# The Semantic Approach and its application to Evolutionary Theory[1]

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## 1. Elements of the Semantic View

The semantic view of theory structure, as developed by Suppes (1957,1967), Suppe (1974, 1976, 1977, 1988), and van Fraassen (1970, 1972, 1980), represents theories as classes of models or structures. These models are, on the version of the semantic approach used here, defined by specifying their laws, parameters, and variables. The semantic approach to theory structure is simply a method of formalizing the content of scientific theories.

## 2. Application of the Semantic View to Evolutionary Theory

In a series of articles and a book, I have analyzed the structure of modern evolutionary theory using the semantic view as a framework (Lloyd 1984, 1986a, 1986b, 1987a, 1987b, 1988, forthcoming; cf. Thompson 1983, 1985,1988). I shall briefly recap the analyses I have done, in order to demonstrate the range of problems accessible to the semantic view.

First, I have used the semantic view to analyze the structure of population genetics models, including kin and group selection models. There is a heated debate in the genetics literature about whether kin selection models should be interpreted as group or as organismic selection models. I have offered an analysis of this problem utilizing the semantic view (1988, Ch. 5).

The semantic view is not applicable exclusively to mathematical models, however. I have also analyzed the basic structure of natural selection models, and the interrelations among the components of these models; these models are characterized non-mathematically (see esp. 1988, Ch. 6). I use this characterization of the basic structure of selection models to offer a new definition of a unit of selection in terms of its actual role in models. This definition, in turn, allows a precise formulation of several controversial problems involving units of selection.

For instance, I use my structural definition of a unit of selection to compare species selection models with other hierarchical selection models (Eldredge and Gould 1972; Gould and Eldredge 1977; Eldredge and Cracraft 1980; Vrba and Eldredge 1984; Vrba 1984; Vrba and Gould 1986). I find that there are certain discrepancies between the structure of species selection models and other selection models, and I suggest a new formulation of species selection models which is consistent with the general structure of selection models (1988, Ch. 6; Lloyd and Gould ms.).

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My analysis of the structure of population genetics models is also useful in understanding what is wrong with genic selectionism (1988, Ch. 7). The semantic view allows a precise formulation of exactly how and why genic selection models are bound to be empirically inadequate, under one interpretation, or trivially different from other models, under an alternate interpretation. In particular, I demonstrate why the attempted resurrection of genic selectionism by Sterelny and Kitcher (1988) misses the point of the debate.

Use of the semantic approach to theory structure allows precise formulation of various different questions about units of selection. Questions about which entities are functioning as replicators, and which as interactors, need to be kept completely distinct, and the semantic view allows the precise translation of this distinction in terms of laws and state spaces.

Finally, I also use the semantic view of theory structure to develop a schema of theory confirmation that is more subtle and sensitive to genuine scientific concerns than traditional approaches in philosophy of science (1988, Ch. 8).

## 3. Is the Semantic View doing any Real Work?

Anyone who wants to argue that the semantic view has done nothing for analyzing biological theories needs to show either that the use of the semantic view is doing no real work in the analyses reviewed above, or that the analyses themselves are ineffective. I would certainly not claim that using the semantic view formally is the only way to make progress on issues in philosophy of biology. I do find it suggestive, however, that the fine analytic work of Brandon, for instance, is completely compatible with, and in many ways, suggestive of, an informal use of the semantic view of theory structure (e.g., Brandon 1981, 1982). At any rate, I have no attachment to the "manifest destiny" of the semantic view. Workers using the semantic view have created careful reconstructions of some important parts of evolutionary theory using this approach, and the use of this analytic framework has, I would argue, contributed to the clarification of a number of issues in the philosophy of biology.

I know of a number of people who have dismissed the utility and value of the semantic view on account of some basic misconceptions. I would like to take some time today to review what I see as the three most common misconceptions about the semantic view.

## 4. Three Common Misconceptions about the Semantic View

By far the most common complaint is the following: "The semantic view necessarily involves anti-realism. I am a realist, therefore I must reject the semantic view".

As a matter of fact, the semantic view, as a view about theory structure, is *neutral* on the issue of scientific realism. There is nothing in the claim that scientific theories are usefully and clearly reconstructed as classes of model types which entails either realism or anti-realism. The epistemic attitude towards these models and the entities within them is distinct from the description of the structure of the models themselves.

It is especially surprising that people cling to the belief that the semantic view of theories is necessarily anti-realist, in the face of Ronald Giere's visible and long standing advocacy of both the semantic view of theory structure and realism (see Giere 1988). Fred Suppe's quasi-realist approach is yet another possible epistemic approach associated with the semantic view (Suppe 1988).

I do take it that Barbara Horan believes that the semantic view and anti-realism must go together. She, at least, has arguments for this view, rather than just assuming guilt by association. I shall address her argument later.

The second most common myth about the semantic view is that "the semantic view can only work with mathematical theories". This was never true, as evidenced by Suppe's analysis (1974) of biological taxonomy early on, and also by my work on Darwin (1983) and on species selection, and most recently, by James Griesemer's work on laboratory and museum models (Griesemer and Wade 1988, Griesemer ms.).

The supposed restriction to mathematical theories is no more true for the semantic view of theories than it is for model theory in general. It is easier to see how the semantic view would represent mathematical models, but this is in no way exclusive.

The third most popular misunderstanding of the semantic view, according to my informal survey is: "there is no substantive difference between the semantic view and the standard, received view of theory structure."

First, one logical point. If the received view is taken to require the use of first order logic only—which is the way it is often conceived—then any theory involving the real numbers is not representable within it, but would be within the semantic view. This formal point does not, however, strike me as getting to the heart of the matter. The sensible response is simply to lift the restriction to first order languages. Then the two approaches would seem to be equivalent.

But this is not quite right, because one of the advantages of the semantic view is that it, unlike syntactic approaches, is not restricted or committed to a particular *linguistic* formulation. This point has consequences for some rather important issues in theory identification and theory change. On the syntactic view, a change in linguistic formulation means that there is now a new theory. One can imagine many cases in which such a change would be trivial, yet the syntactic approach would still call the entity a new theory. On the semantic view, in contrast, two different axiomatic systems that are semantically equivalent—that is, they share the same set of models—constitute *one* theory, not two. In other words, the fact is that you can change the names without changing the meaning relations, and under the semantic view it is the relations which are taken to be *essential* in the description of the theoretical systems. Hence, there is an important difference between the semantic view and various syntactic approaches, namely that the semantic view avoids the worries about theory change and definition which arise from having particular linguistic formulations of a theory.

But let us step aside from these formal issues for a moment. Suppose someone were to say that the problems with committing to a particular language did not bother them. Let us imagine that the two systems of description of scientific theories are formally equivalent, that they contain precisely the same information, and are completely intertranslatable. Are there still reasons for preferring the semantic view? That depends on what you want to do. If you want a view of theory structure that can be used to analyze the empirical content of theories, discuss their interrelations with other theories, and examine how they are used by scientists, the semantic view has a clear advantage.

First, the semantic view is closer to the practices of scientists, and it provides a natural and convenient way of reconstructing theories and claims about those theories.

Second, the semantic view does not require laws of nature—a problematic concept, especially in evolutionary biology—though it also does not preclude the formulation and use of laws of nature.

Third, the semantic view has a better chance of representing scientists' problems in terms accessible to them, because it is closer to the form of scientists' own reasoning.

Fourth, the semantic view allows either a realist or anti-realist (or quasi-realist) interpretation of theories.

Fifth, the semantic view provides the framework for a much more subtle, faithful, and powerful analysis of how data can support or confirm an empirical claim.

In summary so far, I claim that rejections of the semantic view of theory structure often rely on mischaracterizations of this view. Furthermore, I think that Barbara Horan's objections to the semantic view lie in this camp, and I am especially sorry about that, because I also think Horan raises some important issues about theoretical explanations, testing, and the construction of scientific theories.

## 5. Horan's Criticisms of the Semantic View

I see two main problems with Horan's argument. The first is that she does not distinguish between philosophers' reconstructions of scientific theories and the scientific theories themselves. A *philosophical* preference for reconstructing and representing theories a particular way, as classes of models (semantic entities) is identified by Horan with the value of the *scientific* preference for certain abstract, general theories. But normative claims made by scientists about theory construction are *not* the same as normative claims made by philosophers about theory reconstruction. Without this identification, furthermore, Horan has no case against the semantic view.

The basic problem is that Horan identifies high-level theoretical models in population biology with the semantic models of the semantic view. Hence, she seems to think that (1) the semantic view demands these very high level models, and (2) that the semantic view can fairly be saddled with any problems associated with these high level models.

Let us consider the first claim, in which Horan identifies the semantic view with the demand for very high-level, abstract models. Horan writes, "when theoretical models are constructed one eliminates as much of the detail about the case one wants to explain as possible...instead, the model builder selects a few variables and uses them to make the model a successful predictor...ignoring the details of other cases about which one wants to generalize..." (1989, p. 269.)

Horan claims that these models are unrealistic, since they are so abstract and removed from the biological details and complexity of each case. Therefore, according to Horan, the use of models is supposed to result in anti-realism. *This* is the basis of her argument that the semantic view is necessarily anti-realist. In her words, if we accept the semantic view, "which makes theoretical models an integral part of our conception of theories, we must accept anti-realism as well". (1989, p. 274.)

But the claim that the semantic view of theories demands high-level theoretical models is simply false. What a semantic view theorist actually says is this: give me a scientific description of a system—any system—and I prefer to represent that system in terms of its meaning structure. The semantic approach can be used at *any* level of scientific theory, including extremely detailed, low-level theories.

In fact, I take this flexibility of the semantic view to be one of its *virtues*. Furthermore, in my discussion of genic selection, I criticize the reductionist approach by showing that the genic level models do not ordinarily contain enough information to describe accurately the systems they are intended to explain (1988, Ch. 7). In other words, the fact that more complexity and detail are required in the model is clearly delineated and *defended* using the semantic view of theories.

I conclude that the complexity of biological phenomena emphasized by Horan is no problem at all for the semantic view. In fact, it is rather good for business. With many, many mid- to low-level descriptions of natural systems running around, we are unlikely to run out of work anytime soon.

Let me also mention a second basic problem I see with Horan's account of the seman-
tic view, which has to do with theory testing. I find that Horan uses the terms 'realism',
'robustness', and 'explanatory power' differently than I do—but here is my interpretation
of her argument. She claims that the semantic view "licenses a rather problematic infer-
ence", namely, "an inference from the successful prediction by a theoretical model in one
case to the conclusion that this model affords a general explanation that covers a wide
variety of cases". (1989, p. 269.)

Frankly, I do not know anyone who would want to buy that type of inference without
evidence, but let us consider whether it really does arise out of the semantic view. The
problem highlighted by Horan is that predictive success is not sufficient evidence for
truth. In her words, "in the absence of details about the individual cases to be covered by
the explanatory hypothesis, its predictive success cannot be taken as sufficient grounds
for concluding that it will be the correct explanation"; and for Horan, the correct explana-
tion is the true one.

I believe that Horan is unjustified in blaming models themselves for the problems
with realism. This point should be clear from the fact that the tension between explana-
tion and truth is completely divorced from any issues about models. It is a logical point,
emphasized by van Fraassen: as explanatory power goes up, the probability of truth goes
down (see esp. van Fraassen 1985). That is, the logically stronger claim is less likely to
be true. This is a problem for any realist position; it has nothing to do with whether the
theory is presented as a class of models.

Horan then presents and ridicules the overreaching claims of sociobiologists (such as
E.O. Wilson on human evolution) as examples of this problematic inference. Basically,
the problem is that the details of causal mechanisms of these models cannot be assumed
to be real, simply because the predictions turn out well.

I think that Horan is dead right about the problems with these cases. But I think it is
ironic that she is trying to lay the sins of the sociobiologists at the doorstep of the seman-
tic view, given that both Thompson and I have used the semantic view to analyze, in great
detail, precisely what the evidential and theoretical problems are with sociobiological
claims (Thompson 1985, 1988; Lloyd 1988, Ch. 8).

The claim at stake is that the high-level model can describe a range of natural sys-
tems. I made a point of looking at just this sort of case in my paper on confirming evolu-
tionary models (1987a). There, I argued that providing a range of cases, i.e., a variety of
evidence, is one form of confirming empirical claims made about models. But there is
also the problem of supporting the assumptions of the model *independently*. In that paper,
this type of independent testing was explicitly presented as a corrective to the overem-
phasis on prediction practiced by most philosophers, and by some biologists as well, par-
ticularly the ones Horan is talking about.

The usual hypothetico-deductive views of confirmation concentrate on the accuracy
of the model outcome, that is, the prediction. This exclusive focus on the outcome is
extremely misleading, I have argued, in many cases in evolutionary biology. As the
semantic approach to theory structure makes very clear, assumptions made in construct-
ing any model play a major role in theory; I have claimed they should likewise play a
major role in evaluation of evidence.

In sum, I want to agree with Horan on two major issues. First, an obsession with
developing extremely high-level, general models in biology can lead to sloppy and badly
confirmed scientific claims that miss essential elements of natural systems. Second, an
obsession with the predictive power of models, to the exclusion of concerns about the
descriptive accuracy of details of the models, is a bad idea.

But I take it that, since I have argued for these points *using* the semantic view of theories, they provide no evidence against the semantic view. I have argued that Horan's rejection of the semantic view arises from her identifying that view with a particular (and problematic) scientific approach. I can see perfectly well how this might have come about. We semantic view theorists are always saying that the semantic view is good because it is closer to the practice of science. But I would like to set the record straight, and say, "not *that* close...".

I will conclude by mentioning a few areas in which I think the work that Thompson, Suppe, Griesemer and I have done on biological theories can be extended. For instance, what are the relations between the models and explanations in molecular biology and the models of population genetics and macroevolution? The way is paved to explore the question of reductionism with with sophistication and precision. The received view of theories has had a notoriously hard time explaining or describing the apparent reduction of Mendelian genetics to molecular genetics (cf. Hull 1974). Nevertheless, I think Nancy Maull and Lindley Darden defined the interrelations of the fields well, though in general terms (Darden and Maull 1977). Our work on the semantic approach to evolutionary theory provides, I would claim, the analytic tools and background research necessary to analyze the interrelations of these models with precision and sensitivity.

Alternatively, the topic of the interrelation between population genetics and molecular biology could be approached without worrying about reduction per se. The primary task might be, instead, to give a detailed description of how the results of one theory feed into and take from the results of another theory. This sort of analysis is very important, especially for the sorts of work on confirmation advocated by both Thompson and me.

In conclusion, there are many advantages to using the semantic approach to theory structure in biology. I want to urge that this approach can be especially valuable for describing the content of theories, for analyzing the interrelations of complicated theories with many parts (evolutionary theory, for example), and for helping to formulate and clarify the central scientific arguments occurring in evolutionary biology today.

## Note

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