

### Note to the reader

I wrote this paper in 1996-1997. It has circulated quite widely since then, and I have presented various parts of it in talks at various times. One part has been published: "Models and the Semantic View," *Philosophy of Science*, December 2006, 73: 524-535, is a descendant of section 2 of this paper, along with improved versions of some supporting material from section 1 and the introduction. I have plans to publish other parts of the paper, but I am archiving it here in the meantime in response to requests from colleagues who wish to be able to cite some stable public version of it.

This version differs from the 1997 version in only a small number of superficial ways: I have corrected some typos, filled in one or two references, updated my surname (which changed from Jones to Thomson-Jones in 2004), and updated the bibliographical entries for works which were forthcoming at the time and have since appeared in print. As will be obvious, however, I have made no attempt made to update the bibliography (or the content of the paper) to reflect the numerous additions to the literature which have appeared in the interim.

There are of course many changes I would now make, but there is one significant confusion I should address here. Section 1 lays out, at some length, a taxonomy. I say in more than one place (including the title of the section) that the taxonomy is to be taken as a taxonomy of *notions of model*. Understood that way, however, it is woefully inadequate. There are (and were) plenty of notions of model in the literature which are neither used nor mentioned in the laying out of the taxonomy; indeed, many such notions are discussed in the paper.

The right way to understand the taxonomy is as a taxonomy *of models*. The aim is, in part, to have all the objects picked out (or purportedly picked out) by every widespread and coherent use of the term 'model' in the philosophy of science, and in the sciences themselves, fall into one of the categories included in the taxonomy. I think the taxonomy I present comes close to succeeding in this aim, and I hope to improve it so that it comes even closer. (One outstanding is-

sue is to think about how computer models fit in, or how the taxonomy should be modified so that they do.) Of course, the aim in question could be achieved all too easily in various trivial ways, but the rest of the paper makes it clear, I hope, that the distinctions employed in the taxonomy are philosophically useful ones.

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## **Models and the Semantic View<sup>+</sup>**

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### Introduction

The notion of a model has come to play an increasingly prominent role in the philosophy of science of the last thirty years or so. There are at least two identifiable sources of this development. One is the rise of the semantic view of theory structure as an alternative to the syntactic view beloved of the positivists and their immediate descendants; according to the semantic view, a scientific theory should be thought of as made up, at least in large part, of a collection of models. The other is the increased value placed on attention to the details of workaday scientific practice in theoretical, experimental, and other contexts, for much of that practice involves tinkering with detailed models of specific systems, or kinds of system, rather than working on the foundational principles of overarching theories.

It seems to me that there is something right about focussing on the notion of a model in our philosophical work on the sciences, whether we are driven by an ultimate interest in the structure of evidential reasoning, the nature of scientific explanation, the defensibility of some form of scientific realism or anti-realism, or any one of a number of other central issues. I also think that there is something right about the semantic view, and something laudable in attending to the details of scientific practice. Unfortunately, however, the word ‘model’ has become multiply ambiguous along the way. There is now a profusion of different notions abroad which go by that name, and a serious danger that the number will continue to grow unchecked.<sup>1</sup> Such a situation can only

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<sup>1</sup> Thus my opening words were misleading—there is no single notion of model.

engender confusion and thus lead us astray, unless we make an attempt to clarify matters.

With this in mind, I present in the first section of this paper a taxonomy of notions of model which are to be found in one place or another in the philosophy of science. The initial presentation will involve relatively little in the way of commentary; further discussion of some of the notions it includes will be found in the remainder of the paper. The hope is that the taxonomy presented is fairly comprehensive, at least with regard to notions of model currently in active service.<sup>2</sup>

Each of the five basic senses of 'model' I will try to distinguish can be thought of as having two components, with a sort of pattern emerging amongst them. We might loosely characterize the pattern this way: To call something a model, in each of our five senses, is partly to ascribe a certain role to it, and partly to classify it as a certain sort of thing.<sup>3</sup> There is a job to do, and a kind of thing doing it.

Approached from the point of view of this formula, it will become clear that the first two notions of model on my list can be grouped together, because in both cases a model is something which, to one degree or another, does the job of providing an interpretation for a certain set of sentences, and in such a way that those sentences come out true. To be a model in either of these senses is to be a model of a set of sentences made true. To be a model in the third, fourth, or fifth sense, however, is in part rather to do the job of representing a system, or type of system, from the domain of inquiry (whether actual or merely possible)<sup>4</sup>; to be a model in these latter senses is then, in part, to be a representation. Given this, we might abstract out from my fivefold classification two fundamental or (to employ the opposite metaphor) overarching notions of model,

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<sup>2</sup> For earlier attempts at the same project, see Suppes (1960), Suppe (1967), and Achinstein (1968). See also Lloyd (1998), Frisch (1998), and Thomson-Jones (2007, pp. 12-13, sec. 3.1, and *passim*; and cf. 2010), in which I defend the taxonomy presented here against a specific charge of incompleteness.

<sup>3</sup> Of course, the last clause here flirts with emptiness. If it were important to make this characterization more concrete, we might try something like "and partly to classify it with respect to its intrinsic (i.e., nonrelational) properties," where the implicit idea would then be that playing a certain role is (at least in these cases) a relational property. Consideration of the specific examples at hand should suffice to make my meaning clear, however.

<sup>4</sup> Although not necessarily with any veracity.

namely, the notion of model as truth-maker, and the notion of model as representation.<sup>5</sup> To characterize this dichotomy in terms of a more familiar philosophical dichotomy, we might say that the two most general notions of model at play here can be distinguished by the direction in which the 'of' in 'model of' points: in one case, to language, and in the other, to the world.

For many purposes, this relatively coarse-grained distinction between models as truth-makers and models as representations may be all we need. I am presenting a slightly more complex taxonomy nonetheless, and I am doing so for several reasons. First, the extra detail involved in moving to a higher level of resolution gives substance and concreteness to the more abstract, two-fold distinction, and clarifies the way in which that distinction connects to various discussions of the nature and role of models in scientific inquiry. Second, if it is true that the term 'model' is used in a number of importantly different ways in philosophical discussion of the structure of scientific theorizing, and often without much attention to that fact, then one might hope (and I do) that a relatively systematic laying-out of the differences will help to clarify the nature of the claims various authors are making, and the structure of the debates amongst them. Finally, I think that even if it is possible to frame a number of interesting questions purely in terms of the notions of model as representation and model as truth-maker, detailed work on the answers to those questions will require a richer, more fine-grained grasp of the notions of model which scientists and philosophers employ.

After presenting the taxonomy in section 1, I go on, guided by its lights, to discuss the best way of understanding the semantic view (section 2), and then to examine the respective merits of two of the notions in the taxonomy when it comes to giving an account of one central way in which scientists use the term 'model' (section 3). The notion which, I argue, fares less well in that regard is the notion which, according to the argument of section 2, is central to the semantic view. The taxonomy itself will, I

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<sup>5</sup> For an extensive discussion of this distinction and its importance in this context, see Frisch (1998). I am indebted to Mathias Frisch for numerous very helpful conversations.

hope, be of some general clarificatory value regardless of the success of these latter parts of the discussion.

## Section 1 – A taxonomy of notions of model

*1.1 Truth-making maps.* We begin with a sense of the term ‘model’ drawn from logic. Suppose we have a set of sentences in a first-order language whose non-logical symbols are, in an intuitive sense, initially uninterpreted. Then a *structure* (also called an *interpretation*) for the language is a function which (i) maps the universal quantifier to a nonempty set, called the *universe* (or *domain of discourse*), (ii) maps each individual constant to an element of the universe, and (iii) maps each  $n$ -place predicate to a set of  $n$ -tuples of elements of the universe (i.e., to a subset of the  $n$ -fold Cartesian product of the universe with itself). Assuming that a formal definition of the notion of truth in a structure has been provided, a *model* of the set of sentences in question is then simply a structure on which all the sentences of the set come out true.<sup>6</sup>

A simple example will prove useful in contrasting this sense of the term ‘model’ with the next. Suppose we are dealing with a rather impoverished first-order language, one which contains no constants (or function symbols) and just two predicates, ‘P’ and ‘Q’, each of which is unary. Then one structure for this language maps the universal quantifier onto the set

$$S_1 \equiv \{\text{John, Paul, George, Ringo}\},$$

maps ‘P’ onto the set

$$S_2 \equiv \{\text{John}\},$$

and maps ‘Q’ onto the set

$$S_3 \equiv \{\text{John, Ringo}\}.$$

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<sup>6</sup> See, e.g., Enderton (1972), pp. 79-84, for a fuller and more rigorous exposition. For simplicity’s sake, I am supposing that we are dealing with a language which contains no function symbols. The extension to functions is simple enough; see Enderton (*loc. cit.*).

And this structure is a model of the set

$$\{(\forall x)(Px \rightarrow Qx)\},$$

for on the standard definition of truth in a structure,  $(\forall x)(Px \rightarrow Qx)$  is true in a given structure if and only if the extension of 'P' (i.e., the set which the sentence maps 'P' onto) is a subset of the extension of 'Q'.

In this sense of 'model,' then, a model is a mapping which has a certain function: roughly speaking, it interprets the sentences in a given set, and does so in such a way that they come out simultaneously true.<sup>7</sup> I will use the label 'truth-making map' to refer to models of this sort.

We can broaden the "truth-making map" notion of model in at least two ways. First, we can allow for other sorts of formal language. In constructing a semantics for modal logic, for example, we can again define a model to be a mapping from symbols in the language to various sets and members thereof, and one on which all the sentences in a given set come out true, even though the range of mapping will now have a more complex structure.<sup>8</sup> As a second sort of broadening, we can allow the interpretation of the non-logical symbols to proceed in less set-theoretical ways, such as by the provision of a set of "semantical rules" in a meta-language (e.g., "let 'P' mean the same as 'married Yoko Ono'").<sup>9</sup> A model will then still be a mapping of the symbols in the language onto various objects in such a way as to provide an interpretation of the sentences in a certain

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<sup>7</sup> Only roughly speaking because (i) some of the symbols in the sentences in question (such as ' $\rightarrow$ ') already have their meanings fixed, and (ii) it is a matter of controversy in metaphysics and the philosophy of language to what extent we succeed in attaching a meaning to a predicate symbol by fixing its extension, and insofar as 'P' lacks a full interpretation, so too must any sentence in which it appears.

<sup>8</sup> For a usage of this sort in the context of modal logic, see Goldblatt (1993). (I am indebted to Charles Chihara for this reference.) This is by no means a universal terminology, however. Hughes and Cresswell (1968, pp. 167-8), for example, define a model for a first-order modal language (without constants or functions) to be an ordered 3-tuple consisting of a set of worlds, a set of individuals, and a mapping from (i) the variables of the language to elements of the set of individuals, and (ii) the n-place predicate symbols to sets of n+1-tuples (consisting of n individuals and one world). So, although a model in their sense interprets the formal language at hand, and although it contains a mapping as a part, it is not a mapping, and it is not an interpretation which makes a certain set of sentences true; it also provides denotations for the variables of the language, which is not something one of Enderton's models does.

<sup>9</sup> See, e.g., Spector (1965), pp. 276-277, n. 1 and n. 3, and his references to Carnap, Hempel, Nagel, Braithwaite, and Pap, to all of whom he attributes the notion of a semantical rule.

set on which they all come out true the “objects” will now be fully interpreted terms in a metalanguage, or, more provocatively, the meanings of such terms.<sup>10</sup>

*1.2 Truth-making structures.* In order to introduce a second sense of the term ‘model,’ let us return to the first kind of truth-making map discussed, the one exemplified by our example involving The Beatles. In that case, the model was a mapping from three symbols in the language in question (‘V’, ‘P’, ‘Q’) to three particular sets. It is now a simple enough manoeuvre to focus our attention on the sets rather than on the mapping itself. More specifically, we might consider the ordered triple

$$\langle S_1, S_2, S_3 \rangle.$$

This triple is a model of the set  $\{(\forall x)(Px \rightarrow Qx)\}$  in our second sense; this is essentially Tarski’s notion of model.<sup>11</sup>

The ordered triple in question can be said to have a certain sort of internal structure, in that the second and third elements are subsets of the first, and the second is a subset of the third. This gives sense to talk of models of this sort as “structures” onto which we can map the elements of a language in such a way that all the members of a certain set of sentences come out true; accordingly, I will call models of this sort “truth-making structures.” Confusion will ensue, however, if we are not careful to separate this

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<sup>10</sup> There may be a third sort of broadening involved in the way authors such as Nagel (1961) and Braithwaite (1962) think about models, strictly speaking, for they seem to allow that the sentences which function as axioms of a given theory can contain terms such as ‘molecule,’ ‘point,’ and ‘kinetic energy.’ Even though, according to these authors, we might (and, for certain purposes should) treat such terms as uninterpreted, or at most “only implicitly defined” (Nagel (1961), p. 91), they are certainly not terms to be found in any standard formal language.

<sup>11</sup> See his (1953), p. 11, and (1956), p. 417. In order to see how this notion generalizes to the case of less minimal first-order languages, consider the following:

(i) We can always impose an ordering on the predicates, constants, and function symbols, of the language (if any), and then let that ordering determine the ordering of the elements of the tuple. Given such a procedure, it is easy to see that there will be a one-to-one correspondence between models of the mapping variety and models in the “tuple of sets” sense for any given set of sentences in any given first-order language.

(ii) Not all elements of such a tuple will, in general, be sets if we take constants to be mapped to elements of the universe of discourse, as is standard. I will assume, however, that the extension of a function symbol is a set.

(iii) It is standard to allow the predicates, constants, and function symbols to form a set of denumerably infinite cardinality. This simply means that we need to allow our ordered tuples to contain a denumerably infinite number of places.

Furthermore, this notion of model can just as clearly be extended to cases of formal languages with a richer structure, such as modal languages.

use of the word 'structure' from the use embodied in Enderton's definition, discussed earlier, of a structure as a certain sort of mapping. The whole point of the less precisely circumscribed usage just mentioned is that a structure is *not* a mapping, but rather a thing mapped onto.<sup>12</sup>

From there, it is relatively easy to broaden this second sense of the term 'model' in two ways. First, suppose that a certain system such as an electrical circuit, an organism, or the economy of a particular country provides the raw materials, so to speak, for a set-theoretical structure which turns out to be a truth-making structure for some set of sentences in a formal language in the way I have just described; perhaps the universe of discourse (the first set in the tuple) has as its elements only parts of the system in question, to take the simplest case. Then we might, in a derivative way, speak of the system itself, the electrical circuit or economy, as a providing, or even being a model, in the truth-making structure sense, of the relevant set of sentences. After all, the system in question can be said, without too much distortion, to be a "structure" which, in a loose sense, provides an interpretation of the relevant sentences on which they come out true.<sup>13 14</sup>

Secondly, we might then go on to broaden the notion so that a system can count as a truth-making structure if it makes certain relatively fully interpreted sentences of a language such as ordinary scientific English true. Consider, for example, the sentence

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<sup>12</sup> I might have opted for 'interpretation' rather than 'structure' as a label for the relevant sort of mapping, but that choice, too, carries a risk of confusion, as we also need to use the former term in other senses in the present discussion.

<sup>13</sup> Nagel seems to have an intermediate version of the notion in mind when he mentions, as an alternative to the notion he emphasises, that we might think of a certain "*system* of 'things'" as constituting a model for a certain set of postulates (1961, p. 96). The "things" which appear in his example seem to be a set of molecules, the power set of that set, and (a set of?) ratios between various masses. The system in question then does seem to be a set-theoretical entity, but it is not functioning in exactly the same way as  $\langle S_1, S_2, S_3 \rangle$  in our initial example. For Nagel's preferred notion of model, and Braithwaite's, see the end of section 1.4, below.

<sup>14</sup> Elisabeth Lloyd, for example, uses the term 'structure' in this sense, and opposes our second notion of model, broadened in something like this way, to the notion of a truth-making map. (See the new preface to Lloyd (1994), p. vii.) There is also some broadening of the second variety, however, in that her sample sentences (such as "Object A is touching object B") come with interpreted predicates and sortals, and are not written in the vocabulary of any formal language.

(R) Every resistor heats up when a current passes through it.

We might say that a particular circuit is (or at any rate, provides) a model of this sentence if, indeed, every resistor in the circuit heats up when a current passes through it.<sup>15</sup> Note, however, that in regarding the circuit in question as a structure which makes (R) come out true, we are regarding the quantifier 'Every' as implicitly restricted in its scope to the little universe of entities which comprise that circuit. As we move from circuit to circuit, asking whether each is a model of (R), we are in effect constantly reinterpreting the sentence in a certain respect, even though every predicate in it comes already fully interpreted. That is why I classified (R) as a *relatively* fully interpreted sentence.

Note that truth-making structures are, in general, non-linguistic entities; in general, but not without exception, as (a) there are truth-making structures for  $\{(\forall x)(Px \rightarrow Qx)\}$  of the  $\langle S, S', S'' \rangle$  variety in which the members of  $S, S',$  and  $S''$  are, or include, linguistic items, and (b) if structured systems such as economies and electrical circuits can count as truth-making structures for certain sets of sentences once we broaden the notion sufficiently, then presumably linguistic structures (such as languages) will count as truth-making structures for some sets of sentences.

*1.3 Mathematical models.* The third notion of model I wish to consider is one which can be extracted from the writings of Patrick Suppes and Bas van Fraassen in their seminal work on the semantic view of theory structure.<sup>16</sup> In saying this, I mean to be making only the quite minimal claim that one can read certain central passages in the writings of these authors as employing the notion I will characterize. Whether some other way of

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<sup>15</sup> Cf. van Fraassen (1989), p. 218: "A model is called a model of a theory exactly if the theory is entirely true if considered with respect to this model alone. (Figuratively: the theory would be true if this model was the whole world.)"

<sup>16</sup> The notion also appears in the writings of another originator of the semantic view, Frederick Suppe (1967, 1974 a, and 1989), but I will focus on the work of Suppes and van Fraassen here because, as I understand it, Suppe's picture of theory structure is importantly different from those of Suppes and van Fraassen. See Thomson-Jones (2007) for a related discussion of certain problems with Suppe's framework.

reading the passages in question should be preferred is a question which will receive further consideration in section 2, below. Even if the answer to that question is yes, however, it seems to me that the notion I am presenting in this section is a useful one, and one which makes good sense in the context of the taxonomy as a whole.

The notion I have in mind is strongly suggested by, for example, a crucial passage in van Fraassen's 1987 paper, "The Semantic Approach to Scientific Theories":<sup>17</sup>

[T]he systems [in the domain of inquiry] are physical entities developing in time. They have accordingly a space of possible states, which they take on and change during this development. This introduces the idea of a cluster of models united by a common *state-space*; each has in addition...a 'history function' which assigns to [the modelled system] a history, i.e., a trajectory in that space.

The idea here, then, is that the possible states of a given type of system are represented by points in a mathematical space which in this context we call a *state space*. To take the most obvious example, in classical particle mechanics the state space for a system of  $n$  particles will be a  $6n$ -dimensional vector space, the *phase space*, each point of which corresponds to an assignment of three spatial coordinates  $(x, y, z)$  and three components of momentum  $(p_x, p_y, p_z)$  to each of the  $n$  particles.<sup>18</sup> In quantum mechanics, on the other hand, the state space would be a complex Hilbert space of countably infinite dimensionality. We can define a *trajectory* through the state space to be a function which maps points in some interval of the real line to points in the state space. The points in the domain of such a function are taken to represent times, and so a trajectory represents a

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<sup>17</sup> Although I think that the same notion can be found in important early work due to Suppes, there are certain aspects of Suppes's own presentation which would invite confusion at this point, particularly with regard to the essentially straightforward distinction between this new notion of model and the last. For further elaboration of this remark, and some discussion of Suppes's work, see section 2, below.

<sup>18</sup> Actually, the retouching I have performed on the van Fraassen quote was designed to remove the suggestion of an alternative approach, on which the state space in this case would be a 6-dimensional phase space regardless of the value of  $n$ . The history function then assigns a trajectory through this state space to each of the  $n$  particles ('objects' is van Fraassen's more general term) in the system. This fewer dimensions/more trajectories approach is essentially equivalent to the more dimensions/one trajectory approach I have presented, and the choice between them is likely to rest purely on considerations of mathematical convenience.

particular evolution of the state of the modelled system over time. A model is then simply a state space with a trajectory defined on it.<sup>19</sup>

The notion of model I wish to fix upon is obtained by generalising from the characterization just presented in two simple ways: we will not insist that the systems under study be physical entities, thus making room for an uncontentious application of the notion in the context of economics, for example; and we will not insist that a mathematical object represent the evolution of a system over time in order to count as a model in this third sense.<sup>20</sup> We are left with a notion of model on which a model is simply a mathematical structure used to represent the structure and/or behaviour of a system, or kind of system, from the domain of inquiry corresponding to a given discipline.<sup>21</sup> I will call this sort of model a *mathematical model*.

This label is far from perfect, because the first, second, and (as we will see) fourth categories of model in the taxonomy I am presenting also include objects which could appropriately be described as mathematical. The greatest risk is that of confusing the notion of a mathematical model, as just defined, with the notion of a truth-making structure; keeping those two notions apart will, however, prove vital in the discussion of the semantic view in section 2. The important thing to remember is that although a mathematical model is, and a truth-making structure can be, a mathematical entity, the associated functions are quite distinct. A truth-making structure need not be used to represent, and it is no part of the notion of a mathematical model as I have defined it

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<sup>19</sup> Actually, van Fraassen limits this characterization of the notion of model to the case of “non-relativistic” theories (*ibid.*), but as he clearly takes himself to be discussing scientific theories in general, and not just those to be found within physics, this will presumably include the bulk of existing theories (to the extent that a theory in, say, population genetics can be classed as relativistic or otherwise). In this connection, consider the following quotation from Elisabeth Lloyd’s *The Structure and Confirmation of Evolutionary Theory* (1994), a book in which she develops the state space version of the semantic view further and then explores various philosophical issues in the foundations of evolutionary biology through its lens: “The models [of a theory] are mathematical models of the evolution of states of a given system, both in isolation and interaction, through time” (p. 19).

<sup>20</sup> In addition to van Fraassen (in the quoted passage) and Lloyd (quoted in the previous note), Suppe imposes a restriction of the sort I mean to lift: “theory structures...specify the admissible behaviours of state transition systems” (1989, p.4).

<sup>21</sup> Van Fraassen, characterizing Suppes’ approach in 1972, writes: “From this point of view, the essential job of a scientific theory is to provide us with a family of models, to be used for the representation of empirical phenomena” (van Fraassen (1972), p. 310; quoted in Lloyd (1994), p. 15).

that a mathematical model play any role in interpreting or making true any sentences. The conceptual distinction here is clean enough, regardless of whether there are objects to which both concepts apply.

*1.4 Propositional models.* Explicit discussions of the fourth notion of model I wish to consider are less common than discussions of our third notion, partly because it is not the one preferred by proponents of the currently influential semantic view, but partly, I think, because it is simply tacitly presupposed in much philosophical discussion where the primary focus is not on theory structure. One person who has explicitly articulated a version of the notion I have in mind, however, is Peter Achinstein, in his 1968 book, *Concepts of Science: A Philosophical Analysis*; Michael Redhead took up Achinstein's analysis and explored it further in a later paper (1980). Achinstein attaches the name 'theoretical model' to his notion; I will call the somewhat broader notion I wish to fix upon the notion of a 'propositional model.'<sup>22</sup>

In line with the schema described in the introduction, being a propositional model is partly a matter of being a certain sort of object, and partly a matter of having a certain function.<sup>23</sup> In particular, a propositional model, somewhat unsurprisingly, is a set of propositions, the members of which together represent some system from the relevant domain of inquiry as having certain features, behaving in certain ways, and so on. The function of a propositional model is thus the same as the function of a mathematical model—namely, to represent. The difference lies in the sort of thing doing the representing.

Achinstein's characterization of a "theoretical model" begins in much the same way, but it goes further in a number of respects. First, a theoretical model attributes to the system it describes "an inner structure, composition, or mechanism, reference to

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<sup>22</sup> One advantage of the new label over Achinstein's in the context of this paper is that mathematical models of the sort van Fraassen and Suppes consider, for example, are no less "theoretical" than models of this new variety.

<sup>23</sup> See n. 3.

which is intended to explain various properties exhibited by that object or system” (p. 213). Second, such a model is “treated as a simplified approximation useful for certain purposes” (p. 214). Thirdly, it is “proposed within the broader framework of some more basic theory or theories,” in the sense that “when a scientist proposes a theoretical model of X, he appropriates certain principles of some more fundamental and general theory or theories to which he is committed and applies them, along with various new assumptions, to X” (p. 215). I will take it to be a valuable observation that what I am calling propositional models often have one or more of these three features, but I do not wish to build possession of such characteristics into the definition of the notion of a propositional model.

The claim that propositional models are often taken to constitute no more than a useful approximation to the truth comports nicely with the frequently noted fact that scientific use of the term ‘model’ often seems to carry with it a certain degree of epistemic humility; in particular, a model is usually taken to be a more modest sort of object than a theory. However, there are other grounds one might have for such humility than a conviction that the model makes claims which, despite approximating the truth, are strictly speaking false. One might simply be toying with some ideas, and have no views either way about the truth or falsity of the things the model says. Or, as Braithwaite has noted, one might take the assumptions of the model “only to hold *ceteris paribus*,” and call it a model partly to “show ignorance of the conditions which would make this qualification unnecessary” (1962, p. 269). Finally (and perhaps relatedly), models often have much narrower scope than many of the things we call theories; one might take a given model to speak nothing but unqualified truths, and yet to say relatively little.<sup>24</sup> It is this last sort of humility, and this sort alone, which I take to be built into the notion of a propositional model, for the function of a propositional model

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<sup>24</sup> The emphasis here is on ‘relatively.’ A plausible model of the hydrogen atom may make less grandiose claims than the general principles of an overarching mechanics, but it is still a redoubtable achievement.

Incidentally, Braithwaite also mentions this as a reason one might have for calling something a model (1962, p. 269).

is to represent *some particular system* (or specific sort of system, or process) as having certain features, or behaving in a certain way.<sup>25</sup> The distinction implied here, between a “specific” sort of system (“mass-and-spring systems with a linear restoring force”) and very general kinds (“mechanical systems”), has somewhat vague boundaries, but there is no obvious harm in that, and it is worth noting that the notion of a mathematical model as employed by Suppes and van Fraassen also seems intended to apply to mathematical structures which are used to represent specific systems, or kinds of system.

Another valuable observation Achinstein makes about models of this sort is that they often involve an element of analogy. One form such involvement might take is relatively indirect: The model might have been constructed on analogy with some existing model of a distinct phenomenon, or sort of system; or (what is slightly different) the model might have been constructed with a perceived analogy between the modelled system and some distinct system in mind.<sup>26</sup> In such cases it might be typical, for heuristic purposes if nothing else, to mention the relevant analogy when presenting the model, and as Achinstein notes, a good number of models are even named after the analogies they invoke—the “billiard ball model of gases” and the “liquid drop model of the nucleus” are two standard examples (1968, pp. 216-7).<sup>27</sup> Despite all of this, the propositions making up the model itself might make no mention of any analogy, or of any system other than the one it is intended to model. On the other hand, I see no reason to rule out the possibility that a propositional model might contain, amongst the assumptions which make it up, a proposition to the explicit effect that the modelled

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<sup>25</sup> Of course, the system modelled may itself be rather all-inclusive—the universe, for example, is the object of many of the models cosmologists construct.

<sup>26</sup> Note that presumably mathematical models and physical models (discussed in the next subsection) can also involve analogies in these ways.

<sup>27</sup> Achinstein’s discussion of the analogical aspect of models is in many ways presented as though the (implicit) presence of an analogy were a defining characteristic of what he calls “theoretical models,” on a par with the other features I discussed a moment ago. What he actually says, however, is that “[a] theoretical model is often formulated, developed, and even named, on the basis of an *analogy...*”, and the qualifier “often” clearly takes all the bite out of this as a necessary condition on something’s being a “theoretical model” in his sense (1968, p. 216).

system is analogous to some other system, thus involving an analogy in a rather direct way.<sup>28</sup> One might argue that any respectable such model would have to specify the respects in, and degrees to which the modelled system and the “analogue system” are supposedly analogous, and that once such specifications have been made, all explicit mention of the analogy will become superfluous to the purposes of the model (such as explanation, prediction, and so on). But this *does* seem arguable,<sup>29</sup> and, what is more, the present taxonomy is not intended only to be adequate to respectable scientific models.

A precise understanding of the way in which models can involve analogies is, no doubt, a prerequisite for answering the questions of whether and how analogy plays a role in the methodology of model and theory construction, the logic of confirmation, the structure of explanation, and the semantics of the language of science. Such questions will not detain us here, however.<sup>30</sup> Suffice it for our purposes to remark that it is something close to a category mistake to say (as is sometimes said) that analogies are particular kinds of models. And although it is, on the other hand, not only coherent, but even true to say that many models build on analogies in one way or another, that fact is of no special relevance to our present concerns; in particular, there is no need to add a special category to my taxonomy to accommodate it.

Before we leave the propositional notion of model behind, it is worth comparing it with a notion of model to be found in the work of some proponents of the Received View of theories. According to the Received View, a theory should be thought of as consisting, first and foremost, of a set of uninterpreted sentences in a formal language, possibly accompanied by a set of syntactically defined rules for deriving such sentences from one another.<sup>31</sup> Authors such as Nagel (1961), Braithwaite (1962), and Spector (1965)

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<sup>28</sup> Achinstein does rule out precisely this possibility for theoretical models (*ibid.*, p. 217).

<sup>29</sup> One who espoused what Friedman calls the “familiarity” view of explanation might argue with it (although it seems to me that she would not be forced to). See Friedman (1974), pp. 191-2.

<sup>30</sup> For more on these questions, see, for example, Hesse (1966) and (1974), Braithwaite (1962), Achinstein (1968), Campbell (1920), Duhem (1954), Nagel (1961), Friedman (1974), and Nersessian (2005).

<sup>31</sup> Two qualifications: (i) ‘uninterpreted’ is an overstatement, of course, strictly applying only to the nonlogical symbols; (ii) at least for Nagel, the sentences need not be in a formal language, but can instead be sentences of scientific English which we *treat* as uninterpreted (1961, pp. 91-2).

then used the term ‘model’ to denote a set of *interpreted* sentences, derived from such a set of uninterpreted sentences by the superaddition of an interpretation for each of the non-logical terms. There are some differences between authors with regard to the additional conditions to be placed on a model in this sense,<sup>32</sup> but those differences are largely irrelevant here.<sup>33</sup>

Now one might regard this sentential notion of model as closely akin to the propositional notion the merits of which we have been examining; for some purposes, after all, we can regard an interpreted sentence and the proposition it expresses as interchangeable, and a set of interpreted sentences the members of which make claims about the hydrogen atom can surely be regarded as a representation of that kind of system. Unfortunately, things are not so neat. For the authors in question, models in this sentential sense are models of theories, not models of systems from the domain of inquiry. These authors call these sets of interpreted sentences “models” because of a relation the sets stand in to theories, a relation which is clearly centrally to do with interpretation, and, in Nagel’s case, making true; they are not so-called because of any representation relation they might stand in to gases, or economies, or chemical processes.<sup>34</sup> In many respects, then this notion of model seems to belong with the first

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<sup>32</sup> Nagel demands that the interpreted sentences in question be true (1961, p. 96, n. 4); Braithwaite does not (1962, p. 269). Braithwaite (who happily uses the term ‘theory’ to denote a set of fully interpreted sentences) requires that a model be based on an interpretation of the relevant terms other than the intended interpretation which, he takes it, comes with the theory in question (*ibid.*); for Nagel, *every* interpretation, including the intended one (if such there be), results in a model (*idem*). One manifestation of this difference is the fact that for Nagel a model in this sense is a model of an abstract formal calculus, whereas for Braithwaite it is a model of a (distinct) theory, where that theory can be thought of as a set of interpreted sentences, the abstract logical structure of which is displayed in an associated formal calculus. In this latter respect, Braithwaite is perhaps straying slightly from the true path of the Received View. (It is also important to bear in mind that for most proponents of the Received View, talk of an “intended interpretation” would not be unproblematic, especially as for both Nagel and Braithwaite, models seem to be the result of a full, and not merely partial interpretation of the nonlogical terms involved.)

As mentioned in section 1.2, above, Nagel is also willing to call the “system of ‘things’” we use to produce a sentential model from an uninterpreted calculus a model of the calculus in question. He is then using a version of the *second* of my notions of model. See n. 13.

<sup>33</sup> For an extended discussion of the sentential notion of model, see Achinstein (1968, ch. 8). Note, however, that on my reading of Suppes, as outlined in section 2 below, Achinstein is mistaken in reading the (so-called) axioms appearing in Suppes’ definition of the notion of a system of particle mechanics (i.e., in Suppes’ characterization of the class of mathematical structures which he takes to be the theory, classical particle mechanics) as “uninterpreted formulas that might constitute part of a calculus” (1968, pp. 227-8).

<sup>34</sup> Actually, as Achinstein notes, we might derivatively speak of a set of interpreted sentences *about billiard balls* as a model of a gas, when it is a model (in the sentential sense) of a theory of gases (1968, p. 229). But

and second notions we discussed. However, models in this sense are not objects of quite the right sort to be models in either of those senses. So here is one notion of model which can be found in the philosophy of science and which does not fit into my five-part taxonomy without some pushing and shoving. I have chosen not to create a separate category, however, mainly for the reason that this notion of model has, by and large, fallen into disuse. It is too closely tied to the Received View of theories, and had its heyday when empiricist problems about the meaning of theoretical terms and about the logic of analogical inference in theory development predominated, and were framed in terms of that approach to theory structure.<sup>35</sup>

*1.5 Physical models.* Our fifth and final notion of model was the main focus of Ludwig Boltzmann's entry on the term 'model' in the fourth edition of the *Encyclopaedia Britannica* (1902).<sup>36</sup> Boltzmann's opening phrase, after the etymological information, is "a tangible representation," and the emphasis here is on 'tangible':

There is an obvious parallelism with representation by means of models when we express longitude, mileage, temperature, &c., by numbers, which should be looked upon as arithmetical analogies. Of a kindred character is the representation of distances by straight lines, of the course of events in time by curves, &c. Still, neither in this case nor in that of maps, charts, musical notes, figures, &c., can we legitimately speak of models, for these [i.e., models] always involve a concrete spatial analogy in three dimensions. (p. 214)

The objects of which Boltzmann writes are "models of wood, metal and cardboard" (p. 218), and when he mentions models "of thermal, electro-magnetic and other engines," he notes that "[t]he largest collection of such models is to be found in the museum of the Washington Patent Office" (p. 220). His examples include plaster models of the wavefronts resulting from the refraction of light by a crystal, and, in geometry, papier

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note that this is quite at odds with the propositional notion of model, on which the propositions expressed by a set of sentences about billiard balls will constitute a model of billiard balls.

<sup>35</sup> Accordingly, one objection to the sentential notion of model is that it carries with it the danger of confusing what we would ordinarily call a particular formulation of the model with the model itself—an objection which is entirely parallel to a by now standard objection to the syntactic view of theory structure.

<sup>36</sup> The entry was reprinted in the eleventh edition of 1910-11; page numbers given below refer to that edition. Thanks to Hans Sluga for drawing my attention to Boltzmann's piece.

mâché models, and “thread models, in which threads are drawn tightly between movable bars, cords, wheels, rollers, &c.” (pp. 215-6).

Models in Boltzmann’s sense are thus *concreta*.<sup>37</sup> Taking our lead from Boltzmann, then, we shall say that a model in our fifth sense is an actually existing physical object or process which is used to represent the structure, properties, and/or behaviour of a system, process, or kind of system or process from the relevant domain of scientific inquiry.<sup>38</sup> In Achinstein’s typology, models in this sense are called “representational models”<sup>39</sup>; I shall use the term ‘physical model’ instead.

A paradigmatic, and indeed quite famous example of a physical model is the model of the structure of the DNA molecule which Francis Crick and James Watson constructed in the early 1950’s out of copper wire, carefully crafted tin plates, and (in occasional moments of desperation) pieces of cardboard.<sup>40</sup> Once it was finished, Crick and Watson used this edifice to convey their hypothesis about DNA to others,<sup>41</sup> but its role in the discovery was far more than expository. For Crick and Watson, work on the puzzle of understanding the structure of DNA was largely a matter of trying to construct a physical model that would make sense of the data which had been garnered by X-ray crystallography.<sup>42</sup> Clearly, it would be a mistake to underestimate the importance of physical models to certain sorts of scientific work.

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<sup>37</sup> At least at first sight, this feature of the models picked out by our fifth notion serves to distinguish them, on the whole, from models of each of the other sorts we have considered. (The immediate exception comes from the broadened notion of a truth-making structure, on which electrical circuits and such can count; less obviously, one might have a theory of propositions which makes them *concreta* from the outset, more general ontological issues aside.) Of course, should some reductive form of nominalism turn out to be correct, then all models in each of the other senses would be *concreta*, too.

<sup>38</sup> ‘Physical’ here is intended in a broad sense, in that no restriction to the objects explicitly mentioned in the theories of physics is intended; the reason for the qualification “actually existing” will become clearer in the sequel.

<sup>39</sup> “I turn first to the representational model, a three-dimensional physical representation of an object which is such that by examining it one can ascertain facts about the object it represents” (1968, p. 209). Braithwaite also notes the existence of this sense of the term ‘model,’ characterizing it in passing as “the vulgar sense” (1962, p. 270).

<sup>40</sup> See Watson (1969), pp. 45, 67, 97, 108, and 113.

<sup>41</sup> *Ibid.* (1969), pp. 118, 122, and 125, e.g.

<sup>42</sup> “In place of pencil and paper, the main working tools [in certain work of Linus Pauling’s had been] a set of molecular models superficially resembling the toys of preschool children.

We could thus see no reason why we should not solve DNA in the same way. All we had to do was to construct a set of molecular models and begin to play...” (*ibid.*, p. 27).

Apparently this methodological conviction was not universally shared (*ibid.*, *passim*, but see e.g., p. 120-1).

It is worth considering some of the ways in which physical models can differ from this paradigmatic example. First, the copper-wire model of DNA was clearly intended to represent the structure of a certain sort of system—a kind of molecule—but physical models can also be used to represent *particular* systems: consider physical models of the solar system, or a scale model of the Golden Gate Bridge constructed by an engineer or seismologist.

Second, physical models can also be used to represent processes rather than systems. During the 1995 Royal Institution Christmas Lectures in London, for example, the Cambridge geophysicist James Jackson gave an explanation of the formation of the Himalayas which involved pushing the moving side of a shallow box of sand inwards, causing ripples to form on the surface of the sand.<sup>43</sup>

Third, note that Crick and Watson’s model represented the spatial arrangement of the component parts of the DNA molecule, and did so by way of the spatial arrangement of its own constituents. In the passage I quoted from Boltzmann’s essay, he declares this a definitive characteristic of models in his sense (“models...always involve a concrete spatial analogy in three dimensions” (p. 214)), but the notion of a physical model I have given here agrees rather with Achinstein’s notion of a “representational model” in allowing for the representation of other features, and by other means.<sup>44</sup> A good illustration of such modelling is to be found in the representation of acoustical systems by electrical circuits. In the specific example Marshall Spector describes (1965, p. 280), an electrical circuit involving a resistor, a capacitor, and an inductor in series, and subject to a periodically varying electromotive force, is used to represent the behaviour of the air inside the neck of a flask when a sound wave impinges upon it. Consider two

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<sup>43</sup> Braithwaite mentions the case of a computer which is used as a model of the brain, wherein “the switching operations of the computer are taken as corresponding to the ‘firings’ of synapses in the brain”; “the physical processes in the computer,” he remarks, “will be regarded...as echoing, in temporal sequence, the succession of processes in the field which is the subject-matter of the theory” (1962, p. 270).

<sup>44</sup> Despite the explicit pronouncement cited, some of Boltzmann’s examples suggest that he, too, has a broader notion of model in mind at times. Consider, in particular, the example he offers of the analogies which can arise between the “electrical conduction” in an electrical system and the internal friction in a gas, and the fact that he refers to the electrical conduction “in the *model*” (p. 220; emphasis added).

particular aspects of the way in which representation proceeds here: the quantity of charge on the capacitor represents the displacement of the air in the neck of the flask, and the varying electromotive force represents the force acting on the air due to the incoming sound wave. In neither case do we have spatial features of the model representing spatial features of the modelled system, and in the second case neither the represented feature nor the feature representing it is spatial.<sup>45</sup>

Fourth, Crick and Watson's model is intended to represent a kind of system of which, it is presupposed, there are examples. Some physical models, however, might be intended as representations of an uninstantiated kind. Suppose, for example, that we heat a large circular metal plate in such a way that the temperature increases radially from the centre in the right way, and equip it with thermally expanding rulers; we will then have a physical model of a nonexistent two-dimensional universe with a non-Euclidean geometry (Feynman, 1964, v. II, pp. 42-1 to 42-5).<sup>46</sup>

Fifth, note that although Crick and Watson's model was constructed precisely as a representation of the internal structure of the DNA molecule, some physical models might originally have been constructed with other purposes in mind. One might use an electrical circuit as a representation of an acoustical system even if the circuit in question was originally built to aid in the study of electrical circuits, or power a piece of equipment.<sup>47</sup>

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<sup>45</sup> When the quantity doing the representing is distinct from the quantity being represented (as in the example just given, but not in the DNA case), Achinstein calls the model an "analogue model" (1968, p. 210).

<sup>46</sup> One might even regard some physical models employed in geometry (such as those discussed by Boltzmann (pp. 215 and 216) and mentioned above) as representations of abstract objects, depending, of course, on the view one takes of the ontology of mathematics.

<sup>47</sup> That an object *X* has an analogous structure to object *Y*, or exhibits analogous behaviour, is clearly not a sufficient condition for *X*'s being a representation of *Y*; the former relation, after all, is a symmetric one, whereas the latter is not. And in the case in which (physical) object *X*, though analogous in structure or behaviour to *Y*, was constructed with some purpose other than the representation of *Y* in mind, we might be less inclined to say that *X* is a representation of *Y*. In that case, however, it seems to me that we would also be correspondingly less likely to call *X* a model of *Y*. Perhaps we would say instead that *X* "can serve as" a representation of *Y*, and that it "provides" a model of *Y*. I would speculate that in saying such things we are, in effect, claiming that *X* is especially well-suited, in virtue of its intrinsic features, to being used for certain purposes, perhaps primarily by means of its being thought of a certain way, and that only if we begin to use it for such purposes will it become a model, or representation, of *Y*. Similar remarks would apply, I take it, to a case in which *X* is not a constructed object at all—consider the example Achinstein mentions of Maxwell's analogy between the movement of a swarm of bees and molecular diffusion in a gas (1968, p. 210). It thus seems to me that the notion of a physical model I have characterized fits well with ordinary

Two further points about the use of physical models are worth making before we move on. First, we have already noted that the process of constructing a physical model can be a crucial part of the process of developing a new hypothesis; in a similar vein, it is worth remembering that a physical model as a finished product can be put to active experimental use in the testing of hypotheses about the system it models, and in the investigation of the features and behaviour of that system more generally. One might think of wind tunnels as providing a good example of physical models which are used this way. In that respect, then, such physical models are unlike, say, landscape paintings.<sup>48</sup> Secondly, as in the case of propositional models, it is important not to be misled by the talk of representation into thinking that anyone who uses a physical model to represent a system as having certain features is thereby committed to believing that system in question really has those features. Boltzmann, for example, discusses the notorious case of Maxwell's mechanical models of the microstructure of the electromagnetic ether, and Maxwell's apparent disavowal of a realistic attitude to those models.<sup>49</sup> Anti-realism, at least of some notable varieties, remains an option.<sup>50</sup>

*1.6 Summary.* To recap, our five notions of model, listed with authors who have articulated notions which are least closely related, are:

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usage; whether this impression is correct may or may not be important, depending on the philosophical tasks one wishes to accomplish with the aid of the notion.

<sup>48</sup> Achinstein stresses this aspect of the function of physical models (his "representational models"); indeed, it may be that he meant to make it part of the definition of a representational model that it be something which is "considered in depth, subjected to calculation" (p. 211), and possibly experimented on. (This would make sense of the otherwise somewhat redundant phrase "a physical representation of an object which is such that by examining it one can ascertain facts about the object it represents," (p. 209) by packing a lot of content into the notion of examination.) In any case, this is not something I wish to build into the notion of physical model.

<sup>49</sup> See the passages from Maxwell which Achinstein quotes on p. 220, and at p. 221, n. 4., of his (1968). For further discussion, see e.g., Nersessian (2005). See Buchwald (1985) for an extensive history of Maxwell's own understanding of his electromagnetic theory.

<sup>50</sup> One exception, perhaps, would be a form of anti-realism which declared meaningless, or otherwise prohibited, all putative talk of the unobservable, a view which would be at least in tension with the use of Crick and Watson's copper-wire construction to represent the structure of DNA (or the assumption that DNA molecules are unobservable, or that their structure is). Of course, such an anti-realist might offer a construal of Crick and Watson's utterances regarding the construction (the ones expressing their representational intentions towards it) according to which they were making claims about the observable realm. Otherwise (and perhaps even then), the anti-realism in question is of a particularly implausible or hard-to-swallow variety.

- 1) Truth-making map: A model as a mapping from parts of a language which provides an interpretation for, and makes true, some given set of sentences in that language. (Enderton)
- 2) Truth-making structure: A model as a (generally) nonlinguistic structure which provides an interpretation for, and makes true, some set of sentences. (Tarski)
- 3) Mathematical model: A model as a mathematical structure used to represent a (type of) system under study. (Suppes, van Fraassen)
- 4) Propositional model: A model as a set of propositions, the members of which together form a representation of a (type of) system under study. (Achinstein)
- 5) Physical model: A model as a physical object used to represent a (type of) system under study. (Boltzmann)

Equipped with this taxonomy, we are in a better position to think about how we should understand the semantic view of theories, and to think about the adequacy of the different notions of model to various philosophical tasks.<sup>51</sup>

## Section 2 – Understanding the semantic view

If there is a dominant view of theory structure in current philosophy of science, then it is the semantic view.<sup>52</sup> Although there are, of course, a number of variant versions of the

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<sup>51</sup> One additional apologia regarding my choice of terminology (in addition to the one given at the end of section 1.3): As it turns out, the word 'model' appears only in labels corresponding to notions of model which involve representation as a crucial component. No significance should be attached to this; in particular, I do not mean to suggest that the term 'model' should really only apply to objects which function as representations.

<sup>52</sup> For a detailed and relatively recent history of the semantic view and a survey of the literature on it, see Suppe (1989), pp. 5-20.

semantic view on the market, the majority of them centrally involve at least one of two claims. First there is:

A scientific theory is a collection of models. (I)

The 'I' here is for 'Identification,' as this claim identifies scientific theories as being certain sorts of object.<sup>53</sup> The second claim is perhaps a less ambitious one, although one would not want to say that it is entailed by (I). It is a methodological recommendation directed primarily to those who are engaged in philosophical work on the sciences:

A scientific theory is best thought of as a collection of models. (M)

In both cases, 'collection' is sometimes replaced by 'set,' 'class,' 'family,' or 'population,' and some authors may wish to impose restrictions of scope (to the natural sciences, for example), but whenever possible these differences will be set aside in the present discussion.

A more significant new twist is due to Ronald Giere, who suggests that we think of theories as having two components, "(1) a population of models, and (2) various hypotheses linking those models with systems in the real world" (1988, p. 85),<sup>54</sup> and van Fraassen embraces Giere's suggestion (1987, p. 109).<sup>55</sup> On the sort of picture which

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<sup>53</sup> See, for example, Giere (1988), pp. 47-8, especially "This makes it possible [for the semantic view] to identify a theory...with [a] set of models." Note, however, that it is harder to find bald statements of such a thesis in the original writings of Suppes or van Fraassen. In his "What Is A Scientific Theory?" (1967), for example, Suppes simply emphasises the value of "extrinsic characterizations" of theories which proceed by picking out a class of models, whilst leaving room for "intrinsic" and more linguistically-oriented formulations as well. Van Fraassen presents the "identification" claim in his "The Semantic Approach to Scientific Theories" (1987, p. 109), but only tentatively, and in the context of describing Suppes's view; his characterizations of the semantic view in *The Scientific Image* (1980, pp. 44 and 64), and in "On the Extension of Beth's Semantics of Physical Theories" (1970), for example, whilst clearly putting the emphasis on models, fall short of making such a claim.

<sup>54</sup> In Giere's hands, the general idea is that the hypotheses in question make claims to the effect that various real systems (or types of real system) are adequately represented by various of the models in certain respects, and to certain degrees; see also Suppes (1967), pp. 62-4.

<sup>55</sup> Although Giere and van Fraassen are working with very different notions of model (see Thomson-Jones 2010, section 2.2 and *passim*), and as van Fraassen and Giere both recognize, they differ over the typical form

results, the collection of models obviously makes up only one of the two halves of a theory. For simplicity's sake, I will omit this possible amendment to (I) and (M); incorporation would not affect the substance of the discussion.

My initial question, rather unsurprisingly, is now: What notion of model is being invoked when a proponent of the semantic view utters (I), or (M)? As the discussion of this section progresses, we will also consider the distinct and less hermeneutical question: Given the avowed aims of the semantic view, what notion of model should a proponent of the semantic view be employing in making one or both of the claims in question? First, though, let us focus on matters of interpretation.

I take it to be obvious, and will assume without further argument, that no proponent of the semantic view ever intended to invoke (nor ever invoked) either the notion of a propositional model or the notion of a physical model when putting forth their version of (I) or of (M). The notion of a truth-making map can be dismissed almost as quickly: van Fraassen explicitly rejects it on both his own behalf and Suppes' (van Fraassen (1987), p. 109, n. 2); when Suppes invokes a logical notion of model he usually mentions Tarski, whose notion of model, as we have seen, was that of a (set-theoretical) truth-making structure, rather than that of an Endertonian truth-making map (see especially Suppes (1960)); and van Fraassen (*loc. cit.*), Suppes (1967, p. 57), Suppe (1989, p. 4), and Giere (1988, pp. 47-8) all insist that models in the relevant sense are nonlinguistic (or "extralinguistic") entities,<sup>56</sup> whereas a truth-making map is a partially linguistic thing, for it is a function which takes the non-logical vocabulary of a particular language as its domain.

The models on which Suppes and van Fraassen place the most emphasis are mathematical structures of various kinds. It is thus tempting to ask, in light of the

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of a theoretical hypothesis: for Giere theoretical hypotheses make claims about *similarities* between models and real systems, whereas for van Fraassen they make claims of isomorphism and embeddability.

<sup>56</sup> Confusingly, Giere's emphatic claim to this effect follows immediately upon an Enderton-style definition of 'model' which makes models truth-making maps (*op. cit.*, p. 47). I think this must be read as an error on Giere's part.

taxonomy laid out in the previous section, and the options eliminated thus far, whether these mathematical structures, the models of which the semantic view theorist speaks, are to be thought of as truth-making structures of some specific variety, or as mathematical models—i.e., as representations of systems from the domain of inquiry. That question may present a false dichotomy, however. In particular, a third option is that when a proponent of the semantic view puts forth (I), or (M), she means by ‘models’ to be alluding to entities which function *both* as truth-making structures and as mathematical models in the representational sense. From the point of view of my taxonomy, the notion of model employed would then be a hybrid notion, but describing it as such is not intended to be prejudicial in any way.

It is quite clear, I think, that the models of the semantic view theorist are intended to function as representations.<sup>57</sup> What is sometimes less clear is the extent to which the models referred to in (I) or (M) should be understood to be functioning also as truth-making structures. On the one hand, both Suppes and van Fraassen make explicit statements to the effect that the notion they wish to employ is, or is very closely related to, a logical notion,<sup>58</sup> and van Fraassen begins his presentations of the semantic view in both *The Scientific Image* (1980) and *Laws and Symmetry* (1989) by introducing a notion of model on which models are clearly functioning as truth-making structures in one way or another.<sup>59</sup> On the other hand, when it comes to showing the naturalness and plausibility with which theories in the empirical sciences can be viewed as collections of models, it is quite unclear that the models in question are, as constituents of those theories, functioning as truth-making structures in any interesting way. There is ample room for confusion on this score, however, and so it will be worthwhile to take a moment and consider an example in detail.

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<sup>57</sup> See the following discussion of Suppes, and the discussion of van Fraassen in section 1.3, above.

<sup>58</sup> Suppes (1960), p. 289, and (1967), p. 57; van Fraassen (1980), p. 44.

<sup>59</sup> Van Fraassen (1980), pp. 41-44, and (1989), pp. 217-220. In both cases, van Fraassen draws an example from logical and mathematical work in the foundations of geometry.

A good example of the way in which Suppes makes the case that theories can be thought of as collections of models is provided by his 1957 “axiomatization” of Newtonian particle mechanics (1957, p. 294):<sup>60</sup>

DEFINITION 1. A system  $\beta = \langle P, T, s, m, f, g \rangle$  is a system of particle mechanics if and only if the following seven axioms are satisfied:

KINEMATICAL AXIOMS

AXIOM P1. The set  $P$  is finite and non-empty.

AXIOM P2. The set  $T$  is an interval of real numbers.

AXIOM P3. For  $p$  in  $P$ ,  $s_p$  is twice differentiable on  $T$ .

DYNAMICAL AXIOMS

AXIOM P4. For  $p$  in  $P$ ,  $m(p)$  is a positive real number.

AXIOM P5. For  $p$  and  $q$  in  $P$  and  $t$  in  $T$ ,

$$f(p, q, t) = -f(q, p, t).$$

AXIOM P6. For  $p$  and  $q$  in  $P$  and  $t$  in  $T$ ,

$$s(p, t) \times f(p, q, t) = -s(q, t) \times f(q, p, t).$$

AXIOM P7. For  $p$  in  $P$  and  $t$  in  $T$ ,

$$m(p)D^2s_p(t) = \sum_{q \in P} f(p, q, t) + g(p, t).$$

In presenting a theory this way, we are introducing what Suppes calls a “set-theoretical predicate” (1957, ch. 12, esp. §12.2).<sup>61</sup> In this case the set-theoretical predicate is ‘is a system of particle mechanics.’<sup>62</sup> We then pick out a class of set-theoretical entities simply by instructing our audience to consider the class of objects to which the predicate

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<sup>60</sup> Suppes himself does not use the word ‘model’ much in the (1957) discussion. Nonetheless, his remarks on at (1957), p. 299, and his references back to the 1957 discussion on p. 291 of his (1960) make it clear that he wishes to apply the term ‘model’ to the “systems of particle mechanics” defined in the quotation.

For more examples of Suppesian “axiomatizations” of scientific theories, see, for example, Suppes (1974). My use of scare quotes here will be explained shortly.

<sup>61</sup> Suppes is taking it that the notions of real number, vector, function, derivative of a function, and so on have all, at least in principle, been given a set-theoretical analysis in the context of an axiomatic set theory. See (1957, pp. 249-50).

<sup>62</sup> To avoid confusion, it is important to bear in mind that, absent a thoroughgoing nominalism of exactly the right sort, no system of particles enduring over time could count amongst the “system[s] of particle mechanics,” as that phrase is defined in Suppes’ Definition 1, for the latter are ordered tuples whose elements are scalar-valued functions, vector-valued functions, sets of real numbers, and the like. It is thus an unfortunate choice of words, perhaps, to call the objects to which the set-theoretical predicate applies the “systems of particle mechanics”; “models of the theory of particle mechanics” seems a less troubled choice.

applies, or, to put it another way, which satisfy the predicate. And the fundamental idea behind the semantic approach to scientific theories, in Suppes' version of that approach, is a version of the idea expressed by (M): it is that philosophers of science will most readily gain insight into the structure of scientific theories if they think of them as the sorts of things which can be presented in just this way—that is, by way of the definition of a set-theoretical predicate.

Importantly, and as is often stressed, this method of picking out a class of set-theoretical entities allows us to avoid any reference to a set of sentences in any particular language, or any invocation, implicit or otherwise, of a model-theoretic semantics. Thus, in characterizing Suppes' proposal, van Fraassen writes approvingly: "*to present a theory, we define the class of its models directly, without paying any attention to questions of axiomatizability, in any special language*" (1987, p. 109, emphasis in original). Indeed, not only is there no explicit mention of any formal language, or any set of sentences in such a language, but it is quite unclear what set of sentences in what formal language would do the trick, via a corresponding truth-making relation, of capturing the class of structures the definition picks out.<sup>63</sup> And this is in no way a defect of the definition as a means of presenting Newtonian particle mechanics, *if* the point is only to carve out a class of mathematical structures (the models of the theory, in our present sense) which, according to the theory, are adequate in one way or another to the job of representing actual or possible collections of particles as they move under the influence of various forces.

It is important not to be misled by Suppes' own use of the word 'axiomatize.' Suppes is not offering an axiomatization of that theory in the sense in which a proponent of the syntactic view would seek one (—presumably the sense van Fraassen has in mind

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<sup>63</sup> This claim is in no way compromised by the fact that Axioms P5-P7 correspond in fairly straightforward ways to Newton's laws of motion. (See Suppes' discussion at (1957), pp. 296-8.) For one thing, it is clear that the class of set-theoretical structures picked out by Definition 1 is not the class of truth-making structures of any set of sentences in a denumerable first-order language (i.e., it is not an elementary class); to see this, just consider Axiom P2 in the light of the Löwenheim-Skolem theorem (Enderton (1972), p. 141). (Van Fraassen makes this point in a different setting in his (1985), pp. 301-2; see also his (1987), p. 120.)

in his characterization of Suppes' idea). One of the "axioms" Suppes presents, for example, is that 'T', the second element of any tuple which is to satisfy the set-theoretical predicate in question (and so count as a model of the theory) must be an interval of real numbers (Axiom P2). Clearly, then, the "axioms" Suppes presents do not form a set of sentences whose deductive closure can be regarded as capturing the content, or even the logical structure, of Newtonian particle mechanics, for then they would have to be about (or translations into a formal language of sentences about) forces, masses, accelerations, and the like; axioms in the sense Suppes employs are simply components of a characterization of a class of mathematical structures.

Correspondingly, although we might regard a given model in Suppes's sense (i.e., a given mathematical structure satisfying the predicate 'is a system of particle mechanics') as a truth-making structure for sentences such as 'The set *P* [or: The first set in the tuple] is finite and non-empty' and 'The set *T* [or: The second set in the tuple] is an interval of real numbers,' in the same sort of way that an electrical circuit can be regarded as a truth-making structure for the sentence 'Every resistor heats up when a current passes through it' (see section 1.2, above), this is in essence simply to say that it fits the description we have given in our attempt to pick out a certain kind of mathematical structure. And that does not mean that any given model of the theory in Suppes' sense is a truth-making structure for some set of sentences which we might regard as a linguistic formulation of the classical theory of particle mechanics. In one respect, at least, then, the label 'semantic view' is as much a misnomer as the name 'received view' has become.

Van Fraassen makes this point about the different senses of 'axiomatize' quite forcefully ((1980), p. 65; see also (1970), p. 337), and in the same setting describes his own picture of theory structure as closely allied to Suppes':

To present classical mechanics, for instance, [Suppes] would give the definition: 'A system of classical mechanics is a mathematical structure of the

following sort...’ where the dots are replaced by a set-theoretic predicate. Although I do not wish to favour any mathematical presentation as the canonical one, I am clearly following here his general conception of how, say, the theory of classical mechanics is to be identified. (1980, p. 66)

The conclusion I would wish to draw is that, insofar as claims (I) or (M) are supported by the ease with which we can give presentations of empirical theories on which they look like collections of models, the appropriate notion of model is simply that of the mathematical model. The mathematical structures which such presentations pick out are clearly meant to be used for the purposes of representation; but there is no special reason for supposing that they play a role as truth-making structures for any linguistic formulation of the relevant theory.

Reading (I) and (M), the defining theses of the semantic view, as involving only the notion of the mathematical model, so that all talk of truth-making falls away, fits nicely with Suppes’s slogan that “philosophy of science should use mathematics, and not meta-mathematics” (van Fraassen, 1980, p. 65), for model theory is surely meta-mathematics. Such a reading also seems to make the most sense of van Fraassen’s claims that the semantic view is “a view of theories which makes language largely irrelevant to the subject,” and that, on the semantic view, “models are mathematical structures, called models of a given theory only by virtue of belonging to the class defined to be the models of that theory” (1987, p. 108 and p. 109, n. 2).

Most importantly, however, it would seem that employing only the notion of a mathematical model in advancing (I) or (M) would be the best way to vouchsafe the attainment of certain avowed goals of the semantic view. An account of theory structure, I take it, derives its primary philosophical value from the accounts it facilitates of confirmation, explanation, theory-testing, and the like, and from the light it thus sheds on debates over realism and rationality. Proponents of the semantic view have repeatedly claimed that their view of theory structure has at least two advantages when it comes to this sort of philosophical work: that by construing theories non-linguistically

it allows us to sidestep a slew of familiar problems about the meaning, reference, and classification of various terms in the language scientists employ, and that it leads to a way of thinking about theories which maintains greater closeness to scientific practice than the older, syntactic view. Given this, it seems that a proponent of the semantic view ought to prefer the notion of a mathematical model to either the notion of a truth-making structure, or a hybrid notion on which models play both truth-making and representational roles, for if the models to which (I) and (M) refer are to be thought of as functioning crucially as truth-makers for sentences making up some linguistic formulation of the theory, there is surely a significant danger that we will begin to focus once more on the sentences in questions and the language in which they are written<sup>64</sup>; and as we do so, we are likely to drift further and further away from scientific practice.

Overall, then, it seems to me that the semantic view has little to lose, and something to gain, by employing in its central theses a concept of model shorn of the trappings of model-theoretic semantics, and the role of truth-making.<sup>65</sup> The notion of a mathematical model, I thus want to suggest, provides us with the most plausible version of the semantic view. It is largely with that in mind that I now wish, in the remainder of this paper, to explore certain limitations of the notion of a mathematical model. It is worth pointing out, however, that the discussion in the next section may be of independent interest even to one who rejects my conclusions concerning the semantic view. It also seems unlikely that the problems I wish to describe for the notion of a mathematical model would be satisfactorily addressed by replacing it with a notion of model on which it is crucial that a model functions as a truth-maker.

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<sup>64</sup> Accordingly, it will also be difficult to get philosophers of science to stop thinking of laws as fundamental to theories, something van Fraassen, for one, wants very much to do. See his (1989).

<sup>65</sup> A notable exception to this general claim is perhaps the sort of work in which van Fraassen was engaged in his (1970). (See also van Fraassen (1972).) There the state space for a system, which clearly plays a representational role, also plays a role in the proposed formal semantics for the language of the given physical theory. The project, however, is first and foremost a logical one, and is described as such; and interestingly, van Fraassen makes only the importantly qualified claim that the picture of theory structure he presents is “more faithful to current practice in *foundational research* in the sciences than the familiar picture of a partly interpreted axiomatic theory” (1970, abstract, emphasis added; see also pp. 337-8). (The discussion makes it clear that work in quantum logic is the kind of research he has in mind.) One might simply see this as evidence that different philosophical projects call for different notions of model.

### Section 3 – Mathematical models and propositional models

3.1 *The trouble with mathematical models.* The limitations of the notion of a mathematical model I have in mind can be brought effectively to light by asking what sense of ‘model’ is in play when physicists speak of the “Bohr model of the hydrogen atom.”<sup>66</sup>

According to the Bohr model of hydrogen, a hydrogen atom is comprised of two particles, a proton and an electron. The electron, of mass  $m$ , is significantly lighter than the proton, and moves around it in circular orbits. The two particles carry opposite electrical charges,  $\pm e$ , and attract one another in accordance with Coulomb’s law. There are then two additional constraints on the precise way in which the electron orbits the proton. The first is Newton’s second law, which, in combination with Coulomb’s law and the assumption that the electron orbits are circular, immediately allows us to derive the following relation:

$$\frac{mv^2}{r} = \frac{e^2}{r^2} \quad (1)$$

( $v$  is the velocity of the electron,  $r$  the radius of the orbit, and units have been chosen so as to simplify Coulomb’s law.) The second is Bohr’s own contribution, a postulate according to which the orbital angular momentum of the electron must be an integer multiple of a certain fundamental constant,

$$mvr = n\hbar \quad n = 1, 2, \dots \quad (2)$$

It is then a matter of simple algebra to show that, given the assumptions made so far, the electron can only circle the proton in orbits with radii

$$r_n = n^2 \cdot \frac{\hbar^2}{me^2} \quad n = 1, 2, \dots \quad (3)$$

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<sup>66</sup> Also known as the Rutherford-Bohr, or Bohr-Rutherford model. This choice of example was suggested by van Fraassen’s discussion of the “Bohr model of the atom” (1980, p. 44), but note the difference. The reason for this should become clear when I consider van Fraassen’s remarks below. (On a related note, let me stipulate that we are restricting attention to the Bohrean model of atoms of the most common isotope of hydrogen,  $^1\text{H}$ , i.e., hydrogen with a single proton for a nucleus.)

and associated velocities

$$v_n = \frac{1}{n} \cdot \frac{e^2}{\hbar} \quad n = 1, 2, \dots \quad (4)$$

From here, one can go on to make predictions about the structure of the emission and absorption spectra of hydrogen gas.<sup>67</sup>

It seems immediately clear that the term ‘model’ is being used here in a way which is closely tied to the notion of representation; the model in question is, after all, identified as a model of the hydrogen atom. For this rather simple reason, we can lay aside the first two senses of the term laid out above, for in neither of those senses is a model, as such, a representation.

It is important to note the qualifier ‘as such’ here. All sorts of objects can be used to represent, and there is of course no *a priori* reason why something which is a model in one of our first two senses (those of the truth-making map and the truth-making structure) could not also function, in the right context, as a representation. The point, however, is that being a representation is no part of being a model in either of the first two senses. And this is why I take it that neither of those senses is the one in effect when we talk of the Bohr model of the hydrogen atom, for I am taking it to be self-evident that in such a context the word ‘model’ is being used in a way which invokes the notion of representation.<sup>68 69</sup>

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<sup>67</sup> The presentation of the Bohr model just given is based on Cohen-Tannoudji *et al.* (1977), vol. 1, pp. 791-2. It is somewhat partial, omitting (for example) any mention of energy, despite the fact that energy considerations were crucial to the job of explaining the data concerning emission and absorption spectra which provided the original stimulus for development of the model. Such embellishment would be unnecessary clutter from the point of view of our discussion. The presentation given is also historically misleading, but only in entirely standard textbook ways; in particular, as Pais (1986, p. 200) notes, in the original paper the ‘ $mvr = n\hbar$ ’ postulate standardly attributed to Bohr actually appears as a consequence of a different postulate, albeit one to which it is equivalent in the context of the other assumptions of the model. (The original paper is Bohr (1913); see also Bohr (1918)).

Incidentally, note the presence of an implicit idealization here: We have equated the total force on the electron with the electromagnetic (Coulomb) force, thus neglecting the effects of gravitational attraction between the two particles.

<sup>68</sup> Neither veridical nor complete representation is implied, however. We modern people who think that the Bohr model provides a picture of the hydrogen atom which is extremely inaccurate in a number of respects have not thereby felt the need to refrain from calling it a model of the hydrogen atom, and it is doubtful whether anyone ever took it to be exhaustive.

<sup>69</sup> It should also be said that the prospects for plausibly maintaining that the Bohr model of the hydrogen atom is *de facto* a model in one of our first two senses seem dim. Certainly it is hard to read presentations of

Can we suppose, then, that it is our third sense of ‘model’ which is at play in standard uses of the phrase ‘Bohr model of the hydrogen atom’? I want to argue that it is not, and I shall do so by giving some reasons for thinking that the Bohr model of the hydrogen atom is *not* a mathematical model in the sense I have defined. On the assumption that physicists are not mistaken in calling the Bohr model of the hydrogen atom by that name, it follows that it is not the notion of a mathematical model which they are employing in that locution. And if that is right, then we must conclude that the notion of a mathematical model is not well-suited to the central philosophical tasks of understanding how the confirmation of models proceeds, understanding the nature of explanations which proceed by reference to models, and so on; for surely successful completion of these tasks will require some sort of elaboration or explication of the notion (or notions) of model scientists employ in such contexts.

Our question, then, is whether the Bohr model of the hydrogen atom is a mathematical model; is it, that is, a mathematical structure intended to represent a certain sort of physical system? Certainly, as we have emphasized, the Bohr model is intended to represent a certain type of physical system, and certainly it has a significant mathematical aspect. Let us begin by asking, however, whether the Bohr model could be a mathematical model of one of the paradigmatic varieties we encountered in van

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the model, such as the one just given, as presentations of a mapping from terms in some language to objects which thereby provide them with an interpretation, partial or otherwise. It may seem less implausible to insist that the Bohr model is, in some sufficiently loose sense, a “structure,” and one which *could* be used to provide an interpretation for some set of (only) partially interpreted claims or other, in such a way that the claims turn out true. The problem here is that if *that* is all it takes to be a model in our second sense, then very few things would seem to be absolutely excluded from the class of models in that sense. It is therefore more interesting to ask whether the Bohr model is something we *actually* employ to the end of truth-making interpretation (and perhaps to refine our characterization of the second sense accordingly); the answer to that question seems more straightforwardly to be no.

Incidentally, one might go on to argue that the Bohr model is at least not a truth-making model for the axioms or fundamental postulates of any recognized theory by drawing attention to its notoriously semi-classical constitution. Insofar as it constrains electrons to a fixed series of orbits within each one of which the electron can orbit without suffering *Bremsstrahlung*, it is in conflict with classical electrodynamics; and insofar as it posits continuous electron trajectories, with the electron enjoying both a well-defined position and momentum at all times, it is impossible to reconcile with quantum mechanics, at least on any orthodox reading of that theory (and on the majority of alternative approaches, for that matter). Thus, one might insist, there are representational models of systems which are not, in the truth-making sense, models of any associated theory. For an argument to this effect from interestingly and importantly different cases, see Frisch (1998).

Fraassen's writings (section 1.3), or in Suppes' (section 2). There is, I think, a straightforward reason for regarding the answer to this question as being no.

As the presentation given above makes clear, the Bohr model of the hydrogen atom is not a representation of any *particular* evolution a hydrogen atom might undergo. Instead, it contains a significant amount of modal information about hydrogen atoms: the electron can move around the proton in any circular orbit which satisfies the constraints laid out in the course of presenting the model (that is, those circular trajectories whose defining parameters, radius and speed, are given by equations (3) and (4)), and that's all. Nothing else is permitted.<sup>70</sup> The model does allow for an infinite range of possible orbits, but the orbits it allows are a tiny fraction (speaking somewhat metaphorically) of those it rules out. The Bohr model, then, tells us about the possible ways a hydrogen atom can be, but does not single out any particular state, or sequence of states, for special attention.

Just the opposite would be true of a mathematical model of a hydrogen atom of the particular sorts Suppes and van Fraassen describe in their expositions, for the models they describe are what we might call "single history models." On Suppes' characterization of the models of classical particle mechanics (section 2, above), a model of that theory is a mathematical structure intended to represent one particular history of a system of interacting particles, as a single model of the specified sort involves a single function  $S$ , which represents the trajectories of the particles by giving their positions at each of the times represented by the elements of  $T$ ; presumably this feature of the approach would carry over to a Suppesian treatment of the "old quantum theory." And as we have seen (section 1.3), it is built into van Fraassen's characterization of the notion of a model in at least some places that single models represent single histories. Thus a

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<sup>70</sup> I am simplifying for the sake of exposition, but not in any way which affects the essential point being made here. In particular, transitions between such orbits are permitted—as indeed, they must be if the model is to explain the spectral data it is designed to explain—but how those transitions take place was itself regarded as problematic. As the model prohibits the electron from moving along any path other than the circular orbits it picks out, transitions between them were taken to be discontinuous "quantum jumps"; this was one of the radical ways in which the Bohr model diverged from received physical thinking.

mathematical model of the hydrogen atom constructed along the lines suggested by Suppes' and van Fraassen's detailed accounts would be a mathematical structure which, when properly interpreted, would tell us one way things might go with a hydrogen atom, and no more. In particular, it would contain very little modal information about the system in question. We would learn, on consulting such a model, that one particular history, the one represented in the model, is a possible one for the system in question, but that is all; most dramatically, no possible histories would be ruled out. This, then, gives us decisive reason for insisting that the Bohr model of the hydrogen atom is not, and cannot be thought of as, a model of the specific sort Suppes and van Fraassen describe: no such model could contain the kind of representational content the Bohr model contains.

The very formulation of this difficulty, however, suggests a way in which we might be able to think of the Bohr model of the hydrogen atom as a mathematical model in the general sense, despite the conclusion that it is not a mathematical structure of the sort on which Suppes and van Fraassen tend to focus. We might instead propose that the Bohr model of the hydrogen atom is (or should be thought of as) a certain *collection* of single history models, namely, the collection of all those single history models which represent possible histories of a hydrogen atom according to the Bohr model. It is easy to see how such a collection of single history models can be regarded as having the sort of modal representational content a single model lacks: if each structure in the collection is interpreted so as to represent a possible history of a hydrogen atom, then we can understand the collection as a whole to be telling us what all the possible histories are; in principle, we will then be able to read off the sorts of general constraints laid down in a standard presentation of the Bohr model (such as those given in equations (1)-(4)) from the structure of the collection of single history models (although, obviously, that might

be a rather difficult thing to do if the collection is presented to us in a sufficiently oblique way).<sup>71</sup>

Indeed, there may well be more than one way of circumventing the representational limitations of the single history model, and so construing the Bohr model of hydrogen as a mathematical model of some stripe. Another option (one which is, I take it, essentially equivalent to the “collection” strategy) might be to identify the Bohr model with a single state space containing all the trajectories which the Bohr model deems possible. Provided that we are creative enough, then, it seems that all is not lost for an approach to understanding the sense of ‘model’ involved in talk of the Bohr model which is based upon the notion of the mathematical model. At this point, however, it is perhaps worth raising a question about the precise nature of the project we are considering. What exactly is it that the mathematical model approach might be thought to have succeeded at here?

One way of thinking of the project in question is as an attempt to give a relatively precise account of what scientists *mean* by the term ‘model’ when they use a phrase like ‘the Bohr model of hydrogen,’ or (equivalently, I suppose) of what *notion* of model

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<sup>71</sup> This proposal was suggested by a passage in van Fraassen’s *The Scientific Image*:

Scientists too speak of models, and even of models of a theory, and their usage is somewhat different. ‘The Bohr model of the atom,’ for example, does not refer to a single structure. It refers rather to a type of structure, or class of structures, all sharing certain general characteristics. For in that usage, the Bohr model was intended to fit hydrogen atoms, helium atoms, and so forth. Thus in the scientists’ use, ‘model’ denotes what I would call a model-type. Whenever certain parameters are left unspecified in the description of a structure, it would be more accurate to say (contrary of course to common usage and convenience) that we described as structure-type. (p. 44)

Note, however, that the proposal is not the same; nor is it clear that van Fraassen has the “modal content” issue in mind in this passage. Van Fraassen’s worry is rather that scientists speak of the Bohr model of *the atom*, and that there is a question about how such a thing is to be related to models of the hydrogen atom, the helium atom, the lithium atom, and so on. In response, he seems to suggest that we can begin with a model of the hydrogen atom, a model of the helium atom, etc., and then identify the Bohr model of the atom (*simpliciter*) with the collection of these. (Or perhaps there is a more abstract structure from which each of the more specific models can be derived by fixing the values of certain parameters, values which are left unspecified in the abstract structure (“One proton or two?”), and this more abstract structure can be thought of as the Bohr model of the atom.) My point, on the other hand, is that if van Fraassen is thinking of a model as a state space with a trajectory mapped onto it, then there is no single model which he can identify as the Bohr model of the hydrogen atom. Given that, it becomes unclear just what he is proposing in this passage. Perhaps the Bohr model of the atom is a collection of collections of models....

scientists are employing at such times. The proposal that we are considering will then be that sometimes when scientists use the term 'model,' they mean "mathematical structure used to represent the structure and/or behaviour of systems from the domain of inquiry." This claim has a certain initial plausibility to it, even if that is thanks in no small part to the vagueness of 'sometimes.' And, significantly, the plausibility of this claim might be thought to underwrite a standard contention about the advantages of the semantic view of theories over the received view, namely, that the semantic view, with its emphasis on models, provides us with a way of thinking about scientific theorizing which is much closer to actual scientific practice. (Certainly the claim that scientists are often employing the notion of a mathematical model when they use the term 'model' is far more plausible than the claim that scientists mean "deductively closed, axiomatizable set of sentences (understood primarily as uninterpreted syntactic entities) in a first-order predicate language" by the term 'theory'.)<sup>72</sup>

Alternatively, one might shift ground a little, by invoking some form of sense/reference distinction, reading the talk of "meaning" and "notions" in the last paragraph as talk about senses, and then insisting that what is properly at issue here is not the sense of the scientist's term 'model', but its reference. One could perhaps then go on to consider the claim that when a scientist speaks of the Bohr model of hydrogen, she is in fact picking out a mathematical structure of one of the admittedly rather complex sorts we have discussed, whether she thinks of it that way or not;<sup>73</sup> certainly such a claim is not obviously false. And there are other options still: the project might be taken to be rational reconstruction, or Carnapian explication. In each case, an approach based on the notion of a mathematical model might tell us a story about talk of the Bohr model of hydrogen which has some plausibility. The question we should now ask, however, is

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<sup>72</sup> This is not to suggest, of course, that proponents of the received view were making a claim of this sort about the scientists' use of the term 'theory'. The suggestion is rather that they would have needed to make such a claim in order to be giving an account of theory structure capable of making close contact with scientific practice.

<sup>73</sup> In much the same way as one might claim that someone speaking of gold thereby picks out all the stuff with atomic number 57, regardless of whether she is thinking of it that way.

how well such an approach generalizes. If the prospects for successful generalization seem dim, that is one reason for doubting the approach even in our initial, more limited domain.

In this connection, I want to consider a specific problem the mathematical model approach faces when we try to generalize it. I will suggest a way of further extending the approach in the face of this new difficulty, but by then I think it will have become clear that the approach faces a serious plausibility problem whether the issue is sense, reference, rational reconstruction, explication, the mathematical model approach has difficulty in completing it with much simplicity, elegance, or usefulness (which is not to say that it cannot complete some parts of the project in a way which exemplifies these virtues). I will then present two more reasons for doubting that even the Bohr model of hydrogen is a mathematical, before moving on to examine the possibility that the notion of a propositional model can succeed where the notion of a mathematical model has apparently failed.

Let us turn our gaze, then, from physics to a less mathematical science, such as cell biology.<sup>74</sup> There we still find plentiful talk of models, exemplified by presentations of the nuclear model of the cell. To quote Downes: “in most texts a schematized cell is presented that contains a nucleus, a cell membrane, mitochondria, a Golgi body, endoplasmic reticulum and so on” (1992, p. 145). But this sort of model is not presented by means of equations, or any other sort of mathematical talk; rather, it is presented by means of diagrams and descriptive prose. On the surface of it, the nuclear model of the cell simply does not look like a mathematical structure of any sort.<sup>75</sup> How then could the

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<sup>74</sup> There are, of course, parts of biology which are highly mathematical, such as population genetics, but the problem I have in mind appears with the greatest clarity when we consider subdisciplines which are less mathematical than the greater part of physics.

<sup>75</sup> Although he does not put the observation to quite the uses I have in mind, I take my inspiration here from Downes’ paper (1992, esp. pp. 145-6, but on a related point, see also pp. 147-8). In addition to this point (i.e., that in some important areas of science models do not look much like mathematical structures at all), Downes makes a number of other claims which are in sympathy with the present essay, expressing, in particular, considerable scepticism about purported connections between notions of model we find in metamathematics (in my framework, notions (1) and (2)) and notions of model at work in the sciences. On the whole, however, he does not divide up the conceptual territory in the same way.

notion of model which is at work in such contexts be accounted for in terms of the notion of a mathematical model?<sup>76</sup>

Let us briefly consider one technique we might try to accomplish such a thing. The trick I have in mind bears some similarity to the “supervaluation” approach to the logic of vagueness. The idea is this: The nuclear model of the cell clearly represents the cell as having spatial parts which stand in certain spatial relationships to one another—the nucleus lies inside the boundaries of the cell membrane, and the endoplasmic reticulum lies between them, for example. No precise shape is attributed to any of these parts of the cell by the model, and neither does the model represent them as standing in any more exact geometrical relationships than the ones just described; indeed the model does not even specify relative sizes for these components of the system. So the immediate difficulty we face is that any geometrical model we might construct (involving, say, a three-dimensional real vector space with certain sets of points in the space picked out as representing the locations of the nucleus, membrane, and reticulum) would contain too much information. (The utterances of the nuclear model, so to speak, are much more vague.) Perhaps, then, we might try to think of the nuclear model of the cell as (again) a *collection* of such geometrical structures. The collection would be chosen so that there would be enormous divergence between individual structures as to just how they picture the precise spatial relationships holding between the nucleus and the membrane, the membrane and the reticulum, and so on, and as to how they picture the shapes and sizes of the individual components. The only matters upon which all the structures in the collection would agree would be that the nucleus is inside the membrane, and that the reticulum lies in between. Our collection of structures would

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<sup>76</sup> The example of the nuclear model of the cell perhaps poses another, smaller problem for van Fraassen, because (as we saw) it is built into his notion of model that a model of a nonrelativistic theory (which, I suppose, the “theory of the cell” is) exhibits changes in the state of a system over time. Despite the fact that in the nuclear model the parts of the cell are specified at least partly in terms of their functions, and the fact to talk of function is a move in the general direction of talk about behaviour over time, it would seem strained to claim that the model in question represents changes in the state of the system over time. Accommodating this sort of “structural” model would presumably not require sacrificing the spirit of van Fraassen’s approach however; and this aspect of the situation is not even a *prima facie* problem for Suppes.

then be understood to represent the cell as having just those features which every individual structure represents it as having.

Clearly the nuclear model of the cell contains more spatial information than the information I have just focussed on, so that a collection of geometrical models will have to have far more internal structure if it is to have a hope of mimicking the nuclear model of the cell in that respect. More significantly, however, it is unclear how the procedure should be extended so that a collection of mathematical structures can capture the *other* sorts of information contained in the nuclear model (such as, most obviously, information about the functions of the cell's various components). Such technical worries about the procedure fall by the wayside, however, when we reflect again on the question of just what it is we might mean by claiming that the nuclear model of the cell can be identified with such a large and complex collection of mathematical structures.<sup>77</sup> If the claim is even in part that when a cell biologist speaks of the nuclear model of the cell, what the term 'model' means is "collection of mathematical structures" (of the sort just described), then the claim is surely enormously lacking in plausibility. But claims about the meaning of the term 'model' as it appears in the sciences are among our primary concern, for in the present essay, we have attempted to construct a taxonomy of the notions of model which have been employed in the philosophy of science, and, to some extent, to evaluate the usefulness of these various notions. One of the main ways in which an articulated notion of model might make itself philosophically useful is by providing us with the means to give an account of the notion or notions of model which scientists themselves employ; and certainly both van Fraassen and Suppes have claimed

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<sup>77</sup> It is perhaps worth noting that the set of structures in question will presumably have the cardinality of the reals. However, this was also true of the class of mathematical structures we earlier considered identifying with the Bohr model of hydrogen (or, alternatively, of the class of trajectories which would have to be defined on a single space)—consider, for example, all the possible times at which, or ranges of time during which, a given transition might occur.

Note also that the collection of structures would have to be even more complex to deal with models which have both considerable modal content and little in the way of mathematical content. For example, we might let each possible qualitatively described state or history of the system under consideration would be represented by a collection of mathematical structures, along the lines described with respect to the nuclear model of the cell; the range of possibilities would then be described by a collection of such collections.

to be doing just that, at least in part. (Recall again the fact that part of the standard advertising for the semantic view of theories is that it makes greater contact with scientific practice than the syntactic, or received view could manage.) The discussion to this point makes it clear, however, that the notion of a mathematical model, as it is to be found in the writings of Suppes, van Fraassen, and others, does not provide us with a plausible account of the notion of model which a cell biologist is employing when he speaks of the nuclear model of the cell.

As we have already mentioned, it might be maintained that, whether or not the mathematical model approach can provide a good account of the sense of the term 'model' as it appears in various scientific usages, it can provide an account of the nature of the referents of the term. Such a claim is reminiscent of proposals in the philosophy of mathematics to the effect that (for example) whether or not there is any set-theoretical component to most people's concept of the number two, or the concept most people in a first calculus course have of the derivative of some function, they are in fact referring to set-theoretical structures when they use the relevant terms. Another possible claim is that the notion of a mathematical model provides us with the tools for elaborating a rational reconstruction of scientific theories, even in cell biology. Yet another is that the notion of a mathematical model provides an explication in Carnap's sense, of the (or a) notion of model at work in the sciences. Any of these claims is, I take it, less implausible than the claim to be able to illuminate the sense of the term 'model' as used by cell biologists. Nonetheless, in each case there is surely a serious worry about the extent to which we have managed to approximate the ideal of simplicity in our philosophical account, and that worry should be borne in mind as we go on (in section 3.2) to consider the possibility of accounting for scientists' talk of models with the notion of a propositional model.<sup>78</sup>

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<sup>78</sup> Of course, the degree to which simplicity should guide us in adopting a philosophical account is itself a debatable matter. Note, however, that in the specific case of the project of Carnapian explication, simplicity of the explicans is by definition a guiding constraint.

One might respond to this discussion of the nuclear model of the cell by claiming only that the mathematical model approach can account for scientific talk of models in certain locales—primarily, the physics of the last few hundred years. No mean feat, admittedly, but there may be room for further doubts. For one thing, the desire for a unified account of scientific talk of models might lead some to look askance at a mathematical model account of the notion of model even as it is to be found in mathematical physics.<sup>79</sup> For another, it might well be argued that apparently nonmathematical models play an important role in physics, too. If there is a billiard ball model of gases, or a nuclear model of the atom, which has transcended and outlived specific proposals concerning the details of the dynamics, then such a thing might seem quite unmathematical in nature. And the claim that there are models of just this sort has been central to some accounts of the way in which scientific theory change can be guided and constrained, and even to some arguments for scientific realism.<sup>80</sup>

In closing this critique of the notion of a mathematical model, I would like to mention two further reasons for doubting that the Bohr model of hydrogen (or the nuclear model of the cell, for that matter) should be identified with any mathematical model in the sense characterized in section 1.3.

Recall that for both Suppes and van Fraassen a model is a mathematical structure in quite a narrow sense of that phrase: an ordered tuple whose elements are typically things like sets of real numbers, vector-valued functions on the reals, and so on in Suppes' case; a set of points with certain structural features (differing from case to case), together with a mapping from some interval of the real line to elements of the set in van Fraassen's case.<sup>81</sup> Such structures can be completely specified in mathematical terms,

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<sup>79</sup> The emphasis here is on the "might" and "some". Certainly such considerations are unlikely to hold universal sway in these times, when for many the emphasis is on plurality and disunity.

<sup>80</sup> See, for example, Hesse (1966), Kuhn (1970), and McMullin (1985).

<sup>81</sup> At least in the case of nonrelativistic theories. Although Bohr sought to improve the initial model by taking into account relativistic corrections, the starting point was nonrelativistic. In any case, van Fraassen's models are no less mathematical in the case of relativistic theories.

entirely without the employment of physical concepts;<sup>82</sup> yet if we were to ask someone to present us with the Bohr model of the hydrogen atom, and in reply they made no mention of electric charge, or of particles and orbits, we might justly accuse them of not doing their job. Furthermore, it does not strike one as a contingent matter that the Bohr model of hydrogen represents hydrogen atoms as having certain features—nothing could be the Bohr model of hydrogen, we might say, without representing hydrogen atoms as being such-and-such a way—but it seems a highly contingent matter that some mathematical structure should be employed to that purpose. Together, these considerations suggest that there is a sense in which the representational content of the Bohr model is “built in,” whereas, in contrast, a mathematical model only acquires representational content in virtue of certain facts quite extrinsic to it—that we use it in a certain way, or have certain intentions concerning it, for example.<sup>83</sup> And if this is correct, then clearly the Bohr model cannot be identified with any mathematical model.

One might reply to the point about adequate presentation by insisting that although the Bohr model of hydrogen just is a mathematical structure of a certain kind, it is a fact of pragmatics that, in typical contexts, requests to be presented with the Bohr model are to be understood as requests not only to have the right mathematical structure picked out, but also to have its intended interpretation (and so its intended representational content) made clear. This, however, does not address the claim that the Bohr model has its representational content essentially, something no mere mathematical structure of the kinds we have considered can manage. Alternatively,

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<sup>82</sup> This is clear from Suppes’ characterization of the class of models which, according to his “axiomatization,” make up classical particle mechanics—see the passage quoted above, in section 2.

<sup>83</sup> Note that one cannot plausibly claim that any mathematical structure which might be taken to be the Bohr model of hydrogen is a representation of hydrogen purely in virtue of standing in the right relations to hydrogen atoms, relations which leave us out of the picture (such as “structural analogy,” for example), just because the Bohr model of hydrogen is a *bad* model of hydrogen, so that any mathematical structure which might be taken to express the Bohr model (let alone *be* it) will not stand in any such relations. (This point could be made more vividly with models which we now take to be wider of the mark than the Bohr model was—Thomson’s model of the atom for example, or the phlogiston model of combustion.)

Incidentally, this is why I took it to be a problem that it is a contingent matter whether *we employ* a given mathematical structure to the ends of representing hydrogen (or intend it to represent hydrogen, alternatively): it follows, I take it, that any given mathematical structure could have failed to *be* a representation of hydrogen, in the relevant sense, and the problem is that the same cannot be said of the Bohr model of hydrogen.

then, one might try to deal with both problems by proposing that the Bohr model be identified with some *interpreted* mathematical structure.

Suppes seems to suggest a manoeuvre of this general nature in his (1960), during a discussion of the apparent gap between the way “physicists want to think of a model of the orbital theory of the atom” and his own emphasis on the idea of models as set-theoretical entities.<sup>84</sup> His proposal works as follows: Suppose we are working with classical particle mechanics, understood along the lines of the axiomatization quoted above, and that we wish to model the solar system. The models of the theory are ordered sextuples,  $\langle P, T, s, m, f, g \rangle$ ,<sup>85</sup> satisfying a variety of conditions. The first element in a given tuple,  $P$ , is a set of objects intended to represent the “particles” whose properties, interactions, and evolution are represented by the model as a whole. Suppes then suggests that we will have a model of the solar system which is sufficiently “physical” to satisfy the physicist if we just let the elements of  $P$  be the “planetary bodies” (1960, pp. 290-1).<sup>86</sup>

There are several problems with this proposal, however. For one, it can at most succeed in fixing the objects the behaviour and structure of which the model is intended to represent, by making them constituents of the model; clearly the interpretation of the other elements of the tuple is still open, so that the model does not yet say anything determinate about the objects it is intended to represent. (Does  $m(p_i)$ ,  $p_i \in P$ , represent the mass or the charge of the  $i$ th planet, for example?) For another, this approach clearly

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<sup>84</sup> Suppes is not, however, discussing the specific worries I have just raised. It is interesting to note that, in a different connection, van Fraassen explicitly rejects the idea that “the intention, of which sorts of phenomena are to be embedded in which kinds of empirical substructures, be made part of the theory” (1980, p. 66). On the assumption that this rejection extends also to making our intentions with regard to *nonempirical* substructures part of the theory, and given the identification of a theory with a collection of models, it follows that the representational context of our models is not to be regarded as part of the models *per se*. And so, given van Fraassen’s claim that the scientist’s use of ‘model’ corresponds to talk of a *type* or *class* of models in his sense, he is clearly not relying on the inclusion of an interpretation to close the gap between models in his sense and models in the scientist’s sense.

<sup>85</sup> For continuity of exposition I have adopted the apparatus laid out in the (1957) axiomatization, which is the one quoted above; the (1960) discussion involves some alterations which are of no present relevance.

<sup>86</sup> To see that Suppes does mean for the elements of  $P$  to be the planetary bodies, and not just to represent them, consider the following quotes, especially in context: “the physical model may be simply taken to define the set of objects in the set-theoretical model” (pp. 290-1), and, perhaps more tellingly, “The [relevant] abstract set-theoretical model of a theory will [in such a case] have among its parts a basic set which will consist of objects ordinarily thought to constitute the physical model” (p. 291).

cannot get off the ground if in fact there *are* no objects of the sort the model is intended to represent—consider a model of the electromagnetic ether, for example, or of Cartesian vortices. And anyway, even putting aside these two difficulties, it is not clear that this approach delivers models which represent the sorts of things they represent essentially. It is surely only because we adopt a certain interpretive convention that a Suppesian model of particle mechanics in which  $P$  is the set of planetary bodies comes to be about the solar system, and it is just as surely a contingent fact that we adopt such a convention. There is no reason we could not have decided to use a model involving the planets to represent a certain collection of billiard balls, or a particular group of charged metallic spheres rolling around on a bench, or (in a particularly wrong-headed moment) the social organization of a beehive.<sup>87</sup>

This last point will carry over, I suspect, to any other detailed account of how the interpretation of a mathematical structure is to be fixed. An *interpreted* mathematical model is still just a mathematical structure, one to which we have superadded an interpretation. And whatever technique we apply to achieve the interpretive end, it will be clear that we might have chosen to apply the technique differently, so as to provide the structure with different representational content, or not have applied it at all, leaving the structure representationally barren. We still do not have a model which bears its representational content essentially.

It is thus not only that regarding the Bohr model of hydrogen and the nuclear model of the cell as mathematical models results in rather complex and implausible proposals concerning the notion of model scientists employ in such contexts; there are also quite fundamental reasons for thinking that a mathematical model is just not the right sort of thing to have certain features the Bohr model and the cell model apparently have. We shall now see whether the notion of a propositional model fares any better.

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<sup>87</sup> Note, however, with respect to this last difficulty, that Suppes did not design his proposal to address the problem (if it is one) of giving an account of models on which they are sorts of things which bear their representational content essentially.

3.2 *The advantages of propositional models.* My contention is that the notion of the propositional model is far better equipped than the notion of a mathematical model to make sense of scientists' talk of the Bohr model of the hydrogen atom and the nuclear model of the cell. To see this, we need only consider the difficulties we were forced to wrestle with on behalf of the notion of a mathematical model in this regard, and note the fact that none of those difficulties arise for the claim that the Bohr model, or the nuclear model of the cell, is a propositional model.

Let us begin with the Bohr model of the hydrogen atom. Suppose that this model is just a set of propositions about the structure of hydrogen atoms (a sort of system the model presupposes there to be). One of the propositions which makes up the model might be that hydrogen atoms have two parts, a positively charged particle (the proton), and a much smaller, negatively charged particle (the electron) which orbits the nucleus; another might be that orbital angular momentum of the electron can only take one of the values  $n\hbar$ , where  $n$  is a positive integer; and so on.<sup>88</sup>

First, then, note that a full and explicit presentation of such a set of propositions will presumably involve some mention of hydrogen, and of electrons, protons, and negative charge, whereas the same could not be said of even a detailed presentation of a mathematical structure of the Suppes/van Fraassen variety. Indeed, it requires little in the way of either imagination or mental squinting to see standard textbook presentations of the Bohr model as presentations of a set of propositions.<sup>89</sup>

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<sup>88</sup> There are certain worries which arise once we take seriously the job of trying to complete this initial sketch, and I will address at least some of these worries a little later in this section; the ones I have in mind do not impinge on the way in which propositional models avoid the difficulties mathematical models have with accommodating the Bohr model of hydrogen, or the nuclear model of the cell.

Note that a propositional model can have as much mathematical content as we like. Some, or even all of the propositions appearing in some models might be most naturally expressed in mathematical terms, and we need place no upper limit on the complexity or abstractness of the mathematical content, provided only that it occurs in the service of representing systems or processes from the domain of inquiry. I take it that it would be misleading to say that scientific models can contain "mathematical propositions", because as representations of chemical, or biological, or economic systems, such models are *about* (or are models of) such objects and processes as photosynthesis, group selection, and free markets; they are not about vector spaces, or complex-valued functions, or the reals, or Abelian groups.

<sup>89</sup> Or at least, of the members of such a set. There is perhaps some philosophical reconstruction involved in identifying the Bohr model with a certain *set*. See below for a related discussion of problems with the individuation of propositional models.

Secondly, propositional models would seem to have the representational content they have necessarily; we need only assume that a given set necessarily has the members it has, and that a given proposition necessarily has the content it has, and it will follow straightforwardly that no propositional model could have failed to be a representation of the things it actually represents. The two assumptions in question seem uncontroversial, as modal claims go, and so it seems unproblematic to claim that a set of propositions we might be tempted to identify as the Bohr model, unlike any mathematical structure of the Suppes/van Fraassen variety, will have its representational content necessarily, something one might intuitively take to be true of the Bohr model. To employ the locution of possible worlds: in any possible world in which such-and-such a set of propositions about hydrogen exists, it is a set of propositions about hydrogen.<sup>90</sup>

Thirdly, it is clear that a set of propositions can easily have the sort of content we normally take the Bohr model itself to have. There is no limit on the amount of modal content a propositional model can have, and no reason that a propositional model need picture any particular history of the system. As we saw, the mathematical model approach was perhaps able to overcome this problem by suggesting the identification of the Bohr model with a large class of single history models (or with a single state space containing a large class of trajectories), but clearly the identification of the Bohr model with a single set of propositions solves the same problem with greater simplicity, and the corresponding claims about what physicists have in mind when they talk about “the

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<sup>90</sup> Of course, a stronger claim follows in just the same way, namely, that it is a necessary truth that a given propositional model of hydrogen represents hydrogen as being exactly the way it actually represents it as being. This again seems to fit nicely with an identification of the Bohr model such an entity, and it is again a feature that no purely mathematical structure has.

Whether the qualifier concerning the existence of a given propositional model in other possible worlds is required will depend on the details of one’s views about propositions. For example, if one takes it that Napoleon is a constituent of the (timeless) proposition that Napoleon is short, then there are possible worlds in which that proposition does not exist (or at least, not in any world-bound sense of ‘exist’). And clearly one *might* extend that view of propositions in such a way that propositions about hydrogen, or about electrons, only exist in worlds in which there is some hydrogen or some electrons. However, combining such a view of propositions with the idea that scientists’ models are often propositional models might bring us to the unfortunate conclusion that there is no phlogiston model of combustion in the actual world, for example. (David Lewis has proposed a manoeuvre which could be adapted to solve this problem: see Thomson-Jones (2007), pp. 49-51 for discussion of a very similar point.)

Bohr model of the hydrogen atom” and what they are referring to when they use the phrase, have much greater initial plausibility. Indeed, one could even go so far as to insist that understanding talk about the Bohr model as talk about a set of propositions gives us a way of thinking about this sort of scientific theorizing which brings us closer to actual scientific practice than that provided by the mathematical model approach.

The final difficulty faced by the mathematical model approach was that of providing an account of talk of models in the relative absence of mathematics, as in the case of the nuclear model of the cell. Once again, the notion of a propositional model accommodates the “phenomena” without strain: We simply suppose that the nuclear model of the cell is a set of propositions whose content is qualitative rather than mathematical (e.g., that the cell is made up of three major parts, the nucleus, the membrane, and the cytoplasm, that the cytoplasm lies between the nucleus and the membrane, and so on). It is similarly easy to see how a propositional model can represent the posited parts of the cell as having various functional roles and, more generally, how a propositional model can represent systems and processes as involving certain sorts of causal structures. These are things that at least some of the models of which scientists speak are evidently taken to do, and yet they are things that mathematical models seem unable to do without the employment of strategies which are complex and cumbersome at best.

However exactly we are to understand philosophical claims of the form ‘The Bohr model of the hydrogen atom is an X,’ or ‘The nuclear model of the cell is a Y,’ then, there are a number of respects in which these claims are more plausible if we replace ‘an X’ and ‘a Y’ by ‘a set of propositions’ than if we substitute ‘a mathematical structure which we employ for the purposes of representation.’ And this conclusion generalizes quite straightforwardly to cover a wide range of examples of scientific talk of models, for the features of the Bohr model of hydrogen and of the nuclear model of the cell on which the preceding arguments rest are far from peculiar to those models. What is more,

a set of propositions is a much simpler structure to employ in our philosophical work than a large collection of single history models, say.

*3.3 Peaceful coexistence.* The notion of a propositional model thus has much to recommend it, at least in certain areas. Does this mean that we should simply eschew the notion of a mathematical model which can be found in the writings of Suppes, van Fraassen, and others, a notion which, I have argued, is central to the much-discussed semantic view of theories, in favour of a widespread employment of the notion of a propositional model? Shall we say that Suppes and van Fraassen are wrong about what a model is, and that Achinstein is more nearly right? Or even, as a distinct claim, that the notion we find in Suppes' and van Fraassen's writings is of no use to us as philosophers of science, and that the notion of a propositional model is entirely adequate to our purposes?

Any of these responses to the arguments I have put forward would be an overreaction, and I would like to emphasize that I am not recommending any of them. In particular, note that although I have argued that the notion of a mathematical model is not well-suited to the job of accounting for certain central scientific uses of the term 'model' in a simple and plausible way, and that a notion closely related to one which Achinstein articulates is superior in that regard, this does not amount to the much stronger-sounding claim that Suppes and van Fraassen are "wrong about what models are." It seems clear that the term 'model' is used in a number of different ways by scientists, and in yet more ways by practitioners of other disciplines. The issue is not what models *are*, but rather, what notions of model can be distinguished in various scientific and philosophical discussions, and which notions are appropriate to the various philosophical tasks we might have set ourselves. I have been concerned to make one particular claim along the latter lines, namely, that the notion of a propositional

model is more appropriate to the specific task of accounting for certain paradigmatic scientific uses of the term 'model' than the notion of a mathematical model.

It is nonetheless true that mathematical models play an important role in the theorizing of scientists from a wide range of particular disciplines, and that they are used in explanation, prediction, experimental design, and numerous other areas of scientific practice.<sup>91</sup> It would thus be a mistake not to pay attention to structures of that sort in our philosophical thinking about the nature of scientific explanation, of confirmation, of theory change, and so on. However, it is worth noting that it is possible to bring the notion of a mathematical model and the notion of a propositional model together in a way which seems quite intuitively satisfying, and allows us considerable flexibility.

We noted earlier that a propositional model can have mathematical content;<sup>92</sup> for example, if the Bohr model of the hydrogen atom is a propositional model, then we would expect it to contain the proposition that the value of the orbital angular momentum,  $L$ , of the electron is constrained by the equation  $L = n\hbar$  ( $n = 1, 2, \dots$ ). But notice now that a propositional model can contain propositions which refer explicitly to mathematical structures; it can, for example, involve a proposition to the effect that the evolution of such-and-such a system is represented in a particular manner by a certain mathematical structure of the Suppes/van Fraassen "single history" variety (by a trajectory through a state space, to use van Fraassen's preferred terminology), or it may involve one or more propositions to the effect a certain kind of system can only evolve in one of the ways represented by the members of a certain class of mathematical

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<sup>91</sup> Such structures often turn up for work in less formal garb than the costumes they wear for philosophical occasions. Nonetheless, it is easy to recognize them as the very same structures. For example, the physicist who is working with classical particle mechanics will certainly employ differentiable vector-valued functions to represent the force on a particle at a time, but unless she is expressly engaged in foundational work, she is unlikely to describe one of the arguments such a function takes as an element of a set which is an interval on the real line (Suppes' 'T'—see the passage quoted in section 2, above), or to think of the function and such a set as two elements of a tuple; regimentation of that sort however, may be philosophically useful, and it does not seem to introduce any significant distortion of ordinary practice. (Note that van Fraassen's approach is arguably a little more welcoming towards mathematical structures in casual dress.)

<sup>92</sup> See n. 88.

structures. (This latter case is appropriate to propositional models which, like the Bohr model of the hydrogen atom, contain a significant amount of modal information about the possible behaviours of the systems of some specific kind; the first case, on the other hand, is one in which the propositional model in question is a representation of a particular evolution of a particular system, or sort of system.) Thus an approach based on the notion of a propositional model seems more than able to accommodate the widespread importance of the sort of mathematical structures Suppes and van Fraassen have emphasized.

The difference between focussing on a certain mathematical structure (or collection of structures) and a proposition to the effect that that structure (or collection of structures) represents the evolution (or range of possible evolutions) of a certain kind of system may begin to seem like an unimportant one. But in the attempt to combine the two notions of model, we should not lose sight of the advantages of the second. A propositional model has the option of employing other mathematical techniques in the interests of representation, and it can also represent by entirely nonmathematical means, thus making room for such apparently nonmathematical creatures as the nuclear model of the cell; indeed, a single propositional model can even involve propositions of more than one kind, thus combining complex mathematical representation and far more qualitative information.<sup>93</sup> As we have seen, the fact that propositional models bear their representational content essentially makes them more suitable for accounts of certain sorts of scientific talk about models. And, furthermore, unlike mathematical models, propositional models have components which can serve as objects of knowledge and belief, and as bearers of truth and falsity, all of which might plausibly make them more natural entities to consider for various philosophical purposes, such as the study of explanation and confirmation. It is also worth bearing in mind how comfortably the

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<sup>93</sup> One interesting possibility is that certain kinds of pictorial representation might play an ineliminable role in conveying the content of certain scientific models. I will leave it as an open question whether a satisfactory response to such a phenomenon would be to rely on propositions to the effect that a certain sort of diagram represents certain features of the system in question.

notion of a propositional model underwrites certain familiar ways of speaking about models in which both scientists and philosophers are prone to indulge, especially when they are not thinking explicitly about the notion of a model: Consider locutions like “according to the model...” and “the model postulates...,” as well as talk of examining, explaining, testing, deducing the consequences of, and examining the plausibility of the “assumptions of the model.”

In the case of those propositional models which do make reference to mathematical structures, there is an interesting parallel with Giere’s picture of theory structure. For Giere, a theory “compris[es] two elements: (1) a population of models, and (2) various hypotheses linking those models with systems in the real world” (1988, p. 85). In much the same way, we could, perhaps, think of a propositional model of the special sort in question as being composed of a mathematical structure (or collection of structures) and a set of propositions “linking” the structure (or structures) to a system or kind of system in the domain of inquiry. Note, however, that even those propositional models which do involve mathematical structures may also contain propositions which do not; and, more importantly, note also that, as I argue elsewhere, Giere’s notion of model must be emphatically distinguished from the Suppes/van Fraassen notion.<sup>94</sup>

*3.4 Propositional models and theories.* One question which naturally arises at this point is the question of the relationship which obtains between propositional models and theories. Answering this question in any detail would require the choice of a view about the structure of theories. It is perhaps reassuring to note, however, that an emphasis on propositional models seems, *prima facie* at least, to be compatible with more than one picture of theory structure.

If, for example, one thinks of Newtonian mechanics as consisting, in essence, of Newton’s three laws of motion, together perhaps with a number of specific force laws

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<sup>94</sup> See Thomson-Jones (2010), sec. 2.2 and *passim*.

(such as the law of universal gravitation), then the associated propositional models will be additional theoretical constructs, but ones which are constructed at least in part along lines laid down by the theory, a heritage which will manifest itself by the inclusion of laws from the theory (or propositions stating laws, if these are different) in the model itself. (This seems to be something like the picture Achinstein has in mind (1968, pp. 215-6).)

On the other hand, one might propose a variant semantic view of theories according to which a theory is a collection of *propositional* models; in that case, propositional models would themselves be components of theories. Such a view would clearly preserve certain central aspects of the spirit of the orthodox semantic view, which (I have claimed) centrally regards theories as collections of mathematical models, for on both approaches a theory is seen as an assemblage of representations (or structures which are to be used as representations) of specific sorts of system, rather than as a collection of overarching general principles, as a more traditional view would have it. Both the orthodox and the propositional variants on the semantic view thus seem to allow for a greater looseness of structure to theories than the syntactic view, and to make more room for a deflationary attitude to laws (features which, I take it, Suppe's and Giere's version of the semantic view are also intended to have).<sup>95</sup> The propositional variant, however, has at least one decided advantage over the orthodox semantic view, in that it extends straightforwardly to theories with little or no mathematical content. Nonetheless, just because, as we have seen, propositional models can invoke mathematical structures of the Suppes/van Fraassen variety when appropriate, a propositional variant of the semantic view would be quite capable of according such structures a central role in those theories for which it seem plausible to do so.

Some might balk at the idea that a view on which theories are collections of sets of propositions should be called a variant of the semantic view at all. Surely, it might be

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<sup>95</sup> For more on these variants of the semantic view, see Thomson-Jones (2007, 2010), in which I express my doubts about the notions on which they build.

objected, one of the primary motives driving the development of the semantic view in its various forms was the desire to avoid thinking of theories as linguistic structures, for that approach seemingly brought us nothing but trouble.<sup>96</sup> And is a collection of sets of propositions not a linguistic structure?

It is tempting to reply to such an objection on its own terms. For one thing, the semantic view was originally so-called because of a perceived connection between the models which, according to the view, make up theories, and the models which do the work in the formal (“model-theoretic”) semantics of certain familiar formal languages (notwithstanding my insistence that this connection should be severed);<sup>97</sup> the opposition was to thinking of theories as sets of syntactic entities, such as largely uninterpreted sentences of a formal language to which a partial interpretation is then superadded. But propositions are just as much entities drawn from semantics as the models of model theory, even if they are drawn from a different approach; thus, one might insist, the propositional variant of the semantic view is no less deserving of its name than established variants are of theirs. And although propositions certainly seem on the face of it to be more readily classifiable as linguistic entities than the mathematical structures on which Suppes and van Fraassen have focussed, a theory of propositions which identifies them with states of affairs or sets of concrete possible worlds arguably has the consequence that a proposition is no more a “linguistic” entity than Napoleon, who was, after all, the referent of a proper name.

The important issue such an objection raises, however, is not whether a propositional variant of the semantic view deserves to go by that name, but whether such a view of theory structure would entangle us once more in the sorts of problems to which the “received view” gave birth, and which the semantic view was intended to

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<sup>96</sup> See, for example, van Fraassen (1987), pp. 108-110. And at the opening of his 1989 opus, Suppe chooses the following as his initial characterization of the semantic view: “According to the Semantic Conception of Theories, scientific theories are not linguistic entities, but rather are set-theoretic entities” (p. 3).

<sup>97</sup> “[S]emantic’ is used here in the sense of formal semantics or model theory in mathematical logic” (Suppe, 1989, p. 4). As noted earlier, Suppe provides a history of the development of the semantic view in his 1989, pp. 5-20.

avoid. Indeed, some might even fear that the very employment of the notion of a propositional model will reopen Pandora's box,<sup>98</sup> regardless of whether we decide to base our picture of theory structure upon it.

Such fears seem unfounded on reflection. The crucial feature of a proposition is that it is expressible by more than one sentence, and by sentences in more than one language, so that there is no danger here of identifying a model, or a theory, with a formulation of that model or theory in any particular language (cf. van Fraassen, 1987, p. 109); thus there is also no apparent threat that this approach to models and theories will individuate them in a way which is at odds with scientific practice (cf. Suppe, 1989, p. 4).<sup>99</sup> Similarly, there is no assumption built into the propositional approach that the non-logical terms involved in a linguistic formulation of a propositional model will be divisible into the "observational" and "theoretical," or even, for that matter, that there will be a clear distinction between the logical and the non-logical terms; *a fortiori*, employment of the notion of a propositional model does not commit us to solving problems associated with empiricist programs in semantics about the interpretation of some especially problematic class of theoretical terms. Thus we are not automatically cast back into the teeth of old difficulties concerning partial interpretation and

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<sup>98</sup> If I might be permitted to miss the point of the myth almost entirely.

<sup>99</sup> One proviso is called for here: In order to individuate models in a way which conforms to ordinary scientific ways of talking, we will need to think of propositional models as deductively closed. Otherwise, we would be forced into thinking of what are ordinarily called different presentations of the same model as presentations of different models.

To see this, assume, for *reductio*, that the model presented by a given presentation is just the set of propositions expressed by the sentences which make up the presentation. Then trivial matters of punctuation can make all the difference. Two presentations which we would intuitively take to be presentations of the same model might differ only in that one replaces the single sentence ' $\phi$ , and  $\psi$ ' of the other with ' $\phi$ ' and ' $\psi$ ' as two separate sentences. Then the model presented in one case will contain the proposition that  $\phi$  and  $\psi$ , but not the proposition that  $\phi$  nor the proposition that  $\psi$ ; the model presented in the other case will have things the other way around. Thus, by the usual criteria of set identity, they will be different propositional models.

Less trivial versions of this difficulty are not hard to find. Consider, for example, as mentioned in n. 67, that Bohr's original presentation of his model of the hydrogen atom involves the postulate that the kinetic energy of the electron is constrained to the range of values  $\frac{n}{2} \hbar f$  ( $n = 1, 2, 3, \dots$ ), where  $f$  is the electron's orbital frequency. This postulate, it turns out, is equivalent to the more familiar  $L = n\hbar$  in the context of the other assumptions of the model. Identifying the model presented with the deductive closure of the set of propositions expressed by the sentences making up a given presentation clearly avoids difficulties of this sort.

correspondence rules (cf., e.g., Putnam 1962, Achinstein 1968, and Suppe 1972). Use of the notion of a propositional model, and consideration of the propositional variant of the semantic view, does not even commit us to the idea that an interesting distinction can be drawn between observable and unobservable entities, properties, events, or states of affairs.<sup>100</sup>

There is one other oft-cited reason for preferring the semantic to the syntactic view of scientific theories, and it is that the semantic approach keeps us closer to scientific practice. Whether that is a good thing may well depend on the specific task at hand, and it will almost certainly depend more generally on one's conception of the relationship between philosophy of science and science itself. In any case, though, we have already seen that there are reasons to doubt whether a version of the semantic view which regards theories as collections of mathematical models can provide us with analyses of relatively nonmathematical theories in terms which are recognizable to us from the scientific context—consider any biological theory which involves the nuclear model of the cell. Indeed, it even seems plausible to claim that the propositional approach is truer to physics as practiced in the case of the Bohr model of the hydrogen atom, and so the same will go for any theory of which the Bohr model is deemed a part—the “old quantum theory,” perhaps.

Of course, if we think of models as sets of propositions (and, perhaps, of theories as collections of such models), then when our attention turns to questions of confirmation, explanation, testing, and so on, we may well want to study the logical structure of models, and of the propositions which make them up; at that point, we may turn to the formal languages which have been developed with such purposes in mind, and before long we could be representing models with sets of sentences in some formal language. The merest threat of such an eventuality may be enough to convince some that the propositional approach to models is a step in the wrong direction (namely,

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<sup>100</sup> Of course, we are also not prevented from taking on such commitments independently, or making such additional assumptions, should we wish to.

backwards). But this is unfair to the propositional notion of model, for the simple reason that the features of the situation which might lead us to bring the tools of formal logic to bear are not peculiar to the propositional approach. If we think of models exclusively as mathematical structures, and as theories as collections of such structures, then talk of confirming, testing, and explaining with models will presumably make sense only insofar as we can talk of confirming, testing, and explaining with propositions (“hypotheses”) to the effect that this or that mathematical structure represents such-and-such a system in certain respects, and with a certain degree of accuracy.<sup>101</sup> Thinking about how propositions of that sort stand in various logical, evidential, and explanatory relations to various other propositions seems just as likely to deliver us into the hands of the logicians as the study of confirmation, explanation, and prediction would be on the propositional approach.

*3.5 Propositional models and idealization.* There is a feature of much scientific theorizing which is of considerable moment for epistemology and methodology, and I would like to close this examination of the propositional approach to models by considering how that approach might accommodate the phenomenon in question. Specifically, it is common practice across the range of scientific disciplines to construct and employ models which avowedly involve idealizations of one form or another. Does this fact pose any problems for thinking of the models in question as sets of propositions?

Let us distinguish two particular sorts of idealization. First, there are models of kinds of system which go uninstantiated in the world around us, but which are some sense ideal. There are models of “ideal pendula,” pendula whose are bobs attached to massless rods, and which experience neither friction nor air resistance; models of electrons in infinite potential wells; models of economic systems involving perfectly (or “ideally”) rational and all-knowing agents; biological models of populations with

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<sup>101</sup> These are essentially Giere’s “theoretical hypotheses,” although (again) I would claim that his notion of model is a distinct one.

endless resources at their disposal. Secondly, there are models of instantiated kinds, or even of particular real systems, which present an idealized picture of those kinds or systems; there are idealized models of hydrogen atoms, cells, and (in case you regard as contentious the claim that those kinds are instantiated) of the solar system.<sup>102</sup> It may not always be clear which sort of model is being presented on a given occasion—whether, for example, a certain passage in a textbook should be thought of as presenting a model of the ideal pendulum, or an idealized model of real pendula, so to speak—but hopefully the distinction is clear enough for present purposes.<sup>103</sup>

Consider first an idealized model of a particular real system, or of an instantiated kind. Is there any special problem in thinking of such a model as a set of propositions? The answer to this question may depend, in part, on what sort of claims we take such a model to make, and on whether we take it to make claims at all. To see this, consider three (possibly) different ways we might understand the utterances of one presenting such a model: (i) as making simple declarative statements about the actual features, composition, and behaviour of the system or systems modelled; (ii) as expressing counterfactuals about the system; (iii) as constituting fictional discourse—that is, as semantically and pragmatically of a piece with or analogous to the utterances an author

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<sup>102</sup> Clearly, no general characterization of idealization is being offered here; instead, I am presupposing a rough and ready understanding of the notion. See Jones (2005) for further discussion.

<sup>103</sup> There is perhaps a hint of difficulty for the propositional approach here. A particular set of propositions will presumably always fall into one category or the other (if either), so that in the case of ambiguous presentations we either have to find a way of making it plausible that some additional factor external to the presentation singles out the model being presented, or accept the consequence that there is no determinate fact about what model is being presented. The mathematical model approach perhaps has the resources to deal with the phenomenon of ambiguous presentations rather more naturally, by suggesting that it is determinate that a certain mathematical structure (or class of structures) is being presented, but that the precise representational purposes to which that structure (or model) is intended to be put is being left open, within certain limits.

This observation serves to draw attention to another potential difficulty for the propositional approach, one which arises if it is ever correct to say that one and the same model can be or is used to represent different systems (or even kinds of systems) on different occasions. If a set of propositions is a representation of (i.e., is a set of propositions about) a certain system or kind of system on one occasion, then it is a representation of that system (or kind) on every occasion. Indeed, one cannot ever speak very comfortably of “using” a set of propositions to represent a certain system, as the propositions, by their very intended nature, do the job all on their own. Thus, in this respect at least, the propositional approach may have a problem in capturing the way we ordinarily individuate models. And this problem (if it is a problem) springs from one of the very features of propositions we celebrated earlier, namely, that they bear their representational content essentially.

of a work of fiction produces when telling her story.<sup>104</sup> (On some approaches to fictional discourse these three ways might collapse into two, in either of two ways. Note also that I am assuming that it is possible to engage in something like fictional storytelling (as opposed to mere lying) about real systems.) In the first case, at least some of the statements made (e.g., that the electron orbits in a hydrogen atom are circular) will be false, or this would not count as an idealized model; in the second case, if statements are made (e.g., that if the electron orbits had been circular, then the energy level would have been so-and-so), they may all be true. What one says about the third case in this regard will, of course, depend on one's views about fictional discourse.

On the first reading, I take it, idealized models present no special problems for the propositional approach, provided that one's accompanying account of propositions meets the rather minimal requirement that there be false propositions.<sup>105</sup> The model will simply be the deductive closure<sup>106</sup> of the set of propositions expressed by utterances making up a presentation of the model. Difficulties arise only if the second or third approach to idealized models is the right one (if these are different), and if counterfactuals or the utterances in a work of fiction, respectively, should not be thought of expressing propositions. In that case we will not easily be able to regard presentations of idealized models as presentations of sets of propositions, and this would surely be at least *prima facie* reason for rejecting the proposal that we think of idealized models as sets of that sort. Whether such a problem in fact arises for the propositional approach is not something I shall endeavour to settle here.

The question of whether we can treat models of uninstantiated but ideal kinds of systems as sets of propositions involves one or two additional complications, but in essence the threat of a problem derives from the same source. The threat may be greater, however, for the simple reason that it is more difficult to read the utterances involved in

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<sup>104</sup> For a closely related discussion, and an investigation of the idea of treating certain kinds of modelling discourse as fictional discourse, see Thomson-Jones (2007).

<sup>105</sup> Minimal or not, this requirement has been known to cause problems.

<sup>106</sup> See n. 99.

presenting a model of this sort as making straightforward declarative statements concerning only actual states of affairs. The utterance ‘The ideal pendulum has a period which is proportional to the square root of its length’ should surely not be read as having the logical form given by  $(\forall x)(Px \rightarrow Lx)$  (nor the one given by  $(\exists x)(Px \wedge (\forall y)(Py \rightarrow y = x) \wedge Lx)$ ), as it is not something which we take to be true (or false) simply in virtue of the nonexistence of ideal pendula.<sup>107</sup> We might say that the utterance in question is a law-statement, and that law-statements do not have the  $(\forall x)(\phi x \rightarrow \psi x)$  logical form, but then the possibility of giving the utterance a reading on which it does not express a proposition looms once more.<sup>108</sup> Still, it must be remembered that a problem only arises in this way if it is wrong to think of counterfactuals, the utterances involved in fictional discourse, or law statements as expressing propositions.<sup>109</sup>

Incidentally, one point which this reflection on idealization serves to drive home is that it is possible, and indeed important, to distinguish between the things a model

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<sup>107</sup> For discussion of Giere’s view that the ideal pendulum is an actually existing abstract object, see Thomson-Jones (2010), section 3.1. Nor does it seem promising to regard the phrase ‘The ideal pendulum’ as a non-denoting singular term. Note, by the way, that some of the utterances which make up the presentation of a given model may be best regarded as definitional, and thus as either trivially true or truth-valueless. ‘The ideal pendulum experiences no air resistance’ seems a likely candidate for such a treatment. See Achinstein (1968) for an extensive discussion of the varieties and roles of definitions in scientific theorizing.

<sup>108</sup> I am thinking of Ryle’s (1949) “inference ticket” view of law statements, although I am not sure whether one should think of that view as committed to the claim that law statements do not express propositions. On the one hand Ryle says that law statements do not, at least in one sense, state facts; on the other hand, they do have truth values (*ibid.*, pp. 120-121).

There is, of course, a connection here to the view that counterfactuals do not express propositions, as those who reject the claim that  $(\forall x)(\phi x \rightarrow \psi x)$  gives us the logical form of laws (even those which, in English, are expressed by sentences of the form ‘All  $\phi$ ’s are  $\psi$ ’s’) often stress the ability of laws to underwrite counterfactuals.

<sup>109</sup> A different way in which problems might arise in the case of models of uninstantiated ideal kinds (and perhaps in the case of idealized models of instantiated kinds or particular systems) involves a number of complex issues concerning logical form, reference, the metaphysics of kinds, and the nature of propositions. The difficulty I have in mind is this: On certain theories about propositions, Napoleon (the man himself) is a constituent of the (tenseless) proposition that Napoleon is short. Such theories thus have the consequence that the proposition in question would not have existed had Napoleon himself failed to come into being. Now suppose that one endorses such a view of propositions, and extends this feature of the view to the propositions expressed by sentences involving natural kind terms. Suppose, for example, that one takes it that the proposition that gold is expensive has gold, the kind, as a constituent. (Such a view seems at least in keeping with a “direct reference” approach to natural kind terms.) And suppose that kinds are the sorts of things which exist only if instantiated. Then the proposition that gold is expensive might have failed to exist. The troubling question which now arises, by the same token, is how there can *actually* be propositions about uninstantiated kinds. If there cannot, then idealized models which represent the properties and behaviour of actually uninstantiated kinds cannot be sets of propositions about those kinds. Clearly, this is a difficulty for the propositional approach which arises only if one combines it with a specific set of views on a number of abstruse and controversial issues. Still, it is something to be wary of. And note that, if there *is* a problem here, it is a problem the approach has not only with models which wittingly idealize, but with any model which turns out to deal in uninstantiated kinds, such as phlogiston models of combustion, or to speak of nonexistent individuals, such as electromagnetic models involving the ether. (Cf. n. 90.)

says about a given system or kind, and the things that we who employ the model assert about the same system.<sup>110</sup> This distinction is apparent when we consider the fact that many idealizations are quite wilful. We are often happy to employ an idealized model in the firm belief, and perhaps even in the full knowledge, that some of the things it says about the modelled system are false; we surely do not thereby commit ourselves to the (putative) falsehoods in question. I do not see any special difficulty for the propositional approach to models here. To adopt a locution of van Fraassen's, there is no reason we cannot "display" a set of propositions, implicitly claiming for it as representation something less (or other) than complete veridicality—approximate truth, for example, or predictive accuracy in certain respects.<sup>111</sup>

### Conclusion

I have presented a taxonomy of some distinct notions of model which are at work in various places in current philosophy of science, and then argued that one of these, the notion of a mathematical model, is the notion which we should see as central to the semantic view of theory structure. I have also argued, however, that the notion of a propositional model is better equipped for providing an account of a primary scientific notion of model. Should these arguments fail to convince, my hope is that the taxonomy which begins this paper has made it easier to grapple with the issues in question, and will prove of similar use in tackling other issues in the philosophy of science where talk of models plays a central role.

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<sup>110</sup> I do not mean to suggest here that a model can represent a system as being a certain way without our help. The point is rather that we may use a model to present a representation, so to speak, without ourselves asserting that things are the way the representation in question pictures them to be.

<sup>111</sup> See Gideon Rosen's helpful discussion of what he calls "quasi-assertion" (1994, pp. 149-151).

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