Laws, Symmetry, and Symmetry Breaking; Invariance, Conservation Principles, and Objectivity*

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Abstract: Given its importance in modern physics, philosophers of science have paid surprisingly little attention to the subject of symmetries and invariances, and they have largely neglected the subtopic of symmetry breaking. I illustrate how the topic of laws and symmetries brings into fruitful interaction technical issues in physics and mathematics with both methodological issues in philosophy of science, such as the status of laws of physics, and metaphysical issues, such as the nature of objectivity.

1 Introduction

The focus of this Address is on the web of connections that tie together laws, symmetries and invariances, and conservation principles. There are many ways to pursue this topic. My line of pursuit will be somewhat unorthodox, but it has the virtue of connecting a number of fundamental issues in the foundations of physics. Reflecting on these issues prompts a reevaluation of basic issues in metaphysics, such as the nature of objectivity and the nature of change. Perhaps because of the formidable technical challenges they pose, philosophers have tended to shy away from the foundations of physics problems that I will identify. And perhaps because they are embarrassed to be seen to be doing metaphysics, philosophers of science have been reluctant to take up the philosophical issues. My message to both groups is the same: Have courage! The nub of the issues in foundations of physics can be made accessible even to the non-specialist. And there is no shame in doing metaphysics as long as the activity is informed by scientific practice. I will be begin with a brief look at the philosophical literature on laws.

2 Laws: the scandal in the philosophy of science

It is hard to imagine how there could be more disagreement about the fundamentals of the concept of law of nature–or any other concept so basic to the philosophy of science–than currently exists. A cursory survey of the recent literature reveals the following oppositions (among others): there are no laws vs. there are/must be laws[1];

[1]See van Fraassen (1989) and Giere (1999) for different versions of the "no-laws" view, and see Carroll (1994) for the "there must be laws" view.

laws express relations among universals vs. laws do not express such relations[2]; laws are not/cannot be Humean supervenient vs. laws are/must be Humean supervenient[3]; laws do not/cannot contain *ceteris paribus* clauses vs. laws do/must contain *ceteris paribus* clauses[4].

One might shrug off this situation with the remark that in philosophy disagreement is par for the course. But the correct characterization of this situation seems to me to be "disarray" rather than "disagreement." Moreover, much of the philosophical discussion of laws seems disconnected from the practice and substance of science: scientists overhearing typical philosophical debates about laws would take away the impression of scholasticism–and they would be right![5]

There is, however, one place where the philosophical investigation of the concept of laws can make solid contact with science, and that place is to be found in the topic of laws and symmetries. Philosophers of science have done some good work on this topic,[6] but it is only a beginning. And the surface of many important subtopics, such as gauge symmetries and symmetry breaking, has barely been scratched. I will emphasize these neglected topics here.

3 Symmetries and laws: laws of nature vs. laws of science

The topic of symmetries and invariances in physics provides no comfort for those who hanker after *laws of nature* in the sense of critters that embody the goodies on the wish list drawn up by philosophers: laws of nature are supposed to be objective (independent of our interests and beliefs); they are supposed to express a strong form

[2]In favor of the former see Armstrong (1987), and for a skeptical reaction see van Fraassen (1989).

[3]For the former, see Armstrong (1987) and Carroll (1987), and for the latter see Earman and Roberts (2002).

[4]See the articles in the special issue of *Erkenntnis* edited by Earman et al. (2002).

[5]For example, it seems to have been enshrined in philosophical consciousness–largely, as far as I can tell, as a result of historical accidents–that a key feature separating laws from accidental generalizations is that the former but not the latter support counterfactual conditionals. This allows philosophers to inject into the discussion of laws all of the controversy and dubious metaphysics that surrounds counterfactuals. And following the venerable tradition of taking a dubious thing to its extreme, some philosophers have based claims about nature of laws on the principle that the lawhood, as well as the truth, of a proposition expressing a law is preserved under counterfactual assumptions logically compatible with the proposition. Now we get to consider nested counterfactuals and subjunctives (e.g. "If P were the case, then it would be the case that if Q were the case, R would be the case.") Not surprisingly, the subsequent debate takes the form of appeal to intuition, authority (i.e., other articles written in a similiar vein), clever examples and counterexamples involving possible worlds, etc.

[6]See, most notably, van Fraassen (1989).

of non-logical necessity ("nomological necessity"); they are supposed to cut nature at the joints by expressing truths about natural kinds; they are supposed to have the power to explain the why of things; etc.[7] Here I declare myself in sympathy with the milder form of the "no-laws" view: if there are such critters, I see no good reason to think that science can be counted on to corral them, or that we can tell the difference between cases where science has succeeded in corralling them and cases where it has failed to do so.

But by the same token the topic of symmetries and invariances does not support the strong version of the no-laws view, which intimates that both the history of science and its current practice can be understood without giving pride of place to the search for *laws of science*. In the case of physics–which will be my focus–what physicists mean by the *laws of physics* is, roughly, a set of true principles that form a strong but simple and unified system that can be used to predict and explain. As Steven Weinberg puts it

> Our job as physicists is to see things simply, to understand a great many complicated phenomena in a unified way, in terms of a few simple principles.[8] (1980, 515)

Rather than coming at the topic of laws of physics with preconceptions of what these laws must deliver if they are to support favored philosophical accounts of causation, counterfactuals, explanation, etc., historians and philosophers of science would do better to investigate how physicists use the concept of law. It would not be surprising to find that there is nothing neat and precise that corresponds to Weinberg's "few simple principles." But so what? To dismiss the notion of law of physics on the grounds that it is messy and imprecise would be to miss important points not only about the motivation of physicists but about the methodology and content of physics as well. I will not offer here an argument for this claim in its full generality; rather, I will be content to note how the relevant senses of symmetry and invariance in physics presuppose a distinction between the nomological and the accidental in the sense of a distinction between what holds as a consequence of the laws of physics and what is compatible with but does not follow directly from these laws.

This is hardly a novel idea. It pervades Eugene Wigner's writings on symmetries and invariances. Consider, for instance:

[7] See van Fraassen (1989) for a wish-list of goodies that philosophers have wanted laws of nature to deliver.

[8] The reader acquainted with the philosophical literature will notice a resemblance between Weinberg's notion and David Lewis' (1973) analysis of laws as the axioms or theorems that belong to the best deductive system, where "best" means achieves the optimal balance between strength and simplicity.

> The invariance principles apply only to the second category of our knowledge of nature; to the so-called laws of nature ... [I]nvariance principles can be formulated only if one admits the existence of two types of information which correspond in present-day physics to initial [and boundary] conditions and laws of nature. It would be very difficult to find a meaning for invariance principles if the two categories of our knowledge of the physical world [laws vs. initial/boundary conditions] could no longer be sharply drawn. (Houtappel, Van Dam, and Wigner 1965, 596)[9]

My point–and Wigner's point–is that, in the main, the symmetries physicists are concerned with–e.g. Poincaré invariance, time reversal invariance, etc.–typically are symmetries of lawlike connections among events and not symmetries of particular histories (sequences of states) satisfying the laws or of particular states belonging to these histories. If you like slogans, here are a couple:

> The symmetries are in the laws of the phenomena, not in the phenomena themselves.
>
> The phenomena break the symmetries of laws.
>
> "C'est la dissymétrie qui crée le phenomène" (Pierre Curie1894, 401).

If you are fond of the models view of theories, the point can be put in the following way. A practitioner of mathematical physics is concerned with a certain mathematical structure and an associated set $\mathfrak{M}$ of models with this structure. The sought after laws L of physics pick out a distinguished sub-class of models $\mathfrak{M}_L := \mathrm{mod}(L) \subset \mathfrak{M}$, the models satisfying the laws L (or in more colorful, if misleading, language, the models that "obey" the laws L). Abstractly, a symmetry operation is a map $\mathcal{S} : \mathfrak{M} \to \mathfrak{M}$. $\mathcal{S}$ is a symmetry of the laws L just in case it preserves $\mathfrak{M}_L$, i.e. for any $m \in \mathfrak{M}_L$, $\mathcal{S}(m) \in \mathfrak{M}_L$. At this level of abstraction the characterization of symmetries on offer is not very informative; the following sections add content to the characterization by supplying detailed examples. Nevertheless, even at this high level of abstraction the characterization of symmetries is useful in making an elemenary but crucial point; namely, the condition for $\mathcal{S}$ to be a symmetry of the laws L would be satisfied if $\mathcal{S}(m) = m$ for each $m \in \mathfrak{M}_L$, but the condition for $\mathcal{S}$ to be a symmetry of the laws L can be satisfied even though this identity fails for some or all of the models obeying L.[10]

[9]For additional expressions of this idea, see Wigner (1967). I would differ from Wigner only in inserting "laws of physics" for "laws of nature."

[10]Or more precisely, the condition for $\mathcal{S}$ to be a symmetry can hold even if m and $\mathcal{S}(m)$ are not the same up to an isomorphism of the type relevant to the mathematical structure at issue.

But if the above slogans are accurate, one might ask why physicists should set such store by symmetries broken by the phenomena. The obvious–and I think largely correct–answer is that research in physics is guided by Weinberg's injunction to understand complicated phenomena in terms of a few simple principles–that get dubbed the laws of physics–and that physicists have, or think they have, reasons to believe that the laws of physics do and, perhaps, must reflect the symmetry at issue. I will have more to say about the second part of the answer in section 3.

The above remarks also help to explain an otherwise puzzling assertion from Hermann Weyl's book *Symmetry*:

> If Nature were all lawfulness then every phenomenon would share the full symmetry of the universal laws of nature as formulated by the [special] theory of relativity. (1952, 26)

Remove Weyl's reference to the laws of the special theory of relativity and substitute a variable L. If Nature were all the lawfulness of L, then $\mathfrak{M}_L$ would consist of a single model or a single isomorphism type (i.e. any two models of $\mathfrak{M}_L$ would be related by an isomorphism of the relevant mathematical structure.). And if $\mathcal{S}$ were a symmetry of the laws L, then it would have to have to preserve this model or its isomorphism type. Which is to say (in possible world talk) that the $\mathcal{S}$-image of possible world corresponding to the isomorphism type is the same world; which is to say that the phenomena as described by $\mathfrak{M}_L$ "share the full symmetry of the universal laws of nature." So the very fact that actual phenomena do not share the symmetry of what we take to be a symmetry of the laws of physics proves the existence of contingency!

I hasten to add that such talk does not represent a return to laws of nature in the sense of principles that deliver non-Humean forms of necessity. For on the account of scientific laws that I favor, these laws supervene on the Humean base and, thus, carry with them no non-Humean powers. On the other hand, I want to underscore again the point that one cannot even get started on the topic of symmetries and invariances in physics without acknowledging the importance of what I am calling laws of physics. And I want to emphasize that in using the models view of theories I am not endorsing a non-statement view of theories in any interesting sense of that term. Like van Fraassen (1980, 1989), I think that issues in the philosophy of language are largely unimportant for the philosophy of science–in particular, I can't see that any important foundational issue about laws and symmetries turns on whether, say, Newton's laws of motion or Einstein's laws of gravitation are formulated in this vs. that language. What is important is the mathematical structure, and that is brought out by the choice of the type of model in which the laws of the theory are to be situated. But none of this is any comfort for a non-statement view: the relevant class of models consists of those models satisfying some set of laws as expressed (largely, one hopes, non-linguistically) by a set of equations.[11]

4 The status of symmetry principles

The received wisdom about the status of symmetry principles has it that one must confront a choice between the *a posteriori* approach (a.k.a. the bottom up approach) vs. the *a priori* approach (a.k.a. the top down approach). The former approach means that we subject candidate laws of physics to empirical checks and then derive the symmetries from the candidates that have passed muster. The latter approach means that symmetry principles are viewed as being more fundamental than the laws they constrain or as being second order laws that dictate symmetries to first order laws.The choice on offer is not an either-or one. That symmetry principles have a meta-character follows from the characterization of symmetries of laws given in section 2. But viewing symmetry principles as meta-laws doesn't commit one to treating them *a priori* in the sense of known to be true independently of experience. For instance, that a symmetry principle functions as a valid meta-law can be known *a posteriori* by a second level induction on the character of first-order law candidates that have passed empirical muster. From the other direction, the *a priori* of the top-down approach doesn't have to be understood either in the sense of necessarily true (or true for all times) or in the sense of knowable independently of experience; rather it can be understood in the sense a revisable constitutive *a priori*.

The last remark applies to the symmetries of physical laws deriving from the symmetries of spacetime in the pre-general relativistic era. During that innocent era the favored spacetime–say, neo-Newtonian spacetime or Minkowski spacetime–was supposed to be constitutive of physical possibility in that is was supposed to serve as the fixed backdrop for any acceptable theory of physics.[12] Then if the laws of physics are formulated in terms of geometric object fields on spacetime and if laws are to be "general" or "universal" in the minimal sense that they cannot use names or

[11]Much is made of the fact that the relevant set of models for a theory of physics may not be an elementary class in the sense of being the set of models that satisfies some set of sentences of some first order language. This hardly seems to me to undermine the spirit of the statement view of theories. It would be hard to imagine how a class of models could perform the functions of scientific theory unless that class is specified as a set of models as a elementary class in the informal sense of a class of models satisfying some list of conditions that are given in an informal and yet sufficiently precise manner to suit the purposes at hand–e.g. Einstein's general theory of relativity consists of all models of the form $\mathcal{M}, g_{ab}, T_{ab}$ such that $\mathcal{M}$ is a differentiable manifold, g_{ab} and T_{ab} are tensor fields on $\mathcal{M}$ of such-and-such types, and the pair (g_{ab}, T_{ab}) satisfies Einstein's field equations everywhere on $\mathcal{M}$. What sauce for the goose is sauce for the gander here: if the proponent of the semantic view can get away with informal specifications, so can the proponent of the statement view.

[12]Here I am borrowing from–and distorting–Reichenbach (1965, Ch. 5); see also Friedman (1999, Ch. 3).

designators for particular spacetime points or regions, it follows that any acceptable candidate for a law of physics must share the symmetries of the spacetime.[13]

To illustrate the force of the above considerations, consider why Huygens and Leibniz were morally certain that Descartes' candidates for laws of elastic impact cannot be laws of physics, despite the fact that neither Huygens nor Leibniz had done the relevant experiments. The answer does not lie in the facts that Huygens and Leibniz had performed thought experiments on colliding bodies (which they had) and that thought experiments have some mysterious power to generate knowledge about nature (which they don't). Rather the answer is–to use admittedly anachronistic terminology–that both Huygens and Leibniz thought that the symmetries of spacetime include mapping that transform a situation in which two bodies are in relative motion into a situation in which either one of the bodies is in motion and the other is at rest. It follows that Descartes was wrong in postulating that (i) when a moving body collides with a less massive body initially at rest, the more massive body pushes the less massive body along, but (ii) when a moving body collides with a more massive body initially at rest, the less massive body rebounds while the more massive one remains at rest (see Fig. 1). For the before of (i) can be transformed into before of (ii) and vice versa by the said symmetry operation, and since the laws of motion must respect this symmetry, it follows that Descartes' purported laws do not give a consistent and unambiguous answer to what happens when bodies of unequal masses collide.[14]

[13]To make the claim more specific, take the models of the theory to be of the form $\langle \mathcal{M}, A_1, A_2, ..., A_m, D_1, D_2, ..., D_n \rangle$ where $\mathcal{M}$ is the spacetime manifold and the A's are geometric object fields on $\mathcal{M}$ that characterize the structure of spacetime. The first letter of the alphabet is used for these object fields since the spacetime is supposed to be 'absolute': $\mathcal{M}$ and the A's are the same in all the models. The D's are the geometric object fields on $\mathcal{M}$ that characterize the physical contents of the spacetime models. These objects are labeled with a 'D' since they are supposed to be dynamical objects that can vary from model to model. The symmetry group $\mathcal{G}_{ST}$ of the spacetime consists of diffeomorphisms $G : \mathcal{M} \to \mathcal{M}$ that leave invariant the A's, i.e. $G^* A_i = A_i$, $i = 1, 2, ..., m$, where $G^* O$ denotes the drag along of the object field O by the diffeomorphism G. There is an associated group of potential dynamical symmetries $\mathfrak{D}_{ST}$ where the action of an element $\mathcal{D}_G \in \mathfrak{D}_{ST}$ corresponding to $G \in \mathcal{G}_{ST}$ is given by $\mathcal{D}_G\left(\langle \mathcal{M}, A_1, A_2, ..., A_m, D_1, D_2, ..., D_n \rangle\right) = \langle \mathcal{M}, A_1, A_2, ..., A_m, G^* D_1, G^* D_2, ..., G^* D_n \rangle$. With the given spacetime forming the constitutive *a priori* and with the minimal requirement of "universality" in place it follows that any candidate law L must be formulated in terms of the A_i that characterize the spacetime structure and some set of D_j's. Then $\mathfrak{D}_{ST}$ must be a symmetry group of any such candidate law L; for since such an L cannot distinguish between $m = \langle \mathcal{M}, A_1, A_2, ..., A_m, D_1, D_2, ..., D_n \rangle$ and $\mathcal{D}_G(m) = \langle \mathcal{M}, A_1, A_2, ..., A_m, G^* D_1, G^* D_2, ..., G^* D_n \rangle$, it cannot fail to be the case that if $m \in \mathfrak{M}_L$ then $\mathcal{D}_G(m) \in \mathfrak{M}_L$. Indeed, the indistinguishability provides some grounds for taking m and $\mathcal{D}_G(m)$ to describe the same physical situation. Below we will see that under some circumstances there are reasons to avail oneself of this move

[14]What makes the situation complicated is that–again to use admittedly anachronistic terminology–neither Huygens nor Leibniz had a consistent account of the symmetries of space-

The constitutive *a priori* in the sense under discussion is revisable. Pressure for revision of the spacetime structure can come in the form of empirical evidence or philosophical/theoretical considerations indicating that laws do or should exhibit a group of symmetries either larger or small than what would result from the symmetries of the spacetime currently in favor. Here is one example of how the issue of symmetries of spacetime and laws interacted with the debate, that raged from the time of Leibniz and Newton until the 20th century, over absolute vs. relational accounts of motion and of space and time. Suppose that you reject a relational account of space and time in favor of a container view of space: physical bodies are literally contained in space in the sense that shifting them all, say, one mile to the east results in a new physical state. Then it follows that if you want it to be possible that determinism is true, you must attribute enough structure to spacetime that motion is absolute in the sense that not all motion is relative motion of bodies. For example, in neo-Newtonian spacetime with its inertial structure, the acceleration of a particle is a well-defined, non-relational quantity. Stripping the inertial structure from the spacetime removes the basis for absolute quantities of motion, but it also makes the spacetime symmetry group so large that determinism for particle motion becomes impossible (if the container view of space is maintained). For now there are symmetries that are the identity map on that portion of spacetime on or below a given plane of Newtonian simultaneity but non-identity above. Since such a symmetry must be a symmetry of the equations of motion, it follows that for any solution to these equations (solid lines in Fig. 2) there is another that agrees with the first on the particle positions in the container spacetime for all past times but disagrees with it for future times (dashed lines of Fig. 2). On the other hand, if you want to be a relationist about motion and you want to allow for the possibility of determinism, then you must do a *modus tollens* move and reject the container view of spacetime. This amounts to treating the said transformations as gauge transformations in the sense of connecting different descriptions of the same physical content. I will return to this theme below.

There are two limitations to the above thesis about the *a priori* status of symmetry principles. The first is that it does not apply to "internal" or non-spacetime symmetries. Of course, one could try to tell a parallel story about how the structure of internal spaces serve as the grounds a constitutive *a priori* for internal symmetries, but such a story does not have the ring of plausibility. The second is that even for spacetime symmetries it does not survive far into the 20th century. Both the notion of spacetime as the grounds for a constitutive *a priori* and the above way of connecting the symmetries of spacetime and the symmetries of laws disintegrate with the

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time and of laws. Both speak as if uniform rectilinear motion is a well-defined physical quantity; but to make it so requires that spacetime have something equivalent to inertial structure, which in turn makes it possible to define absolute (i.e. non-relational) quantities of motion (such as absolute acceleration) that would contradict their idea that all motion is the relative motion of bodies.

advent of Einstein's general theory of relativity (GTR). In GTR no one spacetime serves as the fixed backdrop for physics since different spacetime structures belong to different solutions to Einstein's field equations. Moreover, most of these spacetimes lack any non-trivial symmetries on either the global or local level. There is still a sense in which Einstein's gravitational field equations satisfy a strong symmetry principle; but, contrary to what Einstein originally thought, this symmetry principle is not a relativity principle that generalizes to arbitrary reference frames the special principle of relativity that is implemented in Newtonian theories by Galilean invariance and in special relativistic theories by Lorentz invariance. Rather the symmetry principle satisfied by GTR is a gauge principle, about which I will have more to say below in section 5. In general, the change that GTR necessitates in our conception of symmetry principles is an underappreciated moral.

5 Symmetries and conservation principles: Noether's first theorem

Do symmetries of laws of motion[15] entail conservation principles and conversely? The answer is yes as shown by Emmy Noether, *if* the laws are in the form of differential equations that are derivable from an action principle, *if* the symmetries are continuous, and *if* the symmetries are variational symmetries. But before turning to Noether's theorems, I want to comment on the first condition.

The first antecedent condition means that the allowable motions are the ones that extremize an action $\mathfrak{A}=\int_{\Omega}\mathcal{L}(\mathbf{q},\mathbf{q}^{(n)},\mathbf{x})d^p\mathbf{x}$, where $\mathbf{q}=(q_1,q_2,...,q_m)$ stands for the dependent variables, $\mathbf{x}=(x^1,...,x^p)$ stands for the independent variables, and $\mathbf{q}^{(n)}$ stands for the derivatives of the dependent variables up to order n with respect to the independent variables.[16] The type of equation of motion that results from setting $\delta\mathfrak{A}=0$ are known as Euler-Lagrange equations $EL_k=0$, $k=1,...,m$. The vast majority of candidates for fundamental laws of motion in physics have this form. But this fact may represent an artifact of scientific theorizing rather than a fundamental feature of nature; for physicists choose equations of motion with an eye to quantization, and the standard route to quantization is via a Hamiltonian formulation, which can be produced once the a Lagrangian formulation is in hand. So one would like to know: (a) what are the necessary and sufficient conditions characterizing those systems of differential equations $\Delta_k=0$, $k=1,2,...,q$, that are equivalent to a set of Euler-Lagrange equations $EL_k=0$ in the sense that there is

[15]By laws of motion I mean to include not just laws describing the motions of particles but also laws that govern the time development of field quantities.

[16]The configuration variables $\mathbf{q}$ might describe positions of particles or values of fields. In many applications the independent variables $\mathbf{x}$ are those of space and time.

an invertible q x q matrix A_k^j of differentiable functions such that $\Delta_k = A_k^j EL_j$, and (b) how hard is it to satisfy these conditions? For second-order Newtonian equations of motion (say, $\ddot{q}_k = F_k(q, \dot{q}, t)$, $\dot{q}_k := dq_k/dt$ and $\ddot{q}_k := d\dot{q}_k/dt$) the problem is known as the *Helmholtz problem*. Helmholtz found a set of necessary conditions that were later proved to be sufficient for the existence of a Lagrangian formulation. In the special case of one degree of freedom it is known that these conditions are always satisfiable, but the general case of Helmholtz's problem remains unsolved. And for more general types of differential equations very little is known about the conditions that are necessary and sufficient for the equations to arise from a variational principle. In sum, we are in a state of ignorance about how hard, or easy, it is to satisfy the first antecedent condition.

A variational symmetry is one that leaves $\mathcal{L}$ form invariant up to a divergence term. Every such variational symmetry is a symmetry of the Euler-Lagrange equations that follow from the action principle (i.e. a variational symmetry carries solutions of the EL-equations to solutions), but in general the converse is not true.[17] Noether's first theorem states that the action admits an r-parameter Lie group $\mathcal{G}_r$ as variational symmetries iff there are r linearly independent combinations of the Euler-Lagrange expressions EL_k, $k = 1, 2, ..., q$, which are divergences. Thus, as a consequence of the Euler-Lagrange equations, there are r *proper conservation laws* of the form

$$Div(\mathbf{J}_j) = 0, \qquad j = 1, 2, ..., r. \tag{1}$$

where $Div(\mathbf{J}_j)$ stands for $\sum_{i=1}^{p} D_i J_j^i$ and D_i is the total derivative with respect to x^i and where $\mathbf{J}_s = (J_s^1, ..., J_s^p)$, $s = 1, 2, ..., r$, with theJ_s^i being functions of the independent and dependent variables. The $\mathbf{J}_s$ are called the *conserved currents*. When the independent variables are those of space and time, the time component of a Noether current can be integrated over space to give a Noether charge, and under appropriate boundary conditions this charge can be shown to be constant over time. It is worth emphasizing that for laws of motion that do not satisfy the presuppositions of Noether's theorem, there is no guarantee that a symmetry of the equations of motion will lead to a conserved quantity or vice versa.

A concrete application of Noether's first theorem is provided by interacting point masses in Newtonian mechanics where $\mathcal{L} = T - V$, provided that T is the usual expression for the kinetic energy of the particles and the potential V depends only on the interparticle distances. The resulting action $\mathfrak{A} = \int \mathcal{L} dt$ admits the inhomogeneous Galilean group as a variational symmetry, and the conservation principles that follow via Noether's first theorem are the conservation of energy, angular and linear momentum, and the uniform motion of the center of mass. Conversely, the existence

[17] The case of scaling transformation is a classic counterexample.

of these conservation laws entails that the action admits a 10-parameter Lie group of variational symmetries.

6 Invariance and objectivity: Noether's second theorem

The overarching theme of Nozick's *Invariances: The Structure of the Objective World* (2001) is that invariance is the root of objectivity: the familiar marks of objectivity–accessibility from different angles, intersubjectivity, and independence from people's beliefs and desires–are all to be explained in terms of invariances. This is a potentially powerful theme. But actual as opposed to potential power can only come from specificity: if objectivity is to be construed as invariance, we need to know what it is that can be, or fail to be invariant, and under what transformations the said things must be invariant if they are to capture objective features of nature. Naturally, Nozick has a good deal to say about these questions, not all of which is successful.

For instance, I think Nozick reaches too hard in trying to make a connection between objectivity and conservation principles:

> Emmy Noether showed that for each symmetry/invariance that satisfies a Lie group, there is some quantity that is conserved ... So [!] it is not surprising that laws that are invariant under various transformations are held to be more objective. Such laws correspond to a quantity that is conserved, and something whose amount in this universe cannot be altered, diminished, or augmented should count as (at least tied for being) the most objective thing there actually is. (81)

Here I must take exception with my teacher. For I do not see that it makes much sense to speak of gradations of objectivity of laws. And even if it did, I don't see how the invariances of laws could provide a means of assigning the gradations. If the spacetime arena is, say, Minkowski spacetime, then all laws of physics must be Poincaré invariant; if the arena is Minkowski spacetime equipped with a distinguished orientation, then a law of physics may or may not be invariant under time reversal, but I would not want to say that a law that is time reversal invariant is more objective than one that is not. And while conserved quantities may play a special role in a theory of motion, I do not see why a quantity that is conserved is any more objective or real than one whose value changes with time.

Nevertheless, I think there is great merit in Nozick's theme that *objectivity = invariance*. I will not attempt any general implementation of this theme but will settle for a more modest goal. In particular, I will concentrate on the question of when a theory of mathematical physics is committed to treating certain mathematical quantities definable in the theory as representing genuine physical magnitudes as opposed to mere mathematical artifice. If we had a criterion for sorting the quantities of theories into these two categories, then we would have a theory-relative criterion of objectivity: according to the theory, what is objective or real in the world is described by the behavior of the values of the genuine physical magnitudes of the theory. A non-theory relative criterion could then, presumably, be obtained by appeal to considerations of truth and completeness of theories. But I will not venture into these dark waters and will stay focused on the theory-relative criterion. Even so I have no general theory-relative criterion in my pocket. However, I do have two things to offer. The first is a theory-relative necessary condition; namely, to count as a genuine physical magnitude (relative to the theory) the quantity must be invariant under the gauge transformations of the theory, that is, the transformations that, from the perspective of the theory, are taken to relate different descriptions of the same physical situation. Of course, this condition is useless unless coupled with a means of identifying the gauge freedom of the theory. This is where my second offering comes into play. There is, I claim, a uniform apparatus which applies to all theories whose equations of motion are derivable from an action principle and which identifies the gauge freedom of such a theory. It is just here that Noether's second theorem comes into play. (Nozick was right that a Noether's theorem is a key to objectivity; he just picked up on the wrong Noether theorem!)

The second Noether theorem concerns the case where the action is invariant under an infinite dimensional Lie group $\mathcal{G}_{\infty r}$ depending on r arbitrary functions of all of the independent variables. It tells us that there are r linearly independent mathematical identities constructed from the Euler-Lagrange expressions EL_k and their derivatives. Such a case is one of underdetermination, a case where there are more "unknowns" (dependent variables) than there are independent Euler-Lagrange equations. When the independent variables are those of space and time the underdeterminism amounts to an apparent breakdown of Laplacian determinism. This can be seen more explicitly by noting that in the cases at issue arbitrary functions of time appear in solutions of the Euler-Lagrange equations so that a unique solution is not picked out by initial data. I say there is an *apparent* breakdown in determinism because the option remains open to blame the appearance of a breakdown on a redundancy in the descriptive apparatus of the theory in the sense that the correspondence between the state descriptions given in the theory and the "real" or "objective" state of affairs is many-one. In particular, one can take the elements of $\mathcal{G}_{\infty r}$ to be gauge transformations: the solutions of the Euler-Lagrange equations are divided into equivalence classes, two solutions belonging to the same class just in case they are related by a gauge transformation generated by an element of $\mathcal{G}_{\infty r}$; and the objective facts about a possible world (as described by the theory) are what are common to all the solutions belonging to a said equivalence class.

In the physics literature it is more usual to carry on the discussion of gauge matters in the Hamiltonian formalism, where gauge transformations connect instantaneous state descriptions rather than entire histories. For sake of simplicity, consider Lagrangians of the form $\mathcal{L}(\mathbf{q}, \dot{\mathbf{q}})$. In cases where Noether's second theorem applies the Hessian $\partial^2\mathcal{L}(\mathbf{q}, \dot{\mathbf{q}})/\partial\dot{\mathbf{q}}_j\partial\dot{\mathbf{q}}_k$ will be singular, and as a result the Legendre transformation from the Lagrangian velocity phase space $\mathcal{V}(\mathbf{q}, \dot{\mathbf{q}})$ to the Hamiltonian phase space $\Gamma(\mathbf{q}, \mathbf{p})$ implies that the canonical momenta $\mathbf{p}_j := \partial\mathcal{L}/\partial\dot{\mathbf{q}}_j$ are not independent but must satisfy a family of identities $\phi_s(\mathbf{q}, \mathbf{p}) = 0$, where $s \leq m$. In order that these *primary constraints* (which follow directly from the definitions of the canonical momenta) be preserved by the motion, *secondary constraints* may have to be satisfied. In order that the secondary constraints be preserved by the motion, *tertiary constraints* may come into play, etc. The subspace $\mathcal{C}$ of the Hamiltonian phase space $\Gamma(\mathbf{q}, \mathbf{p})$ where all the constraints are satisfied is called the *constraint surface.* One then finds the *first class constraints,* i.e. the constraints whose Poisson bracket with any constraint vanishes on $\mathcal{C}$. The gauge transformations are taken to be those transformations of the phase space that are generated by the first class constraints.[18] The gauge independent quantities–a.k.a. the *observables* of the theory–are then those phase functions $F : \Gamma(\mathbf{q}, \mathbf{p}) \to \mathbb{R}$ that are constant along the gauge orbits. The redundant structure of the original theory can be removed by passing to the reduced phase space $\tilde{\Gamma}$ obtained by quotienting the constraint surface $\mathcal{C}$ by the gauge orbits. If we are lucky, $\tilde{\Gamma}$ can be coordinatized by new variables $(\tilde{\mathbf{q}}, \tilde{\mathbf{p}})$ which makes $\tilde{\Gamma}$ the cotangent space of a reduced configuration space coordinatized by $\tilde{\mathbf{q}}$. Now any phase function $\tilde{F} : \tilde{\Gamma}(\tilde{\mathbf{q}}, \tilde{\mathbf{p}}) \to \mathbb{R}$ is an observable in the reduced theory.

I want to emphasize a number of points here. First, the constrained Hamiltonian apparatus is incredibly powerful: it offers a uniform treatment of gauge for all theories whose equations of motion/field equations can be derived from an action principle, i.e. the vast majority of the theories of modern physics. Second, it yields intuitively correct results in familiar cases. For instance, in Maxwellian electromagnetic theory formulated in Minkowski spacetime, the electromagnetic potentials are marked as gauge dependent quantities while the electric and magnetic fields (or more properly the Maxwell tensor) count as observables. Third, the apparatus helps to clarify the classic form of the dispute about the ontological status of spacetime and to make precise the connection of this dispute with the fortunes of determinism. To illustrate, return to the example from section 3 used to illustrate the moral that if determinism is to have a fighting chance in a classical spacetime setting, then an absolute or "container" view of spacetime requires that the structure of spacetime be sufficiently rich as to support non-relational quantities of motion. For someone

[18] This approach to gauge was developed by P. A. M. Dirac. For an authoritative overview, see Henneaux and Teitelboim (1989). For a more user friendly introduction, see my (2000b, 2000c).

who wants to be a relationist about motion this entails an abandonment of the container view. What that abandonment means can be made precise in terms of gauge transformations. Suppose that the equations of particle motion are derivable from an action principle $\mathfrak{A} = \int \mathcal{L}(\mathbf{q}, \dot{\mathbf{q}})dt$. The resulting Euler-Largrange equations are guaranteed to be invariant under the transformations that produce the threat to determinism illustrated in Fig. 2 if they are variational symmetries of $\mathfrak{A}$. But since these transformations induce transformations of the configuration variables $\mathbf{q}$ of the form $\mathbf{q} \to \mathbf{q} + \mathbf{f}(t)$ where $\mathbf{f}(t)$ is an arbitrary function of t, Noether's second theorem comes into play and we have a case of underdetermination for the Euler-Lagrange equations, confirming the intuitive argument of section 3 for an apparent breakdown of determinism. When one works through the Hamiltonian constraint formalism for appropriate Lagrangians, one finds (as expected) that the gauge invariant quantities are relative particle quantities, such as relative particle positions, relative particle momenta, etc. (see my (2002c)). The apparent breakdown of determinism is explained away by saying that what the proponent of the container view took to be two different particle histories is just a single history represented in two different ways in terms of gauge dependent quantities. Of course, it is an empirical matter whether a theory of this kind can save the phenomena involved in actual particle motions. The proponents of relational accounts of space, time, and motion tended to assume that a theory answering the bill was easy to construct. In fact, it wasn't until relatively recently that the details of the construction were worked out.[19]

7 Gauge, objectivity, and the general theory of relativity

Thus far the implementation of part of Nozick's formula *objectivity = invariance* by means of the recommended apparatus has gone swimmingly. But the application to Einstein's GTR yields some surprising and seemingly unpalatable consequences. One is that in the Hamiltonian formulation of the theory the first class constraints generate the motion, which is to say that the motion is pure gauge and that the observables are constants of the motion. Some philosophers and physicists have found this "frozen dynamics" so bizarre that they think it shows that the constraint apparatus, which is otherwise so fruitful and successful in other domains, has gone haywire when applied to GTR. But one lesson of 20th century physics is that results that initially shock our intuitions often have to been accommodated in the scientific image. In keeping with this lesson I want to suggest that much is to learned from trying to accommodate rather than dismissing the result in question (see may 2002a).

[19]See Pooley and Brown (2002) for a discussion of the implications of the absolute-relational controversy of various results due to Julian Barbour.

The problem of time and change in GTR is one aspect of a more general interpretation problem of a kind that philosophers of science claim to take to heart but have shied away from in the case of GTR. Whatever else it means to interpret a scientific theory, it means saying what the world would have to be like if the theory is true, and this in turn means specifying which quantities the theory takes to be "observables" in the sense of genuine physical magnitudes and under what circumstances these quantities take on values. In ordinary QM it is assumed we know more or less how to characterize the observables, and most of the interpretational angst is vested in the problem of how to assign definite values to these quantities in such a way as not to be so profligate as to run into impossibility results of the Kochen-Specker type or so parsimonious as to be unable to account for the outcomes of measurements. In GTR the situation is, so to speak, just the reverse. There is no problem about a value assignment rule: all observables always take on definite values. The problem is rather to construct the observables. Those who know a bit about GTR might guess that we can make a beginning on the construction by starting with "scalar invariants," e.g. things like curvature scalars. But on the line I am pursuing these quantities cannot count as observables since they are not gauge invariant (diffeomorphic invariant) quantities. The gauge invariants constructible from the basic dynamical variables of GTR include highly non-local quantities such as the four-volume integral of the Ricci scalar curvature over all spacetime (assuming such an integral converges), but obviously such quantities are not very useful in describing the outcomes of typical measurements and observations. Another class of diffeomorphic invariants is comprised by what can be called coincidence quantities, a name chosen to reflect the fact such quantities are the counterparts for fields of Einstein's "point coincidences" for material particles.[20] To illustrate, consider a solution to Einstein's field equations with the generic property that the spacetime metric does not have non-trivial symmetries. In that case the spacetime manifold can be coordinatized by the values of four scalar fields constructed from the metric and its derivatives. Then, for example, the taking on of the electromagnetic field of such-and-such a value coincident with the four scalar fields having values such-and-so is a diffeomorphic invariant. Note that an ontology comprised of such coincidence events is rather strange. We are used to thinking of an event as the taking on (or losing) of a property by a subject, whether that subject is a concrete object or an immaterial spacetime point or region. But the coincidence events in question are apparently subjectless. Note also that one doesn't

[20]See Howard (1999) for a disussion of this notion. The idea that physical reality is exhausted by such point coincidences was Einstein's way of trying to overcome the apparent interpretation of GTR due the fact that the diffeomorphism group is a gauge group of the theory. By modern lights Einstein was on the right track in that the gauge invariant quantities of the theory do evolve deterministically. Where Einstein went wrong was in giving a characterization of these gauge invariants that was too crabbed and didn't do justice to the field content of the theory.

verify the occurrence of a coincidence event by first measuring the values of the electromagmetic and the scalar fields in question, and then verifying that the required coincidence of the value of the former with the latter does indeed hold; for by themselves none of these fields are gauge invariant quantities and so cannot be measured. The verifying measurement has to respond directly to the coincidence. What this implication means for measurement and observation obviously requires spelling out, a task I cannot undertake here.

These strange features may be an indication that the interpretational stance I have suggested is on the wrong track. But it is surprising (and disappointing!) to me that philosophers of science think they can know this *a priori.* I propose that one way of testing an interpretational stance for classical GTR is to see how well the stance lends itself to promoting a marriage of GTR and quantum physics that issues in a successful quantum theory of gravity. And here I would like to correct the impression conveyed by the popular media that string story (or M-theory, or whatever it is now calling itself) is the only viable route to a quantum theory of gravity. In fact, the loop formulation of quantum gravity–which uses the interpretational stance I have been pushing–is a viable program. In particular, in contrast to M(ystery)-theory it is a genuine theory rather than a wannabe theory, and it has enjoyed theoretical success (e.g. explanation of black hole entropy).[21] Furthermore, it may be technically feasible to test its predictions in the near future. If this approach to quantum gravity falters for reasons connected with the suggested interpretational stance, then that stance is disconfirmed. But, to repeat, if philosophers think that they can prove *a priori* that this will happen, they have an obligation to submit their results to the *Physical Review* so as to kill off a non-viable program.

8 The cosmological constant, the fate of the universe, and change

The cosmological constant Λ has had a long and checkered history since its introduction into GTR in 1917, with periods where Λ plays an important role in cosmology alternating with periods where it pushed off stage (see my (2001)). We are presently in a period where Λ–or some surrogate for Λ–is holding center stage.

The cosmological solutions to Einstein's gravitational field equations that are thought to capture the large scale features of our cosmos are called the Friedmann-Walker-Robertson (FRW) models. In these models the scale factor a obeys the equation

$$\ddot{a} = -(\rho + 3p)a + \Lambda a \tag{2}$$

[21]For a review of loop quantum gravity, see Rovelli (1998).

where $\ddot{a} := d^2a/dt^2$, t is cosmic time, and ρ and p are respectively the density and pressure of matter. Recent observations of Type Ia supernovae indicate that the rate of expansion of the universe is increasing, i.e. $\ddot{a} > 0$. From equation (2) it then follows that either the cosmological constant must have a positive value or else the universe is dominated by a strange form of matter-energy (commonly dubbed "quintessence") which exerts a sufficiently negative pressure that $\rho + 3p < 0$, in violation of the so-called strong energy condition thought to be satisfied by all normal forms of matter.

For present purposes I will assume that Λ rather than quintessence is at work and will note two consequences of this assumption. The first is that if a positive cosmological constant is indeed responsible for the speeding up of the expansion of the universe, then the universe will expand forever. This might seem attractive since it means that in the future direction time stretches to infinity. However, if a positive Λ is at work then it is doubtful that critters anything like us will be able to take advantage of this eternity since in the distant future the universe will become increasingly inhospitable to life as we know it (see Krauss and Starkman 2000) This is a topic well worth exploring, but I now drop it since I want to emphasize the connection to the issues discussed above.

The connection is forged by asking the seemingly naive question: In what sense is the cosmological constant a constant? It must be a constant in the sense that it has the same value throughout spacetime–this is necessary in order that Einstein's field equations with cosmological constant imply the local energy conservation law in the form of the vanishing of the covariant divergence of the stress-energy tensor. But there is a further sense in which Λ must be a constant, at least if the standard derivation of Einstein's field equations from a variational principle is followed. For that derivation implies that, on pain of setting the volume of spacetime to 0, Λ is not a dynamical variable in the sense that it does not vary from solution to solution.

However, there is nothing to prevent the cosmological constant as being treated as a spacetime constant within each solution but having a value that varies from solution to solution–in effect, the cosmological constant is treated as a constant of integration rather than a fundamental constant of nature. I will use the lower case λ to denote this sense. But recall that we are demanding that a candidate for a fundamental law of motion must be derivable from an action principle. Applying this demand to the λ version of Einstein's field equations leads to some interesting consequences. In particular, it is found that the derivation requires that spacetime of standard GTR be enriched by the addition of new object fields, and when the resulting action is run through the constrained Hamiltonian formalism it is found that the first class constraints are weaker than those in standard GTR and, hence, that the class of gauge invariant quantities is richer (see my (2002d) for details). In fact, in the λ version of GTR the dynamics is "unfrozen" in that there are gauge independent quantities that are not constants of the motion. This finding caused a flurry of excitement in the late 1980's in the quantum gravity community because it was thought that the λ version of GTR would overcome some of the obstacles in the canonical quantization program for producing quantum gravity. But when these hopes were dashed because of technical difficulties, physicists bent on finding a quantum theory of gravity quickly lost interest. Nevertheless Λ vs. λ remains for philosophers of science an interesting illustration of the interconnections among action principles, constraints, gauge principles, observables, etc., and it illustrates the power of the analaytical appatatus I have been touting to reveal these interconnections.

9 Spontaneous symmetry breaking

My final topic brings together several of the themes discussed above–the breaking of a lawlike symmetry by particular states, conservation laws, the Noether's theorems, and gauge freedom. Examples of spontaneous symmetry breaking in classical physics were discussed at this meeting by Chang Liu (2002). I don't have the time to pause to consider such cases. My focus will be on quantum field theory (QFT), and nothing in either classical mechanics or ordinary non-relativistic QM prepares us for what happens in cases of spontaneous symmetry breaking in QFT.

Consider a Lagrangian for a classical field admitting symmetries that form, say, a one-parameter Lie group. We know by Noether's first theorem that there is an associated conserved current. Now suppose that the field is quantized by giving a Fock space representation where there is a distinguished state (the "vacuum state") which gives the ground state of the quantum field and from which excited states are built up by applying creation operators. One can ask whether the action of the one-parameter symmetry group of the Lagrangian[22] can be represented by a one-parameter group of unitary operators on the Fock space.[23] Under very mild and reasonable assumptions the answer can be shown to be in the negative. If there were such a unitary group its generator would be a self-adjoint operator $\hat{Q}$ corresponding to the global Noether charge Q obtained by integrating the time component of the conserved Noether current over all space. But a simple *reductio* argument shows that if the vacuum is translationally invariant and if $\hat{Q}$ commutes with translations, then the existence of $\hat{Q}$ leads to contradiction.[24] This is puzzling to intuitions trained in ordinary QM where "symmetry transformation" and "unitary transformation" are

[22]From here on by "symmetry" I mean internal symmetry. For example, if the Lagrangian $\mathcal{L}(\varphi)$ for a real-valued scalar field φ is $\partial^{\mu}\varphi\partial_{\mu}\varphi$, then it admits the one-parameter group of internal symmetries $\varphi \to \varphi' = \varphi + \beta$, $\beta = const.$

[23]A unitary transformation of Hilbert space can be thought of as a rotation that leaves invariant the inner product of vectors.

[24]For a concrete example of this reductio argument at work and references to the literature see my (2002e).

virtually synonymous.[25] The puzzle deepens when one realizes that the non-unitary implementability of the symmetry leads to the "degeneracy" of the vacuum state, for the vacuum state is supposed to be the unique Poincaré invariant state. Part of the puzzle is resolved by noting that this uniqueness assertion is not contradicted by the relevant sense of degeneracy, which means that there are many unitarily inequivalent representations of the canonical commutation relations of the field, each with its own unique vacuum state. But again, this information is unhelpful to someone operating on intuitions trained on ordinary QM where no such phenomenon can arise.

The apparatus that illuminates the structural features of spontaneous symmetry breaking is the algebraic formulation of QFT. Here the basic object is taken to be an algebra $\mathcal{A}$ (usually a C^**-algebra*) of observables. A *state* ω is a complex valued positive linear functional of $\mathcal{A}$. The more familiar Hilbert space formalism is viewed as a way of representing this abstract algebraic structure. In more detail, a *representation* of a C^*-algebra $\mathcal{A}$ is a structure-preserving mapping $\pi : \mathcal{A} \rightarrow \mathcal{B}(\mathcal{H})$ from the abstract algebra into the concrete algebra $\mathcal{B}(\mathcal{H})$ of bounded linear operators on a Hilbert space $\mathcal{H}$. A fundamental theorem due to Gelfand, Naimark, and Segal (GNS) guarantees that for any state ω on $\mathcal{A}$ there is a representation $(\pi_\omega, \mathcal{H}_\omega)$ of $\mathcal{A}$ and a cyclic vector $|\Psi_\omega\rangle \in \mathcal{H}_\omega$ (i.e. $\pi_\omega(\mathcal{A})|\Psi_\omega\rangle$ is dense in $\mathcal{H}_\omega$) such that $\omega(A) = \langle \Psi_\omega | \pi_\omega(A) | \Psi_\omega \rangle$ for all $A \in \mathcal{A}$; moreover, this representation is the unique, up to unitary equivalence, cyclic representation.

In the algebraic setting a symmetry transformation is realized by an automorphism θ of the algebra $\mathcal{A}$, i.e. a structure preserving mapping of $\mathcal{A}$ onto itself. In relativistic QFT one encounters cases of automorphisms θ and states ω where θ is not unitarily implementable in the state ω in the sense that in the GNS representation $(\pi_\omega, \mathcal{H}_\omega)$ determined by ω there is no unitary operator $\hat{U}$ on $\mathcal{H}_\omega$ such that $\pi_\omega(\theta(A)) = \hat{U}\pi_\omega(A)\hat{U}^{-1}$ for all $A \in \mathcal{A}$.

An automorphism θ of $\mathcal{A}$ can also be viewed as acting on states; viz., given a state ω on $\mathcal{A}$, θ produces a new state $\widehat{\theta\omega} := \omega \circ \theta$. Obviously, if ω is θ-invariant, i.e. $\widehat{\theta\omega} = \omega$, then θ is unitarily implementable with respect to ω; but the converse is not necessarily true. If θ is not unitarily implementable in the state ω, then ω and $\widehat{\theta\omega}$ determine unitarily inequivalent representations in that their respective GNS representations $(\mathcal{H}_\omega, \pi_\omega)$ and $(\mathcal{H}_{\omega'}, \pi_{\omega'})$ are such that there is no isomorphism $E : \mathcal{H}_\omega \rightarrow \mathcal{H}_{\omega'}$ such that $\pi_{\omega'}(A) = E\pi_\omega(A)E^{-1}$ for all $A \in \mathcal{A}$. If the GNS representations of ω and $\widehat{\theta\omega}$ have natural Fock space structures, then the vacuum is "degenerate" but in a sense wholly different from the degeneracy of ground states in ordinary QM: the degenerate vacuum states in QFT belong to different, unitarily inequivalent Hilbert space representations of the field algebra; or, if it is insisted that they occupy the

[25] I say "virtually" because some discrete symmetries, such as time reversal, are implemented by anti-unitary operators.

same Hilbert space, they belong to different superselection sectors.

Weyl algebras are C^*-algebras that capture the algebraic structure of the Weyl form[26] of the canonical commutation relations. Ordinary QM deals with finite dimensional Weyl algebras. The (irreducible, weakly continuous) representation of such an algebra is unique up to unitary equivalence.[27] Furthermore, any automorphism θ of such an $\mathcal{A}$ is inner, i.e. there is a unitary element $U \in \mathcal{A}$ such that $\theta(A) = UAU^{-1}$ for all $A \in \mathcal{A}$. Hence, when only a finite number of degrees of freedom are present, the key features of spontaneous symmetry breaking of QFT–non-unitary implementable symmetries and unitarily inequivalent representations–cannot arise. By contrast, the algebraic formulation of QFT reveals that there features are mathematical commonplaces when an infinite number of degrees of freedom are present. What is interesting to the physicist is that there are physically important instantiations of these mathematical commonplaces in condensed matter physics and elementary particle physics.

If this were all there was to the story it would still hold interesting morals for the foundations of QFT. But there was much more to come. Before various ideas in elementary particle physics could coalesce to form what became known as the Standard Model it was necessary to find a mechanism by which the particles could acquire their mass. It turned out that the answer was suggested by a means of avoiding an embarrassing consequence of spontaneous symmetry breaking. It was discovered that the spontaneous breaking of a continuous symmetry subject to Noether's first theorem together with some standard assumptions of QFT–such as Poincaré invariance, local commutativity, and the spectrum condition–implies the existence of "Goldstone bosons"–massless scalar bosons. Since there was very good evidence that such particles do not exist, it seemed that either spontaneous symmetry breaking had to go or else there has to be some radical modification in the way the business of QFT was conducted. A way out of this uncomfortable situation was found by Peter Higgs, who suggested, in effect, that the problem be changed. He showed that by introducing additional fields the symmetry group of the Lagrangian could be enlarged to an infinite dimensional Lie group whose parameters are arbitrary functions of the spacetime variables. One now is in the domain of Noether's second theorem and gauge transformations. Higgs further showed that the gauge could be chosen so that the Goldstone bosons are suppressed and that in this "unitary gauge" the new field had acquired a mass. As the semi-popular presentations put it, "Particles get their masses by eating the Higgs field."

Readers of *Scientific American* can be satisfied with these just-so stories. But philosophers of science should not be. For a genuine property like mass cannot be gained by eating descriptive fluff, which is just what gauge is. Philosophers of science

[26]This is a kind exponentiated form the CCRs that avoids complications about domains of definition for unbounded operators.

[27]This is a form of the Stone-von Neumann theorem.

should be asking the Nozick question: What is the objective (i.e. gauge invariant) structure of the world corresponding to the gauge theory presented in the Higgs mechanism? From the above discussion we know that there is in principle a way to answer this question. The constraint apparatus I described above in section 6 is applicable: when the shift is made from the Lagrangian to the Hamiltonian formulation, constraints will appear; find all of he constraints and single out the first class constraints; quotient out the gauge orbits generated by the first class constraints to get the reduced Hamiltonian phase space whose phase functions are gauge-invariant magnitudes; finally, quantize the unconstrained system to get a quantum field theoretic description stripped of surplus gauge structure. To my knowledge this program has not been carried out. To indicate why it is important to carry it out, consider the following three-tiered dilemma. *First tier*: Either the gauge invariant content of the Higgs mechanism is described by local quantum fields satisfying the standard assumptions of Poincaré invariance, local commutativity, spectrum condition, etc. or not. If not, then the implementation of the Higgs mechanism requires a major overhaul of conventional QFT. If so, go to the second tier. *Second tier*: Either the gauge invariant system admits a finite dimensional Lie group as an internal symmetry group or not. If so, Noether's first theorem applies again. But (since the other standard assumptions are in place) Goldstone's theorem also applies and, hence, Goldstone bosons have not been suppressed after all. If not, go to the third tier. *Third tier*: Either the gauge invariant system admits no non-trivial symmetries at all or else it admits only discrete symmetries. In either case Goldstone bosons are quashed. In the former case spontaneous symmetry breaking is not an issue since there is no symmetry to break. In the latter case it is possible that the discrete symmetry is spontaneously broken. But the usual argument for symmetry breaking using the conserved Noether current does not apply. And while it is possible that some completely different sort of construction will demonstrate the spontaneous breakdown of the hypothesized discrete symmetry there are no extant demonstrations that have more than a hand waving force.

The list of unresolved issues about spontaneous symmetry breaking includes not only these foundations of physics issues but more general methodological issues as well. High up on the list for the latter category is the issue of the role of idealizations of physics. I would propose as a condition of adequacy on any acceptable account of the role of idealizations that it imply that no effect is to be deemed a genuine physical effect if it is an artifact of idealizations in the sense that the effect disappears when the idealizations are removed. Now add two facts: first, actual systems exhibiting spontaneous symmetry breaking, e.g. ferromagnets and superconductors, are finite in extent; second, all of the more-or-less rigorous demonstrations of the features that make spontaneous symmetry breaking intriguing–namely, non-unitarily implementable symmetries and unitarily inequivalent representations–involve taking an infinite volume limit. The upshot is to bring into question the reality of these features.

10 Conclusion

Philosophers of science have barely scratched the surface of the topic of laws, symmetries, and symmetry breaking. What I find most attractive about this topic is that it brings into fruitful interaction issues from metaphysics, from mathematics and physics, from the philosophy of scientific methodology, and from foundations of physics. By the same token, the fact that all these issues are put into play means that the discussion is very difficult to control and that it is always in danger of getting lost in thickets of technicalia or degenerating into mush. Successfully confronting these dangers requires someone who understands and cares about the philosophy and who not only has a command of the mathematics and physics but can use it to illuminate and advance philosophical concerns. There are young people with these abilities. To them I say: The road ahead will be filled with tribulations and obstacles (not the least of which will be some of your colleagues), and it is uncertain what professional reward, if any, you will earn from traveling this road. But the unless some of you have the courage to make the journey, the discipline will be immeasurably poorer.

* The ungainly title is due to the desire to do homage to my predecessor, Bas van Fraassen, and my teacher, Robert Nozick.

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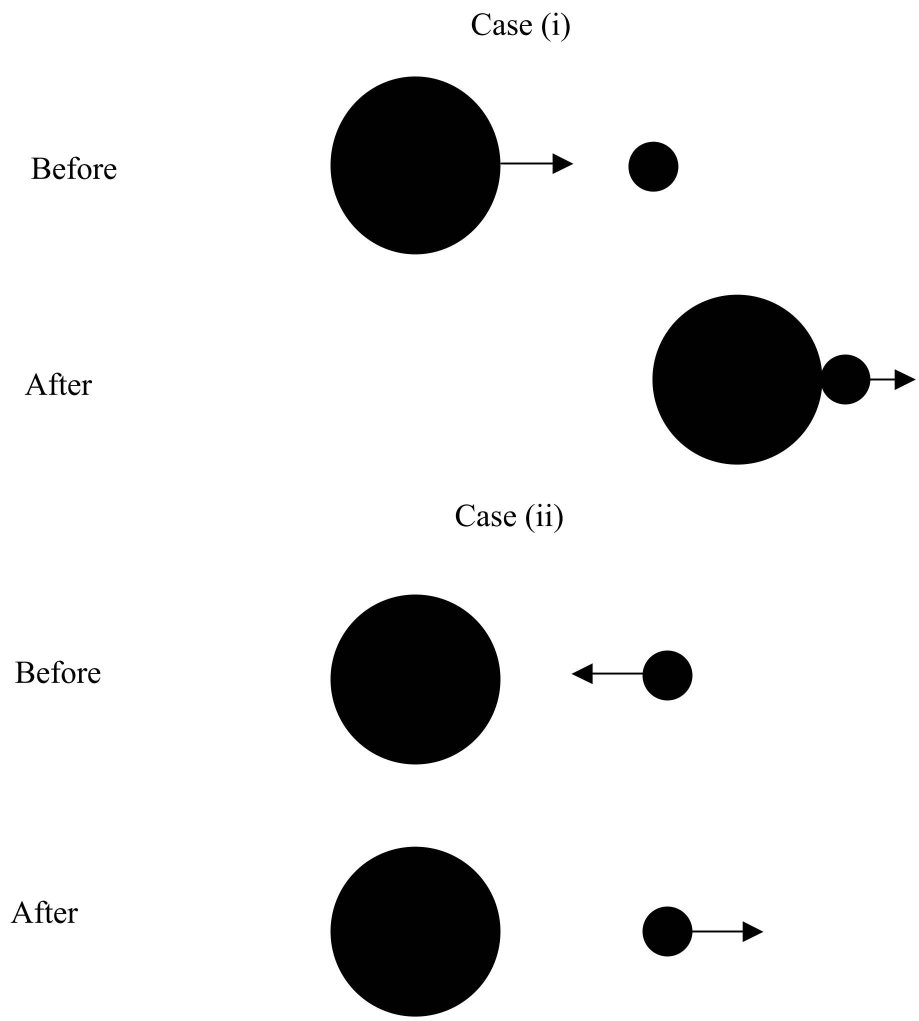

Fig. 1

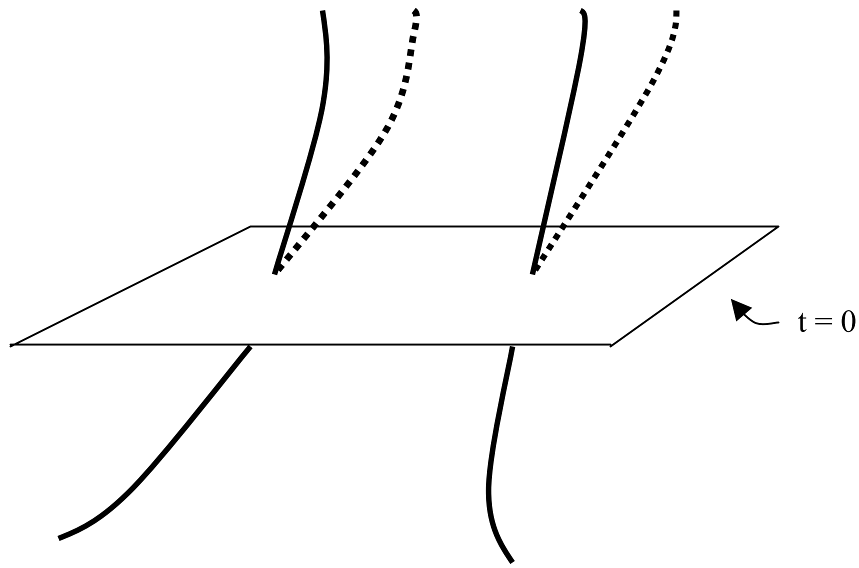

Fig. 2