The Structure of Scientific Theory Change: Models versus Privileged Formulations*

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Two views of scientific theories dominated the philosophy of science during the twentieth century, the syntactic view of the logical empiricists and the semantic view of their successors. I show that neither view is adequate to provide a proper understanding of the connections that exist between theories at different times. I outline a new approach, a hybrid of the two, that provides the right structural connection between earlier and later theories, and that takes due account of the importance of the mathematical models of a theory (the semantic component) and of the various distinct formulations that pick out these models (the syntactic component).

1. Introduction. Discussions of the generic features of theories in the sciences tend to focus on their predictive capacities. Sometimes an eye is turned toward what they predict, and sometimes toward what they rule out. This is to be expected since much of the business of science seems to be about determining what will happen, when, and under what circumstances. Also, not unreasonably, we judge our theories by how well their predictions correspond to subsequent states of the world. At the same time, theories are supposed to be aids to the understanding, used to explain and make intelligible worldly phenomena—and discussion often focuses on this aspect of theories (see Suppe 1977, 1989, and van Fraassen 1989).

In addition to these two aspects of scientific theories, in debates about

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the structure of scientific theories, it is crucial to attend to the role that theories play in the construction of their own replacements. Precisely here is where we find the crucial importance of the distinction between a purely model-theoretic account of theories and an account that takes due notice of how theories are actually formulated. For, as we will see, if we focus merely on the theory at a single time, then any debates about the theory's content that arise from a consideration of alternate formulations can be reexpressed as debates, not about structure, but about interpretation. Then debates about whether particular issues (for example, the question of the 'reality' of certain entities) are to be understood as on the one hand concerning the theory itself, or on the other as concerning merely how best to think about the theory—i.e., whether the issue is to be understood as about the theory or merely our interpretation of it—collapse into a clash of intuitions. Focusing on the diachronic aspects of theories, by contrast, allows us to see that debates about the proper way to formulate a theory often have to do with important aspects of the core theory itself—not metaphysical talk about the reality of entities per se, for example, but rather insights into the way the theory, as expressed in terms of one kind of entity or another, fits into our current scientific practice as that practice is poised to transform itself.

However, in the context of debates over the structure of scientific theories, little attention has been paid to the role that theories play in formulating their own successors. By structure, I have in mind precisely what is at issue between proponents of the syntactic conception of theories on the one hand and the semantic conception on the other. I don't believe it is entirely clear what we mean when we refer to the 'structure of scientific theories', but nor do I believe we should uncritically assume that we mean the classes of models associated with various scientific theories, as the semantic conception would have it. In a rough and ready way, I think 'theory structure' can be taken to refer to those features of theories that are relatively constant across scientific disciplines. For the logical empiricists, theory structure would be the general account of the canonical formulation of the theory, as brilliantly reconstructed by Suppe (1979, 50–52): a first-order language partitioned in the appropriate way into theoretical and observational parts with a semantic interpretation of the observational component that links it up with the world; and a partial interpretation of the theoretical component (involving theoretical postulates and correspondence rules) that links it up with the observational component of the theory. The content of any given theory was to be fit into the structure provided by the canonical formulation. On the other hand the semantic conception takes a theory to have the structure of a class of mathematical models, coupled with an assertion that the class of models contain the system of interest suitably idealized and mathematized.

(I'll have more to say about that shortly.) The content of any given theory is to be fit into the structure of mathematical models, picked out by some way of formulating the theory.

The two traditions provide very different accounts of what theories are, and these differences are what I am calling *differences in structure.* The opposition is that between sentences and the objects that satisfy them; hence, 'syntactic' versus 'semantic'. My thesis about structure is that not all content of a given theory can be shoe-horned into the structure provided by the semantic conception—there is something left out. This something is the class of distinct formulations of the theory. I am not advocating a return to the account offered by the logical empiricists. However, I do claim, and I will attempt to show, that the structure of scientific theories is best taken to involve, in addition to mere models, as well those ways in which the models are picked out, the formulations. I leave for another occasion an exact specification of *distinct formulation,* but roughly, I intend the class of entity referred to in picking out a model class to be included in the specification of the theory itself. For example, I will want to recognize as distinct a Lagrangian formulation of classical mechanics, and a Hamiltonian formulation. On the other hand, I do not wish to include as distinct formulations mere shifts from one romance language to another. For those who suspect that there is no distinction to be made between the cases, I offer the following essay. For those who suspect that much more work needs to be done to spell out the distinction in a way that does not fall prey to standard worries about definability and so forth, I offer my hope to return to the question in a future work. Let it suffice that here I do not wish to have the word 'structure' (as applied to scientific theories) understood as 'classes of model', for it is my aim to challenge the adequacy of the latter as an account of the former.

It should be obvious that existing theories are employed in the act of theorizing about new domains not intended to be covered by these existing theories. It also seems obvious that new theories are constructed using the very theories they are meant to replace. It may be questioned whether their role in theory revision is a fundamental fact about theories, or rather a consequence of the fact that we do not yet possess a complete theory of nature. For if we ever do possess such a theory, then surely it will not be employed to construct a replacement. One reply to such an objection is that there is little basis for assuming that such a thing as a complete—in the sense of finished and immune to revision—theory of nature is possible. While I think the reply is correct, I won't argue for it here. I will rather observe that, if our theorizing about scientific practice is to reflect faithfully that practice, then we should recognize that all of the scientific theories to which we have access are incomplete in various ways, and that a theory of theories should take this incompleteness, and the attendant role that theories play in theory construction, into account.

The purpose of the present work is twofold. Its primary purpose is to indicate that this third role of theories—the construction of their successors—should be of interest to philosophers of science who are concerned with understanding the *structure* of scientific theories. I also wish to show that the current dominant approach to theory analysis, the semantic conception, cannot readily account for the role that theories play in the construction of replacement theories and that its failure follows directly from its defining condition—that the theory is nothing more than its class of models.

Proponents of the semantic view have long recognized that structural features do not exhaustively characterize scientific theories—there is much in scientific theories to study that is not theory structure (Suppe 1979). But both Suppe and van Fraassen maintain that the structure of the theory is the key underpinning of any reflection on scientific theories. Suppe (1977, 3), indeed, goes so far as to say that any results impugning the structural account of theories that is associated with some philosophical program can undermine all results concerning other aspects of theorizing that follow from that program. In other words, whatever other virtues a theory of theories may have, these can be trumped by the vice of failing as an account of theory structure.

Thus examples showing that in the practice of science, theories are employed in ways that are at odds with the semantic conception's account of theoretical structure, would seem to cast some doubt on the adequacy of that conception. If the *use* of a theory (in particular to extend the theory to a new domain and, ancillarily, to properly understand the current theory) very often requires that the theory itself be considered to be something other than what the semantic view calls a theory, then we must be suspicious of that view.

My strategy is as follows: I begin with a summary of the main features of the semantic conception in Section 2. There I outline very briefly how prediction of phenomena is accomplished on the semantic conception. In Section 3, I challenge the semantic conception's ability to make sense of a current debate in physics—the status of particles in quantum field theory. I do not attempt to settle the matter, but merely ask if the semantic conception can make sense of the debate *and* make sense of it as a debate about the theory itself. The answer: yes and no. In Section 4, I consider a more pressing worry—how to account for theory construction and transformation using the semantic conception. Here the semantic conception faces two vexing challenges, neither of which it meets entirely satisfactorily and to the second of which, I claim, it is completely inadequate. The challenges come from episodes of scientific theory change: the transition from Euclidean to non-Euclidean geometry; and the quantization of general relativity. I conclude that while the technical apparatus of the semantic conception is largely sound, its basic tenet, that formulations are irrelevant to theory structure, is unsound and must be modified.

It must be remarked that my treatments of these examples fall far short of detailed case studies. Such studies for these, and related examples, I hope to carry out in the near future. However the claims made for these examples are relatively uncontroversial and, except for the case of quantum gravity, sufficiently well known that such detailed treatment is irrelevant to establishing the main point of contention: that more account needs to be taken of the relevance of theoretical formulation to theory structure. Here, briefly, is a summary of the examples to be used: Debates over the status of particles in quantum field theory, if understood as debates about quantum field theory itself, are best understood as debates about how the structure of the theory is related to the structure of its successor theories. And, these debates are best understood as about the proper way to *formulate* the theory. The development of geometry as an historical science is driven (as early as Proclus) by debates concerning the proper *formulation* of the parallel postulate and its admissibility as a separate axiom. Finally, significant work in the quantum gravity community is done on ways to extend general relativity into the quantum domain. Here one very important approach, loop quantum gravity, proceeds crucially on the basis of treating *formulations* of the theory as part of the structure of the theory. Taken together, these examples and my examination of them, show that formulations are an important part of the structure of our scientific theories.

Before going on, I observe that most of my critical treatment involves van Fraassen's particular way of laying out the issues, and so his views will be the primary target of my considerations. Here I do not consider the semantic conception in full generality, and so one might think that other versions (for example, Fred Suppe's way of working out the view) are themselves tenable even if van Fraassen's is not. In my opinion, such a view would be mistaken. I take van Fraassen's version of the semantic conception to be a minimal version; I find quite compelling his claim that the proper question to be asking about the content of a theory is "how could the world possibly be the way the this theory says it is?" and moreover that here "realists and antirealists can meet and speak with perfect neutrality" (1989, 193). Those who think that the content of a theory (over and above its empirical adequacy) can help us to determine its truth value, may derive comfort from the fact that only van Fraassen's view of theory structure is here explicitly challenged; so be it.

2. The Semantic Conception. We will see later that there is a measure of irony in the fact that the semantic conception began as an attempt to cleave more strongly to scientific practice than did the received view. But first consider the following crucial observation:

> As actually employed by working scientists, theories admit of a number of alternative linguistic formulations—for example, classical particle mechanics sometimes is given a Lagrangian formulation and other times a Hamiltonian formulation—but it is the same theory regardless which formulation is employed. As such, scientific theories cannot be identified with their linguistic formulations; rather, they are extralinguistic entities which are referred to and described by their various linguistic formulations. This suggests that theories be construed as propounded abstract *structures* serving as models for sets of interpreted sentences that constitute the linguistic formulations. (Suppe 1989, 82)

So the semantic view conceives of theories as a certain kind of mathematical structure. In general, a theory is some n-tuple consisting of some sets of objects together with relations defined on their subsets. Appended to theories is a rule for use, or a theoretical hypothesis, asserting that the system of concern is relevantly similar to the members of the class. The standard example here is classical particle mechanics. It is conceived of as an ordered quintuple $\mathcal{P} = \langle P, T, s, m, f \rangle$. P is a set of particles, T is an interval of real numbers corresponding to elapsed times, s is a position function defined on the Cartesian product of P and T, m a mass function defined on P, and f a force function defined on the Cartesian product of P, T, and the set of positive integers (Suppes 1969, 13ff.).

Instead of worrying about ideal linguistic formulations of theories, the semantic conception takes the theory to be the things picked out by any legitimate formulation. Here's a recent formulation from van Fraassen's *Laws and Symmetry*:

> Suppes' idea was simple: *to present a theory, we define the class of its models directly*, without paying any attention to questions of axiomatizability, in any special language, however relevant or simple or logically interesting that might be. . . . This procedure is in any case common in modern mathematics, where Suppes found his inspiration. In a modern presentation of geometry we find not the axioms of Euclidean geometry, but the definition of the class of Euclidean spaces. (1989, 222)

For van Fraassen, the crucial mistake of the received view was to "confuse a theory with the formulation of the theory in a particular language" (1989, 221). And, for him, the great virtue of the semantic approach is that the formulations of a theory recede into the background of our concerns. We now have no interest in how a theory is formulated since, after all, the only crucial questions we could desire to ask are questions about the structure of the models themselves. "Of course," says van Fraassen, "to present a theory, we must present it in and by language. That is a trivial point. . . . But in the discussion of the structure of theories it can largely be ignored" (1989, 222).

For both Suppe and van Fraassen, the virtue claimed for the semantic approach is that it reflects the practice of actual science. Justification for the opinion that the semantic conception more accurately models scientific practice initially came from analogy to one of the practices of modern mathematics—model theory. In model theory, we do not attempt to construct a first-order axiomatizable language for the theory and propose various rules of interpretation. Rather, we define directly the class of models that are taken to be the subject matter of the theory. The case is similar in the empirical sciences. In physics, for example, one rarely hears anything about deductively closed sets of sentences and other such syntactic notions and, in fact, one hears a great deal about spaces of solutions to equations and hears solutions to, say, Einstein's field equations, described as set-theoretic *n*-tuples consisting of a manifold and some geometric entities on the manifold. So at least in the respect of describing *some* structural features of scientific theories, the semantic conception seems to fare well. And, indeed, its proponents are right to insist that much can be learned about scientific theories by paying close attention to their models. But I will argue that its structural account does not *fully* capture what is involved in the structure of a scientific theory.

The central focus of my account of the semantic conception should be clear (and while it does not take into account the subtly of thought of the proponents of that view, it is a fair summary of the main tenets of the approach): According to proponents of the semantic conception, the structure of a theory has nothing to do with how we go about elaborating its models; any formulations that pick out the same (or isomorphic) models (in the sense of mathematical structures satisfying the axioms of the theory) yield the same theory, and thus the formulation itself is irrelevant.

2.1. Prediction and Explanation. In *The Semantic Conception of Theories and Scientific Realism*, Suppe motivates the semantic conception as an answer to the question, What do scientific theories do? His answer:

> In general, a scientific theory has the task of describing, predicting, and (possibly) explaining a class of phenomena. It does so by selecting and abstracting certain idealized parameters from the phenomena, then characterizing a class of abstract replicas of the phenomena

> which are characterized in terms of the selected idealized parameters. These abstract replicas are physical systems. (1989, 67)

How does his answer help us to understand how theories are used to make predictions? The semantic account goes like this (Suppe 1989, 82ff.): We begin with a class of phenomena we want to make predictions about. For example, we may be interested in the behavior of falling bodies. Our initial theory of this class of phenomena will not attempt to characterize it fully. It will instead focus on certain features of this class—say their positions in space as functions of time and their masses. It will not be concerned (at least initially) with other features deemed irrelevant or too complicated (like their colors and shapes, respectively). It then defines from the parameters of interest new systems—*physical systems*—that become the subject matter of the theory.

Thus, the theory is not directly concerned with phenomenal systems, but rather with these abstract replicas, and it is these that it attempts to model. The theory collects all possible values for the parameters of these systems, and each set of assignments of values is a possible state of the system. It then specifies rules for transitions from one state to another. A sequence of states allowed by these rules is a possible history of the physical system. The theory is the set of possible physical systems (with their histories) according to this prescription—the *theory-induced physical systems.* In our example, the collection of all position functions allowed by the rules is the theory, and a theory-induced physical system is an assignment of allowed histories to each member of the given physical system (e.g., the abstractly characterized falling bodies).

To apply the theory to phenomena, we employ auxiliary theories of measurement to construct, from actually measured data, the data that would have been obtained if we had met the idealizing conditions of the theory. If, that is, we had taken data about bodies whose only properties were mass and position. From this new data, we construct yet another class of physical systems—*causally possible physical systems*. These are then compared with the theory-induced physical systems. If the sets are identical, the theory is empirically true, if not, it is empirically false. It is not the theory that selects the causally possible systems. The experimental methodology[1] determines which systems correspond to which phenomena. Thus, the theory never directly confronts the phenomena.

The prescription as presented in this way is quite simple. But in actually carrying out these steps, we see a number of complications. Suppes' de-

1. I do not have time, here, to consider this fascinating aspect of the semantic approach. But its details have little bearing on the main concerns of the paper. See Suppe 1989, 82ff.; Suppes 1993, Chapters 13–18; van Fraassen 1987, 112ff.

tailed analysis of how to carry out the prescription in even the simplest of systems shows just how complicated it can get (see Suppes 1969, Chapter 2). But we already knew that real science is messy, and its messiness doesn't detract from the elegance of the proposal. The simple conceptual story appears quite general, and in each case, the fleshing out of the details will illuminate the often obscure relation between theory and experiment. As a theory of measurement, I believe, the semantic conception is at its best. The clarity of the general outline is a reflection of the role Suppes played in the origin of the approach. Suppes' careful constructions of toy theories to illustrate how mathematics is applied to real-world systems are significant achievements, and the notion of physical system as the correct domain of applicability of scientific theories is a profound insight.

Finally, those who are interested in an account of explanation can find it here. The behaviors of real systems are explained by being constituted sufficiently similarly to these ideal systems that one can say that their behaviors are explained by the lawlike connections existing between the various states of the systems at initial times, and their future states.

3. Understanding Scientific Debates. Before continuing, I want to emphasize that my target here is the account of theories that identifies them with their models. Other accounts of theory structure (which do not make such an identification—even if called 'semantic' accounts) are not in the scope of my argument. The semantic conception, so understood, seems to fare well as an account of scientific prediction and explanation. Of course that would seem to be a minimal requirement. How does the semantic conception fare as an account of understanding the claims made by a theory? Specifically, how does the account fare as an adjudicator of debates about the content of scientific theories? We will see here that it doesn't fare as well.

In this section, I proceed in three stages. I first raise an interpretational question about quantum field theory and naively apply the prescription of the semantic conception in an attempt to answer it. The prescription fails. I then try again to answer the interpretational question using a more sophisticated understanding of the role that the semantic conception is supposed to play in such matters. I claim that the semantic conception still gets things wrong. Finally, I propose that theories should be understand as including, *as structural features*, various methods of formulation. This is a small enlargement of what it is to be a theory, and indeed in terms of content the modification I propose is insignificant. I believe, however, that such a modification is entirely at odds with the philosophical spirit of the semantic conception—we cannot maintain the core claim of the semantic conception that theories are to be identified with their models.

The notion of formulation remains vague; it is to be hoped that even vaguely understood, the notion will suffice for how I use it.

3.1. A Naive View. Let's begin by asking how, on the semantic conception, we are supposed to decide issues about the content of a theory. Van Fraassen gives a simple-seeming formula:

> Now the world must be one way or another; so the theory is true if the real-world itself is (or is isomorphic to) one of these models.
>
> The analysis of structure provided therefore leads also to a schematic description of content. For what a theory says, that is its content. (1989, 226)

His illustrative example for this point comes from general relativity, in which spacetime is curved and the analogues of straight lines in flat space are the geodesics of the curved spacetime. Van Fraassen tells us that in a general relativistic spacetime, the geodesics of the theory are the candidates for the paths of lights rays and freely falling particles (1989, 228). So when we accept the theory of general relativity, we regard the geodesics in the theory as corresponding to literal paths in the world. It is not that we necessarily regard it as *true* that paths in the world are general relativistic geodesics. Rather, we regard the theory itself as making the claim that the world is one in which real paths are described by the geodesics. It is, of course, a further question whether we are to *believe* the claims the theory makes.

Now let us consider the parallel case of particles in quantum field theory. Here we have a significant debate over whether quantum field theory really is a theory of particles. I must emphasize that I am not here interested in continuing the actual debate over the status of particles in the theory. Indeed, I'll be presenting quite a sketchy and stylized version of the debate. Instead, I am here interested in making clear what it is about quantum field theory that allows us to understand the debate about the existence of particles as about the *theory itself*, rather than our interpretation of it. So then, let us ask naively whether we are to regard particles as features of the world *according to the theory*. Well, at first blush, it certainly seems as though we should. In any model of quantum field theory, there are structures that correspond to one, two, etc., particles acting, being acted on, carrying the forces of the theory, and so forth. A naive application of the semantic conception tells us we should take particles as features of the world according to quantum field theory. For, every model admits them; they are preserved under all of the symmetries allowed by the theory. Indeed, they seem perfectly suited to the way one normally employs the theory; we are primarily interested in scattering phenomena involving sharply localized, discreetly interacting systems.

They thus appear to be just those systems that *would* behave as the theory says, *were* they to meet the idealizing conditions of the theory (see Suppe 1989, 82ff.). As van Fraassen says, "the *content* of the theory is what it says the world is like" (1989, 193), and in the case of quantum field theory, the theory seems to say that the world is like one with particles flying about doing things. Moreover, it might seem that particles are, in this sense, precisely analogous to the geodesics van Fraassen considered above: That is, as 'candidates' for the bearers of charge and mass and so forth. We might be tempted, without further reflection, to regard these states as corresponding to literal objects in the world. But there is a problem with this naive view.

3.2. A Real Debate. Given the persistence of the debates over the status of particles in quantum field theory,[2] it seems unlikely that these debates can be so easily resolved, and especially not by reflecting on the nature of scientific theories. The debates are of real significance for how we are to understand a basic feature of the theory. And moreover, as van Fraassen also instructs us, "[s]cientific models may, without detriment to their function, contain much structure which corresponds to no elements of reality at all" (1989, 214).

Let me then start again with one version of the debates as carried out by two leading physicists, Robert Wald and Steven Weinberg. Arguing against understanding quantum field theory as a theory of particles, Robert Wald describes the problem of constructing a quantum field theory on a curved spacetime:

> A student or researcher wishing to learn about these developments [in quantum field theory in curved spacetime] faces a number of impediments. In my view, the most serious among these is the fact that the standard treatments of quantum field theory in flat spacetime rely heavily on Poincaré symmetry . . . and interpret the theory primarily in terms of a notion of 'particles' (1994, ix).
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> In the past, much attention has been devoted to the issue of how to generalize the notion of 'particles' to curved spacetime . . . this issue is irrelevant to the formulation of quantum field theory in curved spacetime. . . . Quantum field theory is a theory of *fields*, not particles. (1994, 2)

The idea is that if we focus only on the models of normal quantum field theory—i.e., those with a certain symmetry—we tend to let our rep-

2. See Halvorson 2001 for a nice presentation of some important moments in the debate.

resentation drive our interpretation in illegitimate ways. Most obviously, we find that every model of quantum field theory in Minkowski space (or static spacetimes generally) comes with a natural particle interpretation. Now the interpretation of the theory as a theory of particles is not just natural, it thrusts itself into view because of the natural symmetries possessed by these models. And so, according to Wald, as a result, quantum field theory 'becomes' a theory of interacting particles. Indeed, the Standard Model of Particle Physics has absorbed this interpretation into its description. We can then see that focusing on entire classes of models tends to obscure the ineluctable addition of incidental features of the process of representation. That is, at least for Wald, it is entirely accidental to the theory that it include states that are naturally interpreted as particle states.

Looking at the world as given by the theory, when we have thought we were seeing particle states, we were really seeing the symmetries of Minkowski space.[3]

The problem I have identified—that quantum field theory is not really a theory of particles but only seems to be because of the symmetries of spacetime—might be addressed by the semantic-view theorist as a failure to look at all models. The semantic view, she might observe, requires one to look at all models before drawing conclusions about the structural features of the theory. The fact that we mistakenly conclude that quantum field theory is a theory of particles indicates only that we need to broaden our class of models to include those of quantum field theory on a dynamical spacetime. So it is our approximation scheme—restricting to flat spacetime—that is to blame. Once we relax that approximation, we no longer have the option of interpreting the theory as a theory of particles, and so we are in the same position, with respect to the issue of particle states, as any other view of theories.

But to respond that we have left some models out of consideration, is to misunderstand the objection. For a given domain of nature, I follow the semantic prescription and produce a theoretical description. Associated to the description are various models, and to decide what the theory has to say about that domain, I recur to these models and read off those features that all have in common. When I do so, I identify the essential features of members of my model class, that is, features common to each model. I take it to be a legitimate inference, by one who regards the theory

3. There are other reasons to worry about the legitimacy of the particle interpretation of quantum field theory. Of major importance is the issue of localization. This issue is at the heart of Halvorson 2001. For the current work, I will consider only the worry about the model spaces themselves, and bracket worries about the proper interpretation of the actual states.

as given by the models, that features possessed by all models are features of the represented system. The approximation scheme used in a particular case comes not from the construction of the theoretical description, but from the model of the data as Suppe calls it. My business *after* producing the 'physical system' is to provide a theory of the system. If the models of that system, which constitute the theory, share certain features, then I need some reason to deny the attribution of these features to the system itself. If, as Suppe and van Fraassen would have it, the intention of physical theory is to produce literal accounts of the world, it would be well to have, in our account of theories, a technique available for separating the artifacts of the modeling process from characteristics of the modeled system.

It is true that when we focus on the class of models itself and choose an exemplar from this class, we can say unintended things about the system on the basis of our interpretation of features of the exemplar. Yet the problem with quantum field theory is not a problem with the particular model, since any exemplar comes with a particular interpretation 'built in'. Rather, it is a problem with the whole class of models that satisfy the axioms of the theory and yet 'say' more than the theory itself does. I take the preceding example to illustrate that, on the semantic conception as commonly understood, the resources to talk about issues of interpretive import are unavailable, for nowhere in the models is a difference to be found between the symmetries of the fields and the symmetries of the framing spacetime—there are only symmetries of the mathematical models.

The inability to engage these interpretive issues is connected with an apparent tension in van Fraassen's view. First our theory is a class of models together with one or more hypotheses asserting that the world is (or is isomorphic to) one of these models. He also maintains that the models may contain much structure with no real-world correlate (1989, 225–228). The tension is resolved by noting that there he means the identity (or isomorphism) to be restricted to allow only correspondence with empirical descriptions. But obviously we then need a way to distinguish those parts of our data models that are to be understood as empirical, from those that are not to be so understood. Without a boundary drawn between the observed and unobserved, it seems we cannot go far in our interpretive enterprise—an enterprise that is, for van Fraassen, of central import in our use of theories (1989, 189ff.). His criticism of the received view is focused on precisely this point. The received view is said to have obscured interesting features of theories by demanding that they be presented in specific form. By redirecting our attention from metamathematics to mathematics, it is thought that we can make more informative and revealing analyses of theories than could otherwise be made.

It is important to ask whether van Fraassen has similarly handicapped himself by requiring that we focus only on those aspects of our models that correspond to empirical substructures. On reflection, the answer is clearly *no*. He has, rather, shifted slightly the meaning of empirical. Here empirical is not to be taken in the sense of merely observable. Instead, the empirical structures are *candidates* for the representations of observational data. It is thus legitimate to take as empirical such statements as: continuous trajectory, etc. But now things have become confusing. For, what are these structural features of the theoretical model without real-world correlate? And how do we find out? Apparently we cannot simply draw conclusions about the world that go beyond the data. In some way or other we construct a 'data model' and our theory is to be responsible only to that. But we began this enterprise with the idea that theories could tell us something interesting about the world.

We have returned to the tension just alluded to. How do we determine which structural features of the models do not have real-world correlates? Since, for van Fraassen, the theory just is the class of models, it seems that, at least on his version of the semantic conception, we cannot.

Clearly the above version of the semantic conception is unable to shed light on the ontological commitments of quantum field theory. But a proponent would claim that the problem is to suppose that it should. A further look at van Fraassen's discussion of what is real for a theory reveals that he is unwilling to make any pronouncement. If one responds to questions like, what's real for the theory?, any answer is bound to be an interpretation (1989, 226). "We are also not altogether clear on where exact content ends and extrapolation begins. The demarcation is vague, and comes to light mainly when rival interpretations appear" (1989, 227). One can conclude that van Fraassen's version of the semantic view is simply too weak to specify fully the ontological claims of the theory, and should be used only to provide a convenient framework for organizing precisely these debates about the claims the theory makes,[4] and that the debate is not, strictly speaking, about the theory itself, but rather about how to interpret it.

The role of the philosopher in interpretive debates, then, is not answering the question of content but making clear the issues involved in the question of content. We do so by using the semantic conception's image of a theory to frame the debate between users of the theory as a debate about which features of the models are to be taken seriously. Here, then, is how the question of the significance of particles for quantum field theory should be addressed: There is a debate in the physics community

4. In a personal correspondence in 2000, Lisa Lloyd suggested this as the correct way to understand the semantic conception vis-à-vis questions of content.

about how we are to understand quantum field theory. According to quantum field theory, are there interacting particles that carry the forces of the theory? There is general agreement that the solution space of quantum field theory has certain features, one of which is a natural particle basis. But how to understand this feature is a matter of some dispute. On one side of the debate are those whose views are similar to Wald's: what quantum field theory says is that it is a theory of fields, and that the description of nature in terms of particles breaks down before its description in terms of quantized fields. On the other side are those, like Weinberg, who believe that the particles are a more certain feature of the theory than are the fields themselves—that quantum field theory itself becomes ineffective as a description of nature before the particle aspect loses its certainty. That is, some take Poincaré invariance to be of central importance, and thus wish to view the theory as one about particles, while others believe that, since we lose this invariance when working in a dynamical spacetime, we should not interpret the theory this way, but should instead view it as a theory of fields and not particles. This debate will be resolved not by philosophers but by physicists; the philosophers' contribution is to make clear what is being debated.

The dispute about particles is apparently quite easy to understand on the semantic conception as I have just outlined it. The diagnosis is that different physicists have different ideas about which features of the models are to be interpreted as features of the world and which arise merely through considering too narrow a range of values of the parameters. Thus Wald's objection to particle physicists is supposed to be that if one considers only flat spacetimes, then one is naturally liable to see particles as fundamental, and this problem is easily corrected by recognizing that the metric of spacetime is not fixed at some definite value but is free to change—so the curvature must be understood as a parameter, not a fixed feature of the theory. Even this account of the debate is difficult to reconcile with the semantic conception's claim that theories are just their models. For nothing in the model carries the information that the metric is one particular value of a parameter, rather than a fixed feature of the theory.

Still, the response is better than the naive caricature with which I began. Analysis of the structure of a theory should not, perhaps, be expected to pick out the correct interpretation of the theory. But, even this more sophisticated response fails to explain properly what is at issue between Wald and Weinberg. The debate is not about which features of the models to take seriously. Instead, the debate is about which formulations of the theory are to be preferred.

3.3. Formulating the Debate and Formulating the Theory. Particle theorists don't see their role as 'interpreting' the theory in terms of particles, they *begin* there. Steven Weinberg sets out with the task of constructing a theory of particle interactions. He says:

> I start with particles in this book because they are more *certain*, more directly derivable from the principles of quantum mechanics and relativity. (1995, 1)

> If we want to know why quantum field theories are the way they are, we have to start with particles. (1995, 2)

And also for Wald, apparently, the issue is how best to *formulate* the theory. Very roughly, the distinction between the two views is the following: For Wald's construction, one begins with a classical field theory (already special relativistic) and quantizes it. For Weinberg's construction, one begins with particles and asks how their states are represented after applying the rules of quantum mechanics and special relativity. What are we to make of this difference? We have two leading physicists considering (ostensibly) the same theory, and yet their views of how it is best formulated are largely at odds with one another.

In quantum field theory, prior to work on its algebraic version, one could not simply abandon the particle picture with the obvious device of removing the simple spacetime symmetry and allowing the metric to vary. For, in at least some treatments of this theory, especially Weinberg's development of it, particle states do not arise as a result of imposing the symmetry. Rather, in these cases, the states arise from the attempt to deal with quantized particle states in a special relativistic setting. That is, these theorists begin with classical particle states and then attempt to impose the symmetries. Indeed, it is precisely for this reason that, as Wald observes, much effort has been spent trying to generalize the notion of particle for curved spacetimes. Particle physicists are seeking, but not finding, these generalizations not because the models of quantum field theory on Minkowski space are the way they are, but because they are attempting to extend the particle formulation to curved spacetime domains. Without the algebraic version of quantum field theory (or something else not yet discovered), one cannot produce a satisfactory account of quantum fields on a (general) curved background. In presenting a scientific theory, one does not present its models directly and then examine the features of these models. Instead, one must display the models as solutions to sets of axioms, or equations, or whatever. Far from being the triviality mentioned by van Fraassen, this feature of theories is crucial. To understand the theory, it is necessary to understand how its models can be generated. To know, for example, that the theory of quantum fields can apply to a general curved background spacetime, one must produce a formulation of it that has an appropriate solution space. The algebraic formulation has it; the particle formulation, apparently, does not.

The example of quantum field theory makes two things clear. First, it shows that when using a scientific theory, one may pay very little attention to the models of the theory. Of course, it is true that the models are carried along with every appropriate set of axioms or given formulation. So one could argue there is little incentive to distinguish between focusing on the models and focusing on the axioms from which they arise. Second, it shows that users of the 'same' theory, may have very different ideas about what that theory is. Now, neither Wald nor Weinberg takes quantum field theory seriously as a fundamental theory of nature. So the underlying issue in their debate concerns the appropriate direction in which to look for a new, deeper theory. But even the surface issue is not about how to interpret the states that appear in models of quantum field theory. They both agree that these states are usefully and naturally interpreted as particle states. Instead, at issue is how seriously to take these states, and that debate is one over which formulation is more appropriate for this theory.

I see only two ways to characterize their disagreement. Either they are considering two different theories or they are approaching the same theory in distinct ways, each emphasizing one particular technique of formulation above the others. So is quantum field theory one theory or many? The answer to this question is, I suggest, a matter of taste. One could take a new formulation of quantum field theory as a new theory—albeit a theory that has models equivalent to those of older theories. Then, of course, it would be obvious that the theory itself has more structure than that of its models, viz., the algebraic formulation leading to these models. On the other hand, one could take quantum field theory as a single theory that is now more completely understood—because we recognize a broader class of formulations of it. But, in either case, to present the theory one needs to recognize explicitly the variety of its formulations in order to separate out the features that are essential to the theory from those that are not. I do not believe the semantic conception has the resources to meet this demand. There is more to the theory than the models.

Can I be sure that I have recognized all possible formulations in arbitrary cases? Certainly not. Ideally, I suppose it would be best on my suggestion to include all possible relevantly distinct formulations within the theory. Instead, we are left with merely those distinct formulations that have been used by actual theorists. But to enable us to understand (more clearly than does the semantic conception) what is involved in disputes about the content of a theory, these formulations will suffice. On the other hand, if I may consider only the class of models in discussions about the content of a theory, I can arrive at a distorted picture of the debate. In the case of quantum field theory, I would mistakenly conclude that the real issue is over the symmetries: to relax or not to relax them. Instead, the dispute is best characterized as over how to *understand* the symmetries. Is quantum field theory about special-relativistic quantum particles or is it about quantized fields?

I don't claim that, to understand the issues involved in this debate, it is necessary to adopt my suggestion of including formulations in our concept of a theory. I do claim that, if we want to view the debate as a debate about the theory, the semantic conception gets the story wrong. If the semantic conception is correct that the entire structure of theories is contained in their models, we cannot view this particular interpretive issue as being about the theory. This is an unpalatable result, but semantic conception theorists need not choke on it. The results of the next section are not so easily swallowed.

4. Theory Change and Theoretical Formulations. The answer, as I see it, to the question of how to understand quantum field theory, turns on recognizing the peculiar connections that exist between quantum field theory and earlier (and later!) theories. The same models can arise from traditions that take either the fields or the particles as primary. The theory itself still does not force us to view particles either as real or as artificial. Instead, the theory exhibits itself as the crossroads of two traditions. By including, within the theory, information about its various formulations, we can recognize the connections between the current theory and its successors and predecessors. If we restrict ourselves to considering only the models, these connections are unnecessarily obscured.

I claim that my suggestion—that we include formulations within our account of theories—helps to illuminate what is going on when we try extend theories. The answer we adopt to the question, What is the quantum theory of fields? tells me in what direction I should look to find an extension of the theory—do I look for particle theories in curved spacetime or do I extend my field theory to that domain? In either case, I extend, not by directly extending my class of models in order to widen the existing theoretic structure as van Fraassen suggests (1989, 228, 230), but by extending a favored formulation of the theory in particular ways. What's the difference? As I already said, for van Fraassen its a trivial matter to observe that I have to extend using some linguistic formulation or other. But his prescription is still focused on the models. The idea is that I specify in some way what features I want the new models to have, and I use a formulation to pick them out. But as we will see, in some cases the choice of initial formulation of the original theory is essentially related to the class of models that gets picked out for the new theory: different formulation, different successor theory. By any token such a situation is far from trivial. By understanding the theory as given with its formulations, I can understand this process as truly extending the theory.

Old theories may be used in several ways to devise their own replacements. Among other techniques are using analogies between old regimes and new (for example, attempting to construct a theory of the atom by analogy to the solar system), and employing mathematical tools and techniques that are familiar and well understood (for example, constructing quantum mechanics using the Hamiltonian framework from classical mechanics). These techniques are familiar and make no particular demands on how we are to understand the structural features of scientific theories. Another technique is to treat the variables from the old theory in ways appropriate to new situations (for example, picking a favored formulation of the old theory and extending it into new regimes). It is on the last mentioned that I wish to focus. For this, the case of quantum gravity is intrinsically interesting especially in the version I address.

Before I turn to quantum gravity, I revisit briefly a familiar example from the history of science that provides an interesting challenge to the semantic conception.

4.1. Euclidean to Non-Euclidean Geometry. Here I consider the transition from Euclidean to non-Euclidean geometry, and argue that formulations are crucial to understanding this development, and that therefore the semantic conception gets things wrong. The semantic conception has an explanation for its failure, although not everyone will find the explanation entirely satisfying.

Since the appearance of Euclid's *Elements*, there has been broad agreement that the parallel postulate is problematic. It does not, many felt, carry the same sense of obviousness and certainty as the other axioms. However, there was universal agreement that the postulate was true. Here already is an illustration of the importance of formulations of theories in geometry—the system of demonstrations comprising Euclid's system was not in doubt, and yet the formulation made people uneasy. I won't press this point.

One result, however, of these two convictions—that the parallel postulate is true, and that it is not axiomatic—was a lively industry of attempts to prove the postulate from others of Euclid's axioms, or to find a replacement axiom that would allow the parallel postulate to be proved but which would itself possess the obviousness and certainty that the other lacked. Indeed it was their participation in this industry that led Gauss, Bolyai, and Lobachevski to reject the parallel postulate and to develop non-Euclidean geometry (see Bonola 1955, especially Chapter 3).

The historical case here is very clear. Sustained attention to how best to formulate Euclidean geometry was crucial in the development of non-

Euclidean geometry. Thus the case for the importance of formulations in constructing new theories using older theories is made. Not quite. Van Fraassen has given his response to this example already. He says, "In a modern presentation of geometry we find not the axioms of Euclidean geometry, but the definition of the class of Euclidean spaces" (1989, 222).

Thus while historically correct, my account of the transition from Euclidean to non-Euclidean geometry is irrelevant to the concerns of the semantic conception. It ignores the crucial fact that the ancients (and early moderns) did not do geometry the modern way. Constructing new sets of axioms by modifying old sets is a method that has been superseded in modern science. We would now understand the relation between Euclidean and non-Euclidean geometry as follows: Begin with Euclidean geometry, i.e., its class of models. Now note that there is a parameter in the theory—the curvature—which has a value zero. Allow the parameter to vary from negative to positive infinity. The process generates a new model class—the class of all constant curvature geometries.

One could quibble with the semantic conception's response by demanding to know how the curvature parameter is to be recognized in the model class of Euclidean geometry. Indeed the example of the transition to quantum field theory on curved spacetime is quite similar in this respect. It is not clear how, by an examination of the model classes of quantum field theory, one is supposed to recognize the restriction to flat Minkowski space. Likewise, it is not clear how one is supposed to see that the class of Euclidean spaces is only a special case of the constant curvature geometries. I'll leave this issue aside and merely observe that the problem of how to recognize that a theory is a special case, by observing *only the models,* needs to be addressed by semantic theorists who take seriously the importance of theory structure for theory construction.

4.2. Loop Quantum Gravity. I turn now to a case that does not appear to have such an obvious (even if not entirely satisfying) interpretation under the semantic conception.

In the effort to construct a quantum theory of gravity, there are several distinct approaches. An important and promising one is the so-called loop quantum gravity. This approach is the most successful to date of those that begin with classical general relativity and attempt to construct a quantum analogue (a broad class of approaches). While I do not wish here to advocate for one approach over the others, I will claim that the loop quantum gravity approach is fruitful enough, and theoretically well-grounded enough, that it can be used to illuminate our conception of scientific theories as that conception is used to interpret the work of constructing new theories. While there is little consensus in the scientific community about what the future theory of quantum gravity will look like, the loop approach is not particularly atypical insofar as it exemplifies how one goes about looking for new theories. For my purposes, the most important aspect of the loop program is how it begins. It starts with general relativity as a theory of connections on a manifold (connections are what let you parallel-transport things in the theory), *not* as general relativity is normally introduced as a theory of metrics (which allow you to measure intervals). We can think of this, very roughly, as taking the Christoffel symbols as the dynamical variables and then solving for the metric, rather than the other way around. Classically this is of little importance since the two approaches coincide under plausible auxiliary conditions (see Ashtekar 1991, 51 for details). In effect they are two distinct formulations of the very same theory. Yet, as tools for quantizing gravity they are strikingly different.

Ashtekar (1991, 38–39) summarizes the advantages of the connection approach as follows: 1. The equations are easier to solve when stated in terms of connections and, in part for this reason, more generally applicable; 2. It makes the theory more closely resemble conventional quantum field theories (Yang-Mills theory), and hence allows us to use many of the techniques developed for these theories—techniques unavailable to standard formulations; 3. The theory is more easily interpreted geometrically, and geometry is precisely what we want quantized in a quantum version of general relativity. Of these, perhaps the second is the most important from a conceptual point of view. By focusing on just one formulation of the theory, technical matters are more tractable, but more importantly we gain a road map for how to proceed at successive stages of theory construction. The details of this construction, while fascinating, are not important for the present discussion. What is important is how the loop approach employs classical general relativity in producing a quantum version. Here we do not identify general relativity with the various models it admits. The work is done in this case by the careful reconstruction of one exemplar formulation of the existing theory and extending *it* in certain ways—we extend the formulation *itself* using standard techniques of quantization to produce a formulation of a possible quantum gravity. If, when this is done, the new formulation is satisfiable, there will be some class of possible models for the semantic theorist to consider. But while it isn't at all clear what role these models played in the construction of the theory, the role of the formulations in that construction is obvious.

The really crucial point is the following: When one applies the standard tools of quantization that Ashtekar alludes to above to the metric version of general relativity one gets a mess, not a nice theory. The problems with the result are well-known, but I'll just mention that we don't know what the equations say, and moreover they are provably nonrenormalizable, and apparently unsolvable by any other means. By sharp contrast, the equations that result from applying these same tools to the connection version of general relativity are apparently interpretable (as equations describing quantum geometry)[5] and solvable. Indeed, the loop version of the Wheeler-deWitt equation is the only one I know of that has been solved.

We started from the same theory and applied the same construction techniques. But, because we began with different formulations, our results are strikingly different. It is time to recognize that there is no such thing as extending the class of models of a theory simpliciter. Instead we extend the theory as a whole. And, in important and apparently typical cases, we do this by extending one of its preferred formulations into a new domain.

5. Conclusion. As I remarked above, the theoretical work done in the quantum gravity community is not an attempt to extend the class of models of quantum mechanics and general relativity. Nor is it some misguided attempt to find the overlap between their models. It begins with a theory and produces a particular formulation of it. Sometimes the theory is general relativity, sometimes quantum field theory. This formulation then forms the basis for efforts to extend the theory. When, and if, a satisfactory theory of quantum gravity is produced, it is certain that the models of the theory will not have, as a subclass, those of general relativity. It is to be expected that quantum gravity will be applicable to the regime in which general relativity has had success, so they may share *data models*. But the kinds of thing general relativity is about are very different from those to be described by quantum gravity. We see that the case of geometry is perhaps not so anachronistic after all, for at the very leading edge of modern physics we find the same method again employed as we try to extend the theory into new domains.

Van Fraassen's incomplete description of how new theories come about from older ones is symptomatic of his exclusive focus on theories as sets of models. By instead including the class of formulations of the theory in our account of its structure, we can more naturally account for the utility older theories have in the construction of replacements. When the models of a new theory have little to do with those of the old, it is difficult to see, on the semantic account, what these theories have to do with each other. The 'physical systems' constructed from them will be very different and they will be related only insofar as, in regimes where they are both

5. I don't claim that all issues of interpretation are transparent on this view, but rather that we seem to have a good place to start. Consult Ashtekar and Lewandowski 1996 for an introduction to the quantum theory of geometry.

applicable, they share data models. On the other hand, the inclusion of formulation schemes into the theory allows them to be naturally connected sub rasa as successive attempts to represent the same reality. We might fruitfully compare scientific theories to organic structures like mushrooms for example. What those who advocate the semantic account of theories have been focusing on are merely the fruiting bodies, so to speak, of a vast network of hidden structure. I submit that the most natural way to view this hidden structure is as an integral part of the theories themselves. Sometimes perhaps new theories result from the sporing of these momentary excrescences, i.e., the direct manipulation of the discrete collections of models. Much more typical in my view are those cases when the models of a new theory are connected with those of the old by way of this network of hidden structure—the formulations.

The issue here is that theories are being used to construct their own replacements and this is possible, not in spite of their alternate linguistic formulations, but because of them. To understand this use, it is crucial to recognize that these formulations are key aspects of theories that provide their users with the flexibility to adapt them to new situations. That theories are presented in some formulation or other is not a trivial point about language users that merits little attention. Rather, the elaboration of alternative formulations undergirds an important element of theoretical structure.

The crucial distinction between my account of theories and that of any version of the semantic conception is that what people do when they theorize is reflected more faithfully on my account; the semantic conception answers the question, How is the new theory compatible with the old one? What I will call the *privileged formulations account* answers the question, What role is the old theory itself playing in the search for a new theory? Another way to put the point, and slightly less tendentiously: when both the semantic conception and a conception taking due note of privileged formulations ask what role the old theory is playing, the semantic conception answers that the models are used to construct new models, and the evidence used to justify this claim is the way that various formulations of the theory are appealed to by theorists. The privileged formulations account stands in sharp contrast: rather than attempting to discount the varieties of theoretical formulations, it notes that scientists are constructing new theories in ways that rely crucially on which formulation is being used. We see theorists privileging some formulations over others; our account of theories should do the same.

I have not argued that any particular components of the semantic view must be dismantled to accommodate this extra structure, and so I suppose I could be seen as suggesting an augmentation, rather than a replacement, for the semantic view. I do not know whether that is really what I am doing. If a proponent of the semantic view is willing to countenance the inclusion of formulations into the structure of a theory, then this could be seen as an augmentation. But, recall how Suppe introduces *The Structure of Scientific Theories*:[6]

> It is only a slight exaggeration to claim that a philosophy of science is little more than an analysis of theories and their roles in the scientific enterprise. A philosophy of science's analysis of the nature of theories, including their roles in the growth of scientific knowledge, thus is its keystone; and should that analysis prove inadequate . . . [a]t the very least, it calls for a reassessment of its entire account of scientific knowledge. (1977, 3)

It is trivial to observe that science is not interested in theories for their own sake, but only for the things that can be done with them. It is widely claimed, both by proponents of the semantic conception and others, that two very important uses for theories are explanation and prediction. I have proposed a third: theory construction. At least in the area of quantum gravity, the latter is of great importance. For here there is little data against which to check the predictions of the theory, and so comparison with the predictions of other theories (used as approximation schemes) is crucial. Moreover, one cannot, from scratch, produce a viable theory of quantum gravity. Instead theorists employ older theories, in ways I have partially elaborated above, to guide the construction of their new theories. I take the point that theories are used in theorizing to be, by itself, too obvious to warrant much attention. However, the insights gained by examining in detail the ways in which theories are so employed, and the crucial role of formulations therein, have significance for the way we understand the nature of scientific theories.

I claim that to understand a theory is, in part, to understand the structural connections that exist between it and older theories for which it is a replacement, and also to understand the various ways in which the theory might be modified in turn. To understand these structural connections, we must move beyond a narrow focus on the models of the theory and broaden our view to include the possible ways of formulating the theory that result in these models.

6. It is only a slight exaggeration to claim that Suppe equates the nature of theories with their structure.

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