symmetry. Therefore, if the observational consequences of the theoretical states of theoretical symmetries constituted by such theoretical transformations are able to adequately represent the constitutive differences and the constitutive invariant features of the same observable empirical symmetry (which implies in particular that these theoretical symmetries are observationally incomplete), such theoretical symmetries can be said to have DES on the basis of this observable empirical symmetry.

But firstly, that we have thus got a matching of such theoretical symmetries with an observable empirical symmetry is actually a failure of establishing the ontological DES in the theoretical approach, because what we have established is only the observational DES. Secondly, ultimately this is perhaps the observational DES in the theoretical approach as well, but originally this is the observational DES in the empirical approach, because it is established on the grounds that our theoretical symmetries are able to adequately represent some observable empirical symmetry. Thirdly, this DES only holds provided observational consequences of an observationally incomplete theoretical symmetry in form some observable invariance, some putative observable indicators and the change in them compatible with that invariance are instantiated in our world, but this is not guaranteed in the theoretical approach. And fourthly, from the perspective of the theoretical approach we could as well try to establish a DES of observationally complete theoretical symmetries, but as on the one hand these would be best instantiated by observationally complete empirical symmetries, and on the other hand I restricted empirical symmetries to observationally incomplete empirical symmetries by reasons of identifiability, such theoretical symmetries will be unable to have any DES and will instead be only able to have a matching with a physical symmetry (if a suitable physical symmetry happens to exist). This illustrates in agreement with my general discussion above that the theoretical approach requires the existence of something in the world that is not guaranteed to obtain; that it may fail to lead to any DES; and that for its leading to a DES we should pass by the empirical approach, which suggests that one better starts from the empirical approach directly.

Chapter III. 3. Theoretical symmetries with the observational DES in the empirical approach

The empirical approach not only guarantees us a DES, but also allows us to replace not sufficiently elaborated and unsatisfactorily motivated suggestions from the literature on DES as to which theoretical symmetries have DES by a justified and detailed account of how a generic theoretical symmetry with the observational DES looks like, as I will now show.

Section III. 3. 1. A strengthened observational DES in the empirical approach

To begin with, consider what happens if we repeat the move above using the empirical approach, but instead of asking what it takes to adequately represent an empirical symmetry ask the same question of an observable empirical symmetry more precisely, as is needed to obtain the observational DES. As we deal again with a kind of physical symmetry, by the assumption of minimal structural similarity we get again a *constraint* by which the adequately representing theoretical thing should be a theoretical symmetry. However, because of dealing with an observable empirical symmetry more precisely we now have an additional detail, namely that there are observable constitutive features, of which we know moreover that these features belong to physical states. Hence the assumption of minimal structural similarity implies *an additional constraint* on theoretical states, namely that they have theoretical elements that either are observational (in the sense of describing something observable) or induce observational consequences in a way allowing to represent these observable constitutive features. Now arguably an adequate representation of observably identical features includes representing them by identical observational elements (consequences), and an adequate representation of observably different features includes representing them by differing observational elements (consequences). Hence *the relevant theoretical symmetry has to be constituted by observationally inequivalent states, and so is not a gauge symmetry in the sense of observationally complete theoretical symmetry*. This agrees with my conclusion from the first chapter of Part III where a similar (in the premiss concerning an adequate representation of observable constitutive differences) but not identical reasoning was used to arrive at the conclusion that theoretical symmetries with the observational DES are non-gauge in the sense just mentioned. *Thus the empirical approach coupled to the reasonable assumption of minimal structural similarity and to some understanding of what counts as an adequate representation allows us to get as much as the observational DES of non-gauge symmetries in the sense of theoretical symmetries that are not observationally complete, while from the theoretical approach we were not sure to get any DES at all.*

More can be obtained from the empirical approach if an extra restriction is made. The need for it comes from the fact that the constraints we have just deduced are rather weak. In particular, the last constraint of having suitable observational elements (consequences) can be satisfied by a theoretical symmetry coming from physics, but also, perhaps, by a theoretical symmetry constituted by simple verbal descriptions of the constitutive features of an observable empirical symmetry. However, here we are interested specifically in theoretical symmetries from physics, and so we need an additional requirement to single them out from the class of all the theoretical symmetries with the observational DES obtained via the empirical approach. One may try to ensure such a restriction by requiring that we only leave those theoretical symmetries that provide the most adequate representations of observable empirical symmetries and by arguing that it is only theoretical symmetries from physics that satisfy the latter requirement. But here I will take a simpler but arguably also admissible option and will introduce the desired a restriction on pragmatic grounds. Thus I will now strengthen the observational DES in the empirical approach into *the observational DES in the empirical approach involving theoretical symmetries from physics* by simply requiring on pragmatic grounds that the constraints above that we have deduced from the empirical approach, the assumption of minimal structural similarity, the fact that we deal with observable empirical symmetries and the notion of adequate representation be satisfied in the way this is usually done in physics.

To describe a bit more precisely what this way consists in, it is necessary to use some knowledge of physics, so this becomes a theoretical approach in a sense, but not at all in the sense of the theoretical approach that I have been opposing to the empirical approach above. We thus ask what is the usual way in physics of adequately representing the constituents of an observable empirical symmetry, namely the physical states which contain the observable constitutive invariant features and the observable constitutive differences, as well as the possible physical transformation between these states. Answering this question turns out to be sufficient to get a detailed description of what a theoretical symmetry from physics with the observational DES looks like in the empirical approach.

Section III. 3. 2. Representing a physical state of an observable empirical symmetry

We will concentrate first on what it takes to represent the observable constitutive features of one of the physical states constituting an observable empirical symmetry.

General knowledge of physics tells us that first of all one has to introduce variables, associate to them ranges of values, formulate the rules for assigning a value, or a collection of values, of a variable (or of several variables) to a physical system, and specify *observational consequences* in form of predictions (whether trivial or not) as to what it implies, observationally, to represent a physical system as characterised by a given value, or a given collection of values, of a variable (or of several variables). Ascriptions of values of the relevant variables then constitute the *theoretical underpinnings* of these observational consequences. To represent the observable constitutive features we next have to ascribe values of variables to the system or its parts in such a way that their observational consequences adequately represent the features in question.

Usually ascriptions of values are organised in *momentary theoretical states*, i.e. collections (or distributions over a space) of values indexed by the same value of a time variable. Momentary theoretical states are in turn usually organised in a *sequence*, i.e. ordered by monotonically increasing values of the time variable, into *a(n extended (in time)) theoretical state*. This should however be *a restricted (in time) sequence* because it only has to contain momentary theoretical states indexed by the values of the time variable corresponding to the period of time during which the physical state of a system is defined. Usually this is itself a restricted period of time, especially if one uses the first way of realising empirical symmetries and so has to transform one physical state of a system into another.

Next, the representation of features differs according to whether they are *observably dynamical* or *observably static*. Here it is not meant whether a feature pertains to a dynamics but whether it comprises an evolution of observable features. In the former sense both the constitutive invariant features and the constitutive differences are often dynamical because they involve a dynamics, but this does not mean that they are necessarily dynamical in the latter sense. Rather, it is the case that either kind of features may involve observably dynamical and observably static component features. For example, the identical observable dynamics within a Galileo's ship mostly consists of observably dynamical features, but it also comprises relevant observably static features such as e.g. shapes and kinds of objects. Meanwhile, in 't Hooft's beam-splitter empirical symmetry the identical interference patterns are observably static in that once an interference pattern is formed it remains as it is, and besides there are other observably static features such as the distances between the source of the beam, the two slits, and the screen where the interference pattern is formed which contribute in determining which pattern it going to be produced; but the formation of an interference pattern is observably dynamical, and we need to account for this too. As to the observable constitutive differences, at least in the recognised macroscopic empirical symmetries they have an observably dynamical component. For instance in Galileo's ship empirical symmetry it consists in that a ship changes its position with respect to the shore as time goes on. And even in 't Hooft's beam-splitter empirical symmetry the functioning of a switched-on solenoid is dynamical, although whether this is the case of half-wave plates as well is unclear (unless in so far as particles are supposed to pass through a half-wave plate at different times). Besides, there is in any case an observably static component coming from the relevance condition above by which a constitutive difference should persist throughout the time during which an observable constitutive invariant feature and/or a dynamics leading to it unfolds. In Galileo's ship empirical symmetry this component consists in that the pace with which a ship moves with respect to the shore should be the same as long as we perform experiments inside (until the ship is brought into another physical state due to being physically boosted in the first way). In 't Hooft's beam-splitter empirical symmetry the constitutive differences on my account are the presence or absence of half-wave plates or switched-on solenoids, and these facts are likewise observably static. Thus even if we only represent the observable constitutive features we need to represent observably dynamical and observably static features alike.

The difference in the representation of observably dynamical and observably static features is that observably dynamical features have to be represented by evolving theoretical underpinnings whose observational consequences evolve, while observably static features can be represented either by static theoretical underpinnings whose observational consequences are static, or by evolving theoretical underpinnings whose observational consequences are static. Here theoretical underpinnings and observational consequences are said to be *evolving* or *static* according to whether they differ or not for different values of the time variable within the relevant restricted range. The change or absence of change with time of values of the other variables across an extended theoretical state can be expressed respectively by a non-trivial or a trivial *dynamical theoretical transformation* that passes from the initial momentary theoretical state of an extended theoretical state through all the intermediate momentary theoretical states of that extended theoretical state to the final momentary theoretical state of the same extended theoretical state.

A sequence of momentary theoretical states with evolving theoretical underpinnings can be *generated* from an initial momentary theoretical state and a final momentary theoretical state by extremising the action in between these momentary theoretical states or from a single momentary theoretical state using differential equations of motion (in the latter case the equations specify the dynamical theoretical transformation from a given momentary theoretical state to the momentary theoretical state indexed by the next value of the time variable). It is possible that a sequence generated in such ways cannot represent all the observable features we need to account for, in particular if accounting for different observable features requires using different means of generating sequences. For instance, the observable dynamics relevant to the constitutive differences and the observable dynamics relevant to the constitutive invariant features may require using different equations or actions to generate sequences accounting for them due to these dynamics involving different kinds of phenomena. Likewise to generate sequences with evolving theoretical underpinnings suitable for representing observably static features one may need other equations than those which may be used to generate sequences with evolving theoretical underpinnings suitable for representing observably dynamical features. In such cases one needs to combine separate representations of various observable features to get a representation of all the features to be accounted for. Often it is possible to combine these separate representations so that they form a single compiled sequence of momentary theoretical states, i.e. a single extended theoretical state.

Thus in physics *a theoretical state capable of adequately representing the observable constitutive features of one of the physical states constituting an empirical symmetry* is typically an extended in time theoretical state that consists of a compiled restricted in time sequence of momentary theoretical states such that its theoretical underpinnings and observational consequences accounting for the observably dynamical features evolve, while its observational consequences accounting for the observably static features are static whether their theoretical underpinnings are static or evolve, where theoretical underpinnings typically consist of ascriptions of values of variables to a system or its parts, and are typically generated using equations of motion or actions.

Section III. 3. 3. The proliferation of representations of physical states

We know from how physics works that *the way of representing any given observable feature is usually not unique*, and this also holds in our context. For instance, to represent a constitutive difference of Galileo's ship empirical symmetry in Newtonian or Galilean mechanics or special relativity one has a choice among an infinity of observationally equivalent different assignments of velocity values to the ship and the shore. Meanwhile, to represent a constitutive difference of Faraday's cage empirical symmetry one has a choice not only between an infinity of values but also between different variables such as various potential and field strength variables as explained in my analysis of Healey's article above.

More generally, representing observable features of an extended physical state requires an extended theoretical state with suitable observational consequences, and there usually are plenty of theoretical underpinnings that allow to obtain such observational consequences. If the theoretical underpinnings are evolving, the non-uniqueness may arise because one starts with different observationally equivalent momentary theoretical states while using the same deterministic equations or with different couples of observationally equivalent momentary theoretical states while using the same action, but it may also arise if one starts with the same momentary theoretical state or the same couple of momentary theoretical states but uses different equations or actions, or when indeterministic equations are used. And as the constitutive features of one of the physical states constituting an observable empirical symmetry are just a particular case of observable features of an extended physical state, *there is going to be a proliferation of theoretical states capable of representing the constitutive features of a physical state constituting an observable empirical symmetry because there are usually many theoretical underpinnings that can lead to the same observational consequences.*

A theoretical state has to provide a representation of all the observable constitutive features of a relevant physical state, and for this it suffices to provide one representation per feature. Therefore, *the quantity of different theoretical states per physical state can be estimated to be equal to the number of ways in which all the representations of all the features to be accounted for can be combined so that one representation per feature is provided and so that all the features to be accounted for are represented.* Given that there are many features to represent and given that the representation of each feature is usually non-unique as just explained, the number of different theoretical states per physical state can thus be extremely large.

We can now use the same procedure to obtain theoretical states capable of representing the observable constitutive features of the other of the physical states constituting an observable empirical symmetry. The difference with the representations of the first of the physical states will necessarily concern the representation of the observable constitutive differences and will consist in the difference in the relevant observational consequences together with a difference in their theoretical underpinnings, given the principle by which different observational consequences cannot come from the same theoretical underpinnings (which also holds for probabilistic theories).

However, theoretical states representing the two physical states may additionally differ in the representation of the observable constitutive invariant features, because we need not and sometimes cannot make the same representational choices. For instance, to predict the same interference pattern in the setup of 't Hooft's beam-splitter empirical symmetry where one of the physical states features a switched-on solenoid and the other a half-wave plate it may be more appropriate or even necessary to use a theory with the electromagnetic interaction for the first state and a theory without the electromagnetic interaction for the second state. Still, such dissimilarities do not preclude a couple of extended theoretical states representing each a distinct one of the two physical states constituting an observable empirical symmetry from constituting a theoretical symmetry. For *the observational consequences which serve to represent the observable constitutive invariant features need to be the same in the two theoretical states* even when the theoretical underpinnings of these consequences are different, *and so the two theoretical states will anyway be partially identical as is required for them to constitute a theoretical symmetry.*

By now we have not discussed transformations yet, but it is already clear that there is going to be *a proliferation of matchings of theoretical symmetries with an observable empirical symmetry* due to a proliferation of matchings of theoretical states with a physical state and due to the possibility of making different representational choices for a couple of theoretical states representing a couple of physical states (even though sometimes this possibility is going to be limited as just explained). In fact, at least *the representation of all the recognised empirical symmetries is subject to a radical non-uniqueness even if one concentrates on a single instance of a particular realisation of a specific empirical symmetry and keeps conventions fixed.*

This one-to-many relationship between an empirical symmetry and theoretical symmetries does not show a weakness of the empirical approach but rather *shows a weakness, i.e. permissiveness, of the observational DES* as a means to match empirical symmetries with theoretical symmetries. The latter weakness comes from the non-uniqueness of theoretical underpinnings giving rise to suitable observational consequences as well as from the fact that the observational DES by contrast to the ontological DES only imposes requirements on the observational consequences and not on the theoretical underpinnings. In the theoretical approach this weakness can be overlooked, because there one seeks for any empirical symmetries matching with a given theoretical symmetry, and if all such empirical symmetries are found, the enquiry is over. But this does not make the one-to-many relationship between an empirical symmetry and theoretical symmetries disappear, for if one was to start with another theoretical symmetry, one could have found that it can be matched with the same empirical symmetry. However, it does hide this relationship, because one is not motivated to start with another theoretical symmetry if one is only interested in a given theoretical symmetry. Meanwhile, in the empirical approach one seeks for any theoretical symmetries matching with a given empirical symmetry, and that is how *the empirical approach makes the one-to-many relationship between an empirical symmetry and theoretical symmetries apparent.*

Section III. 3. 4. Representing the possible physical transformation of an observable empirical symmetry

Concentrating now on transformations, a physical transformation in the first way of realising empirical symmetries (where the same system is physically transformed) is most adequately represented by a theoretical transformation. The form of the theoretical transformation is determined in particular by what from the physical transformation it has to represent. Usually it is not required to represent how the physical transformation is being performed. For example, we do not usually represent whether oars or an engine were used to physically boost a Galileo's ship and how much time it took to bring it into a boosted state. Moreover, it is possible that we cannot represent in physics such aspects of the physical transformation as how one has decided to boost a ship. Thus what has to be represented by the theoretical transformation is rather the net effect of the physical transformation. Therefore, this theoretical transformation should not pass through intermediate theoretical states, by contrast to the dynamical theoretical transformation within an extended theoretical state defined above that leads from an initial momentary theoretical state to a final momentary theoretical state through all the intermediate momentary theoretical states. By that reason I will call the theoretical transformation we are now discussing *the main theoretical transformation* to distinguish it from dynamical theoretical transformations within extended theoretical states. More precisely, the net effect of the physical transformation consists in passing from one to the other of the physical states constituting an empirical symmetry. Thus the main theoretical transformation representing this should send an extended theoretical state which represents the physical state initial with respect to the physical transformation to an extended theoretical state which represents the physical state final with respect to the physical transformation. This means in particular that if we call a theoretical state *initial* or *final* according to whether it represents an initial or a final physical state, a theoretical state initial in that sense will also have to be initial with respect to the main theoretical transformation, i.e. the one it acts upon, and a theoretical state final in that sense will also have to be final with respect to the main theoretical transformation, i.e. resulting from its action. This also means that the main theoretical transformation should send all the differing theoretical elements of the initial theoretical state to the corresponding theoretical elements of the final theoretical state, whether these theoretical elements contribute in the representation of the observable constitutive differences or of the observable constitutive invariant features. Besides, corresponding to the fact, mentioned when discussing the formalisation of the notion of empirical symmetry above, by which physical transformations in the first way are often invertible the main theoretical transformations may likewise have the property of being invertible.

In the second way of realising empirical symmetries (where two systems in different physical states are compared) there is no physical transformation of the kind there was in the first way. Instead there is a weaker link between the physical states consisting in how one of the states compares to the other. However, on my view this does not necessarily lead to any difference at the theoretical level in comparison with the first way. More precisely, I wish to argue that the link between the physical states in the second way can be adequately represented by the main theoretical transformation which I have just introduced to represent the physical transformation in the first way. Indeed, this theoretical transformation is able to represent the link between the two physical states because it links the two theoretical states representing these physical states. The link consists in comparing physical states in a given order, and the main theoretical transformation also induces an order of theoretical states because of being directed. Comparing physical states in a given order or in an opposite order is possible, and likewise it may be possible for a main theoretical transformation to have an inverse. In the second way no intermediate physical states are considered, and similarly a main theoretical transformation does not pass through intermediate theoretical states. Perhaps a main theoretical transformation leaves something unrepresented from how we compare states, but arguably this loss of detail is not greater than the loss involved, in the first way, in representing the net effect of a physical transformation instead of representing a particular physical transformation with our reasons for performing it, the process of implementing it and so on. So if the latter loss of detail is acceptable, a fortiori the former should be acceptable as well.

Thus the main theoretical transformation should be able to adequately represent as much the physical transformation in the first way as the link between the physical states in the second way. This allows to treat the two ways as equivalent at the theoretical level, which is consistent with my discussion of the formalisation of the notion of empirical symmetry above where it was argued that the two ways must be treated as equivalent also at the empirical level. Given that both ways thus admit using theoretical transformations on my account, both ways give rise to the usual kind of theoretical symmetry and to the usual kind of DES.

By contrast, if a representation of an empirical symmetry in the second way could not feature a theoretical transformation, that representation would not be *a theoretical symmetry* in the usual sense of a theoretical construction consisting of two theoretical states and of a theoretical transformation between them, and hence it could not *have DES* in the usual sense of being a theoretical symmetry matched with an empirical symmetry. So we would have either to restrict my defence of the empirical approach from the previous chapter to the first way, or, for the second way or for both, to weaken the definitions of theoretical symmetry and/or of DES to admit DES of theoretical constructions that do not feature theoretical transformations.

However, as the link between the two physical states in the second way can be adequately represented by the main theoretical transformation as just shown, it follows that in the second way it is possible to adequately represent an empirical symmetry by a theoretical symmetry in the usual sense and therefore to have DES in the usual sense. Thus the defence of the empirical approach above goes through for the second way as much as for the first way without weakening the definitions. To highlight this extended validity of the defence we can even reformulate its premiss by which an empirical symmetry has a physical transformation as a premiss by which an empirical symmetry has a conservative physical transformation in the first way or the link between the physical states in the second way. The defence also seems to hold if one replaces the two alternative relationships between the physical states in the two ways by their common basis (e.g. a modal relationship). For if the main transformation can adequately represent the kinds of relationships involved in the two ways, arguably it can adequately represent their common basis as well.

Summing up, we have that in physics *a theoretical symmetry capable of adequately representing the constitutive features of an observable empirical symmetry* instantiated via any of the two ways typically consists of two theoretical states as described above, as well as a main theoretical transformation linking these theoretical states which brings about, without passing by intermediate theoretical states, all the differences in the theoretical underpinnings and observational consequences there are between these two theoretical states. *It is therefore theoretical symmetries of this kind which get the observational DES in the empirical approach.*

Section III. 3. 5. Consequences for the literature on DES

The description of theoretical symmetries with observational DES in the empirical approach just obtained receives a clear justification through my defence of the empirical approach and my formalisation of the notion of empirical symmetry. Besides, it is much richer than all the previous descriptions of theoretical symmetries with DES that we have seen above. But in addition, it shows that when compared with the theoretical symmetries having the DES we are discussing these descriptions are mistaken in crucial respects.

Namely, firstly *contrary to some of the literature on DES such as Kosso's as well as Brading and Brown's articles theoretical symmetries with the observational DES are not symmetries of laws, at all or in general*, at least if by 'laws' one means equations of motion or actions. Indeed, for theoretical states of a theoretical symmetry to represent the observable constitutive features they only need to include sequences of momentary theoretical states that can be generated by equations of motion or actions, not these equations of motion or actions themselves. And even if we were to include equations of motion or actions themselves in the theoretical states of a theoretical symmetry with the observational DES, these theoretical states would not necessarily feature the same equations of motion or actions, because it is possible and sometimes desirable or even necessary (as in the example with a setup of 't Hooft's beam-splitter empirical symmetry given above) to use different equations of motion or actions to represent the observable constitutive features of the two physical states constituting an empirical symmetry.

And secondly, *contrary to all the literature on DES the theoretical transformation linking the theoretical states actually does not play a significant role in ensuring the observational DES* *(and hence probably the ontological DES) in comparison with the theoretical states themselves*. For this theoretical transformation cannot represent observably static constitutive features because it should bring about a change in observational consequences to yield an observationally incomplete theoretical symmetry; cannot represent neither those kinds of observably dynamical features which are only involved in the constitutive invariant features nor the intermediate observable physical states of observably dynamical features; represents very little of a conservative physical transformation in the first way; and in the second way represents something much weaker than an actual physical transformation. By contrast, theoretical states are able to represent observably static constitutive features; can represent observably dynamical ones more fully than a theoretical transformation could do; and are even able to represent the result of applying the conservative physical transformation in the first way, as well as the physical basis of the link between the two physical states in the second way (namely the differing observable features of the two states to be compared). Moreover, as will be explained below, a transformation alone cannot determine a symmetry (is not sufficient to construct it) because many different couples of states can be linked by the same transformation, while a couple of states can determine a symmetry because it determines a transformation (and sometimes does so uniquely). So one cannot transfer the representational weight from theoretical states to a theoretical transformation by arguing that the latter determines the former. Hence it must be recognised that theoretical states contribute much more than theoretical transformations between them in ensuring the representation of an empirical symmetry and thus the observational DES in the empirical approach. One consequence of this is that the proposal above to allow DES for theoretical constructions that do not feature theoretical transformations becomes innocent (although as explained my account is not dependent on that proposal). But most importantly, *most of the literature on DES thus happens to be mistaken in the very question it is trying to solve. For it seeks a property of theoretical transformations which would ensure a DES, while given the present analysis of the observational DES in the empirical approach one rather has to ask whether a property of theoretical states exists which could ensure that DES*. Therefore, in particular *the traditional and so far most popular position from that literature* according to which only theoretical symmetries constituted by global theoretical transformations have DES must be reassessed, and this will be made in the remainder of Part III.

Chapter III. 4. Global and local transformations and states

The traditional position from the literature on DES relies on the idea that the global/local distinction has some relevance to DES, but it concentrates on this distinction as applied to theoretical transformations, while on my analysis just made it is theoretical states that matter most for the observational DES in the empirical approach and hence possibly for the ontological DES in the empirical approach, which are the two kinds of DES that as I argue should be the most important for us. Therefore, if we wish the traditional position to have a chance to hold in that context we should *extend the global/local distinction from theoretical transformations to theoretical states.* This is what I will be doing in this chapter. Note that for around a half of it I will be assuming theoretical transformations, states and symmetries to be already given, while the questions of specifying theoretical states and transformations as well as of constructing theoretical transformations and obtaining theoretical symmetries will be addressed in the other half of the chapter.

Section III. 4. 1. Which global/local distinction for theoretical transformations is possibly relevant to DES

Whether we wish to evaluate the traditional position in its original formulation referring to theoretical transformations or to make the extension of the global/local distinction for theoretical transformations to theoretical states as just proposed, we should anyway begin by determining which global/local distinction for theoretical transformations has a chances to be relevant to DES, particularly to the two kinds of DES that as I argue should matter most for us.

Firstly we should remind ourselves that the global/local distinctions for theoretical transformations used in the literature on DES are not the same as what I call the universal/restricted distinction whether on the domain of the whole universe or on some other domain. For the latter is a distinction about how wide are the domains to which theoretical transformations apply, while the global/local distinctions are usually about the sorts of theoretical transformations or the way they apply.

Next, as we saw in the literature on DES one finds many definitions of the global/local distinction for theoretical transformations, but perhaps the most common are as follows. *Firstly, a continuous group of transformations is said to be global or local* according to whether it depends on a finite number of parameters or functions respectively (where group is a mathematical notion as defined in Part I). *Secondly, a transformation is said to be global or local* according to whether it is specified using a finite number of parameters or functions respectively. *Thirdly, a transformation is said to be global or local* according to whether its effective prescriptions of change in value are the same or vary within its domain of application.

The first definition is surely important for discussing the ontology of theoretical symmetries in such contexts as the gauge argument, Noether's theorems and Hamilton's principle. However, because of concerning groups of transformations it *is too general for my analysis here* in which I wish to be the most specific possible, so that I concentrate on symmetries consisting of just two states, on representing a specific instance of a particular realisation of a given empirical symmetry, and also on just few theoretical transformations. I think that this is worth doing because by considering too large categories one looses the sight of nuances. This is best seen in Healey's idea [2009, pp. 700-701] that the ability of theoretical boosts to represent Galileo's ship empirical symmetry bestows his weaker DES and his stronger DES on theoretical symmetries constituted by Lorentz transformations (which form a group consisting of theoretical boosts and theoretical spatial rotations). Here he ascribes DES to a very wide category including Lorentz transformations that are not theoretical boosts and theoretical boosts that are unable to represent Galileo's ship empirical symmetry, and this only because some theoretical boosts happen to be able to represent Galileo's ship empirical symmetry. This cannot be the right approach if one wishes to be precise as to which theoretical transformations lead to DES and which do not, so I will not adopt it here. Thus I will only be seeking a global/local distinction which can concern single transformations. This is of no danger to the generality of my analysis, because I am considering generic theoretical transformations in the same way as I am considering generic symmetries, generic instances of realisations of empirical symmetries and so on.

Next we have the second definition, which is like a reincarnation of the first definition but applied to single transformations rather than groups of transformations. Is that what we need? Well, often this definition happens to designate what the first definition means but in less precise terms, so that we are back to the same critique plus the reproach of being vague. But when it does not it is often unclear why what the second definition designates should matter. Thus in particular in our context the second definition seems to yield a distinction not very relevant to the analysis of DES, at least when compared with the distinction yielded by the third definition. This is because *the second and the third distinctions concern different variables* involved in a theoretical transformation and *only the variables concerned by the third definition are directly relevant to DES.*

To see this consider for instance the theoretical symmetry $\psi_0 \rightarrow \psi_1 = \psi_0 e^{iq\Lambda}$, where ψ is a wavefunction and Λ may stand for a parameter θ or for a function $\theta(\mathbf{x},t)$. Here the theoretical phase transformation of ψ, which is more precisely $\cdot e^{iq\Lambda}$, is as we know able to represent the change in the constitutive differences of 't Hooft's beam-splitter empirical symmetry and preserve the representation of the constitutive invariant features of the same empirical symmetry. Now the second global/local distinction above classifies this transformation as global or local according to whether Λ stand for a parameter θ or for a function $\theta(\mathbf{x},t)$. Meanwhile, the third global/local distinction above classifies this transformation as global or local according to whether the effective prescription of change in ψ is the same at any point $(\mathbf{x},t)$ within the domain of application of our phase transformation or different at distinct points within that domain. But what matters for the representation of 't Hooft's beam-splitter empirical symmetry is the change in ψ and not Λ. Therefore, *it is the third global/local distinction for theoretical transformations* and not the second global/local distinction for theoretical transformations *that captures something potentially relevant for DES.*

It is sometimes thought that the second definition and the third definition are equivalent, but this is obviously not the case intensionally, and neither is this right extensionally (i.e. from the point of view of how two definitions classify the same transformations). To be sure, *we expect a transformation local on the third definition to be also local on the second definition, and a transformation global on the second definition to be also global on the third definition.* This is a property that can be useful when one wishes to extrapolate suitable results about one of the two distinctions to the other. However, as we see this property does not exhaust all the comparisons we could make of the two distinctions, so the remaining comparisons are going to say that the two distinctions are not equivalent. Indeed, firstly a transformation global on the third definition can be global or local on the second definition, i.e. described using a finite number of parameters or functions. And secondly, a transformation local on the second definition can be local or global on the third definition.

The first of these issues may arise from the fact that a function non-trivial elsewhere is constant on the domain of application of a transformation. This is to the disadvantage of the second definition, because it shows that the distinction it speaks about may be irrelevant to the properties of a transformation as applied to a particular domain. But it is important to take the domain of application of a transformation into account, because some theoretical transformations involved in representing the recognised empirical symmetries (in particular, theoretical boosts, electrostatic potential shifts and phase shifts) preserve observational consequences when applied on the whole domain available and change observational consequences when applied on a more restricted domain. Thus they only constitute non-gauge symmetries in the sense of observationally incomplete theoretical symmetries when applied on a restricted domain, and hence lead to the kinds of DES such as the observational DES and thus possibly also to the ontological DES only in that case. This is probably one of the reasons why the authors like Greaves and Wallace may think that only restricted theoretical transformations are relevant to DES. However, in fact *whether a theoretical transformation is restricted or universal does not necessarily determine whether it leads to DES*, because as explained in my analysis of Brading and Brown's article there are also theoretical transformations that bring about a change in observational consequences even when applied on the whole universe (e.g. spatial reflections), and besides there are other theoretical transformations that do not bring about a change in observational consequences even when applied on a restricted domain (e.g. some transformations of the electromagnetic potential). Yet, it does follow that *one has to take the domain of application into account in so far as for some transformations it has an impact on their capacity of bringing about a difference in observational consequences* and hence of being relevant to the observational DES and possibly also to the ontological DES. *From this point of view the third definition is also preferable*, because it is explicitly specialised to domains of application while the second definition is not. Indeed, the third definition speaks of a transformation as applied to a specific domain, not as a transformation in itself.

In addition, however, both issues mentioned above may arise even when a function concerned by the second definition is non-trivial on the domain of application of a transformation. It is then *the fact that the second definition and the third definition are concerned with different variables* that *may lead to them classifying differently the same transformation.* As an example, consider the theoretical symmetry $A_{\mu\,0} \rightarrow A_{\mu\,1} = A_{\mu\,0} + \partial^{\mu}\Lambda$, where A_{μ} is the electromagnetic potential and Λ may stand for a parameter θ or a function $\theta(x_{\mu})$ (which is the relativistic notation for $\theta(\mathbf{x},t)$). As we know, the theoretical electromagnetic potential transformation of A_{μ} from that theoretical symmetry, which is more precisely $+\,\partial^{\mu}\Lambda$, is able to represent the change in the constitutive differences of Faraday's cage empirical symmetry and preserve the representation of the constitutive invariant features of the same empirical symmetry, as well as can be combined with the phase transformation given above so that to represent the change in the constitutive differences of 't Hooft's beam-splitter empirical symmetry and preserve the representation of the constitutive invariant features of the same empirical symmetry. Healey [ibid., p. 700] was proposing to consider a specific version of this transformation with $\Lambda = kt$, where k is a constant, and noted that the effect of this choice of Λ is to leave invariant the spatial component of A_{μ}, namely the magnetic vector potential **A**, while changing the time component of A_{μ}, namely the electrostatic potential φ, by the same amount k everywhere within the domain of application of the transformation. In other words, the electromagnetic potential transformation $+\,\partial^{\mu}\Lambda$ with $\Lambda = kt$ *effectively reduces* to the electrostatic potential shift $+\,k$. Healey classified this specific version of the electromagnetic potential transformation as local [ibid.], and this can be warranted by the second global/local distinction above, for it implies that the specific version of the electromagnetic potential transformation in question is local because $\Lambda = kt$ is a function of time rather than a constant. However, on the third global/local distinction above the same specific version of the transformation should be classified as global, because it effectively reduces to the electrostatic potential shift $+\,k$ and the change in value prescribed by that effective transformation is the same for any item characterised by φ within the relevant domain. Thus, although Healey did not make such a remark, we see that the specific version of the transformation he is discussing is global on the third definition while being local on the second definition, which shows that *the two definitions are not equivalent even extensionally.* Therefore, we have to choose which of these definitions to concentrate on.

As argued above, it is the third definition that yields a distinction potentially relevant to the analysis of DES both because of the variables it concerns and because of its being explicitly specialised to domains of application. But as we now see *the third distinction also allows us not to be concerned with how we express effective transformations*, because it classifies an effective electrostatic potential shift in a unique way whether it was originally formulated as an electrostatic potential shift or as a specific electromagnetic potential transformation. This is to be contrasted with the second distinction, where the same effective transformation is classified as global or local depending on how we express it. But effective transformations matter more than the way of expressing them, which may well be arbitrary. Therefore, also by the latter reason it is the third distinction that is worth concentrating on. Hence it is this distinction that has to be used to define an extension of the global/local distinction to theoretical states.

Section III. 4. 2. Extension to a possibly relevant global/local distinction for theoretical states

Note that I adopt a more general version of the third definition than usual, allowing the domain of application of a theoretical transformation to consist of any kind of *items* rather than just spatiotemporal points as in the examples above. Note also that we can distinguish between what I call *the first understanding of transformations*, by which a transformation is a collection of prescriptions of change in values of a variable, and what I call *the second understanding of transformations*, by which a transformation is a difference between final values and initial values of a variable. It is the first understanding of transformations that was used in the third definition above, and I will continue to use it for the moment.

Thus on the definition adopted *a given theoretical transformation in the first understanding* is *global* or *local* with respect to a variable depending on whether the prescriptions of change in value of that variable are the same for all items or different for at least two items within the domain of application of the transformation. Therefore, the most straightforward extension to states consists in saying that *a given theoretical state* is *global* or *local* with respect to a variable depending on whether the values of that variable are same for all items or different for at least two items within the domain of application of the state.

Arguments in favour of this way of defining the extension to states are as follows.

Firstly, in this case the only significant difference between the two distinctions, and perfectly natural, is that the distinction for transformations is about prescriptions of change in value while the distinction for states is about values. The difference between the domains of application that occurs in the two definitions is also natural, but is less significant because the domain of application of a theoretical transformation in the first understanding can be taken to be the domain of application of the values this transformation will be acting upon, but values are just what constitutes a theoretical state, and so the domain of application of the transformation will be the same as the domain of application of the theoretical state constituted by these values, or a restriction of the domain of a theoretical state constituted by these and other values. The relevant theoretical state will be initial with respect to the theoretical transformation, because it is a state on which the transformation acts, as well as initial with respect to a physical state, because we are interested in cases where the theoretical transformation is a main theoretical transformation and hence a theoretical state on which it acts is a theoretical state capable of representing the initial physical state of an observable empirical symmetry. Thus the domain of application of a transformation in the first understanding will be the same as, or a restriction of, the domain of application of an initial theoretical state in either sense of 'initial'. So *a theoretical transformation in the first understanding* can be said to be *universal* or *restricted* according to whether it applies to the whole domain of the initial theoretical state or on a restriction of the latter domain.

Secondly, the global or local character of given states and transformations can be determined using the same tool, namely *the evaluating function* which estimates how values or changes in value differ for two items. To illustrate, in my example of phase transformations $\psi_0 \rightarrow \psi_1 = \psi_0 e^{iq\Lambda}$ the theoretical phase transformation is as said more precisely $\cdot e^{iq\Lambda}$, while ψ_0 and ψ_1 are actually theoretical states. So when applied to the theoretical phase transformation the evaluating function will be determining whether the effective prescriptions of change in ψ differ from one item to another within the domain of application of the transformation (because this is what the third definition for transformation is concerned with), by contrast to *the function from the second definition above* which was just a specification of Λ. Meanwhile, in the case of theoretical states such as ψ_0 and ψ_1 the evaluating function will be determining whether ψ differs from one item to another within the domain of application of a state, provided we are evaluating the global or local character of states in the same variable as the variable of interest to us in the case of transformations. *The conditions for having a global or a local character according to the evaluating function* follow from the adopted definitions of the global/local distinction for states and transformations and will again be similar for states and transformations. Namely, *a given theoretical state* will be *global* or *local* according to whether the difference in value as estimated by the evaluating function respectively is or is not zero for respectively any or at least some two items within the domain of application of the state. And *a given theoretical transformation in the first understanding* will be *global* or *local* according to whether the difference in change in value as estimated by the evaluating function respectively is or is not zero for respectively any or at least some two items within the domain of application of the transformation. Sure, the answers of the evaluating function may fail to be well-determined, as happens in theories with non-trivial topology such as Weyl's. But then again this will affect the characterisation of states and transformations alike. And if the evaluating function only fails to give a unique estimation for two distant items from the same domain, as is common in particular to many general-relativistic topologies, restricting the evaluating function to comparisons to neighbouring items, i.e. making it a kind of derivative on the domain, can help to determine the character of both states and transformations.

Thirdly, the global/local distinction for states as defined above needs the same precondition for being well-defined (i.e. for allowing either option to be realised) as the global/local distinction for transformations, namely that *the domain of application should always be composed of several items.* Indeed, if the domain is composed of a single item, a state or a transformation will always be trivially global, for there will not be any possibility for values or for changes in value to differ within the domain, as is needed to get a local state or a local transformation. For example, as we know the theoretical symmetry $v_0 \rightarrow v_1 = v_0 + v$, where v is velocity and $+ v$ is the theoretical boost transformation, is able to represent the constitutive differences of Galileo's ship empirical symmetry, as well as the change in them and the inertial character of the observable dynamics within the ship in each of the two physical states constituting that empirical symmetry. Here the theoretical states v_0 and v_1 as well as the theoretical transformation $+ v$ are all usually interpreted as applying on a ship, i.e. on a single item, and so we have here a theoretical symmetry where as much the states as the transformation are trivially global. If on the other hand we reinterpret the domain as composed of parts of a Galileo's ship, then again as much the states as the transformation of that theoretical symmetry are going to be global, because arguably we will have to reinterpret the states as assigning the same value of velocity and the transformation as assigning the same change in velocity to any part of a Galileo's ship. However, neither the states nor the transformation are going to be trivially global on the reinterpreted domain, because that domain also allows values and changes in value to differ for different items, i.e. for different parts of a Galileo's ship, as is needed to get a local state or a local transformation.

More precisely, *if we are to specify a local transformation or a local state, we need a domain such that it is composed of several items and moreover these items are distinguishable*, so that we be able to say to which item which value or change in value is to be ascribed (*for specifying global transformations and states this requirement is not necessary* because we can ascribe a value or a change in value to all items in a domain without being able to distinguish among them). This distinguishability can be ensured for instance by spatiotemporal relationships of objects with respect to a spatially asymmetric gradually evolving reference object. Besides, we may wish to reflect this distinguishability on the theoretical level in case we do not wish to make an appeal to the link between the physical level and the theoretical level each time we specify values or changes in value.

One way of reflecting the distinguishability among items on the theoretical level is to use *identifying values of variables*. Example of identifying values of variables are spatiotemporal positions of items, because within a reference frame any spatiotemporal position can be attributed to at most a single physical item. In general, *values of variables are identifying if each item gets a unique combination of these values.* Often the identification requires more than one variable, e.g. in the case of spatiotemporal positions values of four variables are used. Such a small number of variables happens to be used because for each variable each of its values is used only once, while if that was not the case, more variables would be needed to identify each item uniquely. In other words, *to use less variables one better uses states local in the variables whose values are to ensure the individuation, although the individuation using states global on their domains of application, or on parts of them, is also possible provided these domains are suitably chosen.* To specify identifying values of variables we can make appeal to the link between the physical level and the theoretical level. For example, we can specify correspondence rules as to which spatiotemporal relationships with respect to a reference ensuring the distinguishability of items should lead to which assignments of values of spatiotemporal values. The specified identifying values of variables will next allow to predicate values of other variables, and changes in them, on items as identified by the identifying values of variables instead of predicating them on items directly. This is how theoretical states and transformations, in particular local ones, can be built using some identifying usually local state as a basis instead of using correspondence rules for all the variables directly. We will return to identifying values of variables in *my proof* below.

Note that the precondition concerning the distinguishability of items, together with its possible reflection on the theoretical level, is again common to local transformations and states, similarly to how the precondition about the number of items is common to the global/local distinctions for transformations and states. In sum, *the proposed extension of the global/local distinction from transformations to states is very conservative and allows to treat transformations and states in the same way.*

Section III. 4. 3. Given and constructed theoretical transformations, states and symmetries

Let us now consider the global/local distinction for the second understanding of transformations. In this case a theoretical transformation is considered not as a collection of prescriptions of change in value but as a difference between final values and initial values of a variable. Thus by the same argument as above the domain of a theoretical transformation in the second understanding will be the same as or a restriction of the domains of the initial and the final theoretical states. By that reason it was more convenient to use the first understanding of transformations, which concerns a domain of a single state instead, when extending the global/local distinction to a single state. Still, the discussion above can also apply to the second understanding of transformations, provided when speaking of transformations we replace the change in value by a difference between a couple of values belonging each to a distinct state, and an item by a couple of items belonging each to a domain of a distinct state. Thus in particular *a given theoretical transformation in the second understanding* will be *global* or *local* with respect to a variable depending on whether the differences between couples of values of that variable are the same for all couples of items or different for at least two couples of items within the domain of application of the transformation.

When both the theoretical transformation and the theoretical states are already given, as I have presupposed above for the most of this section, the two understandings are just two interpretations of the same theoretical transformation. The first understanding of a theoretical transformation is then more suitable when one represents using this theoretical transformation the physical transformation in the first way of realising empirical symmetries, and the second understanding is more suitable when one represents using this theoretical transformation the link between the physical states in the second way (this is in agreement with my discussion above where it was proposed to use the same transformation differently interpreted for the two ways).

On the other hand, when theoretical transformations are not yet given, different understandings are more than differences in interpretation because they lead to different methods of constructing theoretical transformations and hence to different ways of obtaining theoretical symmetries.

The first method of constructing theoretical transformations relies on theoretical transformations in the first understanding. It consists in that one specifies prescriptions of change in value of a variable on some domain that is or can be characterised by the variable. If next one applies the constructed transformation to an initial theoretical state constituted by values of that variable characterising at least all the items within the domain of application of the transformation, and possibly other items in addition, then one gets a final theoretical state and hence possibly a theoretical symmetry (provided something, e.g. some or all of the observational consequences, is left invariant by the transformation). This *first way of obtaining theoretical symmetries* is convenient in that the final theoretical state in that case is uniquely determined by the application of the constructed theoretical transformation in the first understanding to a given initial theoretical state.

The second method of constructing theoretical transformations relies on theoretical transformations in the second understanding. It consists in that one takes two collections of values, decides which of them will be final and which initial with respect to the transformation to be constructed, and specifies by which amount the values from the final collection differ from the values of the initial collection. If the initial and final collections of values are given, they will constitute an initial and a final theoretical states, thus by constructing the theoretical transformation one can also get a theoretical symmetry (provided something, e.g. some or all of the observational consequences, is shared by the states). In general in this method of constructing theoretical transformations a transformation will not be uniquely determined even when the collections are given, but instead will depend on how values from two collections are matched in couples, which may in turn depend on how items characterised by values are matched in couples. Thus in general this *second way of obtaining theoretical symmetries* does not yield a unique theoretical symmetry even when the states are given. In case one wishes the transformation and the theoretical symmetry to be uniquely determined, one needs to use extra restrictions on how to match values or items in couples. For instance, one may use an appeal to identifying values of variables for this purpose, but this is not the only option.

We will return to the two methods of constructing theoretical transformations in *my proof* below.

Note in the meantime that the first method of obtaining theoretical symmetries uses an initial theoretical state and a theoretical transformation, while the second uses two theoretical states. So while the theoretical states are sufficient to obtain a theoretical symmetry (via the second method) because they determine the theoretical transformation (although in general non-uniquely), a theoretical transformation is not sufficient to obtain a theoretical symmetry (via the first method) because it cannot determine a theoretical state without using another theoretical state in addition. To be sure, on can say that a theoretical transformation determines a class of theoretical symmetries because it determines a class of initial and final theoretical states that can be linked by the transformation, in the same way as a couple of theoretical states determines another class of theoretical symmetries because it determines a class of theoretical transformations that can link these two theoretical states. But as said it is also sometimes possible in the second way of obtaining theoretical symmetries to get a unique transformation when starting with a certain couple of theoretical states, so that the two theoretical states will then determine a specific theoretical symmetry rather than a class of theoretical symmetries, while for a theoretical transformation it does not seem possible to do so, or at least this would have to be much less common. So for our focus on specific theoretical symmetries theoretical states seem more powerful than theoretical transformations. Hence *as announced at the end of the previous chapter one cannot transfer the representational weight from theoretical states to a theoretical transformation by arguing that the latter determines the former or that the latter determines a theoretical symmetry while the former does not – for things are rather the other way round!*

Note also that whatever the understanding of a theoretical transformation when obtaining a theoretical symmetry, once it is obtained the two understandings become different interpretations of the theoretical transformation and one can use either of them depending on the way in which an empirical symmetry is realised.

Chapter III. 5. Why would one attribute DES to (properly) global symmetries alone

With the extension of the possibly relevant global/local distinction for theoretical transformations to theoretical states that we have just carried on it is possible to better evaluate the traditional position in the context where DES is mostly ensured by the theoretical states. To do so we will discuss in this chapter the traditional position and its reformulation for such a context.

Section III. 5. 1. A critique of the traditional attributions of DES to global symmetries alone

To remind, *a theoretical symmetry is usually said to be global or local* according to whether the theoretical transformation constituting it is global or local. *A traditional and so far more frequent position* in the literature on DES then consists in saying that only global but not local theoretical symmetries have DES. This position is shared by as much authors as Kosso [2000], Brading and Brown [2004], Healey ([2007], [2009]), Friederich ([2015], [2017]), and Ladyman and Presnell (manuscript [2016/01/08]). *A newer alternative position* consists in saying that both global and local theoretical symmetries have DES. This position has been defended by Greaves and Wallace [2014] as well as Teh [2016].

Although the traditional position is so far the most popular in the literature on DES, it is hard to find in this literature any convincing reason for holding it. Indeed, as we already saw Kosso's, Brading and Brown's as well as Healey's reasons for holding that position are all unsatisfactory. For Kosso it was actually possible to hold that position simply because he was using the theoretical approach to DES and could thus only choose to establish DES for global symmetries. In fact he tried to establish DES of local symmetries too, but failed to do so because of only considering the phenomena that did not satisfy his observable difference condition. Brading and Brown's denial of DES for theoretical symmetries constituted by local theoretical transformations was in part due to the last reason and in part to some other also unsatisfactory reasons. These included considering a too restricted class of local transformations (which comprised only local phase transformations on the whole beam) and a too restricted class of theoretical symmetries (which comprised theoretical symmetries of laws but excluded for instance the theoretical symmetries to which I have attributed the observational DES in the empirical approach above). As to Healey, his preference for global symmetries was due to the extra requirements that as I showed above were not motivated within his framework and even contradicted to it.

Friederich argues that local symmetries do not have DES using the following ideas. Firstly, any local theoretical transformation on a region can be extended to a theoretical transformation on a wider region at whose boundaries it approaches the identity. Secondly, the theoretical states on the wider region linked by the extended transformation must be matched with the same physical state. Thirdly, the theoretical states on the former region linked by the former transformation must then also be matched with the same physical state (where this physical state is presumably a restriction of the physical state with which the theoretical states on the wider region are matched). However, this argument does not hold either. For firstly, there can be local transformations acting on the whole universe, so that there is no region towards which to extend them. These are not the theoretical transformations Greaves and Wallace are interested in, so Friederich does not concentrate on them because he is answering to Greaves and Wallace, but if we wish to know whether any local theoretical transformations do not have DES, such theoretical transformations must be considered as well. Secondly, even if we do concentrate on restricted local theoretical transformations for which the first of Friederich's ideas listed above holds, his second idea does not follow. In [2015, p. 548] Friederich motivates this passage by saying that theoretical states on the wider region linked by the extended transformation must be matched with the same physical state because this transformation does not act on the boundary of the wider region and beyond it. However, the latter property is insufficient to ensure the former matching, because that matching also presupposes that the theoretical change on the interior of the wider region does not match with the change of the physical state, and this last presupposition is not warranted in any way. Thirdly, if the second of Friederich's ideas listed above fails to hold, the same happens with his third idea provided the subregion of the wider region concerned by the change of the physical state intersects with the region on which the local transformation acts. Fourthly, whether Friederich's argument holds or not, this should be so for restricted local transformations and restricted global transformations alike, because for any restricted global transformation Friederich's first idea should work as well and because the two other ideas do not seem to depend in any way on whether the original theoretical transformation is local.

Ladyman and Presnell's claim that only global symmetries have their stronger DES is also based on unconvincing arguments. One of them says that we cannot assign their stronger DES to local symmetries unless they represent some empirical symmetry that global symmetries are unable to represent. But this does not seem to follow, because their stronger DES does not seem to have anything to do with how much theoretical symmetries are able to represent a given empirical symmetry. Another argument says that if representations using global symmetries and those using local symmetries are equally empirically adequate, this is no justification for assigning the ontological significance to the latter. But then neither is this a justification for assigning it to the former.

Thus *currently the traditional position by which only global symmetries have DES has no satisfactory justification.* But why could this position seem so attractive, and is not there any other justification of it that we can provide?

Section III. 5. 2. A reformulation of the nuanced traditional position

Before giving my answer to that question I would like to introduce some amendments to the traditional position so that to make it more viable in the context of my analysis.

Firstly, let us concentrate on the most recent update of the traditional position found in Ladyman and Presnell's manuscript [2016/01/08]. I will call its generalised version *the nuanced traditional position* and will take it to consist in that while both global and local theoretical symmetries can have a weaker DES, only global symmetries can have a stronger DES.

Secondly, let us make the kinds of DES from that definition more precise. To see why this should be done note that actually there is always a way to make only theoretical symmetries of some kind to have some DES. For this is suffices to define that DES so that it be impossible for other theoretical symmetries to have it. The easiest way to do so is to explicitly preclude the other symmetries from having it. Adding this as a requirement to some already existing DES may moreover result in strengthening it, so that the resulting DES will indeed be a stronger DES with respect to the original DES which will then indeed be a weaker DES. For instance, if we add to the definition of the observational DES in the empirical approach that to have our new DES a theoretical symmetry should have the observational DES in the empirical approach and besides be global, then local symmetries will be unable to have the resulting DES and that DES will be stronger with respect to the observational DES in the empirical approach. However, this would only be a trivial way of making the nuanced traditional position right, while what matters instead is whether it is right with respect to kinds of DES of interest to us. As I have been arguing at length, the kinds of DES that should be of special interest to us are the observational DES in the empirical approach and the ontological DES in the empirical approach. Hence I will understand a weaker DES from the definition of the nuanced traditional position to refer to the former and a stronger DES from the same definition to refer to the latter.

Thirdly, as already mentioned the traditional position and its nuanced version assign some importance for DES to the global/local distinction for theoretical symmetries, and that distinction is based on a global/local distinction for theoretical transformations, but in our context theoretical transformations are much less important than theoretical states, hence if the traditional position and its nuanced version are to keep the idea that some global/local distinction for theoretical symmetries is important, that should be a global/local distinction also based on a global/local distinction for theoretical states. We did obtain the last global/local distinction below, and beforehand we determined which global/local distinction for theoretical transformations is potentially relevant to our context. Therefore, we have to introduce a new global/local distinction for theoretical symmetries that would be based on our global/local distinctions for theoretical states and transformations, and then say what should be the attitude of the nuanced traditional position to different kinds of theoretical symmetries as classified by the distinction we will have introduced.

To do so note that despite the traditional dichotomy between global and local theoretical transformations it is actually possible to have *a mixed theoretical transformation* that acts non-trivially on several variables with respect to one or some of which it is global while with respect to another or some other of them it is local. Hence it is likewise possible to have *a mixed theoretical symmetry with respect to the global/local distinction for theoretical transformations*, i.e. a theoretical symmetry constituted by a mixed theoretical transformation as just defined. The same holds for a theoretical state, which can also be *a mixed theoretical state* because of being global with respect to some variables and local with respect to some others. Therefore, our new distinction for theoretical symmetries should likewise comprise not only some global symmetries and some local symmetries but also some mixed symmetries. The proposed distinction is then as follows: *a theoretical symmetry will be called properly global, properly local or mixed with respect to the global/local distinctions for theoretical states and transformations adopted above* according to whether among the theoretical transformation and the theoretical states constituting it all are global in all the relevant variables, or all are local in all the relevant variables, or neither of the previous options holds because the global/local distinction classifies the three constituents or their restrictions to different relevant variables differently.

Here we are discussing the possible importance of main theoretical transformations and of the effect they may induce on theoretical states, so we have to only concentrate on those variables whose values are non-trivially changed by a main theoretical transformation. But in addition, we have to exclude from consideration those of the latter variables which concern the representation of the observable constitutive invariant features (for the case where this representation is done differently in the two theoretical states). Indeed, those parts of theoretical states that are responsible for the representation of the observable constitutive invariant features are anyway mostly local, because to represent an observable dynamics one usually ascribes different values of suitable variables to different items. Hence we would not give any chance for the traditional position to be right because we would make a choice of variables which hugely privileges the local character over the global one. Besides, when the representation of the observable constitutive invariant features uses different values of variables in the two states, the part of the main theoretical transformation acting on them is a gauge transformation in the sense of a theoretical transformation not bringing about any new observational consequences, and hence this part is anyway unable to ensure any DES, and so what could ensure a DES is only the part which acts on the variables relevant to the representation of the observable constitutive differences. Hence *the relevant variables for the evaluation of the properly global, properly local or mixed character of a theoretical symmetry* in our context are going to be those whose values are non-trivially changed by the main theoretical transformation and which are relevant to the representation of the observable constitutive differences. I will call theoretical symmetries, transformations and states *restricted* when we only consider those of their parts that concern the relevant variables as just defined. The effect of defining the relevant variables as just proposed is that the focus of our ontological discussion gets replaced on the role of restricted symmetries.

To illustrate, suppose that a main theoretical transformation only acts non-trivially on the wavefunction and on the electromagnetic potential and these are the only variables relevant to the representation of the constitutive differences of the setup of 't Hooft's beam-splitter empirical symmetry where a half-wave plate and a switched-on solenoid are added in the final physical state. Then the restricted main theoretical transformation will be the main theoretical theoretical transformation as restricted to its action on the wavefunction and electromagnetic potential variables, and we will only consider whether this theoretical transformation is global or local in these two variables, and similarly we will thus only consider whether the theoretical states as restricted to those two variables are global or local in them. The theoretical symmetry constituted by that full main theoretical transformation and by the full theoretical states linked by it will then be mixed for instance if its restricted main theoretical transformation is local in both variables but one of the restricted theoretical states is global in both variables, or if the restricted main theoretical transformation or at least one of the restricted theoretical states is global in one of the two variables and local in the other.

As said, in light of this more complete classification of theoretical symmetries one has to make precise of each of the mentions of global or local symmetries contained in the definition of the nuanced traditional position given above which kinds of theoretical symmetries this mention comprises as far as the global, local or mixed character of theoretical states is concerned. This amounts to replacing such mentions relying on the old global/local distinction for theoretical symmetries based on theoretical transformations alone by other mentions relying on the new global/local distinction for theoretical symmetries based on both theoretical transformations and theoretical states.

Considering firstly the mentions of global and local symmetries to which the nuanced traditional position assigns the ability to have a weaker DES, the option to interpret them as comprising properly global and properly local symmetries is not acceptable, because it prohibits the combinations of mixed or local states with global transformations and of mixed or global states with local transformations while there was no such restriction in the original formulation. Another option of taking these mentions to designate properly global and properly local symmetries together with mixed symmetries with global or local transformations solves the previous problem, but still precludes transformations from being mixed. This would be justified if the nuanced or original traditional position was ever denying DES to symmetries with mixed transformations explicitly, but in fact it seems to have overlooked them completely. When faced with the question whether such symmetries can have DES my guess is that proponents of the nuanced traditional position would agree that they can, given that they already agree to attribute a weaker DES to theoretical symmetries with global transformations and to theoretical symmetries with local transformations separately and given that a theoretical symmetry with a mixed transformation is just a theoretical symmetry whose transformation is global in some variable(s) and local in some other(s). Hence the best option is to take the nuanced traditional position to claim that properly global, properly local and mixed symmetries in the sense of the new global/local distinction for theoretical symmetries all can have a weaker DES.

As to the mention of global symmetries to which the nuanced traditional position assigns the ability to have a stronger DES, we have a choice between interpreting them as only properly global symmetries, or only mixed symmetries with global transformations, or both. But arguably the two last choices go against the spirit of the nuanced traditional position, which consists in that a certain DES is a function of the global character of a component of a theoretical symmetry, because these interpretations admit that theoretical states may be mixed or local, and as in my framework it is theoretical states that ensure most of DES, these interpretations thus allow DES to be primarily ensured by non-global components. Therefore, I propose to interpret the mention of global symmetries to which the nuanced traditional position assigns the ability to have a stronger DES as designating properly global symmetries alone. This actually also finds some justification in the usual treatment of Galileo's ship empirical symmetry from the literature on DES, for as explained in the previous chapter it is usual to take the theoretical states v_0 and v_1 as well as the theoretical boost transformation $+ v$ to apply on the whole ship, which as explained implies that the theoretical symmetry $v_0 \rightarrow v_1 = v_0 + v$ is properly global either trivially or after the reinterpretation of the domain as consisting of parts of the ship. Note that this is a restricted theoretical symmetry as defined above, and my other examples of theoretical symmetries from the chapter on the global/local distinctions are likewise restricted theoretical symmetries in that sense.

If we now explicitly integrate the proposed interpretations and my choices of a weaker DES and of a stronger DES into the definition of the nuanced traditional position, we get *a reformulated nuanced traditional position* by which while properly global, properly local and mixed symmetries can all have the observational DES in the empirical approach, only properly global symmetries can have the ontological DES in the empirical approach. It is of this position that I wish to ask whether it may seem attractive and whether we may provide a justification for it.

Section III. 5. 3. Why would one support the reformulated nuanced traditional position

Here is why one could find attractive the claim that only properly global symmetries have the ontological DES in the empirical approach in cases where both they and the other theoretical symmetries have the observational DES in the empirical approach.

Suppose we wish to restrict the proliferation of matchings to the minimum by considering representations of just a single instance of a particular realisation of a given observable empirical symmetry. As argued above, even in this case there will be plenty of theoretical symmetries that have the observational DES by virtue of adequately representing the observable constitutive features of that instance. For example, there will be an infinity of different assignments of values of the electromagnetic potential that are able to represent the constitutive differences of an instance of Faraday's cage empirical symmetry. The question is then how to determine which among the theoretical symmetries that have the observational DES on the basis of a given instance have the ontological DES on the basis of that instance, where as before we are interested specifically in the ontology yielded by restricted theoretical symmetries.

To answer the latter question we may firstly consider finding some criteria relevant to the latter DES with respect to which these theoretical symmetries may perform differently, so that we be able to discard some of them and leave some others, normally a single one per instance, that would have the ontological DES. What could these criteria be? One criterion could be *empirical adequacy*, i.e. the ability to adequately represent observable features. But usually it does not allow to single out just one theoretical symmetry per instance as having the ontological DES, because all or at least an infinity of theoretical symmetries with the observational DES on the basis of a given instance are going to be observationally equivalent. Other criteria may be the so-called *theoretical virtues* such as simplicity, explanatory power and so on. But there is usually no significant difference between the relevant theoretical symmetries with respect to such theoretical virtues either (although if there was, this would not be conclusive because the link of these virtues with the ontology would still have to be demonstrated). In particular, in our example of the representation of the constitutive differences of an instance of Faraday's cage empirical symmetry by assignments of values of the electromagnetic potential there will be plenty of such assignments that are both observationally equivalent and arguably identical with respect to the other theoretical virtues, so neither of the criteria just mentioned would be of any help for diminishing the proliferation of matchings.

Next we could consider choosing a single theoretical symmetry arbitrarily, but this would hardly lead us to the ontology in any substantial sense. And if on the other hand we attribute the ontological DES to more than one theoretical symmetry, this will yield an incoherent ontology because usually theoretical symmetries are not complementary but contradictory with respect to each other (e.g. they assign different values of a variable or different changes in value of a variable to the same item).

The next option can be to hold as ontologically faithful what all the theoretical symmetries agree on. This allows us to escape both the arbitrariness and the incoherence, but the problem is now that the resulting ontology may be very poor, because theoretical symmetries may happen not to have much in common as far as their theoretical underpinnings are concerned. And that is where assigning the ontological DES to properly global theoretical symmetries alone becomes advantageous, because in fact this seems to be the only case where the ontology does not come out as poor.

Indeed, if *theoretical states* are ascriptions of values of variables to some collection of items, theoretical states belonging to different theoretical symmetries may agree or disagree at least as to which values of which variables are assigned to which items, what is the difference in value of some variable for two items and whether a theoretical state is global, local or mixed. Similarly, if *theoretical transformations* are ascriptions of changes in values of variables to some collection of items, theoretical transformations belonging to different theoretical symmetries may agree or disagree at least as to which changes of values of which variables are assigned to which items, what is the difference in the changes of value of some variable for two items and whether a theoretical transformation is global, local or mixed. Therefore, theoretical symmetries may likewise agree or disagree at least on any of the six aspects just listed. Consequently, the ontological proposals induced by theoretical symmetries may agree or disagree at least on the physical features and on the physical transformation corresponding to the values and to the changes in values in question, on the physical relations corresponding to the differences in the values and in the changes in values in question, as well as on whether the distribution of these physical features and the action of this physical transformation is of global (uniform), local (non-uniform) or mixed character.

Now if the theoretical symmetries whose common core we wish to bestow with the ontological significance comprise all properly global, properly local and mixed theoretical symmetries, they will disagree on all the six aspects listed above. If we take mixed theoretical symmetries alone, the same thing happens. If we take local symmetries alone, i.e. those having local transformations, they will obviously agree on the character of the transformation, but disagree on the remaining five aspects. If we take properly local symmetries alone, they will agree on the local character of the states and the transformation, but disagree on the other four aspects. If we take global symmetries alone, i.e. those having global transformations, they will obviously agree on the global character of the transformation, and usually on the changes in values of variables prescribed by it, but will disagree on the other four aspects. But if we take properly global symmetries alone, they will usually agree on all aspects except one. Indeed, firstly they will obviously agree on the global character of the states and the transformation. Secondly, they will obviously agree on the difference in value and in the change in value between two items, namely on the fact that there is no such difference. And thirdly, they will usually agree as to which prescriptions of change in value are assigned by the transformation. So they will only disagree as to which values are assigned to the items by the theoretical states. Consequently, *provided we omit the physical properties that would correspond to the assignments of values by theoretical states to items from our ontology, holding that only properly global symmetries have the ontological DES yields a non-arbitrary, coherent and the richest ontology, and that is why the reformulated nuanced traditional position is so attractive.*

This argument has not been explicitly proposed in the literature on DES, but I suspect that something like it is implicitly taken into account by the defenders of the traditional position and plays a role in their attachment to it. However, this is only *a pragmatic argument*, because it says that the reformulated nuanced traditional position yields us an ontology which has some desirable qualities, but nothing guarantees that the ontology in any substantial sense really is how we would like it to be, and in particular that it is as rich as the choice of properly global symmetries implies. To establish the ontology in a substantial, not purely pragmatic sense we need something more.

Here is then a different, *unpragmatic argument* as to why only properly global symmetries must have the ontological DES. The possibility for making this argument comes from the fact that there does not seem to be any necessary link between adequately representing the constitutive features of an empirical symmetry using a properly global theoretical symmetry and adequately representing them using a non-properly-global theoretical symmetry. So although I was arguing above that any of the recognised empirical symmetries should be matched as much with global symmetries as with local symmetries, it may still seem that for some empirical symmetry it may happen that its constitutive features can be adequately represented by properly global symmetries alone, in which case only properly global symmetries will have the observational DES with respect to that empirical symmetry. One could then argue that we are justified in also assigning the ontological DES to properly global symmetries in that case because there are no other theoretical symmetries that have the observational DES with respect to that empirical symmetry. Moreover, one could further argue that for example by reasons of consistency the ontological DES should always be attributed to a single kind of theoretical symmetries. Hence, the argument would go, only properly global symmetries should have the ontological DES also in the case of those empirical symmetries with respect to which both properly global and non-properly-global theoretical symmetries have the observational DES. In short, only properly global symmetries would have to always get the ontological DES because of them having the observational DES with respect to a greater number of empirical symmetries.

Perhaps this is not the strongest argument ever proposed, but at least it provides a promising alternative to the unsatisfactory arguments for the traditional position from the literature on DES discussed above. Moreover, this argument is capable of yielding an ontology in a substantial sense because of its not relying on pragmatic considerations. And at the same time, as the ontology it leads to is the one following from properly global symmetries alone, we get all the benefits highlighted by the pragmatic argument given above as well. So the reformulated nuanced traditional position is both attractive and tenable provided the unpragmatic argument holds. But does it?

Chapter III. 6. Constructing more theoretical symmetries with the observational DES

The crucial assumption of the unpragmatic argument is that it should be possible to have a situation where the constitutive features of an empirical symmetry can be adequately represented by properly global symmetries but not by non-properly-global symmetries. In other words, it should be possible to have a situation where only properly global symmetries have the observational DES in the empirical approach with respect to some empirical symmetry. If the crucial assumption does not hold, the situation just described should be impossible, and so what will hold is on the contrary that each time properly global symmetries have the observational DES in the empirical approach with respect to some empirical symmetry, non-properly-global symmetries also have this DES with respect to that empirical symmetry. That is exactly what I am going to prove in this chapter.

More precisely, if I would just prove that the non-properly-global symmetries in question are global symmetries, i.e. those having global transformations, then a proponent of the traditional position could revert to its original formulation by which what matters for DES is the global character of transformations alone rather than global character of both transformations and states. Also proving that the non-properly-global symmetries in question are theoretical symmetries with local or mixed transformations and global states could make one claim that what matters is the global character of states and hence that the property of being global retains its importance. So *what I will be proving is that each time time properly global symmetries have the observational DES in the empirical approach with respect to some empirical symmetry, properly local symmetries also have this DES with respect to that empirical symmetry.* However, *while proving that I will also prove that* each time time properly global symmetries have the observational DES in the empirical approach with respect to some empirical symmetry, *mixed symmetries also have this DES with respect to that empirical symmetry.*

Section III. 6. 1. How the proof works

The first idea helping to prove that is that a theoretical symmetry has the observational DES in the empirical approach with respect to some empirical symmetry by virtue of having suitable observational consequences, hence any other theoretical symmetry can have the same DES with respect to the same empirical symmetry provided it has the same observational consequences. The second idea is that to obtain from a theoretical symmetry with given observational consequences another theoretical symmetry with the same observational consequences we have to use *gauge transformations* in the sense of theoretical transformations that do not bring about any new observational consequences; this will be the sense in which I will use the term "gauge transformation" in my proof. The third idea is that to obtain from a properly global theoretical symmetry with given observational consequences a properly local theoretical symmetry with the same observational consequences we should impose additional requirements on the gauge transformations such that they ensure that the resulting theoretical symmetry is properly local when applied to a properly global theoretical symmetry. My proof shows in particular what are these requirements and under which conditions they will always be satisfied.

To see how the ideas just described may work consider again the restricted theoretical symmetry $v_0 \rightarrow v_1 = v_0 + v'$, where v is the velocity variable, v_0 is the initial theoretical state consisting of the initial ascriptions of velocity, v_1 is the final theoretical state consisting of the final ascriptions of velocity, $+ v$ is the theoretical boost transformation consisting of prescriptions of change in velocity on v_0 or in differences in velocity between v_0 and v_1, and all the ascriptions of velocity and prescriptions of change in velocity are made in the same reference frame. As said, this theoretical symmetry is able to represent the constitutive differences of Galileo's ship empirical symmetry, as well as the change in them and the inertial character of the observable dynamics within the ship in each of the two physical states constituting that empirical symmetry. If completed with a representation of the observable invariant features, which may be the same in the two states but defined relative to different reference frames, this theoretical symmetry will be able to adequately represent the constitutive features of Galileo's ship empirical symmetry and so will have the observational DES with respect to it. Moreover, this complete theoretical symmetry will be properly global in the variables acted on by the main transformation and relevant to the representation of the constitutive differences, i.e. in the velocity variable, because v_0, v_1 and $+ v$ are all usually taken as global either trivially or not as explained above. Hence we have here a properly global theoretical symmetry with the observational DES.

Suppose now that we apply to this theoretical symmetry a gauge theoretical boost transformation. This is just like the theoretical boost transformation $+ v$ from our theoretical symmetry, and it may even be numerically the same (say both prescribe +5 km/h for the same direction and units), or it may be numerically different, but most importantly it is going to apply not on the initial theoretical state v_0 alone but on it together with the final theoretical state v_1. More precisely, I will count theoretical transformations applied to v_0 and v_1 as two gauge transformations. The result in another theoretical symmetry $v_0' \rightarrow v_1' = v_0' + v'$, where $+ v'$ is the same as $+ v$ not because our gauge transformations are numerically equal to the transformation $v_0 \rightarrow v_1$, but because the states we have obtained in that case give rise to the same transformation between them as the original states. The point is that as we obtained the resulting theoretical symmetry via gauge transformations, the resulting theoretical symmetry will have the same observational consequences as the original one. Hence both will have the observational DES with respect to the same empirical symmetries. The idea is now to show that the gauge transformations can be so chosen that the resulting theoretical symmetry will be not properly global like in the example just given but properly local instead.

Again, to avoid any vagueness I will be very specific and will presuppose that we deal not just with an empirical symmetry but with a certain instance of a particular realisation of a specific empirical symmetry. This is of no risk for the generality of my proof, because which is that instance, realisation and empirical symmetry will be left completely unspecified. The only thing that we presuppose about the empirical symmetry is that this is an identifiable empirical symmetry in the sense that its constitutive differences and its constitutive invariant features are both observable and both have to be adequately represented.

Section III. 6. 2. The proof

I am going to prove that from a properly global theoretical symmetry which has the observational DES by virtue of adequately representing the observable constitutive features of a particular instance of a specific realisation of an observable empirical symmetry a properly local theoretical symmetry with the same status with respect to the same instance can always be constructed using suitable gauge symmetries in the sense of observationally complete theoretical symmetries, i.e. theoretical symmetries constituted by theoretical transformations which do not induce any change in observational consequences, and hence which link observationally equivalent theoretical states.

The construction recipe is as follows:

(1) construct new theoretical states and ensure they are observationally equivalent to the original theoretical states;

(2) ensure the new theoretical states are local in all the variables relevant for the representation of the observable constitutive differences;

(3) construct a theoretical transformation between these local states;

(4) ensure this theoretical transformation is local in all the variables relevant for the representation of the change in the observable constitutive differences.

Remarkably, all these stages will be achieved by simply imposing requirements on gauge transformations. There is hardly anything else that will be used, except perhaps the identifying values of variables and a simplifying assumption that the domains of all the states involved have the same cardinality (without putting restrictions as to how it is chosen). The proof thus consists in showing that requirements on gauge transformations allowing to achieve all these stages can be formulated, formulate them, and show that they can always be satisfied.

The notation in the proof is as follows. In the original theoretical symmetry the initial theoretical state is labelled by S_0, the main theoretical transformation by T, the final theoretical state by S_1. The common part of the two states is denoted by S. In the resulting theoretical symmetry the same labels with a prime are used. The (gauge) transformation linking S_0 and S_0' is labelled by T_0, the (gauge) transformation linking S_1 and S_1' is labelled by T_1. After all these labels, variables acted on by a transformation or involved in a state can be indicated in brackets. Variables (or a single variable) are denoted by A if they are acted on by the restricted main theoretical transformation (i.e. contribute in the representation of the observable constitutive differences), by B if they are acted on by the full but not restricted main transformation (i.e. contribute in the representation of the observable constitutive invariant features), by C if they are involved in the states but are not acted on by the main theoretical transformation, and by N if they are not involved neither in the main theoretical transformation nor in the theoretical states prior to introducing the gauge transformations. These letters are unprimed or primed depending of whether the involvement is with respect to the states and the transformation of the original or of the resulting theoretical symmetry. Unspecified variables are denoted by X, while I stands not for variables but for values of variables ensuring the individuation of the items for the state of which these values make part.

In this notation our task reads as follows: starting with a theoretical symmetry with observational DES whose restricted symmetry is constituted by $S_0(A)$, $T(A)$, $S_1(A)$ all global in A, we should arrive at another theoretical symmetry with observational DES whose restricted symmetry is constituted by $S_0'(A')$, $T'(A')$, $S_1'(A')$ all local in A'. I am going to obtain this using gauge transformations T_0, T_1 as depicted in Figure 1 in the Appendix to the present work.

(1) We firstly need gauge transformations T_0, T_1. One option is to construct T_0 and T_1 using the first method of constructing transformations, which consists in attaching prescriptions of change in value of some variable to some or all of the items which belong to the domain of a state initial with respect to a transformation to be constructed, i.e. to the domain of S_0 for T_0 and of S_1 for T_1.

That T_0, T_1 are not given in advance implies in particular that a theory used to build to the original symmetry does not tell us anything about the observational impact of T_0, T_1. Therefore we can always impose as a requirement that T_0 and T_1 be such that they do not induce any change in observational consequences when applied to respectively S_0 and S_1.

Alternatively, we can recur to gauge transformations already given by a theory used to build the original symmetry or by another theory. For instance, we can recur to Galilean mechanics or special relativity, classical electrostatics or classical electromagnetism, and quantum mechanics which say that respectively boosts, electrostatic potential shifts and phase shifts are gauge transformations when they are global and applied on the whole domain available (here the domain of S_0 and S_1 for respectively T_0 and T_1), or we can recur to general relativity and classical electromagnetism which say that local transformations of respectively spatiotemporal variables and the electromagnetic potential are gauge transformations if they have a specific form.

The option of recurring to already given gauge transformations may seem to imply that a properly local symmetry that we wish to construct is already given too. However, if T_0, T_1 are given as prescriptions of change on the domains of S_0, S_1, then S_0', S_1' are still not given until we apply T_0, T_1 to S_0, S_1, i.e. until we apply to a value associated by a state to a given item a prescription of change in value associated by a transformation to that item. Moreover, neither giving T_0, T_1 nor applying them to S_0, S_1 may be sufficient to define T'. For the use of S_0' and S_1' to define T' may result in the possibility for T' to be defined in different ways (see the subsection on constructing theoretical transformations above, as well as (3) below), and thus T' will not be defined until a choice among these ways is made. Besides, not all T_0, T_1 lead to properly local symmetries, otherwise we would not need all the requirements on them specified below. Thus using already given T_0, T_1 is admissible.

Next, as a given or constructed T_0 is gauge when applied to S_0, the resulting state S_0' is observationally equivalent to S_0, and likewise the state S_1' resulting from the application of T_1 to S_1 is observationally equivalent to S_1. Thus S_0' represents the initial physical state as good as S_0 does, and likewise for S_1' as compared to S_1. Moreover, S_0 and S_1 are distinct observationally inequivalent states, for otherwise the same theoretical state would be allowed to represent two observationally distinguishable physical states, and this would contradict the assumption (implied by the observational DES) that the observable constitutive differences are adequately represented. Therefore S_0' and S_1' are distinct observationally inequivalent states, and if they can be linked by a main theoretical transformation, it will be non-trivial and will have to bring the difference in the observational consequences of these states, which will be the same as the difference in the observational consequences brought by T.

(2) We next need to determine which variables T_0 and T_1 should act on non-trivially, and relatedly which variables i n S_0' and S_1' are relevant to the representation of the observable constitutive differences in these states, i.e. play the role of A'.

Firstly, it is clear that T_0 and T_1 should act non-trivially on some variables from S_0 and S_1 and that the variables playing the role of A' should be among those on which T_0 and T_1 act non-trivially. For the variables on which T_0 and T_1 do not act non-trivially will have the same values in S_0' and S_1' as the values they have in respectively S_0 and S_1, and so, supposing that (some of) these variables play the role of A' in S_0' and S_1', the same variables should likewise play the role of A in S_0 and S_1, i.e. be the only relevant ones to the representation of the observable constitutive differences in S_0 and S_1. Hence these variables will be the only relevant ones to the representation of the constitutive differences in both of S_0 and S_0', as well as in both of S_1 and S_1', and thus we will not be able to say that the observable constitutive differences are represented differently in S_0' with respect to S_0, as well as in S_1' with respect to S_1, while that is what we want to be able to do.

Secondly, T_0 and T_1 should act non-trivially at least on A. For otherwise A will have the same values in S_0' and S_1' as the values they have in respectively S_0 and S_1, and so we will not be able to say that the observable constitutive differences are represented essentially differently in S_0'

with respect to S_0, as well as in S_1' with respect to S_1, while that is what we want to be able to do.

Therefore, as T_0 and T_1 are gauge transformations, the variables playing the role of A' should be those the non-trivial action of T_0 and T_1 on which is sufficient to make S_0' and S_1' observationally equivalent to respectively S_0 and S_1 as far as the representation of the observable constitutive differences is concerned, and this while T_0 and T_1 act non-trivially on A. It is clear that the variables which can satisfy the sufficiency requirement just stated can be either A alone, or A together with some of B and/or C and/or N, or some of B and/or C and/or N alone. For completeness I will suppose that the variables playing the role of A' can be among any of A, B, C and N.

Thirdly, as here we are only interested in the representation of the observable constitutive differences, we should only allow T_0 and T_1 to act non-trivially on those variables the non-trivial action of T_0 and T_1 on which is sufficient to make S_0' and S_1' observationally equivalent to respectively S_0 and S_1, and this while T_0 and T_1 acts non-trivially on the variables playing the role of A', as well as on A (whether it makes part of A' or not). This sufficiency requirement is not equivalent to the previous sufficiency requirement above because now we require observational equivalence on the level of whole states and not on the level of the theoretical elements responsible for the representation of the observable constitutive differences alone. Likewise, the variables that can satisfy this more comprehensive sufficiency requirement are not necessarily going to be A' and A alone. This is because the non-trivial action of T_0 and T_1 on A' and A can make S_0' and/or S_1' incompatible with the way features other than the observable constitutive differences are represented in respectively S_0 and/or S_1. In this case respectively T_0 and/or T_1 will also have to act on the theoretical elements accounting for the latter features to make respectively S_0' and/or S_1' observationally equivalent to respectively S_0 and/or S_1 as far as the representation of these features is concerned. Therefore, T_0 and T_1 should act non-trivially at least on the variables playing the role of A', together with A, but in addition they may also act non-trivially on other theoretical elements provided this is due to the reason just explained.

For example, consider a setup of 't Hooft's beam-splitter empirical symmetry with two half-wave plates in the final physical state and none in the initial physical state, where the change in the constitutive differences is represented by a global phase shift on the whole beam, so that the observable constitutive differences are represented by the fact that a wavefunction ψ_1 in S_1 has a phase factor with respect to a wavefunction ψ_0 in S_0. Here a wavefunction variable plays the role of A. As T_0 and T_1 should act non-trivially on A, we can make T_0 and T_1 to modify ψ_0 by a second phase factor and ψ_1 by a third phase factor. Thus T_0 and T_1 may firstly consist of different phase transformations. However, this may be insufficient to make S_0' and S_1' observationally equivalent to respectively S_0 and S_1 as far as the representation of the observable constitutive differences is concerned (unless T_0 and T_1 reduce to global phase shifts) if the observational consequences of modifying a phase factor are constrained by a theory used to build S_0 and S_1 or by another relevant theory. On the other hand, if we rely on the classical electromagnetism with matter more precisely, it says that we can make S_0' and S_1' observationally equivalent to respectively S_0 and S_1 as far as the representation of the observable constitutive differences is concerned provided in T_0 and T_1 the phase transformations have the form $\cdot e^{iq\theta(\mathbf{x},t)}$ and are supplemented with the electromagnetic potential transformations of the form $+ \partial\theta(x_\mu)$. Thus we can make either of T_0 and T_1 to consist not of a transformation of a wavefunction alone but of a joint transformation of a wavefunction and of an electromagnetic potential. If we do so, the wavefunction variable and the electromagnetic potential variable A_μ will together play the role of A'. Moreover, if the electromagnetic potential variable is not used in S_0 and S_1 prior to us introducing the joint transformations in question, the electromagnetic potential variable will play the role of N. For T_0 and/or T_1 to be able to act on N we need to redefine S_0 and S_1 as containing values of that variable, but this redefinition should be made in such a way that it does not change the contribution of other variables involved in the original theoretical symmetry to the representation of an observable empirical symmetry concerned. Arguably, the latter condition is satisfied if S_0 and S_1 are supplemented with zero values of N across both of them and T does not act on N. Now if we included the means of generating representations of the observable constitutive invariant features, e.g. equations, in theoretical states, then it is possible that T_0 and T_1 would have to act not on the wavefunction variable alone and not even on the wavefunction variable and the electromagnetic potential variable alone, but on them both and on equations as well. This is because the action of T_0 and T_1 with the electromagnetic potential variable playing the role of N makes us pass from a context without electromagnetism to a context with electromagnetism, and this may require using different equations to generate representations of the observable constitutive invariant features. In that case, for T_0 and T_1 to ensure that respectively S_0' and S_1' are observationally equivalent to respectively S_0 and S_1, T_0 and T_1 would also have to send equations used in S_0 and S_1 to other equations suitable for being used in S_0' and S_1', in addition to acting on the variables playing the role of A', namely on the wavefunction variable playing the role of A and on the electromagnetic potential variable playing the role of N. Or course, one may argue that a new theoretical symmetry that we are constructing is representationally worse than the original theoretical symmetry in some respects, e.g. because it uses more variables to account for the same features in comparison to the original theoretical symmetry. On the other hand, one may also argue the other way round, i.e. that the new theoretical symmetry is representationally better than the original symmetry, e.g. because particles in 't Hooft's beam-splitter are usually supposed to be charged and thus electromagnetism has to be there anyway. But the point of the proof is not to determine whether properly local symmetries are representationally better overall than properly global symmetries or vice versa. Rather, it is to show that the former symmetries can be constructed out of the latter using requirements on gauge symmetries, and thus to show that one cannot take properly local symmetries to be representationally worse than properly global ones on the grounds that only the latter can have the observational DES with respect to some observable empirical symmetries.

We next need to make both S_0' and S_1' local in A'. This does not mean that both S_0' and S_1' should be local in all the variables on which T_0 and T_1 act non-trivially. Indeed, firstly, if A does not make part of A' in both S_0' and S_1', then both S_0' and S_1' may be global in A. More precisely, it is plausible that the variables playing the role of A in S_0 and S_1 will always be among A' unless the value of A is zero across both S_0' and S_1'. Thus I will only be considering the case where A is either local in both S_0' and S_1' because of its making part of A', or global, and more precisely zero, across both S_0' and S_1' because of its not making part of A'. Secondly, if as explained above the action of T_0 and/or T_1 on A' and A makes S_0' and/or S_1' incompatible with the way features other than the observable constitutive differences are represented in respectively S_0 and/or S_1, S_0' and/or S_1' need not be local in all the theoretical elements involved in the representation of the latter features. Thus it is only the case that S_0' and S_1' have to be either both local in A' including A (when A is among A'), or both local in A' and zero in A (when A is not among A').

To ensure this, recall that being global means attaching the same (change in) value to each item within the domain of application and being local means attaching a different (change in) value to different items within the domain. It follows that, within the domain of application of the transformation in the first understanding and with respect to the variable(s) acted on by it, /1/ a global transformation applied to a global state always yields a global state; /2/ a global transformation applied to a local state always yields a local state; /3/ a local transformation applied to a local state can yield a local or a global state; and /4/ a local transformation applied to a global state always yields a local state.

Therefore, /1/ allows to put values of A to zero by requiring that $T_0(A)$ and $T_1(A)$ be suitable global transformations. Meanwhile, for making S_0', S_1' local in B and C in which S_0, S_1 are local, either we should require using /2/ that T_0, T_1 be global, or using /3/ we should require that T_0, T_1 be local and exclude the possibility in this context for S_0', S_1' to be global. For this, as T_0, T_1 are given or have been already constructed, we can simply check different S_0', S_1' obtained using different local T_0, T_1 and only retain those local T_0, T_1 which yield local S_0', S_1'. Or else we can require for this that T_0, T_1 be identity on some subset of items of S_0, S_1. Finally, /4/ allows to make S_0', S_1' local in A and N, as well as in B and C in which S_0, S_1 are global, by requiring that T_0, T_1 be local in these variables.

For T_0 or T_1 to be local this transformation should associate different amounts of change to different items characterised by S_0 or S_1, and this means that these items should be distinguishable beforehand. To ensure this we will presuppose that S_0 and S_1 include or are supplemented with the individuating distributions of values I allowing to distinguish between the items. E.g. values of spatiotemporal variables can be used because within any given coordinate frame any two items always get associated to them different sets of values of these variables. Of course, associations between sets of values and items vary with coordinate frames, so distinct coordinate frames can associate the same set of values to distinct items. Thus S_0 and S_1 can share the same I (as part of C) e.g. because they represent objects participating in the interior dynamics within two systems (e.g. Galileo's ships or 't Hooft's beam-splitters) as having the same locations in the coordinate frames defined by the observers of these systems.

(3) As by now S_0' and S_1' have already been obtained, we can use the second method of constructing transformations and define T' as a difference, for any variable by values of which S_0' and S_1' differ, between a given final value and a given initial value of that variable, where final values come from S_1' and initial values from S_0'. Which variables T' acts upon is determined by a *strong key fact* by which T' should be equivalent to the combination of the reverse of T_0 followed by T and T_1 in case T_0 is reversible, given that both T' and the combination just mentioned should lead from the same S_0' to the same S_1', or at least by a *weak key fact* by which the combination of T_0 and T' should be equivalent to the combination of T and T_1 given that both combinations should lead from the same S_0 to the same S_1'. Either fact means T' should only act on those variables which are needed to make the square from Figure 1 commute. E.g. if $T_0(N)$ and $T_1(N)$ are identical, T'(N) is identity, and if neither of T, T_0 and T_1 act on C, nor can T'. Thus variables on which T' does not act (denoted by C') and variables on which T' acts (denoted by A'&B') are those whose change by T' is respectively incompatible or compatible with one of the key facts.

We need not have I in S_0' and S_1' to define T', but their presence together with the key facts determines whether T' is defined uniquely. Firstly, if there are I among the values of C' (e.g. if the same I are incorporated in S_0 and S_1 (as part of C) and if T_0, T_1 preserve these I), T' is unique. Secondly, if C' do not provide I and A'&B' do but only for one of S_0' and S_1', then some items characterised by the same values of C' will be distinguishable in one state because of having different values of A'&B' and indistinguishable in another state because of having the same values of A'&B'. In this case T' preserving C' is seemingly underdetermined as to which of the distinguishable items should be matched with which of the indistinguishable items. But due to the indistinguishability of the items the matching options are indistinguishable too, hence no real underdetermination arises. Thirdly, the same holds if neither C' nor A'&B' provide I for either of S_0' and S_1', i.e. when indistinguishable items are matched. Fourthly, if C' does not provide I and A'&B' do for either of S_0' and S_1', then T' is indeed underdetermined because there are several distinguishable ways of matching the distinguishable items of the two domains (or, more precisely, of matching values of A'&B' associated to these items) even if C' is required to be preserved given the key facts. This underdetermination is however not a problem provided among the ways of defining T' we can single out those which make it local in A'.

(4) We deduce from /1/-/4/ that /1'/ two global states can be linked by an identity or a global transformation, /2'/ two local states can be linked by an identity or a global or a local transformation, /3'/ a global state and a local state can only be linked by a local transformation.

Thus by /2'/ T' is not automatically local in all the A' in which S_0' and S_1' are local. Moreover, in the third case above, i.e. when neither C' nor A'&B' provide I for either of S_0' and S_1', T' limited to the matchings of indistinguishable items (i.e. limited to those parts of S_0' and S_1' that are global in both C' and A'&B') cannot be local by /1'/, although the whole T' can still be local.

On the other hand, in the second case above, i.e. when C' do not provide I and A'&B' do but only for one of S_0' and S_1', T' limited to the matchings of indistinguishable items with distinguishable items (i.e. to those parts of S_0' and S_1' where one of S_0' and S_1' is global in both C' and A'&B while another is global in C' and local in A'&B') is necessarily local by /3'/, and hence the whole T' is necessarily local. So one way of making T' local is to ensure that the second case above obtains. For this, if S_0, S_1 have the same I (as part of C), we can e.g. make T_0, T_1 both act on C to alter the same value (or the same set of values) of I into another value among those used in I, while we make say T_0(A,N) identity and T_1(A,N) local on the items whose values are concerned, and both T_0(A,N) and T_1(A,N) local on the other items. (That local transformations act on I needed to define them is not a problem.) However, this hardly suits for I such as values of spatiotemporal variables, so I will also describe another way of ensuring T' is local which it suitable for them too.

Namely, it follows from the definition of global and local transformations adopted above that if one transformation is equivalent to the combination of a second and a third transformation, then /1"/ if from first and the second transformations, or from the second and the third, both are global, respectively the third or the first is either identity or another global transformation; /2"/ if from the first and the second transformations, or from the second and the third, one is global and the other local, respectively the third or the first is another local transformation; /3"/ if the first and the second transformations are local, the third is identity or global or local. Therefore we can make a transformation global or local by considering it as part of a triangle of transformations where the global or local character of the other transformations is suitably chosen.

A triangle is available directly for variables not acted on by T, i.e. C and N, for which by a *reduced weak key fact* T_1 is equivalent to the combination of T_0 and T' (Figure 2). For A and B we can transform our square into two triangles. For this we define T'': $S_0 \rightarrow S_1$' using the second method of constructing transformations and determine the variables T'' acts upon using another reduced weak key fact by which T'' is equivalent to the combination of T and T_1. T'' also gets involved in a third reduced weak key fact by which T'' is equivalent to the combination of T_0 and T' (Figure 3).

Note that by construction S_0, T'', S_1' is another theoretical symmetry observationally equivalent to both the original theoretical symmetry S_0, T, S_1' and the resulting theoretical symmetry S_0', T', S_1'. Besides, the theoretical symmetry S_0, T'', S_1' can be a mixed theoretical symmetry, i.e. such that its states and transformation are neither all the three global nor all the three local in the variables playing the role of A'', i.e. responsible for the representation of the observable constitutive differences in that symmetry. Indeed, we get a mixed theoretical symmetry if S_0 is global in at least one variable which is among those playing the role of A'' and among those playing the role of A', given that S_1' will then have to be local in the same variable. Thus while constructing from a properly global symmetry a properly local symmetry we have also constructed a mixed theoretical symmetry.

The character of T' can now be determined from the fact that in both triangles where it is present it is the third transformation from the rules /1"/-/3"/ above. Thus in general T' can be made local via /2"/, or via /3"/ e.g. if we require the other two transformations to be identical on some restriction of their domain (which precludes the third transformation from being global) while not being identical on their whole domain (which precludes the third transformation from being identical). However, not all solutions suit for particular variables.

T'(A) cannot be made local via /2"/, for if T_0(A) is global and T''(A) local, by /2"/ T_1(A) should be local and so S_0' will be zero in A while S_1' local in A contrary to (2); and if T_0(A) is local and T''(A) is global, by /1"/ T_1(A) should be global and so S_0' will be local in A while S_1' zero in A contrary to (2). Thus T'(A) should be made local by choosing a local T_1(A), which by /2"/ makes T''(A) local, and a local T_0(A), which allows to use /3"/ provided the requirements for /3"/ above are satisfied, i.e. provided we require that T_0(A) be different from T''(A) except for some subdomain of S_0. In other words, although we allowed the case where T_0 and T_1 both bring A to zero, if we wish

T'' to be local in A, T_0 and T_1 should only act so as to make both S_0' and S_1' local in A. And this is what we expect: it would be strange if we got a local T''(A) in the case where S_0' and S_1' are both zero in A.

Similarly, T'(N) or T'(C) with global S(C) cannot be made local via /2''/, for if one of T_0 and T_1 is global in C or N and another local in C or N, by /1/ one of S_0' and S_1' will be global in C or N and by /4/ the other of S_0' and S_1' will be local in C or N contrary to (2). Thus T'(N) or T'(C) with global S(C) should be made local via /3''/ by choosing both T_0 and T_1 local in N or C and by imposing on them the requirements for /3''/ above.

Also T'(C) with local S(C) can be made local via /3''/ as just described provided we ensure that S_0'(C) and S_1'(C) are local. It is fortunate that one of the ways of ensuring this proposed in (2), namely to require that T_0(C) and T_1(C) be identity on a subdomain of S, implies (if for T_0(C) and T_1(C) the same subdomain is concerned) that these transformations are identical on this subdomain and so allows to satisfy the requirements for /3''/ at the same time. Besides, T'(C) with local S(C) can be made local via /2''/ by choosing one of T_0(C) and T_1(C) to be global and another local while ensuring, e.g. as proposed in (2), that the state resulting from the action of the latter transformation is local in C.

Finally, if S_0(B), T(B), S_1(B) are all global, T'(B) can be made local exactly as it was done with T'(A). While if S_0(B) is global and S_1(B) is local, and hence T(B) local by /3'/, we can choose T_1(B) to be global, which makes T''(B) local by /2''/, then choose T_0(B) to be local and use /3''/ and the requirements above, i.e. require that T_0(B) be different from T''(B) except for some subdomain of S_0. Meanwhile, if S_0(B) is local and S_1(B) is global, and hence T(B) local by /3'/, we can choose T_1(B) to be local, and make T''(B) local by using /3''/ and the requirements above, i.e. require that T_1(B) be different from T''(B) except for some subdomain of S_1 and some subdomain of S_0 which share the same I (as part of C), then choose T_0(B) to be global and use /2''/. Finally, if S_0(B), T(B), S_1(B) are all local, we can choose T_1(B) to be global, which makes T''(B) local by /2''/, and choose T_0(B) global, which makes T'(B) local by /2''/.

This concludes the proof.

Chapter III. 7. Consequences of the proof

What is shown by the proof most directly is that whenever a properly global symmetry has the observational DES with respect to an instance of a particular realisation of an observable empirical symmetry, a properly local symmetry and a mixed theoretical symmetry can always be constructed which have the observational DES with respect to the same instance of the same realisation of the same observable empirical symmetry. Therefore, for all the recognised empirical symmetries that have been matched with properly global symmetries so far it should be possible to find or construct other kinds of theoretical symmetries as well which will be able to represent the observable constitutive features of these empirical symmetries as good as properly global symmetries do. More generally, this motivates a search for more theoretical symmetries with the observational DES. Finding them is useful to evaluate how different various theoretical symmetries with the observational DES may be. Of course, the search can be extended to the representations of new observable empirical symmetries as well, which can themselves be found by checking which other phenomena satisfy the definition of generic empirical symmetry given above.

Secondly, before the proof I formulated an unpragmatic argument to the effect that only properly global symmetries have the ontological DES, which is a claim making part of the reformulated nuanced traditional position. This argument was based on the crucial assumption by which some observable empirical symmetries yield the observational DES to properly global symmetries alone. It followed from this crucial assumption that only properly global symmetries can have the ontological DES in such cases because no other candidates are available, and it was suggested to extend this conclusion to cases where such candidates are available as well. However, the proof shows that this crucial assumption does not hold. Hence the unpragmatic argument given above cannot work either. And as also shown above, no other convincing argument to the same or similar effect has been proposed so far. Therefore, a second consequence of the proof is that the traditional position in any of the formulations considered cannot be accepted at present. In particular, at that moment nothing warrants the claim that only properly global symmetries have the ontological DES. Thus it is possible that properly local symmetries or mixed symmetries have the ontological DES as well or alone.

Thirdly, the road now gets cleared for considering other position than the traditional one. If we put pragmatic preferences aside, we see that not only is it highly questionable whether properly global symmetries have the ontological DES, but it is also questionable whether the distinction between theoretical symmetries that have the ontological DES and those which do not in any way parallels the global/local distinction for theoretical transformations, the global/local distinction for theoretical states or these two distinctions taken together. Thus not only should we consider options other than the traditional one among those yielded by the global/local distinctions, but we should also go beyond the global/local distinctions and consider other possibly useful distinctions. For instance, for the question of which theoretical symmetries have the observational DES and which do not the most relevant distinction is between gauge symmetries and non-gauge symmetries in the sense of respectively observationally complete and observationally incomplete theoretical symmetries, while neither of the global/local distinctions is directly relevant to that question. Therefore, likewise the distinctions other than the global/local distinctions should be sought that are relevant to the question of which theoretical symmetries have the ontological DES, and more generally novel ways of addressing this question should be explored.

That does not necessarily mean that the global/local distinctions should be completely dismissed, but the proof shows that at least they are not decisive. This joins other reasons by which the choice of global/local distinctions as a basis for one's ontology may have to be reconsidered, such as a certain contingency in whether a theoretical transformation or a theoretical state is classified as global or local. For instance, the adopted global/local distinction for states is dependent on conventions, and so a global state may become a local state and vice versa if they are changed (in the language of fibre bundles the relevant situation is where one changes a cross-section without changing the connexion). And given the definition of the global/local distinction for transformations we have adopted, the global or local character of a transformation defined by a couple of such states is thus dependent on conventions as well. But the second definition of the global/local distinction for transformations that we have not adopted above fares no better, because as explained whether a transformation is global or local on the second definition depends on how we express it. This is not the case for the definitions we have adopted, and they also have the advantage of giving a unique answer as to whether a transformation or a state are global or local once conventions are fixed. So some global/local distinctions may fare better than some other global/local distinctions on some points, but it is unclear whether any is fundamental. Thus whether one is convinced that some global/local distinction matters or not, the choice of the relevant distinction has to be made with care.

Fourthly, the proof shows the importance of theoretical states and theoretical transformations in constructing other theoretical states, theoretical transformations and theoretical symmetries and in determining their properties, such as their global or local character and the variables acted on by the main transformations. So this illustrates and develops the discussion of construction of transformations and states given above. In particular, we see the usefulness in this respect of the attention to theoretical states, which is itself a consequence of the empirical approach as shown above, for it is the inclusion of states in the analysis that has allowed us to formulate a proof as it is (although considering transformations alone is not useless given in particular the rules /1"/-/3"/ above). Besides, we see from the proof that the same theoretical state (S_0) can give rise to a non-gauge symmetry (S_0, T, S_1) and to a gauge symmetry (S_0, T_0, S_0') in the sense of an observationally incomplete and an observationally complete theoretical symmetry respectively. Thus whether a theoretical symmetry is non-gauge or gauge in that sense is determined by a state and a transformation or by two states rather than by a single state. This parallels the way an empirical symmetry happens to be identifiable or non-identifiable. Indeed, as we saw above, the same state of a square tile yields a non-identifiable empirical symmetry when coupled to a 90-degree rotated state and an identifiable empirical symmetry when coupled to a state rotated by a number of degrees that is not equal to 90 degrees multiplied by a natural number. This shows that identifiability of an empirical symmetry likewise depends on a state and a transformation or on two states rather than on a single state. More generally, according to the two ways of obtaining theoretical symmetries given above, a theoretical symmetry can be obtained from a state and a transformation or from two states. One may argue that similarly an empirical symmetry can be obtained from a state and a transformation or from two states, particularly if these two means are applied in order to obtain empirical symmetries instantiated via the first way and via the second way respectively.

Fifthly, the proof crucially relies on gauge symmetries in the sense of observationally complete theoretical symmetries. So it shows that gauge symmetries and gauge transformations are highly relevant to the analysis of DES, despite the claims in the literature on DES going in the opposite direction ([Greaves and Wallace 2014, p. 87]; [Teh 2016, p. 116]) and despite the fact that, as explained above, gauge symmetries are unlikely to have any identifiable DES. More precisely, gauge symmetries are shown by the proof to be relevant to the analysis of DES as follows. Firstly, we see from the proof that they are a powerful means of generating theoretical symmetries with the observational DES given a single theoretical symmetry with the observational DES. Secondly, they make possible the proof above, and that proof in turn invalidates the crucial assumption of the unpragmatic argument concerning the ontological DES. Thirdly, the proof also implies that which theoretical symmetries have the ontological DES may depend on the interpretation of gauge symmetries. This last point has remained in shadow up to now, so let us consider it in a bit more detail.

In our basic square we have three theoretical symmetries with the observational DES with respect to the same observable empirical symmetry, namely a properly global symmetry (S_0, T, S_1), a possibly mixed symmetry (S_0, T'', S_1') and a properly local symmetry (S_0', T', S_1'), as well as two gauge symmetries (S_0, T_0, S_0') and (S_1, T_1, S_1'). Now a theoretical symmetry can be interpreted *passively*, i.e. with the two theoretical states redescribing the same physical state and the theoretical transformation describing a passage between these redescriptions, or *actively*, i.e. with each of the two theoretical states describing a different physical state and the theoretical transformation describing a passage between the two physical states, whether in the sense of a comparison or in the sense of a genuine physical transformation. Thus if both gauge symmetries are interpreted passively, S_0 and S_0' describe a single physical state, and likewise S_1 and S_1' describe a single physical state observationally distinguishable from the first, so we have just one empirical symmetry here. Then, if at most one theoretical symmetry can have the ontological DES with respect to a given instance of a particular realisation of a specific empirical symmetry, it follows that at most one of the three non-gauge theoretical symmetries has the ontological DES in this situation, although all the three theoretical symmetries have the observational DES here. If on the other hand both gauge symmetries are interpreted actively, then we have two instances of a given particular realisation of the same empirical symmetry here, and so at most two of the three theoretical symmetries with the observational DES will have the ontological DES in this situation. Finally, if one of the gauge symmetries is interpreted passively and another actively, we will have the latter situation again as far as the number of instances and the maximal number of theoretical symmetries with the ontological DES is concerned. However, now the instances will share one of their physical states, while their other physical states will be observationally distinguishable from the first physical state and observationally indistinguishable but unobservably different from each other. Therefore, at most three of the four theoretical states S_0, S_0', S_1, S_1' will now be able to adequately represent the unobservable underpinnings of the three distinct physical states, while the remaining theoretical state among the two linked by a passively interpreted gauge transformation will be unable to do so. In sum, the proof thus shows that the ontological interpretation of non-gauge theoretical symmetries as well as of their states and transformations may depend on the ontological interpretation of gauge symmetries.

Conclusion

I started in Chapter I. 1 with explaining why we should study the ontology of theoretical symmetries in physics. This was because such a study is an important instance of a reasonable 'partwise' approach towards the ontology of science. The importance of this instance was said to lie in particular in theoretical symmetries' links with other theoretical elements and in the fact that theoretical symmetries are present in virtually any theory within physics.

In Chapter I. 2 I presented some of the ways in which the ontology of theoretical symmetries is usually addressed. These include the discussion of the universal and restricted observationally complete external symmetries, the discussion of symmetries involving 'indistinguishable' and distinguishable objects, the gauge argument, as well as the IES based on Noether's theorems and Hamilton's principle. This presentation allowed me to dwell on some topics which are useful for the subsequent discussion but which it would not be convenient to present later in the text. It also provided me with some examples of assignments of the ontological significance with which DES could next be compared.

Another role of the same presentation was to illustrate the application of my vocabulary of theoretical states, observationally complete symmetries and so on to some traditional topics and hence to justify its use for the topic of DES and beyond. On the other hand, some freedom was left concerning how wide the application of this vocabulary should be. My distinction between the theoretical approach and the empirical approach, for instance, could be applied with some stretching to more topics among those I have discussed. For example, as explained in Section I. 2. 2 Leibniz and Clarke's debate can be analysed in the direction going from the physical level to the theoretical level while Earman and Norton's hole argument goes in the opposite way, and likewise the link between observation and observational consequences of a theory mainly addressed in Chapter I. 1 in the direction from theory to the world could also be considered in the opposite direction. However, I only concentrated on the application of the distinction between the two approaches to the discussion of 'indistinguishable' and distinguishable objects, because this is where the theoretical approach seems to have a potential for being more efficient than the empirical approach, and so this is a context which can be contrasted with DES where it is the empirical approach that is going to be exhibited as more efficient.

The brief presentation of DES occupies Chapter I.3. One aim here is to motivate the choice of texts to be analysed in Part II. Another is to give an overview of DES as described in the literature on DES and to raise some questions about DES which are to be answered in what follows. A third aim is to compare DES with the topics from the previous chapter, and by now we are able to see that this comparison is to the advantage of DES just as announced.

Indeed, as explained none of these other topics seem to attribute the actual ontological significance to all the constituents that I have distinguished within a theoretical symmetry, namely to the identical part of the theoretical states, the differing parts of the theoretical states and the theoretical transformations between the states. The strongest attribution we have seen is in the Newtonian interpretation of theoretical symmetries, according to which the differing part of the theoretical underpinnings of one of the theoretical states can be matched with what obtains in the world, the differing part of the theoretical underpinnings of other theoretical states can be matched with what could obtain in the world and theoretical transformations inducing these differences between theoretical underpinnings can be matched with passages either from an actual location in the world to a possible location or from a possible location to another possible location. As said, this interpretation is only a weak strengthening with respect to the Leibnizian interpretation of theoretical symmetries, and yet this strengthening seems to bring enough trouble in form of Leibniz's objections and the hole argument, in particular.

It is even more noteworthy, then, that DES can bring about much stronger assignments of ontological significance than those of the Newtonian interpretation and this while being perfectly acceptable. To see this note that a theoretical symmetry has the observational DES in the empirical approach if it is able to adequately represent both the constitutive invariant features and the constitutive differences of an observable empirical symmetry, and so given the assumption of minimal structural similarity this implies that this theoretical symmetry should have partially identical theoretical states (some of) whose invariant theoretical elements match with the observable constitutive invariant features and (some of) whose theoretical differences match with the observable constitutive differences of that empirical symmetry. Hence assigning the observational DES to a theoretical symmetry implies that both the invariant part of its theoretical states and the differing parts of its theoretical states have actual counterparts in the world, and similarly for theoretical transformations bringing about these theoretical differences particularly when we deal with the first way of realising empirical symmetries. In this way all the constituents of a theoretical symmetry, namely the identical part of the theoretical states, the differing parts of the theoretical states and the theoretical transformations between the states, get assigned actual counterparts in the context of the observational DES. So in terms of these constituents the observational DES can be said to bestow on theoretical symmetries not only an ontological siginificance stronger than that bestowed by the Leibnizian and the Newtonian interpretations, but also the strongest ontological significance there can be, because nothing can be stronger given some constituents than the attribution of the actual ontological significance to the totality of these constitutents.

But at the same time as promised in Chapter I. 3 the analysis of DES allows us to go beyond even this strongest assignment of the ontological significance to theoretical symmetries. This is because it suggests us to adopt a more nuanced classification of the constituents of theoretical symmetries than that I was mentioning so far. This other classification is obtained by distinguishing within each of the previous constituents between observational consequences and theoretical underpinnings. The observational DES then turns out to assign the strongest ontological significance only to the identical observational consequences, to the differing observational consequences and to the transformations between the latter. The question which remains is then whether the theoretical underpinnings of all of the latter constituents have an independent ontological significance in addition, and answering it requires to establish whether the ontological DES holds for a theoretical symmetry concerned.

In Part II the main literature on DES is summarised and analysed. This part thus shows in particular how the discussion of DES originated and was developing, and so it also has a historical interest. But the main aim is to understand whether the proposed accounts of DES are adequate, and if not, where and why they fail and how to amend this. It is when trying to make precise what the literature on DES speaks about that one obtains such elements of my account as the definition of DES in the two approaches via the notions of representation and instantiation as well as the distinction between my two kind of DES. And it is also when assessing this literature that the need becomes especially apparent for an account of DES based on clear principles rather than on unsystematised discussions of particular cases.

More precisely, here is what is unsatisfactory about the previous analyses of DES: empirical symmetries are only given through examples and their definitions are imprecise; a distinction between a weaker DES and a stronger DES is not clearly defined and the identifiability of empirical symmetries and DESs is not discussed; different approaches to determining which theoretical symmetries have which kind of DES are neither clearly distinguished nor argued for; it is not explained what a theoretical symmetry should look like in general for it to have a certain kind of DES and why, as well as why it is theoretical transformations rather than theoretical states that would ensure a DES; the global/local distinction presumed to be relevant to the analysis of DES is given many definitions without studying whether they are equivalent and if not which one may be of relevance to the analysis in question; the traditional position by which only global theoretical symmetries have (a stronger) DES is not supported by any convincing motivation whatsoever; the importance of gauge symmetries is completely overlooked.

So instead in Part III I present a comprehensive set of novel results: a detailed formalisation of the notion of empirical symmetry; a discussion of two ways of realising empirical symmetries; a discussion of identifiability of empirical symmetries; a demonstration of why gauge symmetries are unlikely to have any identifiable DES; a distinction between the observational DES and the ontological DES; a distinction between the theoretical approach and the empirical approach; a general argument as to why the former is more promising than the latter in determining how a theoretical symmetry with the observational DES looks like; a description of what a theoretical symmetry with the observational DES looks like in the empirical approach and an explanation of how this description arises from the representational needs and the physics; an evaluation of the quantity of theoretical symmetries with the observational DES per observable empirical symmetry; a discussion of the representation of the relationship between the physical states in two ways of realising empirical symmetries; an explanation of why it is theoretical states and theoretical elements responsible for the representation of the constitutive differences that ensure most of DES; a motivation for a choice of the global/local distinction of possible relevance to DES, and its extension from theoretical transformations to theoretical states; a critique of the main arguments in favour of the traditional position and the viable reformulation of the nuanced traditional position; an assessment of the support to the reformulated nuanced traditional position via a pragmatic and an unpragmatic argument; a constructive proof showing how to generate specific novel theoretical symmetries with the observational DES from the original ones; an assessment of the presumed importance of the global/local distinction for the ontological DES in light of the proof; and an explanation of how gauge symmetries are relevant to the analysis of both the observational DES and the ontological DES.

Where does my analysis lead us? Some roads are indicated towards the end of this work, where it is suggested to seek for more empirical symmetries, for more theoretical symmetries with the observational DES and for other distinctions potentially relevant to the ontological DES, in particular. But I would like now to also mention a road which goes beyond the tasks internal to the analysis of DES itself. This other road involves explicitly analysing the opposition between DES and IES which usually lurks at the background of the discussions of DES.

One way to do so is to ask what is direct about DES and what is indirect about IES. My idea is then to notice that in the case of DES ontologically significant observational consequences are taken to belong to a theoretical symmetry itself while in the case of IES they are taken to belong primarily to another theoretical element implied by a theoretical symmetry, such as a Euler-Lagrange equation or a continuity equation. Hence what is indirect about IES is that another theoretical element is interposed between theoretical symmetries and the relevant observational concequences. And so what DES implies about a theoretical symmetry is primarily implied by IES of that theoretical element instead. This suggests that DES and IES function in the same way but have a different focus: DES is concerned with the ontological significance of theoretical elements closest to relevant observational consequences and IES with the ontological significance of theoretical elements more remote from these observational consequences. This allows to generalise IES so that it applies to such remote theoretical elements whether they are theoretical symmetries or not. This also allows to actually overcome the binary opposition between DES and IES in favour of an analysis of the ontological significance of a range of theoretical elements classified according to their remoteness from given observational consequences. Whether that makes us happy or not, this means that we are back to such traditional questions for the general philosophy of science as to how remote theoretical elements can be for them to still enjoy an independent ontological significance on the basis of the observational consequences' empirical success.

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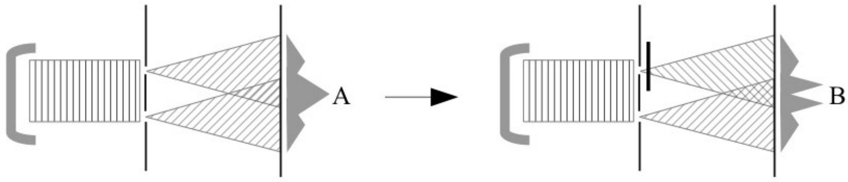
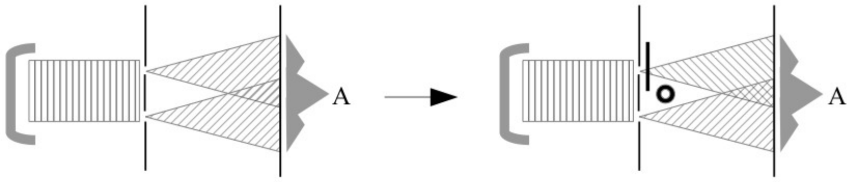
Appendix

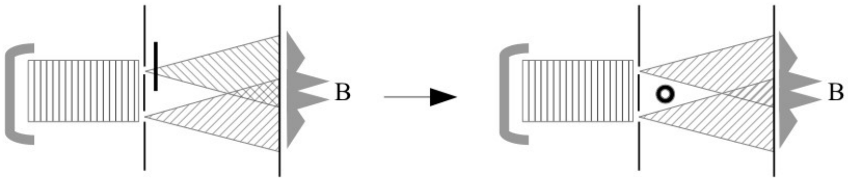
Setups of 't Hooft's beam-splitter empirical symmetry



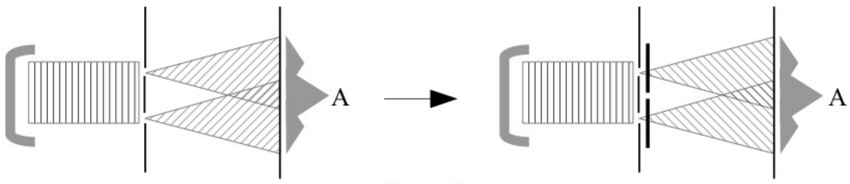
Setup 1



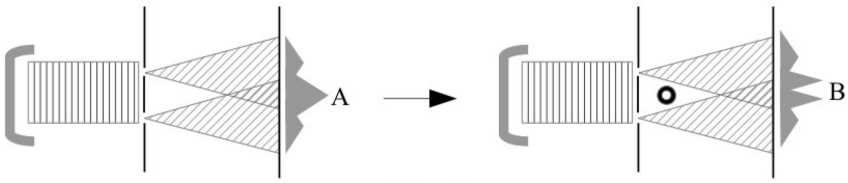
Setup 2a



Setup 2b



Setup 3



Setup 4

Table: The recognised empirical symmetries[8]

<table>
<tr><td>Recognised empirical symmetry</td><td>Faraday's cage</td><td>Galileo's ship</td><td colspan="3">Einstein's elevator</td><td>'t Hooft's beam-splitter</td></tr>
<tr><td>Scale</td><td colspan="5">#Macroscopic</td><td>#Microscopic</td></tr>
<tr><td>System</td><td>*Cage</td><td>*Ship</td><td colspan="3">*Elevator</td><td>#A beam-like system</td></tr>
<tr><td rowspan="5">Constitutive invariant features</td><td colspan="3">Observable dynamics within a system plus its outcomes</td><td>-</td><td>Observable dynamics within a system plus its outcomes</td><td>Outcomes of a dynamics within a system</td></tr>
<tr><td colspan="3">*Dynamics mechanical, gravitational, electromagnetic</td><td>-</td><td>*Dynamics mechanical, gravitational, electromagnetic</td><td>*Dynamics quantum, electromagnetic</td></tr>
<tr><td>*Lighted candles</td><td>*Falling drops</td><td>*Whether the light bends, whether we stand on the ground</td><td>-</td><td>*Whether the light bends, whether we stand on the ground</td><td>#Interference pattern</td></tr>
<tr><td colspan="4">Relationship of a system to a suitable reference</td><td>-</td><td>-</td></tr>
<tr><td>Whether there are sparkles</td><td>Whether there is a certain relative uniform rectilinear displacement</td><td colspan="2">#Whether there is a certain relative displacement</td><td>-</td><td>-</td></tr>
<tr><td rowspan="4">Kinds of constitutive differences</td><td rowspan="4">#Differences in relative charge</td><td rowspan="4">#Differences in relative uniform rectilinear displacement</td><td rowspan="2">-</td><td rowspan="2">-</td><td>Differences in relative accelerated displacement</td><td rowspan="2">Some differences in equipment</td></tr>
<tr><td>#</td></tr>
<tr><td colspan="3">Combinations of observable indicators (massive bodies and switched-on engines)</td><td rowspan="2">#Combinations of observable indicators (half-wave plates and switched-on solenoids)</td></tr>
<tr><td>#</td><td>#</td><td>*</td></tr>
<tr><td>*Constitutive differences compatible with the</td><td>- No / some sparkles on the outer boundaries</td><td>- No / some relative uniform rectilinear</td><td>- (1') No massive body, no working engine (free</td><td>- (2a') No massive body, no working</td><td>- (1') No massive body, no working engine (free float) / a massive</td><td>- (2a) No half-wave plate, no switched-on solenoid / a half-</td></tr>
</table>

[8] Proposed primary specifications are denoted by "#", secondary specifications by "*". All "/" denote a passage from one physical state to another. All "no / some" cases can also be replaced by the cases with two different realisations of the relevant observable differences.
For Galileo's ship and Faraday's cage examples are taken from Galileo [1632/1967] and Maxwell [1881] quoted by Healey [2009, pp. 698-699]. For Einstein's elevator empirical symmetry the three columns from right to left correspond to the realisations of this empirical symmetry satisfying respectively the first, second and third observable invariance conditions from my analysis of Brading and Brown's article. For Einstein's elevator and 't Hooft's beam-splitter empirical symmetries the phenomena and setups are numbered as in my analysis of Brading and Brown's article.

constitutive invariant features		displacement - No / some friction - No / some wake	float) / a massive body, no working engine (free fall) - (3') No massive body, no working engine (free float) / two massive bodies, no working engine	engine (free float) / a massive body, a working engine - (2b') A massive body, no working engine (free fall) / no massive body, a working engine	body, no working engine (free fall) - Standing still on the surface of a massive body / no massive body, a working engine	wave plate, a switched-on solenoid - (2b) A half-wave plate, no switched-on solenoid / no half-wave plates, a switched-on solenoid - (3) No half-wave plates, no switched-on solenoids / two half-wave plates, no switched-on solenoids
*Similar observable differences incompatible with the constitutive invariant features	- Possibly, sparkles of varying intensity on the outer boundaries in one of the states	- No / some accelerated relative displacement	- (4') No massive body, no working engine (free float) / no massive body, a working engine			- (1) No half-wave plates, no switched-on solenoids / a half-wave plate, no switched-on solenoids - (4) No half-wave plates, no switched-on solenoids / no half-wave plates, a switched-on solenoid

Figures for the proof

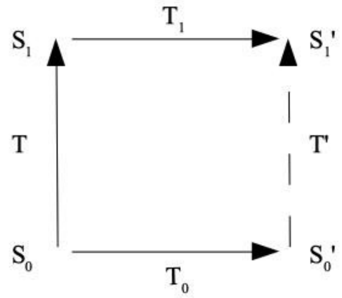


Figure 1

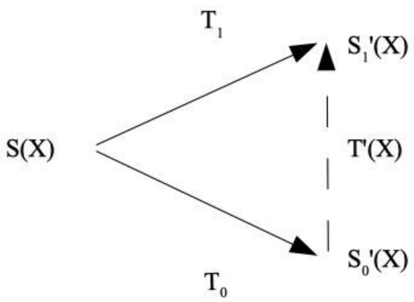


Figure 2

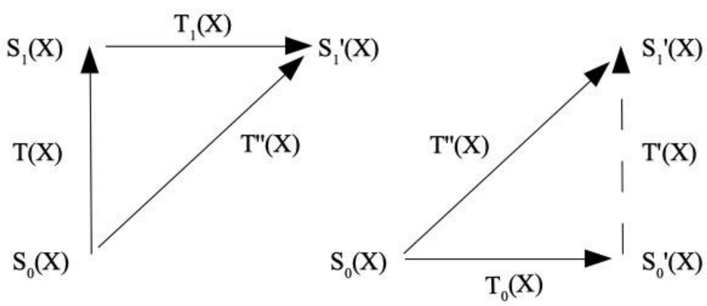


Figure 3