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THE METHODOLOGICAL CHARACTER OF SYMMETRY PRINCIPLES[1]

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Abstract

In this paper, I argue that symmetry principles in physics (in particular, in quantum mechanics) have a methodological character, rather than an ontological or an epistemological one. First, I provide a framework to address three related issues regarding the notion of symmetry: (i) how the notion can be characterized; (ii) one way of discussing the nature of symmetry principles, and (iii) a tentative account of some types of symmetry in physics. To illustrate how the framework functions, I then consider the case of the early formulation of quantum mechanics, examining the different roles played by symmetry in this context. Finally, I raise difficulties for ontological and purely epistemological interpretations of symmetry principles, and offer a methodological alternative.

1. Introduction

There's no doubt that symmetry principles play a crucial role in physics. They also play a significant role in the philosophical reflection about physics. One can develop different perspectives about the nature of theoretical practice by examining different roles that symmetry principles play in scientific activity (see, for instance, van Fraassen 1989: 233-289, Hughes 1989, and Brading and Castellani (eds.) 2003). Not surprisingly, there have been a number of discussions about the nature of symmetry principles (see, e.g., Weyl 1952, Wigner 1967, and more recently, Kosso 2000*a*, 2000*b*, and 2000*c*). For the most part, the discussion has focused on specific details of the physics, which can only be praised, of course. But, to obtain a broader perspective, it's important also to examine whether symmetry principles have an ontological significance as well.

In this paper, I will discuss this aspect of the nature of symmetry principles: which ontological consequences (if any) they have. First, I sketch a framework to conceptualize the issue, reviewing, in particular, the distinction between symmetries in the laws and

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symmetries in the solutions. I then apply the framework to discuss the different uses of group theory in quantum mechanics by Weyl (1931) and Wigner (1931). I argue that Weyl developed a foundational approach with regard to symmetries in the laws, whereas Wigner used group theory for applications of quantum mechanics, emphasizing symmetries in the solutions. Both approaches (Weyl's and Wigner's) are necessary to understand the content of quantum mechanics, but they are not sufficient, given that the approach via Hilbert spaces developed by von Neumann (1932) (or something along its lines) is also needed. I close the paper by returning to the issue of the nature of symmetry principles in quantum mechanics, and I argue, in light of the discussion of Weyl, Wigner and von Neumann, that symmetry principles play an important heuristic and methodological function, but they play no ontological role.

2. A Framework: Symmetry in the Laws and Symmetry in the Solutions

In order to examine the role of symmetry in physics—and for brevity considerations, I'll focus the discussion only on quantum mechanics—it's important to have a framework to provide some guiding questions and tools to analyze how symmetry is used in the formulation of quantum theory. The framework has three main components: (i) a characterization of the notion of symmetry, (ii) a discussion of the nature of symmetry principles, and (iii) a tentative account of some types of symmetry in physics. These components provide a context to examine the broader issue of the nature and role of symmetry. The usefulness of the framework will emerge from the way it illuminates the latter notion in physics. Briefly, the framework can be presented in the following terms.

2.1. The notion of symmetry

In its most general formulation, symmetry is a transformation that leaves the relevant structure invariant (see van Fraassen 1989: 233-348, van Fraassen 1991: 21-76, and Weyl 1952). Of course, what counts as *relevant* changes from context to context. In this sense, the notion of symmetry has an inherently *pragmatic* character. It depends on what users take to be relevant in a given context, and this will determine which features of the structure in question we aim to preserve, and which we don't. This doesn't mean that it's enough for users to *decide* which features are going to be preserved for the latter to be actually preserved. There are *objective* constraints to be met—constraints whose obtaining doesn't depend on the particular users or context. For example, no transformation leaves the cardinality of natural numbers invariant while establishing a one-to-one and onto mapping from natural to real numbers. In this sense, it's an objective fact about these structures that no such invariance can be found. One cannot simply *decide* to create invariance. However, the pragmatic character remains. After all, it's still crucial to decide which components of a structure are taken to be *relevant* in the structure preservation process. For the same reason, the notion of invariance is also context dependent. In different contexts, different kinds of structure are preserved—that is, different parts of the structure are left invariant.

As is well known, this notion of structure preservation receives its mathematical formulation via the notion of isomorphism. It's thus not surprising that isomorphism is also a *context-dependent* notion: different kinds of structure are preserved in different contexts (see also van Fraassen 2002: 22-24). For instance, in arithmetic, what is preserved between two isomorphic structures, besides the ordering among natural numbers, are the properties of the arithmetical operations over such numbers. In set theory, the crucial features to be preserved by isomorphic structures are quite different, having to do with the properties of the membership relation among sets. Or, in linear algebra, other properties are preserved: the isomorphism preserves the mathematical properties of vectors, which *strictly speaking* have nothing to do with the properties of set membership.[2]

When the notion of invariance is highlighted, it's important, again, to be clear about the *kind of things* that are left invariant. An important type of invariance here is the invariance of truth-values. A mathematical formulation of this type of invariance is provided by the notion of elementary equivalence. That is, if two first-order structures are elementarily equivalent, then every sentence that is true in one structure is true in the other, and vice

[2] Unless one adopts a radical form of set-theoretical reductionism, according to which vectors *are* a particular kind of set, the two domains (set theory and linear algebra) are distinct from one another. But this radical reductionism doesn't seem to be justified. In particular, it leaves a number of issues about mathematical

versa. The invariance, in this case, has to do with the truth-values of the sentences under consideration.

But what do the notions of symmetry, isomorphism, and elementary equivalence have in common? The discussion above implicitly provides an answer to this question. The three notions can be put into important *heuristic* uses. If two structures are isomorphic, then they are elementarily equivalent.[3] Suppose that a sentence *P* is true in a structure *S*. Then by establishing that *S* is isomorphic to a structure *S′*—or is elementarily equivalent to *S′*—we can immediately conclude that *P* is also true in *S′*. This is a *transfer principle*, a principle that allows one to transfer results from one domain into another. What symmetry provides is precisely a form of transfer principle. By leaving some structure invariant, symmetry allows us to extend results from one domain into a different one, based on the structural similarity between the domains. For example, with the formulation of nonstandard analysis, Abraham Robinson explored various forms of transfer principles, using in particular model-theoretic results, such as the compactness theorem, as the basis for model construction in mathematics.[4] By judiciously choosing the models to explore, Robinson managed to establish important new results, including results about the mathematical foundations of quantum mechanics, such as the invariant subspace problem[5] (see Robinson 1974: 195-201).

Thus, due to the heuristic use that the notions of symmetry, isomorphism, and elementary equivalence can be put into, there is an important relation between them. Symmetry principles provide a way of exploring the heuristic role of these notions.

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practice completely unsettled. Nothing in the practice *requires* such a reductionism, and typically, vectors are *not* formulated in terms of sets.

[3] The converse is not true in general, of course, given the existence of nonstandard models of arithmetic. Any two such models are elementarily equivalent, but their domains typically have different cardinalities, and so they are not isomorphic.

[4] The compactness theorem provides a systematic strategy to build models of various kinds of theories. According to the theorem, if *M* is a set of sentences such that every finite subset of *M* is consistent, then *M* is consistent (for a discussion, see Robinson 1974: 13-14).

[5] What Robinson proved, in joint work with A. Bernstein, was the following theorem: Let *T* be a bounded linear operator on a Hilbert space *H*, and let $p(z) \neq 0$ be a polynomial with complex coefficients, such that $p(T)$ is compact. Then *T* leaves invariant at least one closed linear subspace of *H* other than *H* or $\{0\}$ (for a discussion and proof, see Robinson 1974: 195-201). It's worth noting that this result is established based on symmetry considerations.

2.2. Symmetry principles

There are, of course, several ways of formulating symmetry principles. The formulation I'll adopt is the one that highlights the *heuristic* role of symmetries. As I'll argue below, this is the most significant role of symmetries in physics (particularly in quantum mechanics), and it's important to be clear about this role to begin with.

But even emphasizing the heuristic role is not enough to provide a unique formulation of a symmetry principle. There are two, closely related, principles that need to be captured here (see van Fraassen 1989: 233-246, and van Fraassen 1991: 24-33):

(SP1) *Problems that are essentially the same must have essentially the same solution*.

This formulation already emphasizes the heuristic role of symmetries: they guide the formulation of a problem and indicate strategies for its solution. On the one hand, (SP1) emphasizes the importance of looking for symmetries in a problem, and identifying the relevant features that characterize the problem. These features are then used to generate a *new* problem—relevantly similar to the original problem. By solving the new problem, (SP1) then allows us to *transfer* the solution to the original problem. Basically, (SP1) suggests a general guideline for problem solving, and by following this guideline, more "structure" is generated—given that a new problem is formulated. In turn, by using the additional structure, a different, but related, problem is solved, which then provides the solution to the first problem—given (SP1).

The second formulation of the symmetry principle underwrites the fact that symmetry is indeed a matter of structure preservation. If some symmetry is broken, this results from the introduction of a previously ignored asymmetry (see, again, van Fraassen 1989: 239-243). In other words:

(SP2) *Any asymmetry in a problem must come from another asymmetry*.

By identifying the "original", more basic asymmetry, one can obtain additional information regarding the overall characterization of the problem at hand. This, in turn, may provide additional resources to solve the problem. Together, (SP1) and (SP2) stress the heuristic role that is played by symmetry: the importance the latter has in problem solving.

2.3. Types of symmetry

Finally, there is a significant distinction between types of symmetry. It is the distinction between (i) symmetries in the laws, and (ii) symmetries in the solutions (Kosso 2000*a*: 359, Kosso 2000*b* and 2000*c*, Wigner 1967, and Ismael and van Fraassen 2003). In some cases, symmetries are found in the very formulation of fundamental principles in physics—in basic "laws of nature".[6] These are the symmetries in the laws. In other cases, symmetries are found in the solutions of relevant equations. These symmetries may or may not be found in the fundamental laws themselves. But the fact that the symmetries may be found in the solutions is significant, and deserves mention.

The distinction between these types of symmetry is important, given that it highlights the different levels in which symmetries may occur. Although symmetry itself already indicates some level of generality, the generality of the symmetries may come in different levels—and this is exactly what the distinction emphasizes. As we will see, the distinction will play an important role in the discussion that follows.

3. Three Uses of Symmetry Principles in Quantum Mechanics: The "Sandwich" Model

In light of the framework above, I will consider the case of quantum mechanics, and illustrate how the framework can illuminate the topics under consideration. In particular, I'll examine three related uses of symmetry principles in quantum theory. (i) I'll discuss

[6] I am using here the terminology of "laws of nature" that became established in the literature, without assuming the existence of anything like real modalities in nature. Whether there is anything corresponding to a philosophically robust notion of *law of nature* is not at all clear. Personally, I think van Fraassen has mounted a persuasive skeptical challenge for anyone who wants to claim that there are such laws (see van Fraassen 1989: 17-128).

Weyl's use of group-theoretic principles in the *foundations* of quantum mechanics, indicating that it provides a case of symmetry in the laws. (ii) I'll examine Wigner's use of group-theoretic principles in the *applications* of quantum mechanics, indicating that it is a case of symmetry in the solutions. (iii) I'll discuss von Neumann's introduction of Hilbert spaces in quantum mechanics, indicating that it illustrates the heuristic role of symmetry principles—in the sense of (SP1) and (SP2) discussed above. This will pave the way for a discussion of the nature of symmetry principles in quantum mechanics.

In 1925 and 1926, two entirely distinct formulations of quantum mechanics were devised. In 1925, Heisenberg, Born, Jordan, and Dirac formulated matrix mechanics in a series of papers; in the following year, Schrödinger articulated wave mechanics also in a series of works.[7] The two formulations couldn't be more different. Matrix mechanics is expressed in terms of a system of matrices defined by algebraic equations, and the underlying space is discrete. Wave mechanics is articulated in a continuous space, which is used to describe a field-like process in a configuration space governed by a single differential equation. Despite the differences, the two theories seemed to have the same empirical consequences. For example, they yielded coinciding energy values for the hydrogen atom.

But how is it possible that theories that are so different yield the same results? The natural answer is to claim that the theories are equivalent. Schrödinger and Dirac made partially successful attempts in this direction. Ultimately, the attempts didn't succeed. In Schrödinger's case, what was established was only a mapping assigning a matrix to each wave-operator, but not the converse (Schrödinger 1926; for a discussion, see Muller 1997: 49-58). Dirac, in turn, did establish the equivalence between the two theories, but his method required the introduction of the so-called δ-function, which is inconsistent (Dirac 1930).

It is in this context that von Neumann introduced a radically different proposal, formulating his approach in terms of Hilbert spaces (von Neumann 1932). Von Neumann

[7] For a detailed critical discussion, and references, see Muller (1997).

noted that the mathematical spaces used in the formulation of wave and matrix mechanics were very different (one space was discrete, the other continuous). However, if we consider the *functions* defined over these spaces, we obtain particular cases of Hilbert spaces. This suggested that the latter provide the appropriate framework to develop quantum mechanics. And von Neumann's celebrated proof of the equivalence between wave and matrix mechanics established an isomorphism between the corresponding Hilbert spaces.

But there was a further reason for the use of Hilbert spaces. They provide a straightforward setting for the introduction of probability in quantum mechanics. This is a crucial issue, given the irreducibly probabilistic character of the theory. And in fact, in a paper written in 1927 with Hilbert and Nordheim, the problem of introducing probability into quantum mechanics had been explicitly addressed (see Hilbert, Nordheim and von Neumann 1927). The approach was articulated in terms of the notion of the amplitude of the density for relative probability (for a discussion, see Rédei 1997). But it faced a serious technical difficulty (which was acknowledged by the authors): the assumption was made that every operator is an integral operator, and therefore, Dirac's problematic function had to be assumed. As a result, an entirely distinct account was required to introduce adequately probability in quantum mechanics. This provided additional support for the use of Hilbert spaces.

At this point, the problem faced by von Neumann was clear:

(1) How to provide a mathematically consistent and well-motivated formulation of quantum mechanics that explains why matrix and wave mechanics yield (basically) the same empirical results?

In order to solve this problem, von Neumann found a problem that was essentially the same:

(2) Is there a mathematically consistent framework to formulate both matrix and wave mechanics, and one in which the equivalence between the two theories could be established?[8]

By noting that the space of functions defined on the mathematical spaces that underlie matrix and wave mechanics are Hilbert spaces, and by noting that the latter yield a natural way of introducing probability in quantum mechanics, von Neumann found a way of solving problem (2). Thus, given (SP1)—namely, the claim that essentially the same problems have essentially the same solutions—von Neumann was able to solve problem (1). In fact, von Neumann's construction provides a beautiful example of (SP1) at work.

Moreover, von Neumann's proof of the mathematical equivalence between matrix and wave mechanics also illustrates the *context dependence* and *structure preservation* of symmetry. Without identifying the right framework—the right context—to run the proof, it wouldn't be possible to establish the equivalence result (as indicated by the unsuccessful attempts by Schrödinger and Dirac to prove the latter). And once the right context is determined, the equivalence proof is a matter of preserving the right structure, a matter of establishing the structural equivalence—the isomorphism—between the relevant Hilbert spaces (see von Neumann 1932: 28-33). Ultimately, what von Neumann did was to use a *transfer principle*, bringing in structure from the theory of Hilbert spaces to provide a mathematically acceptable foundation for quantum mechanics.

So, by 1927, quantum mechanics could be seen as a semi-coherent assemblage of principles and rules for applications. And von Neumann provided a systematic approach to overcome this situation. Around the same time, Weyl provided a different approach. His 1931 book was an attempt to impose a degree of coherence via the introduction of group-theoretic techniques.[9] Weyl's approach, similarly to von Neumann's, was concerned with

[8] Strictly speaking, von Neumann only proved the equivalence of the *mathematical* frameworks used to formulate wave and matrix mechanics. He disregarded the extra ontological assumptions made by each theory. For a discussion of this issue, see Muller (1997).

[9] Dirac's 1930 work represents a further attempt to articulate a coherent basis for the theory. However, as von Neumann perceived, neither Dirac's nor for that matter Weyl's approaches offered a mathematical framework

foundational questions, although not exactly the same sort of questions. As Mackey points out (1993: 249), Weyl distinguished two questions in the foundations of quantum mechanics (see Weyl 1927): (a) How does one *arrive at* the self-adjoint operators that correspond to various concrete physical observables? (b) What is the *physical significance* of these operators, i.e. how are physical statements deduced from such operators? According to Weyl, (a) had not been adequately treated, and is a deeper question; whereas (b) was settled by von Neumann's formulation of quantum mechanics in terms of Hilbert spaces. But to address (a), Weyl needed a different framework altogether: he needed group theory.

According to Weyl, group theory "reveals the essential features which are not contingent on a special form of the dynamical laws nor on special assumptions concerning the forces involved" (1931: xxi). And he continues:

> Two groups, the group of rotations in 3-dimensional space and the permutation group, play here the principal role, for the laws governing the possible electronic configurations grouped about the stationary nucleus of an atom or an ion are spherically symmetric with respect to the nucleus, and since the various electrons of which the atom or ion is composed are identical, these possible configurations are invariant under a permutation of the individual electrons. (*ibid*.; italics omitted.)

In particular, the theory of group representation by linear transformations, the "mathematically most important part" of group theory, is exactly what is "necessary for an adequate description of the quantum mechanical relations" (*ibid*.). As Weyl establishes, "all quantum numbers, with the exception of the so-called principal quantum number, are indices characterizing representations of groups" (*ibid*.; italics omitted). Moreover, as he shows, Heisenberg's uncertainty relations and Pauli's exclusion principle can be obtained via group theory (Mackey 1993, and French 2000). Given these considerations, Weyl's conclusion is not at all surprising: "We may well expect that it is just this part of quantum

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congenial for the introduction of probability at the most fundamental level, and (initially at least) this was one of the major motivations for the introduction of Hilbert spaces.

physics [the one formulated group-theoretically] which is most certain of a lasting place" (*ibid*.).

Weyl is clearly concerned with the formulation and derivation of fundamental principles of quantum theory, including, as just noted, Heisenberg's uncertainty relations and Pauli's exclusion principle. So, the use of group theory he developed provides a clear case of *symmetry in the laws*.

But it is not only in the foundations of quantum mechanics that group theory has a decisive role; it is also crucial for the *application* of quantum theory. Wigner, in particular, explored this role (see Wigner 1931). Here we find an important difference between Weyl's and Wigner's use of group-theoretic techniques in quantum mechanics (Mackey 1993, and French 2000). Weyl explored group theory at the foundational, indicating how to obtain group-theoretically quantum mechanical principles. Wigner, on the other hand, was particularly concerned with the *application* of quantum mechanics—this is the main theme of his 1931 book. As he argues, we cannot apply the Schrödinger equation directly, but we need to introduce group-theoretic results to obtain the appropriate idealizations (French 2000). In Wigner's own words:

> The actual solution of quantum mechanical equations is, in general, so difficult that one obtains by direct calculation only crude approximations to the real solutions. It is gratifying, therefore, that a large part of the relevant results can be deduced by considering the fundamental symmetry operations. (Wigner 1931: v)

In particular, group theory allows physicists to overcome the mathematical intractability of the many-body problem involved in a system with more than two electrons. In this way, via group theory, it is possible to relate quantum mechanics to the data (French 2000). We have here a clear case of *symmetry in the solutions*. Thus, group theory enters both at the foundational level and at the level of application.

However, in order for the use of group theory to get off the ground, one has to adopt the prior reformulation of quantum mechanics in terms of Hilbert spaces. It is from the representation of the state of a quantum system in terms of Hilbert spaces that a group-theoretic account of symmetric and antisymmetric states can be provided (Weyl 1931: 185-

191).[10] The group-theoretic approach also depends on the Hilbert space representation to introduce probability into quantum mechanics. Moreover, at the application level, despite the need for idealizations to apply quantum theory, the Schrödinger equation is still crucial—putting constraints on the accepted phenomenological models—and the representation of states of a quantum system in terms of Hilbert spaces has to be used. In other words, group theory is not an independent mathematical framework to articulate quantum mechanics: the Hilbert spaces representation is required. Roughly speaking, we can say that von Neumann's Hilbert spaces representation is "sandwiched" between Weyl's foundational use of group theory and Wigner's application program. Hence, there is a close interdependence between group theory and Hilbert spaces theory in the proper formulation of quantum mechanics.

The "sandwich" model highlights the close interconnection in quantum mechanics between the symmetries in the laws and the symmetries in the solutions. These two levels of symmetry are connected in two steps. First, the Hilbert space formalism links the symmetries in the laws explored by Weyl with the symmetries in the solutions that Wigner articulated. Second, von Neumann's ingenious use of the symmetry principle (SP1) provides an additional connection between these two kinds of symmetry. After all, (SP1) is the sort of heuristic principle that guided so much of von Neumann's work, shaping, in particular, the introduction of the Hilbert space formalism. In this way, by exploring (SP1), one can identify the mathematical framework that implements the first step.

But the symmetry principle (SP2) also played a significant role in this story. After motivating and introducing the Hilbert space formalism in quantum mechanics, von Neumann was eventually dissatisfied with it. The trouble is that, on von Neumann's view,

[10] As French points out: "the fundamental relationship underpinning [some applications of group theory to quantum mechanics] is that which holds between the irreducible representations of the group and the subspaces of the Hilbert space representing the states of the system. In particular, if the irreducible representations are multi-dimensional then the appropriate Hamiltonian will have multiple eigenvalues which will split under the effect of the perturbation" (French 2000: 108). In this way, "under the action of the permutation group the Hilbert space of the system decomposes into mutually orthogonal subspaces corresponding to the irreducible representations of this group" (*ibid*.: 108-109; see also Mackey 1993: 242-247). As French notes, of these representations "the most well known are the symmetric and antisymmetric, corresponding to Bose-Einstein and Fermi-Dirac statistics respectively, but others, corresponding to so-called 'parastatistics' are also possible" (*ibid*.: 109).

the formalism didn't allow the proper introduction of probability in the case of quantum systems with infinite degrees of freedom (see Rédei 1997). To provide an alternative framework that extended, in a unified way, his original approach to the case of infinite degrees of freedom, von Neumann used the symmetry principle (SP2): an asymmetry must always come from an asymmetry. Von Neumann realized that the logical structure of quantum mechanical systems with a *finite* number of degrees of freedom is a projective geometry, which is isomorphic to the projective geometry of all subspaces of a finite-dimensional Hilbert space. But this is not the case of systems with an *infinite* number of degrees of freedom. To determine the logical structure of those *infinite* systems, von Neumann eventually provided a generalization of projective geometry, leading to what he called *continuous geometry*[11] (von Neumann 1960, and Bub 1981: 89-90). The asymmetry found in the case of the Hilbert space formalism—the fact that probability couldn't be properly formulated in systems with an infinite number of degrees of freedom—was traced back to the limited geometric structure of Hilbert spaces, and a new, more general structure (von Neumann algebras) was devised. Again, symmetry principles, now in the form (SP2), have been crucial.

4. The Nature of Symmetry Principles in Quantum Mechanics

Given the considerations above, we can now address to the issue of the nature of symmetry principles in quantum mechanics. To examine the issue, I'll consider three questions: (a) Are symmetry principles *ontological*? (Do they tell us something about the *nature of the world*?) (b) Are symmetry principles *epistemological*? (Do they tell us something about the nature of our *knowledge* of the world?) (c) Are symmetry principles *methodological* and *heuristic* only? (Are they useful to solve problems and provide guidelines for theory construction in physics?) I'll answer questions (a) and (b) negatively; but question (c) will receive an emphatically positive answer. Of course, given the latter answer, it's crucial to discuss the ontological and epistemological consequences of the heuristic nature of

[11] We now call these structures *von Neumann algebras* of Hilbert space operators.

symmetry principles, and why the heuristic nature of these principles *doesn't* undermine the answers given to questions (a) and (b). I'll examine these issues in turn.

4.1. Why symmetry principles are not ontological

As we saw above, symmetry plays an undeniable role in the formulation of quantum mechanics.[12] It's natural enough to expect that symmetry considerations have both ontological and epistemological consequences. The strongest of these claims is, of course, the ontological one: the claim that symmetry considerations allow us to "carve nature at its joints"; that symmetry principles allow us to uncover the underlying regularities of nature. Here is the argument to this effect: If symmetry principles didn't entail something substantially right about the workings of nature, it wouldn't be possible to explain the success of empirical predictions based on symmetry considerations. And there's no doubt that symmetry plays a major predictive role in quantum mechanics, underlying fundamental discoveries in particle physics, not to mention the crucial role that symmetry plays in the formulation and application of quantum theory (explored, respectively, by Weyl and Wigner).

This is, of course, a version of the no-miracles argument, revamped to explain the success of symmetry considerations in physics (for the traditional version of the argument, see e.g. Putnam 1979). According to the no-miracles argument, the best explanation for the empirical success of scientific theories (their success in yielding right, novel predictions about the world) derives from the fact that these theories are true (or approximately so). In fact, the argument goes, it would be a miracle if our best scientific theories, despite making novel predictions,[13] still turned out to be false. For how could such theories possibly make novel predictions without also being substantially right about the underlying causal

[12] For further discussion of the role of symmetry in theory construction, see also van Fraassen (1989: 233-289), van Fraassen (1991: 21-76, 177-184), and Ismael and van Fraassen (2003).
[13] These are predictions of phenomena the theory was not originally constructed to make.

mechanisms and properties they identify in the world? In other words, it would be a miracle if *novel* predictions were made by *false* theories. Or so goes the argument.[14]

The no-miracles argument bears an important connection with another contentious argument, now in the philosophy of mathematics: the indispensability argument (see Quine 1960, Putnam 1979, and for a thorough discussion, Colyvan 2001). According to the indispensability argument:

(P1) We ought to have ontological commitment to all and only those entities that are indispensable to our best scientific theories.

(P2) Mathematical entities are indispensable to our best scientific theories.

(C) We ought to have ontological commitment to mathematical entities.[15]

The indispensability argument provides reason to believe in the existence of mathematical objects for those who have reason to believe in the entities that are indispensable to our best scientific theories—typically, *scientific realists* are in this category. The no-miracles argument, in turn, provides an indirect argument for *scientific realism itself*, given that it shows that realism explains the empirical success of scientific theories. In this way, the no-miracles argument can be viewed as giving reason to believe in the existence of unobservable entities in science. The indispensability argument then extends this belief to unobservable *mathematical* entities. Thus, the two arguments work together to reinforce and expand the commitment to realism, justifying the claim that we have reason to believe in unobservable entities, whether they are concerned with mathematical or physical domains.

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[14] I'm simplifying things here. According to John Worrall, the no-miracles argument shouldn't even be constructed as a *deductive* argument for scientific realism (see Worrall 1989). The argument provides, at best, indirect evidence for realism, given that it indicates how realism manages to explain the predictive success of scientific theories. But, for the purposes of the current discussion, there's no need to enter into the intricacies of this issue here.

[15] I'm following here the version of the argument presented in Colyvan (2001: 11). There is an unanalyzed modal locution ("ought") in (P1) and (C), but the argument can be reformulated without this modal idiom. Given that nothing hangs on this, I'll stick to Colyvan's formulation.

Now, as we saw above, symmetry is a *mathematical* property (ultimately, it's a matter of structure preservation), but it also has important *empirical* consequences, as the quantum mechanics case beautifully illustrates. So, to be able to get ontological conclusions from the successful use of symmetry in physics, something like the indispensability argument and the no-miracles argument need to be in place. After all, without something like these arguments, it's not at all clear how we can justify the claim that we ought to be committed to the results based on symmetry considerations. Consider, for instance, the case of an anti-realist about such symmetry principles. This would be someone who asserts that the use of symmetry principles is just part of an empirically adequate (but not true) theoretical package. Hence, the truth of the results that are obtained via these principles is never asserted—particularly the results dealing with unobservable outcomes. A more robust strategy to justify an ontological reading of symmetry principles is therefore needed.

In terms of the two arguments for realism just discussed, it's easy to mount an ontological justification for the use of symmetry principles. Consider the following indispensability argument:

(P1′) We ought to have ontological commitment to all and only those entities and principles that are indispensable to our best scientific theories.

(P2′) Symmetry principles are indispensable to one of our best scientific theories (namely, quantum mechanics).

(C′) We ought to have ontological commitment to symmetry principles.

In (P2′), the sense in which symmetry principles are *indispensable* should be clear enough. As indicated above, the formulation and application of quantum mechanics seem to require such principles. And so, quantum theory, as we know it, couldn't even get off the ground without symmetry considerations. Hence, it's concluded that we ought to have ontological commitment to the latter.

Similarly, a no-miracles argument to the effect that the best explanation as to why symmetry principles are so successful in quantum mechanics could be articulated as well.

The central point of this argument highlights the fact that by adopting the ontological reading of symmetry principles in quantum mechanics one can explain why such principles are so successful. And there's no doubt that the principles are indeed successful.

Given the fact that the indispensability and the no-miracles arguments are invoked by the ontological reading of symmetry principles, by resisting the former two arguments, it's possible to resist the ontological conclusion from symmetry principles as well. From the discussion above, it's also clear that the no-miracles argument and the indispensability argument have similar patterns. Thus, they also receive similar responses.[16] And by providing these responses, it's possible to undermine the claim that symmetry principles have an ontological import. After all, the claim that they have such an import presupposes a style of argument—the indispensability and the no-miracles arguments—that ultimately doesn't support the conclusion. Let's see why this is the case.

The main problem faced by both the no-miracles argument and the indispensability argument is that they assume that the evidence supports equally well all the components of a theory. And so, in the case of the indispensability argument, it's assumed that the evidence that supports the conceptual machinery of a given theory also supports the unobservable mathematical entities used to formulate the theory. In the case of the no-miracles argument, it's assumed that all the theoretical components of a scientific theory receive indiscriminately the same support from the evidence, which is required to explain the success of the theory given the available evidence. But, in both cases, the assumption is false.

As it has been frequently pointed out, scientific theories don't receive indiscriminate support to all of their parts. There are components of a theory—such as idealizations and mathematical terms—that play no causal role in the description of the phenomena, and so are not taken to receive any support from the evidence (see, e.g., Maddy 1997: 133-160). As a result, these "idle" components cannot be claimed to have ontological significance. But are symmetry principles among the "idle" parts of a theory?

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[16] Here is a heuristic use of a *meta-symmetry* principle!

It's hard to argue that they are. After all, symmetry principles *do* play a role. But it's far from clear that this role warrants the conclusion that symmetry principles are *ontologically* significant. After all, to help the formulation of quantum theory, or to help to get certain solutions from the Schrödinger equation, are *pragmatic* uses of the relevant mathematics. Why should these uses have any *ontological* significance? The fact that a theory allows us to express certain relations is not sufficient to guarantee that the theory is true. Fictional discourse allows us to express various relations without any such ontological implications (see Bueno 2005, and Azzouni 2004). As a result, the first premise of the indispensability argument, (P1), as well as the first premise of the argument for the ontological reading of symmetry principles, (P1′), are not true in general, and should be rejected. Even granting that symmetry principles are indispensable to quantum mechanics, it doesn't follow that we ought to be ontologically committed to them. Symmetry principles provide useful devices to describe the foundations of quantum mechanics and to obtain solutions to quantum mechanical equations. But these features alone license no ontological conclusion about what is going on in nature. (Exactly the same point applies to the no-miracles argument.)

Moreover, even though symmetry principles can be separated from other components of a theory, they never work in complete isolation. So, no ontological credit can be assigned to symmetry alone. This is the topic of the next section.

4.2. Why symmetry principles are not epistemological

If symmetry principles don't necessarily have an ontological character, is their nature purely *epistemological*? That is, can we say that symmetry principles provide, at best, epistemological guidelines—guidelines to increase our *knowledge* of the world? In a fascinating study, Kosso (2000*a*) argues that symmetry principles do have an epistemological character, but their epistemology is in no way notable. In particular, an important use of symmetry principles in physics—the case of spontaneously broken symmetries—doesn't introduce any new epistemological issue. As Kosso points out:

> The epistemology of the cases of spontaneously broken symmetry presented here, even the esoteric case of the electroweak gauge theory, ends up as rather basic, meat-and-potatoes scientific method. An argument by analogy is used to suggest the hypothesis, but the analogy is not of much epistemic value. Rather, it is the subsequent hypothetico-deductive testing that serves as the source of justification. The credibility comes from the success of precise and novel prediction. (Kosso 2000*a*: 374)

What happens is that symmetry principles are added to empirical assumptions about a given domain. As a result, the use of these principles is substantially constrained by empirical considerations made in the domain in question. In other words, symmetry principles are assessed *in a package* together with the relevant empirical information. Thus, the application of symmetry principles alone cannot raise any special sort of epistemological issue. The *whole package* of physical principles, auxiliary hypotheses, initial conditions, and symmetry principles is used to obtain the intended predictions. Credit or blame needs to be assigned to the package as a whole.[17]

Even though Kosso doesn't present the point in the way I just did, he is exactly right in making his point. Symmetry principles need to be assessed in a broader context—we need more than symmetry considerations alone to get any interesting predictions.

Despite this fact, there seems to be something peculiar about symmetry principles: their deceptively a priori appearance. In fact, in Weyl's view, "all a priori statements in physics have their origin in symmetry" (Weyl 1952: 126).[18] For Weyl, these a priori statements are quite limited, though. He mentions two very simple examples: the conclusion that Archimedes draw, based on a priori considerations, that "equal weights balance in scales of equal arms";[19] and the case of casting dice that are perfect cubes, where we can be sure that,

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[17] For simplicity, I've been describing the theoretical situation in terms of the traditional "syntactic" picture of theories. But essentially the same description could be obtained emphasizing the *modeling* process (following the "semantic" approach), where the "syntactic" description given above would be formulated in terms of *restrictions* in the relevant *models*. For example, the initial conditions force one to consider only models in which certain situations are satisfied (see van Fraassen 1989: 217-232, van Fraassen 1991: 1-17, 24-33, and Hughes 1989: 79-82).

[18] Kosso (2000*b*) provides a captivating discussion of the issue raised by Weyl.

[19] In fact, as Weyl points out, "the whole configuration is symmetric with respect to the midplane of the scales, and therefore it is impossible that one mounts while the other sinks" (Weyl 1952: 125).

due to the symmetry of the situation, "each side has the same chance, 1/6" (Weyl 1952: 125-126). But these results don't hold in general:

> Sometimes we are […] enabled to make predictions a priori on account of symmetry for *special cases*, while the *general case*, as for instance the law of equilibrium for scales with arms with different lengths, can only be settled by experience or by physical principles ultimately based on experience. (Weyl 1952: 126; italics added.)

Weyl's conclusion in the *general* case conforms to the point made in the discussion of quantum mechanics above. As we saw, the use of symmetry emerges in a context dominated by empirical considerations—in particular, by all the relevant physical principles of the theory. Whether we are considering symmetries in the laws or symmetries in the solutions, additional physical principles are always found: from the need to find certain solutions to the Schrödinger equation to von Neumann's way of introducing probability in quantum mechanics. And so, symmetry principles are not operating in an empirical vacuum, as it were.

Van Fraassen presents the point very elegantly:

> Once a problem is modelled, the symmetry requirement may give it a unique, or at least greatly constrained solution. The modelling, however, involves substantive assumptions: an implicit selection of certain parameters as alone relevant, and a tacit assumption of structure in the parameter space. Whenever the consequent limitations are ignored, paradoxes bring us back to our senses—symmetries respected in one modelling of the problem *entail* symmetries broken in another model. As soon as we took the first step, symmetries swept us along in a powerful current—but nature might have demanded a different first step, or embarkation in a different stream. (van Fraassen 1989: 317)

The point is important, and van Fraassen had already reached the same conclusion in an earlier discussion:

> We [can] exploit symmetries only after deliberate choice of a model—and then the symmetry carrie[s] us swiftly to the end—but that initial choice has no a priori guarantee of adequacy.
>
> Symmetry arguments have that lovely air of the a priori, flattering what William James called the sentiment of rationality. And they are a priori, and powerful; but they carry us

> forward from an initial position of empirical risk, to a final point with exactly the same risk. The degree of empirical fallibility remains invariant. (van Fraassen 1989: 260-261)

In other words, even if symmetry principles were a priori, the fact that they ultimately depend on empirical considerations highlights their ultimate fallibility.

Where does this leave us with regard to the issue of the epistemological nature of symmetry principles? One way in which symmetry principles could have an epistemological nature is by establishing that they are a priori. After all, a priority is a thoroughly epistemological notion. Weyl characterized the issue exactly in that way. In his view, the conclusions he reached about symmetry—both the general and the special cases discussed a few paragraphs above—are epistemological. And, indeed, that's what the conclusions are. The problem is that this still leaves the issue quite open. After all, as Weyl himself acknowledged and van Fraassen emphasized, in order to apply a symmetry principle, it's crucial first to model the intended situation, and in setting up the model, various empirical assumptions need to be introduced. As a result, we cannot claim *in general* that the *use* of symmetry principles is simply a priori. Even if the principles were a priori, it would be misleading to leave the issue at that, given the need for additional empirical information for symmetries to get actually off the ground. Given the need for the broader context to get symmetry principles going in physics, the use of these principles isn't purely an epistemological matter after all.

4.3. Why symmetry principles are methodological and heuristic

If the nature of symmetry principles isn't purely epistemological or ontological, what is it? Briefly, the nature of symmetry principles is ultimately methodological and heuristic. As we saw above, symmetry—in the form of group-theoretic techniques—plays two crucial roles in quantum mechanics: (a) It provides crucial tools for the foundations of quantum theory (this is the upshot of seeing Weyl's program as a case of symmetry in the laws). And (b) symmetry is crucial for the solution of the Schrödinger equation (and here we looked at Wigner's approach as a case of symmetry in the solutions). In both circumstances, symmetry is never used alone, but it depends on additional empirical and conceptual resources—in particular, as we saw, the Hilbert space formalism is required for both (a) and (b). All of these outcomes are clearly *methodological* and *heuristic*: (a) increases our ability to understand and formulate quantum theory, whereas (b) helps us solve problems using the resulting formalism.

It might be argued that helping to improve the formulation of the theory and to solve equations are indeed important roles that group theory plays in quantum mechanics. In fact, the argument goes, these roles are *so important* that we need to reify them, and claim that they provide reasons to believe in the existence of the corresponding objects (symmetries, vectors in Hilbert spaces, probability functions etc.). The case for the indispensability and the no-miracles arguments for symmetry principles, discussed above, rested exactly on this move.

However, are these roles *really indispensable*? Even if they were, it doesn't follow that we need to believe in the existence of unobservable entities (whether they are mathematical or physical). After all, as we noted, solving equations and understanding a theory are ultimately *pragmatic* features of our use of that theory. These features are in no obligation of telling us anything substantive about the world. As such, they don't require commitment to the existence of the entities denoted by the theory under consideration. And so, once again, there is no need to—and we should not—reify the description provided by symmetry principles. Ultimately, their role is methodological and heuristic.

4.4. Ontological and epistemological consequences of symmetry principles?

Even if the nature of symmetry principles has to do with heuristics and methodology, the point still stands that these principles seem to have ontological and epistemological consequences. In particular, new particles have been discovered based on symmetry considerations. Doesn't this undermine the claim that the nature of symmetry principles is only methodological and heuristic?

I don't think so. As indicated above, the whole package of physical principles, auxiliary hypotheses, initial conditions, and symmetry principles is used to draw empirical consequences. In isolation, symmetry principles are completely silent. So, strictly speaking, it's misleading to say that symmetry principles have ontological or epistemological consequences. The consequences that follow from the symmetries are actually consequences of much broader theoretical packages. Moreover, as discussed above, to draw ontological conclusions from symmetry principles, something like the indispensability and the no-miracles arguments is required. But, as we saw, these styles of argument fail to establish the intended conclusion. As a result, we should resist the temptation of reading too much into the symmetries alone.

5. Conclusion

Much more could (and should) be said about the nature of symmetry principles in physics; in particular, by extending the discussion simply sketched here beyond quantum mechanics. But I hope enough was said to motivate the claim that whatever use symmetry principles might be put to, there is no need—and no justification—to draw ontological conclusions from them alone.

As any type of principle in physics, symmetry principles are fallible and contingent. They presuppose empirical assumptions about the domain to which they are applied, and they are no more secure than the assumptions on which they rest. Symmetry principles are elegant, ingenious, inventive. But their beauty alone, as the beauty of any other physical principle, is never enough to give us reason to conclude anything beyond the fact that these are *methodological* principles. Symmetry principles are useful, important, perhaps even indispensable *tools* for theory construction. But they are nothing more than that.

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