

CHAPTER ONE

THE NOTION OF "MODEL" IN PHILOSOPHY OF SCIENCE

1. THE INTERPRETATION AND USE OF THE NOTION OF "MODEL" IN PHILOSOPHY OF SCIENCE

One intuitive idea of a model is a possible interpretation in which a theory is satisfied in the Tarskian sense, that is, according to Tarski (1956) a model of a sentence in some appropriate language is a possible interpretation of the language in which the sentence of the language are satisfied. This is the basis of model-theoretic analyses of scientific theories, i.e. a model of a theory is a possible interpretation in which all sentences of the theory are satisfied (i.e., in which the sentences are "true"). Model theory was initially developed for explicitly constructed formal languages with the purpose of studying certain mathematical issues, until Evert Beth and others initiated the application of model theory to semantic analyses of so-called "empirical" scientific theories.¹

Suppes (1960, p.290) points out that the most important distinguishing feature of the relation between a theory and a model of a theory is that a theory is a linguistic entity consisting of a set of sentences, and models are non-linguistic entities in which the theory is satisfied. The notion of "model" is used in widely different ways by philosophers of science and scientists alike. However, if one examines these various interpretations and uses a little more closely, it is possible to find common features in the ways in which the notion of a model is applied that show the advantages of this notion in terms of clarifying the intricacies of the processes and products of science.

Suppes (1960, pp.289,290) remarks that the concept of "model" in the sense of Tarski may be used without distortion and as a fundamental concept in any discipline. In this sense he claims that the meaning of the concept of model in mathematics is the same as in — or very close to that of — the empirical sciences, and finds the difference rather in the various uses of the concept, in the sense that "mathematicians ask a certain kind of question about models and empirical scientists tend to ask another kind of question" (ibid.)². Added to this should be that rather than obscuring the role of models in rational reconstructions of science, these different kinds of "questions" that may be asked about models merely illustrate and strengthen the validity of using the notion of "model" in these kinds of rational reconstruction of scientific theories.

Suppes (in Morgenbesser, 1967, p.57) seems to make much the same point, although perhaps with certain reservations, since he remarks that

... quite apart from questions about direct empirical observations, it is pertinent and natural from a logical standpoint to talk about the models of a theory ... [since these] models are highly abstract, non-linguistic entities, often quite remote in their conception from empirical observations.

Therefore, he acknowledges the apparent logical problem underlying questions concerning that which the concept of a model actually has to add to the usual discussions of empirical interpretations of theories. He comments (*ibid.*) that mostly philosophers find it easier to talk about theories than about models of theories, because mostly, their examples are simple in nature and can be discussed in purely linguistic (first-order logic) terms, while introducing the notion of the model(s) of a theory adds a higher mathematical element to the discussion.

However he (in Morgenbesser, 1967, p.58) points out that when dealing with more complicated theories, like quantum mechanics, and classical thermodynamics, we are not always involved with "simple" examples at all. In these cases he claims (*ibid.*) we also need the general results of set theory as well as many results concerning the real numbers, since direct formalisation of such theories in first-order logic is rather impractical, given that the degree of complexity of theories of this sort is similar to that of theories studied in pure mathematics. He writes (*ibid.*):

In such contexts it is very much simpler to assert things about models of the theory rather than talk directly and explicitly about the sentences of the theory, perhaps the main reason for this being that the notion of a sentence of the theory is not well-defined when the theory is not given in standard formalisation.

Also, some calculations are simply too complex to do in any other context than in that of some model of the theory in question — think for example of a leaf blowing in the wind. To predict where it will come to rest, i.e. to describe its movement, it is possible to use Newton's laws of motion and his law of gravitation³. However, the actual calculations would be really complex, and therefore it would be more practical to work in some model of his (i.e. Newton's) "theory" that focuses only on certain of the aspects of the relevant real system.

2. HEURISTIC USES OF THE NOTION OF "MODEL"

Redhead (1980, p.146) comments that early depictions of "theoretical models", such as Achinstein's (1968), almost always implied these models to be false. The reason for this is that mostly, at that stage in philosophy of science⁴, models were used in the heuristic sense of suggesting new ways to look at certain problems. Thus the notion of "theoretical model" was generally explained in terms of a set of assumptions about some system, attributing some kind of inner structure to the system, and thus being a "simplified approximation" (Redhead, 1980, p.146) of the system modelled. Also, these models were used — and are of course, still used for these reasons — to justify the production of predictive theories — i.e. theories formulated to explain one group of phenomena (one kind of real system) which then prove to have the ability to predict events concerning the same or another type of system.

In other words, after the positivist reign, most of the initial philosophical reflection concerning models was concerned with the role of models within some kind

of "logic of analogy". In these cases it is obvious that models will turn out to be "false" of the system in reality on which the development of the theory in question is focussed, since they are not interpreted in terms of mathematical models (and the empirical reducts) of the theories concerned. Therefore the former usage of the notion of model should not be confused with the latter Tarskian interpretation of the notion in the sense of models "making" the theories they interpret "true".⁵

A related way in which the notion of "model" has also entered into philosophy of science debates is via the well-known use of "iconic" models by Hesse (1963), Achinstein (1968), and Redhead (1980). Both Nagel (1961) and Hesse (1963) view the empirical interpretation of a scientific theory in terms of mathematical and iconic models. Achinstein (1968) defines an iconic model as a set of simplified and approximate assumptions about some system. These assumptions attribute a certain inner structure to the system and are proposed within the context of some more "basic" theory. Hesse (1963, p.19) depicts these models as also possibly exhibiting analogies between the system modelled and some other system. Da Costa and French (1990, p.258) explain these notions in terms of the example of the billiard ball model of kinetic gas theory. Viewed as an iconic model, it offers an image or representation (picture) of a system of gas atoms. There also exists positive analogy in Hesse's terms since there is a similarity between certain aspects of the system itself and certain aspects of the model. The billiard ball model is, however, also a mathematical model of the kinetic theory of gases, in the sense that a semantic interpretation is given to the theory in terms of the billiard ball system such that the axioms of the theory are true under that interpretation.⁶

The main problem with this kind of approach is, as Da Costa and French (1990, p.258) point out, a realist one. The mathematical model is supposed to somehow say something about how the world is (offer an empirical interpretation of the theory), while the same is not expected from an iconic model. Van Fraassen, for instance, ignores these kinds of problem by arguing that scientific theories are indeed nothing more but iconic models of phenomena (observable objects). Da Costa and French (1990, p.259) go on to claim that on the "realist view" it seems that the difference between a theory and models of the theory is that the theory may be true and its models can only be false. This is the same kind of explication that Redhead (1980) offers. The problem is that these kinds of approach view the role of models not semantically, but purely in instrumental terms, that is, in terms of clarifying the axioms of the theory, or extending the domain of a theory, or perhaps even in limiting the domain of theory.

A model-theoretic realist, however, I claim, understands that the role of models in the scientific process is far more important than that. Such a philosopher of science acknowledges that the use of the notion of model in the Tarskian sense as a mathematical interpreting structure is by far the most important one as far as discovering possible links with reality is concerned, and thus such a philosopher only speaks of theories being true in particular models, and not of either theories or models being true or false *per se*, in the literal absolute sense of the word.⁷ Do not take me wrong, the powerful role of these kinds of (iconic) models and also of models as "approximations" to theories — Redhead's (1980, p.147) "impoverishments" of theories⁸ — in elaborating the domains of scientific theories, cannot be ignored.⁹ However, it should be noted that

this is a different use of the term than that of the term as a mathematical model, and also that mathematical models can indeed also be used with these kinds of aim in mind.

Redhead (1980, p.162) makes an interesting remark about the role of models in science, that reminds much of Cartwright's (1983) kind of approach. He (1980, p.162) writes

If we always tried to solve every problem with absolute accuracy, and neglecting no 'accidental' aspects, science would never get started. Science depends on the possibility of ignoring accidents, of isolating certain key features in a situation. These are captured by models, although in the very act of idealisation or approximation we convince ourselves that the model is indeed false. It is moreover in respect of modelling that the imaginative and intuitive element in theoretical physics is most clearly seen ... Modelling is certainly an art, involving a number of logical gaps

Of course mathematical models are also idealisations of some real system. The problem that I have with this kind of remark is however that the interpretative models of some theory, despite their potentially idealised nature, may still be found to be related to some real system(s). I argue that it is not "truth" in terms of so-called "false" (or "distorting") idealisations of real systems that philosophers of science should be concerned with at all, but that rather they should concentrate on the Tarskian notion of truth in terms of satisfaction. This kind of (satisfaction) relation establishes (semantic) links between some theory and the models of the theory. It also may determine relations of empirical adequacy in terms of a modified version of Tarskian satisfaction established by (possible) links between the models of the theory and some system(s) in reality. See Chapters 2,3, 4, and 5 for more on this issue.

Redhead (1980, p.147) also discusses the notion of "enriching models". This kind of model is more interesting for my purposes, since it seems to play a role very close to that which I ascribe to the "initial" or "intended conceptual" models in my analysis of the process of science.¹⁰ Redhead claims (ibid.) that these enriching models may come into scientific play whenever a theory seems to be "incompletely specified" such that "considerable latitude" is allowed in the selection of the detailed structure of the theory. He (ibid.) offers the example of axiomatic field theory where any number of fields may possibly satisfy the axioms of the theory, and claims that an "enriching model" may then be introduced to fill in "missing detail". Obviously here the model is not believed to be contradicting the theory, since no "completed" theory has yet been formulated. However, as soon as the model "resembles an exact theory" it is called a "theory" and not a "model" any longer.¹¹

Another more heuristically related use of the notion of "model" is the application of the notion in the construction of Gedanken experiments — usually done in the empirical sciences. Here, again, I see this as mainly done in the beginning stage of theory development, except when, as Suppes (1960, p.296) points out, the notion of model is used in arguments against the general plausibility of a theory. In these cases the theory is (conceptually) extended to a new domain (where the scientists expect the results in the new domain to be different from those predicted by the theory) by constructing a model of the theory in that domain. I agree with Suppes that this aspect of the use of models need not however be restricted to Gedanken experiments.¹²

Giere (1991, pp.23-27) also discusses the various kinds of model that scientists use through the course of the scientific process. He identifies four different kinds of model, but I shall here discuss only the two more heuristic ones: Scale models are physical models built to scale and are rarely used, although they may play important roles in the formulation of theories, for example the model of DNA, built by Watson and Crick. Then, secondly, analog models are most useful in the beginning stages of scientific research, since they are usually discarded after theory formulation. He (Giere, 1991, pp.23-24) cites the familiar example of the solar system as an analog model for an atom. In this model the electrons and the nucleus of an atom are said to be analogous to the planets circling the sun. (The idea being that if an atom could be magnified by whatever measure required, one would have an observable object with a structure similar to that of the solar system correspondingly reduced.) This model was very fruitful to scientists working in the first half of the century, especially in the ways in which it showed atoms *not* to be analogous to the solar system, the general point being that "a good analogy leads to its own demise" (Giere, 1991, p.24).

3. STATEMENT AND NON-STATEMENT VARIATIONS OF THE NOTION OF "MODEL"

Wójcicki (1979, p.158) sums up the different usages of the notion of model that philosophers of science should keep in mind as follows:

... while the semantic concept of a model is in common use among logicians and mathematicians, empirical scientists almost unanimously opt for the 'mathematical' meanings of 'model' in its *syntactic* (a set of equations) or *semantic* (a mathematical entity which is to represent an intuitive concept) sense.

The interpretation of the notion of a model in the Tarskian sense does not really play a big role in statement depictions of the nature of scientific theories or the process of science. For instance, as an example of a model, Nagel (1961, p.94) offers Bohr's theory of the atom and comments that "... the *theoretical* notion of an electron jump is linked to the *experimental* notion of a spectral line". Nagel claims that more than merely being an example of correspondence rules, the above example also illustrates what he means by a model or interpretation of a theory. He (*ibid.*, p.95) writes:

The Bohr theory is usually not presented as an abstract set of postulates, augmented by an appropriate number of rules of correspondence for the uninterpreted nonlogical terms implicitly defined by the postulates. It is customarily expounded, as in the above sketch, by way of relatively familiar notions, so that instead of being statement-forms the postulates of the theory appear to be statements, at least part of whose content can be usually imagined. ... in such an exposition the postulates of the theory are embedded in a model or interpretation. It should nevertheless be clear, despite the use of a model for stating a theory, that the fundamental assumptions of the theory provide only implicit definitions for the theoretical notions employed in them. ... Moreover, the presentation of a theory by way of a model does not make any less imperative the need for rules of correspondence for linking the theory to experimental concepts. Although models for theories have important functions in scientific imagery ... models are not substitutes for rules of correspondence.

He claims (*ibid.*, pp.96,97) that all expressions that form part of the formulation of models can be called "meaningful", and so a theory supported, as it were, by a model

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