The Importance of Models in Theorizing: A Deflationary Semantic View[1]

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1. Introduction

It is commonly acknowledged in science that model construction is one of the most important components of theorizing. Philosophers of science are gradually coming to acknowledge this situation, spurred on by holders of the semantic view of theories. In this paper I wish to defend a very deflationary version of the semantic view of theories, which is more or less a re-statement of the above commonplace. I reject the view encapsulated in the identity statement "scientific theories are families of models," although acknowledging the useful insights into science that holders of this strong position have given us. My position derives from a critique of various of the semantic views of theories, and further from a guiding presupposition that rather than providing necessary and sufficient conditions for what a theory is, philosophers should focus on the nature of scientific theorizing. Theorizing is carried out by practicing scientists, and we cannot say what scientific theories are unless we appreciate the myriad ways they are used and developed in all of the sciences.

The paper proceeds by investigating some key aspects of the semantic view of theories. I concentrate particularly on the notion of a model, and less on the various notions of "theory" that appear in the literature. Having cleared up some issues to do with the nature of models, I defend the claim that mathematical and meta-mathematical models are clearly different from scientific models. Next, I criticize the strong view that theories are families of models by looking at various examples that show the semantic view "merely" provides descriptions of particular cases of theorizing, rather than providing a general account of the nature of theories. Finally, I propose a more liberal or deflationary view, which is consistent with a naturalistic approach to the philosophy of science.

2. Working Definition of the Semantic View of Theories

In this section I introduce a working definition of the semantic view of theories. There are several formulations of the semantic view and many differences between each of these formulations. In the definition given here I am erring toward a strong version of the semantic view, but it is not a version held by any particular semantic theorist. The definition simply helps distinguish the view from other prominent views

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such as the "received view" (Suppe 1977), which is, roughly speaking, the view derived from logical empiricists that theories can be adequately reconstructed as sets of axioms and correspondence rules.

Here is the definition: Scientific theories consist of families of (mathematical) models including empirical models and sets of hypotheses stating the connections between the empirical models and empirical systems. Empirical models are models that specifically purport to have relations to an empirical system. There are many models in science that clearly do not purport to represent empirical systems and yet are still important in scientific theorizing. From here on I will follow Van Fraassen's (1980a) usage and refer to "empirical systems". Giere (1988) uses "real systems" in its place and I think this leads to misleading presuppositions about realism. The use of the phrase "empirical systems" allows for the discussion in this paper to remain mute about the realism/anti-realism debates (cf. Lloyd 1988).

The above definition is closest to Giere's (1988) and perhaps neither Van Fraassen nor Suppes would include the second clause. The important thing to note is that the models are the central feature. The view emphasizes semantic objects over syntactic objects, taking the lead from semantics in meta-mathematics. On the semantic view, whatever linguistic components a theory has, they are incidental to any serious understanding of that theory's nature. In the next section I give some examples of models to illustrate the diversity of the concept.

3. Theories and Models

In this section I will pay most attention to models, but a brief word is in order about the other side of the definition "theories". Predominantly philosophers have worked with a somewhat unexamined notion of theories. Although much work has gone into attempting to answer the question "What are scientific theories?" there is no real consensus over what the scope of the investigation is. Much work in philosophy of science has proceeded under the assumption that Classical Mechanics is a good example of a scientific theory, and so if we can explain what that is, we have a start on explaining what theories are in general. There are many axiomatizations of classical mechanics (e.g., Simon 1954) that under the received view count as attempts to answer the question.

What has been revealed by closer examination of the practice of science is that the term "theory" does not simply denote the finished product of years of research formulated in its most elegant fashion as served up in advanced text books. Many fields have general overarching theories, middle level, and low level theories. Biology provides an example of a field in which there are clearly several different levels of theorizing, each of which are crucially inter-related. Further, there is no clear candidate for "*the* theory of evolution" (cf. Hull 1988).

Another direction that discussion of theories has taken has been to promote anyone's account of anything as presupposing a theory. Examples abound in the philosophical literature on cognitive science of our folk theories of psychology, physics, and even middle sized every day objects (e.g., Churchland 1986). For example, on this account, I not only possess a theory of the cell, which I invoke in recognizing cells, but also a theory of chairs.

A middle course between the extremes of accepting only fully developed and formalized scientific theories, and promoting any set of concepts that guide perception seems the most sensible. How one identifies candidate theories should be derived

from an investigation of scientific practice. There is no really clear guide in scientific practice to what counts as theorizing, but several rules of thumb can be adopted to recognize such practices. Obviously many theoretical presuppositions are invoked in even the most mundane types of scientific practice, but it seems clear that tissue slicing and staining are not theorizing. Predicting various matches between a mathematical model and a yet to be established experimental set-up clearly is theorizing. Most proponents of the semantic view take off from a close look at a particular piece of scientific theorizing when developing their account of the nature of theories in general, and this is a reasonable approach.

There are many referents for the term "model", and it is my contention that there are far greater differences between models in mathematics and logic and models in science than holders of the semantic view have been prepared to admit. Both Suppes and Van Fraassen have played down the distinction between models in logic and in science. Suppes says "I would assert that the meaning of the concept of model is the same in mathematics and the empirical sciences" (1960, p. 289). Van Fraassen makes a similar point: "... the usages of model in meta-mathematics and in the sciences are not as far apart as has sometimes been said" (1980, p. 44). Suppes and Van Fraassen's views derive from the way they approach the philosophy of science, and this is a topic I will return to later. First, I will elaborate what do seem to be some clear differences between models in science and models in meta-mathematics. I use some elementary examples to set out some of the differences, in the sections below I will introduce some more examples as I develop my criticisms of the strong version of the semantic view.

Let us look at two straightforward examples of models in mathematics and meta-mathematics. Consider the following set of postulates:

1. Any two members of K are contained in just one member of L.
2. No member of K is contained in more than two members of L.
3. The members of K are not all contained in a single member of L.
4. Any two members of L contain just one member of K.
5. No member of L contains more than two members of K. (Nagel & Newman 1958, p. 16)

A model for these postulates is a triangle with vertices K and sides L. The model satisfies any theorems derived from these postulates and shows us that the postulates are consistent. The model provides us with a semantics for the set of postulates.

Now consider a slightly more complex example. We can derive all of arithmetic from the Peano Axioms (let us just for arguments sake ignore Godel's result here). The set that provides a model for the Peano Axioms is the set of integers plus zero. Things are slightly more complex in this case, as we need various orderings on the set to satisfy theorems derivable from the Peano Axioms. One way to do this is by Tarski's method of sequences, so the set {2,2,4.......} satisfies "2+2=4". So a model for arithmetic is the set of sequences produced from various orderings on the integers.

These very elementary examples give us much of what we need to understand how models work in meta-mathematics. The crucial relation is a relation of satisfaction between a set of postulates, theorems, or axioms and a set of some type of objects or other. The latter set can be expressed as a more concrete geometric model as in the first case, or specified in a more abstract manner as in the second case. The one further notion that is important for our purposes is "embedding". Think of postulates 1 through 5 above as a describing a theory T; this theory is satisfied by the triangle, but the triangle can be embedded in the larger structure of a Euclidian plane because it is

isomorphic to part of that plane. On this account our theory T can be embedded in the larger theory T', which is Euclidian Geometry, because the model for T is isomorphic with part of the model for T' (cf. Van Fraassen 1980a, p. 43). Embedding is a relation between models, and relies on the isomorphism of one model with part of another. In our other example the sequence {2,2,4....} can be embedded in the set of all sequences of integers. The notion of embedding works well in the meta-mathematical cases because isomorphism is so well defined. In the scientific case isomorphism is a harder relation to define.

Let us now consider a few models from science. Again these are very simple examples. The first is taken from Maynard-Smith by Lloyd (1988). The description of population growth in ecological theory can be expressed mathematically by "the logistic equation", $dx/dt = rx(1 - x/k)$. Here x is the population density at time t, r is the intrinsic rate of increase, and k is the carrying capacity of the environment. As Lloyd points out, Maynard-Smith observes the following: "[the equation] was not derived from any knowledge of, or assumptions about, the precise way in which the reproduction of individuals is influenced by density; it is merely the simplest mathematical expression for a particular pattern of growth" (1988, p. 15). The equation presents a set of relations between particular mathematical objects, and it is this structure that the equation defines that is a model.

We can go on to make comparisons between the model and actual experimental systems. If we take an experimental population and plot its growth, and the curve matches closely the curve for the logistic equation, then we can claim that the model is isomorphic to the real system. The term isomorphism is being used in a different sense here than in the mathematical examples, and I will comment on this below. What we need now to note is that the model is a mathematical structure defined by the logistic equation, and it may or may not have relations to particular empirical systems.

Giere introduces an example from classical mechanics. Consider the following equation: $m\, d^2x/dt^2 = -(mg/l)x$. This is the equation of motion for the horizontal component x of the motion of a pendulum with length, l, mass, m, where gravity is g. This equation is for a small angle of swing, a, $\cos(a) = 1$. Such a pendulum is a model. The equation describes this particular pendulum, or in the language of the meta-mathematical examples the pendulum satisfies the equation.

In this example there are no empirical systems corresponding to the pendulum. The equations describing the pendulum in my high-school physics laboratory, or any form of existing linear oscillator, require many added parameters for the resistance of the air and the size of the angle, a, and so on. The pendulum that satisfies the equation of motion above is an abstract system or a model that satisfies just that equation.

Finally, consider a typical biology textbook drawing of a cell. In most texts a schematized cell is presented that contains a nucleus, a cell membrane, mitochondria, a Golgi body, endoplasmic reticulum and so on. In a botany text the schematized cell will contain chloroplasts and an outer cell wall, whilst in a zoology text it will not include these items. The cell is a model in a large group of inter-related models that enable us to understand the operations of all cells. The model is not a nerve cell, nor is it a muscle cell, nor a pancreatic cell, it stands for all of these.

Many other models are presented in cell biology when one graphically zooms in on the inside of the cell. For example, when energy transfer is considered we look at a model of a mitochondrion. In the case of the cell there is no mathematical object, and there are no equations describing it, and yet the schematic drawing is not of any one

particular cell; it is an idealized cell or model. Just as the model of the mitochondria is not a drawing of any particular mitochondria.

We have enough examples now to bring out some of the important differences between mathematical and the scientific models. There are two important differences, one centering on satisfaction and the other centering on isomorphism and the related concept of embedding. Let us take the satisfaction issue first.

In the mathematical case the triangle satisfies the postulates of theory T, and this is all the work it has to do. In the case of the logistic equation what we have is a system that satisfies an equation also, but this is not all it does. The difference between the mathematical and the scientific case can be put schematically as follows: Let $\Longrightarrow$ be "satisfies".

Mathematics:
MODEL $\Longrightarrow$ {Linguistic description of theory, Equations ,...}
Science:
{Empirical System}=?=MODEL$\Longrightarrow${linguistic description of theory, Equations,...}

First, consider the cell example as an instantiation of the second schema. There is no clear place for the satisfaction relation in the cell biology case. A set of sentences may describe the cell model, but the cell model does not satisfy the description in any specifiable sense. The notion of satisfaction is a technical term from meta-mathematics with no correlate in many cases of scientific model construction.

Further, in the scientific case we are interested in more than just the satisfaction relation, for an account of theories that does any justice to scientific practice we need to say something about the relation between models and empirical systems they purportedly model. Of course holders of the semantic view have such a concept, my symbol =?= above is captured by isomorphism. Before turning to this a brief note is in order about the pendulum case.

We observed that in the case of the pendulum there was no real system that corresponded to the equation. In fact as Giere has pointed out (1988) there are few models in classical mechanics that bear a close relation to empirical systems. In this case perhaps the similarity between the scientific and the logical case is more apparent. Giere's weaker version of Suppes and Van Fraassen's claims above is that the terminology in classical mechanics "overlaps nicely with the usage of logicians, for whom a model of a set of axioms is an object, or set of objects, that satisfies the axioms" (1988, p. 79). But even in the case of classical mechanics there is some sense in asking the question "What is the relation between the pendulum satisfying the equation, and the pendulum in my high-school physics laboratory?" No such corresponding question can be asked in the meta-mathematical case.

Notice, also, that the notion of embedding partially captures the relations between the pendulum and other linear oscillators. The equation for any linear oscillator is a more general equation than the equation for the horizontal component of the pendulum, and a model for a linear oscillator satisfies the equation for the pendulum. The pendulum is embedded in the linear oscillator because the pendulum is in some sense isomorphic to a sub-structure of the linear oscillator.

Giere, following many other semantic theorists, separates out appropriate questions philosophers should ask about scientific theories: "'What are scientific theories?' and 'How do theories function in various scientific activities'" (1988, p. 62).

Proposing that theories in meta-mathematics are very similar to theories in science is to pursue the former structural issue unconstrained by the latter procedural issue. Most holders of the semantic view, despite their position on the relative status of meta-mathematics and science, do have a story to tell about the relation between models and empirical systems. The semantic view is an attempt to give a general account of the nature of scientific theories, and to give an account that only worked for classical mechanics would not be sufficient.

Isomorphism is a relation between mathematical structures. If there is a function that maps each element of one structure onto each element of another the structures are isomorphic. What is more useful is the idea that some structures can be isomorphic to a sub-structure of a larger structure. A case of this relation was introduced in the geometry example, where we saw that the triangle is embedded in the euclidian plane as it is isomorphic with a substructure of the theory of Euclidian geometry. When we turn from the mathematical to the scientific case there are some problems with the use of isomorphism.

Reconsider the following schema:

{empirical system} =?= MODEL ===> {theory, equation,...}

The operator =?= is generally taken by semantic theorists (Suppes 1967, Suppe 1977, Van Fraassen 1980a) to stand for isomorphism, but scientific cases strain the clear mathematical sense of isomorphism. Certainly when dealing at the level of the relations between parts and levels of a theory isomorphism is a useful relation. As long as the theory is expressible mathematically and has clearly delineable models, then its sub-theories and lower level theories can be shown to be isomorphic to sub-structures of the larger theory. This is clear in the classical mechanics case.

Where the semantic view needs isomorphism to do most of its work is between models and empirical systems. Lloyd, who is aware of the difficulties with the notion of isomorphism (1988, p. 14), gives an example of how the relation works in the logistic equation case. As we saw above she claimed that if the growth curve for an empirical system, say a population of yeast, was the same as the curve from the logistic equation, then the empirical system and the model are isomorphic. But here Lloyd herself notes "in practice, the relationship between theoretical and empirical model is typically weaker than isomorphism, usually a homomorphism, or sometimes an even weaker type of morphism" (1988, p. 14 fn. 2).

The kind of relation that exists between empirical systems and theoretical models is simply not the isomorphism of mathematics. There are a few reasons for the tendency to hang onto the term isomorphism. One stems from Van Fraassen's anti-realism. Van Fraassen (1980a) speaks of empirical systems rather than real systems, so that he can consistently argue for his view that theories need only be empirically adequate. Rather than theories mapping onto real systems, they map onto empirical structures derived from observations. So the isomorphism at work is always between well defined structures.

A further reason for the usage is that holders of the semantic view have concentrated on highly mathematical theories. For example, the empirical structures that Van Fraassen refers to are expressible entirely mathematically. If, by contrast, we consider what relation there is between a schematized cell model and what I observe of a stained piece of muscle tissue through a light microscope, we have a case where the application of the notion of isomorphism looks very strained. And yet this is closely

analogous to the relation that holds between the curve for the logistic equation and the curve for the yeast population.

Giere (1988) believes he has an answer to these kinds of problems. He substitutes the term "similarity" for isomorphism. Models are similar in various respects and degrees to empirical systems (1988, p. 81). But Giere is also more sanguine about real systems. He invokes similarity between models and real systems, because he is a realist. Without passing judgment on whether models are similar to real systems or empirical systems, we can still assess what is gained by introducing similarity as the candidate relation for =?=.

Giere's introduction of the notion of similarity gets him out of the difficulties associated with the strong relation of isomorphism, at the expense of distancing his account of science from any account of logic or meta-mathematics. At best we can say that the use of models in mathematics inspired the semantic view of theories, but we are not entitled to any of the strong claims that models in science and mathematics are the same.

What appears to be the case is that on the semantic view one needs an account of the relation between theoretical and empirical models that can handle all the kinds of models that occur in science. In many cases neither the models nor the empirical systems are, or can be, expressed mathematically, so isomorphism will not do the trick. Giere's similarity relation appears to solve some of these problems, so for now let us assume that this is the kind of relation we are looking for.[2] I now turn to a specific set of criticisms that lead to a deflationary version of the semantic view of theories.

4. A Deflationary Semantic View

Several holders of the semantic view (Suppes 1960, Suppe 1977, Van Fraassen 1980a) propose a general account of the nature of scientific theories. My illustrations of the difficulties with the notion of models above leads to the following question: Is such a strong generalized version of the semantic view tenable? I will address this question by showing how different versions of the semantic view arise when different sciences are examined. I then ask whether holders of the semantic view should be satisfied with a more piecemeal and descriptivist version.

Suppes (1960), Van Fraassen (1980a) and Suppe (1977) share the view that an important part of the philosophical investigation of science is to give an account of the structure and content of scientific theories. Van Fraassen refers to this task as "foundational work" (1980a, p. 2). Much of what was at stake in the initial presentations of the semantic view of theories was to provide an analysis that would answer all the questions logical empiricists had about the nature of theories in a more coherent fashion. Marc Ereshefsky (1991) has clearly laid out the issues at stake here. There is a problem with emphasizing this motivation for the adoption of the semantic view, which is that the explanatory agenda is not being laid out by explananda in science, rather by explananda that were of interest to logical empiricist philosophers. When Ereshefsky assesses the semantic view he does so by contrasting it with the received view thus reinforcing these terms of contrast. For, although he is not a defender of the received view, one could conclude from his criticisms that a return to the received view is in order as the semantic view does not hold up.

An alternative motivation for the semantic view, emphasized by Giere (1988) and Lloyd (1988), and more recently by Griesemer (1990), is that the view provides a more adequate description of theories as they are represented in actual scientific prac-

tice. This motivation leads to a proliferation of views under the rubric of the semantic view. Philosophers of science closely following a particular piece of scientific theorizing present an account that most closely resembles that particular science. This is fine if no general account of the structure and content of scientific theories is required, but if it is, then there is a problem. Let us first consider some examples.

Lloyd (1988) presents a detailed analysis of evolutionary theory, specifically population genetics. Her aim is to demonstrate the logical structure of evolutionary theory by elucidating the structure of its models. In doing this Lloyd claims that she "demonstrate[s] the usefulness of a precise analysis of the structure of evolutionary theory" (1988, p. 23), and that she offers "further evidence for the appropriateness and utility of the semantic view of theories" (1988, 23).

The account Lloyd presents closely follows those in the biological literature. For example, her account of the general structure of population genetics closely follows Lewontin's (1974). Certainly such close attention to the detail of biological theorizing produces useful results, in that Lloyd provides new and useful insight into many problem areas in both philosophy and theoretical biology, especially the units of selection issue. The question is to what extent her approach is generalizable, and, more specifically, generalizable as a version of the semantic view of theories.

Lloyd emphasizes, as do Suppes and Van Fraassen, the importance of mathematical models, and the use of a precise formal approach. In the kinds of cases these authors consider this is a reasonable emphasis. The problem is that, although Lloyd (1988, p. 16) claims that the semantic approach does not attempt to delineate scientific from non-scientific theories, there is something of an implicit demarcation criterion at work here. As we saw in the discussion of models above, there are distinctly non-mathematical models in science. Elucidating theorizing involving these models cannot rely on a formal approach of the kind Lloyd presents, and even less on such a specific formal approach involving state spaces, laws, and parameters.

What is at stake here is not the value of Lloyd's contribution to understanding evolutionary biology, rather the integrity of a strong version of the semantic view of theories. Certainly she holds that model construction is crucial to our understanding of theorizing, but this is a much weaker position than one that says that all theories are families of mathematical models. It is my contention that gaining descriptive and explanatory insight into scientific theorizing is an adequate goal for philosophers of science, even in the absence of a general account of *the* nature of scientific theories. Let us now look at some more examples to try and strengthen the claim that there is no one strong semantic view of the nature of theories.

Giere's discussion of linear oscillators that we considered above is clearly a candidate for a formal analysis in terms of mathematical models. This example can be quite easily accommodated in a state space type account such as Lloyd's, this was shown previously by Van Fraassen (1980a) in his discussion of classical mechanics. If this was the only kind of case Giere (1988) examined it would perhaps lend support to the idea that there was one semantic approach, but Giere examines several different cases notably the revolution in geology.

Giere's (1988) version of the semantic view contains a twist that no previous semantic theorists endorse, which is a certain kind of naturalistic approach with an emphasis on cognitive science. This leads to a confusion over just exactly what models are for Giere. When he discusses classical mechanics he claims that scientific theories are families of models, and that models are constructed abstract entities. So far he directly follows Van

Fraassen. He goes on to say that models are "socially constructed entities" (1988, p. 78). Later, Giere introduces models as kinds of mental representations. So when he discusses the geological revolution he refers to the "mental models" various individual geologists possessed. Here he leans on accounts of representation derived from cognitive science. Certainly at this point we have a proliferation of views on the table.

Models can now be mathematical objects, which are socially, or otherwise, constructed entities, and mental representations of some sort. Now in Giere's geological case study there were two large scale crude models, the static model and the mobile model. Each of these consisted of families of more detailed models of various geological phenomena, for example on the mobilists account a clear pictorial model of sea floor spreading was developed (Giere 1988, pp. 249-270). These kinds of models, irrespective of whether they are mental models, are non-mathematical models, and Giere's investigation of them is not a formal analysis such as that of Lloyd, Van Fraassen or Suppes.

Giere is obviously working with a deflationary version of the semantic view. His notion of models extends over all manner of objects, including the mental models of the cognitive psychologists (e.g., Johnson Laird 1983). This is strategic on his part, as he not only wishes to dissociate himself from logical empiricist philosophy of science, but also from approaches that rely only on a formal analysis of the structure of theories. Giere's naturalism leads him to investigate scientific theorizing in all its forms. Although Giere claims that "theories are families of models", his view could be more adequately characterized as the deflationary: model construction is an important component of scientific theorizing. Far from following through the alleged similarities between scientific theories and those of meta-mathematics, Giere brings in as many interpretations of the term "model" as suit his ends. I will comment on his reference to mental models at the end of the paper, but first I provide a third example of a proponent of the semantic view who contributes to the proliferation of semantic views.

Griesemer (1990) used a case study of the naturalist Joseph Grinnell to "enrich the semantic conception of theories" (1990, 11). Griesemer adds the following two important model types to all the above models involved in scientific theorizing. The first are entities as models, of which one of the most famous is Watson and Crick's wire and bead model of DNA. These kinds of models are different from the schematized cell referred to above, because their representational capacities are enhanced by our ability to physically manipulate them. The second type of model, and the most important for Griesemer's paper, are "remnant models". An example of this kind of model is a museum specimen of an animal or plant. Griesemer's introduction of these further types of model are a result of his move away from specifying theories in terms of the kinds of logical structures they are, and towards accounts of scientific theorizing. This is the move I advocated at the outset of the paper, one that Giere makes to a certain extent, but Griesemer pushes further.

One puzzling aspect of Griesemer's account is his insistence that he is enriching the semantic view of theories. By the time we have admitted laboratory specimens and physical objects to the domain of models, the idea that theories are families of models becomes quite inclusive. The less liberally inclined might be moved to claim that the semantic view is a non-view, because just about anything counts as an extension to it, or an enrichment of it. My approach is to claim that there is no such thing as one semantic view of theories, but work on such an approach led us to a fruitful way to study scientific theorizing and that is to investigate model constructing practices.

This is almost the position Griesemer arrives at, but not quite. His emphasis on theorizing as a practice is evident in the following passage:

> We can ... distinguish two semantic routes to theory introduction, one in terms of abstract models and one in terms of material models. On the classical semantic view, a theory is specified by defining the mathematical structures that satisfy the propositions of the theory. On an extended semantic view, a theory can be specified by defining a class of models as the set of physical structures constructed according to a given procedure or tradition. (1990, p. 12)

A clear way of lessening the contrast between these two views, is simply to re-phrase the definition of the classical view in terms of socially constructed entities also. Then all investigations of scientific theorizing involve the investigation of the practice of constructing models. And also on this view all investigations would have to go beyond the individual theorizer as a locus, as Griesemer (1990, p. 5) has pointed out.

What we have seen from the three examples is not so much a series of extensions to one semantic view of theories, but a set of alternative proposals for the investigation of scientific theorizing. Lloyd's proposal is to pay close attention to the formal structures produced by theorists, Giere's is to develop an account of theorizing as the manipulation of mental models, and Griesemer's to account for theorizing that involves the practices of using physical objects as models. I claim that what is common to all these views is best understood as the somewhat weak claim that model construction is an important part of scientific theorizing. This claim is not weak when we consider the progress various versions of the semantic view have made over the received view. The key mark of this progress is the ability for philosophers to provide insight into detailed and specific cases of scientific theorizing. What could be considered the chief source of weakness is the lack of a general account of the nature of theories. I will close with some concluding remarks about the potential fruitfulness of the deflationary account for naturalized philosophy of science.

5. Conclusions

My criticisms are not an outright rejection of the semantic view of theories, in fact I embrace some of the insights of the semantic view, but only in so far as they are contained in the deflationary semantic view. That is that model construction is an important part of scientific theorizing. What lies behind my criticisms is a view of how to approach issues in the philosophy of science. Any reconstruction of a domain of scientific practice in terms of identity statements such as "theories are families of models" can only give us limited purchase on making sense of science. The deflationary version of the semantic view of theories is an attempt to leave room for accounts of types of scientific theorizing that may not fit the model building mould. The strong view has to provide an analysis of scientific theorizing that forces any and all types of theorizing into this mould. Although I happily acquiesce to the claim that much theorizing in science involves the development and testing of models, I do not agree that a sufficient account of scientific theorizing is allowed by the claim that all scientific theories are simply families of models.

What is implicitly rejected on my view is that there are a special set of issues, referred to by Van Fraassen (1980b) as internal issues, whose investigation supplies sufficient activity for philosophers of science. Once theorizing is focussed on, rather than theories as completed formal entities, distinctions between internal and external issues become hard, if not impossible to make. On the deflationary view, philosophers of science form a loose confederacy for studying scientific theorizing, gathered around the common insight that model building is one of the most important components of such theorizing.

This kind of approach, which I take to be naturalistic, opens up many avenues of inquiry. As Griesemer has pointed out, echoing many sociologists of science before him, the examination of scientific theorizing will require an examination of the social nature of scientific practice. Giere's focus on the notion of models as mental representations also deserves attention. It does not appear to be the case that an account of models as simply some kind of mental representation will do justice to scientific theorizing, as the Griesemer case should indicate, but Giere's emphasis on the psychological does point to an important set of problems about scientific theorizing. For example, it is not clear just exactly what the representational status of theories is, or if they are representational at all. Certainly models are representations in some sense, but even their representational status is unclear. This is emphasized by the difficulty of finding a suitable alternative to isomorphism, and whether, once arrived at, such an alternative will apply to mental models in the same way it applies to socially constructed abstract models. Further, it is an open question just exactly how much scientific theorizing can be usefully and fruitfully carried out by individual scientists without the assistance of their peers. This leads to questions about our individual cognitive capacities, and how they are enhanced by the addition of other theorists.

If one pays attention to only the fine detail of the formal construction of theories, I contend that the above issues remain unaddressed. It could be argued that the formal investigations are sufficient, but I propose that they will only be useful if they go hand in hand with the other kinds of investigation encompassed by a naturalistic approach to the philosophy of science.

Notes

[2]Space prevents me from going into the reasons for rejecting Giere's similarity relation, which is also inadequate for its proposed task. Suffice to say that similarity avoids some of the problems associated with isomorphism, but may bring with it many more of a different nature. Cummins' (1989) discussion of similarity as a representation relation provides a good introduction to some of these problems.

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