# MODELS IN THE POSITIVIST TRADITION

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A great deal has been written on models in the last one hundred years. Much of what has been written, it would seem, is concerned with "saving the concept from triviality."[1] This is the case because the most strongly entrenched position on the structure of theories portrays models in such a light as to render them 'useful fictions', 'temporary psychological aids',[2] 'disreputable understudies for mathematical formulas'[3] or 'props for feeble minds'[4] who can't take the equations 'straight'.[5] This has caused philosophers, such as Max Black, to ask the question:

> Should we think of the use of models as belonging to psychology - like doodles in a margin - or as having its proper place in the logic of scientific investigation?[6]

It is my contention that this problem of the status of models has arisen from a mistaken interpretation of science in general, that may be loosely characterized as the 'positivistic tradition'. This tradition is deep rooted, going back to Bacon, Hume, Comte, and J.S. Mill - becoming more explicit with Hertz and Mach - and reaching to its height with the members of the 'Vienna Circle', in the early part of the 20th century. In this paper, I will attempt to sketch the position of the 'positivist tradition', along with certain basic assumptions regarding theories and models. I will then briefly critique these assumptions, while presenting a more realistic view of models.

<u>The Position</u>: A PHYSICAL THEORY IS TO BE ANALYZED AS AN EMPIRICALLY INTERPRETED HYPOTHETICO-DEDUCTIVE SYSTEM OR FORMAL CALCULUS. This position is rife throughout the works of (the early) Rudolf Carnap, Carl Hempel, Ernest Nagel, E.H. Hutten, R.B. Braithwaite.[7] It was force-

fully put by Pierre Duhem, 1914, thus:

> A physical theory is not an explanation. It is a system of mathematical propositions, deduced from a small number of principles, the aim of which is to represent as simply, as completely, and as exactly as possible a group of experimental laws.[8]

This position requires the following fundamental distinction to be made, expressed by Carnap thus:

> In discussion on the methodology of science, it is customary and useful to divide the language of science into two parts, the observation language and the theoretical language. The observation language uses terms designating observable properties and relations for the descriptions of observable things or events. The theoretical language, on the other hand, contains terms which may refer to unobservable aspects or features of events, e.g., to microparticles like electrons or atoms, to the electromagnetic field or the gravitational field in physics, to drives and potentials of various kinds in psychology, etc. [9]

This fundamental assumption was extremely attractive because it claims to present a clear and convenient schema for analyzing terms, laws, theories, and their relationship to each other and the world.

The next claim usually made is that it is necessary to distinguish between 'The Three Major Components in Theories', the calculus, the semantical rules, and the empirical interpretation.[10]

The <u>calculus</u> is the 'logical skeleton' or mathematical formalism devoid of any empirical meaning. This syntactical system is usually characterized as being (a) a set of primitive formulas (for Carnap and Quine, <u>sentences</u>) which are taken as the postulates, and (b) other formulas which are obtained by derivation from the postulates, in accordance with specified rules of transformation. According to Marshall Spector,

> When this calculus, or syntatical system, is given an empirical interpretation, or meaning, it becomes a system of empirical statements having the structure of a hypothetico-deductive system. The primitive formulas become empirical

> hypotheses; and the derived formulas become empirical statements which will be true if the hypotheses are true.[11]

This empirical interpretation is given to the calculus by semantical rules (rules formed in a suitable meta-language, usually 'ordinary' English or German) which provide the meaning of the terms by stating what properties, relations, or individuals the terms designate. An example would be 'the term "P" of the calculus designates the pressure of a sample gas.' According to Carnap,

> We have formulated the semantical rules of the descriptive signs by stating their designata, for the logical signs by stating truth conditions for the sentences constructed with their help. And therefore we shall say that we understand a language system or a sign, or an expression, or a sentence in a language system, if we know the semantical rules of the system. We also say that the semantical rules give an interpretation of the language system.[12]

It is maintained further that (a) not all of the terms of the calculus of a theory need be given semantical rules, and (b) not all the terms can be given semantical rules. For the positivist, only such terms which represent the observational terms will be given semantical rules, never the theoretical terms (theoretical = unobservable) which cannot be 'understood in themselves', but must be understood or given their meaning in an indirect manner, through the role they play in the theory. Theoretical terms obtain their 'indirect' or 'partial' interpretation if and only if they appear in sentences in the calculus which are given semantical rules: the observation terms. Such sentences are known as correspondence rules.

For example, the sentence 'All gases are composed of molecules' can be transposed thus: '(x) (Gx⊃Qx)'. The term 'G' is given the semantical rule ' "G" ', which designates the property of being a sample gas. The term 'Q' (symbolic for 'is composed of molecules') is a theoretical expression which is not given a semantical rule but is said to obtain 'partial' meaning by virtue of its occurence in a sentence which contains 'G', whose meaning is directly given by a semantical rule.

Thus far we have examined the distinction of observational terms vs. theoretical terms and how, through their relation in an uninterpreted calculus, unobservables came to have 'partial' meaning, via the empirically interpreted observation terms which, of course, reduce to 'sense-data' or perhaps stimulus meaning'.[13] We shall next consider how the concept of 'model' enters this framework, but first we must distinguish between the various usages of the word.

The word 'model' can be roughly defined as a symbolic representation of selected aspects of the behavior of a complex system for a particular purpose. Four types are generally distinguished: (1) experimental, (2) logical, (3) mathematical (4) theoretical. (A fifth, computer models, are noted by some (e.g. Barbour) but do not have relevence here.)

(1) An experimental model, or 'scale' or 'working' model, refers to something actually constructed in order to test certain hypotheses or derive quantitative data related to some system.

(2) A logical model consists of a particular set of entities that satisfy the axioms and theorems of a formal deductive system. An example of a logical model would be a set of points and planes in Euclidean geometry.

(3) A mathematical model is sometimes said to lie between the first two types of models, and has been defined as a 'symbolic representation of quantitative variables in physical or social systems' used chiefly to predict behavior.[14] It is interesting here to consider the manner in which two proponents of what is being characterized as the 'positivistic tradition' view mathematical models and physics.

E.H. Hutten holds the view that

> There are, however, no mathematical models in physics: the equation by itself is not the model....It is the inter-

pretation which is attached to the equation due to previous application that we need for describing our experiments.[15]

According to P. Bridgmann,

Formerly mathematical theories usually had in the background a physical model of some sort, as, for example, the kinetic theory of gases had in the background a model consisting of idealized molecules....The molecules were so simplified that they were amenable to mathematical treatment. We had then a sort of double theory - a mathematical theory of the idealized model, and then a physical theory consisting of the statement that there was a correspondence between the idealized model and the actual physical system sufficiently close so that certain properties of the physical system were reproduced by the model. The point in making such an idealized physical model was that it had a mathematical theory simple enough to be handled.

It presently appeared to reflection, however, that there was an unnecessary step here - since all that could be done in any event was to set up certain correspondences between the results of the mathematical manipulations and the physical system, why have the intermediate step of the idealized physical model, since correspondence to a correspondence is also a correspondence? This of course is exactly what has been done in recent wave and mechanical theories, particularly that of Dirac, and we know that such a procedure has been brilliantly successful. I think that the reason that this change of the attitude was so long deffered was that it was not realized that there was an intermediate step-...the model was actually identified with the physical system.

What we now have is in effect mathematical models rather than physical models...[16]

If we make a comparison between Bridgmann's relatively extreme position and that of Hutten, in his account The Language of Modern Physics, we will find that the difference is not as great as might be expected.

Physical theory developed by gradually overcoming the limitations of the original model; but this does not mean... that the original model is completely abandoned....What remains is the wave equation, together with a minimum interpretation in terms of experience upon which the application of the equation rests. It is like the grin and the Cheshire Cat: the picture of the cat has receded into the background, but knowing that there was once a cat we can understand that the residual phenomenon may be interpreted as a grin.[17]

It is just these dynamics involving theoretical models and the formal calculi, theories, etc., in the positivist tradition that we wish to explore and critique.

The classical picture of the theoretical model in this tradition is represented by R.B. Braithwaite in his Scientific Explanation.

> If we have before us two deductive systems which are each interpretations of the same calculus, in the first of which the interpretation of the initial formulae containing the theoretical terms is epistemologically prior to that of the derived formulae not containing these theoretical terms, whereas in the second interpretation the reverse is the case, the derived formulae being the epistemologically prior, the first deductive system will be said to be related to the second deductive system as MODEL is to THEORY. The first deductive system will be said to be a model for the theory which is the second deductive system, and the second deductive system a theory for the which the first deductive system is a model. A theory and a model for it, or a model and theory for which it is a model, have the same formal structure, since theory and model are each represented by the same calculus. There is a one-one correlation between the propositions of the theory and those of the model; propositions which are logical consequences of propositions of the theory have correlates in the model which are logical consequences of the correlates in the model of these latter propositions in the theory and vice versa.[18]

There are three assumptions regarding the nature of theoretical models that stem from the positivist view of theories. We shall first' outline them more explicitly, then commence our critique.

Assumption I: A MODEL IS DESIGNED TO PRODUCE AN INTERPRETATION FOR AN OTHERWISE UNINTERPRETED FORMALISM OR CALCULUS.

Nagel speaks of a model as "an interpretation...for the abstract calculus which supplies some flesh for the skeletal structure in terms of more or less familiar conceptual or visualizable materials."[19]

E.H. Hutten states that "the model gives a possible interpretation to the symbols (in an equation or formula) which thereby aquire a meaning, and we can then apply the equation or formula and test it."[20]

And Braithwaite succintly put it, "an alternative and equivalent explication of model for a theory can be given by saying that a model is another interpretation of the theory's calculus."[21]

Assumption II: A MODEL IS ALWAYS PROPOSED WITH REFERENCE TO SOME

THEORY, WHERE THE MODEL HAS THE SAME FORMAL STRUCTURE AS THE THEORY.

According to Max Black,

> ...The key to understanding the entire transaction (the relations between the 'described model' and the original domain) is the identity of structure that in favorable cases permits assertions made about the secondary domain to yield insight into the original field of interest.[22]

The formula is roughly, '<u>suspicion of isomorphism</u>, <u>imputation of structure</u>'. That is to say, "one suspects, on the basis of certain inconclusive clues, an isomorphism of structure between a rudimentary and a finished theory. One then imputes the structure of the latter to the former. If the suspicion was correct, one immediately gets analogues in the rudimentary theory of all the known deductions in the finished theory."[23]

This assumption is made throughout the literature on models, with very few exceptions. Nagel, for instance, defines a model for a theory T as a set of <u>true</u> propositions having the same formal structure or calculus as T.[24] Similarly, Braithwaite (op cit) states that

> A theory and a model for it have the same formal structure, since the theory and the model are represented by the same calculus.[25]

Assumption III: A MODEL HAS THE CHARACTERISTICS OF A FORMAL ANALOGY.

A formal analogy has been defined by Hesses as a "one-to-one correspondence between different interpretations of the same formal theory."[25] One of them main characteristics of the formal analogy is that, in addition to being isomorphic, it is <u>symmetrical</u>. This is to say that if $\frac{a}{b}::\frac{c}{d}$ (read 'as a is to be, c is to d') then $\frac{c}{d}::\frac{a}{b}$ -- without remainder. Thus a theory $T_1$ and a model for it $T_2$ are related by a formal analogy (symmetrically isomorphic), if there exists a one-to-one function (g) mapping the structure (i.e. relations and functions) of $T_1$ onto $T_2$ such that the inverse ($g^{-1}$) of the function maps the structure of $T_2$ onto $T_1$. Therefore we again have a case of one calculus with two interpretations. The analogy relation is one of identity, rather than similarity.

Before outlining a critique of the notion of a model in the 'positivist tradition', it should be noted that there is also serious dificulty entailed by the fundamental assumption of the observational/theoretical dichotomy in the first place. This dichotomy has been strongly critiqued (notably by P. Feyerabend and H. Putman). As this topic deserves special treatment in its own right, we will simply mention Feyerabend's conclusion here.

> The distinction between observational terms and theoretical terms is a pragmatic (psychological) distinction which has nothing to do with the logical status of the two kinds of terms. On the contrary,..the terms of a theory and the terms of an observational language used for the tests of that theory give rise to exactly the same logical (ontological) problems. There is no special 'problem of theoretical entities'.[26]

Feyerabend contends therefore that "logically speaking, all terms are 'theoretical'."[27]

If one accepts Feyerabend's conclusion, the concept of 'partial interpretation' or 'indirect meaning' loses all significance. If a little irony may be interjected by quoting Hertz, himself very much in line with this tradition: "When these painful contraditions are removed, our minds, no longer vexed, will cease to ask illegitimate questions."[28]

Critique:

Assumption I: A MODEL IS DESIGNED TO PRODUCE AN INTERPRETATION FOR AN OTHERWISE UNINTERPRETED FORMALISM OR CALCULUS.

This sort of interpretative role is not characteristic only of models - the same role is played by theories, many of which would not be classified as models. For example, quantum mechanics provides interpretation for certain terms and equations in thermodynamics but quantum theory is not usually called a model.

When a theoretical model is used to provide (or because it provides) an interpretation for equations, these equations will not be mere uninterpreted strings of symbols. The Bohr model of the atom, for instance, can be used to assign meaning to the term R appearing in Balmer's equations, describing a relationship among the wavelengths of the lines in the Hydrogen spectrum. Yet the term R has already been given meaning independently of the Bohr model, by reference to the ideal gas law.

Despite the fact that a theoretical model is sometimes used to supply (or because it supplies) new interpretations for terms in certain equations, it would be extremely misleading to consider this to be one of the principle reasons for its use. The Bohr model was not used because it supplied interpretations for terms such as 'elliptical orbit', 'mass', 'charge', etc. Rather, it is a structure that employs previously interpreted terms to describe the relations between parts of the atom, analogous to a solar system.

Let us instead define a theoretical model as an imagined mechanism or process postulated by analogy with familiar mechanisms or processes and used to construct a theory to correlate a set of observations. To illustrate this, we may use the historical example of the model for the kinetic theory of gases. The model proposed, in this case, was that gases consisted of 'tiny elastic spheres'. This is often called the billiard ball model of gases. A familiar situation (billiard balls bouncing) is postulated by analogy, to correspond to the molecules of a gas. Pressure would then be analogous to the transfer of momentum when a 'sphere' collides with the wall of a containing vessel. This relation may be expressed thus: $\frac{\text{'tiny elastic sphere'}}{\text{bouncing}} :: \frac{\text{'gas molecules'}}{\text{pressure}}$, where the horizontal relation between terms is that of similarity, and the vertical relation causal.[29] With this type of model, it is possible to derive several laws of gases (e.g. Boyle's law that pressure is proportional to volume where the temperature is a constant). The model thus leads to a theory which can be tested by observations to see what, if any, of the postulated analogy applies.

Three types of analogy relation may be made apparent. First, the positive analogy, consisting in the observed similarities - e.g. 'elastic', 'hard', 'bouncing', etc.). Second, the negative analogy - that is - the relations that don't 'carry over' from the model to the theory ' e.g. the color of the billiard balls, the necessity of cues, etc.). And thirdly, the neutral analogy, consisting of those relations or properties that are not immediately apparent but require further investigation (e.g. do these 'elastic spheres' have attractive forces?). The above analogy relation is usually called a substantative or material analogy as distinguished from the formal analogy.

Assumption II: A MODEL IS ALWAYS PROPOSED WITH REFERENCE TO SOME THEORY, WHERE THE MODEL HAS THE SAME FORMAL STRUCTURE (SYMMETRICALLY ISOMORPHIC) AS THE THEORY.

According to this assumption, we obtain a model if, and only if, we interpret the calculus of the theory. Were this the case, since any set of propositions constitutes an interpretation of its own calculus, and if a new theory, $T_1$ can be constructed, only by a reinterpretation of an already formalized theory, $T_2$, then each and every existent theory would share the same formal structure or calculus. This obviously is not the case, but the positivist account provides no mechanism for the growth and extension of theories. In addition, many theoretical models, for example, that of the atom, are not interpretations of any known theory of the sort required.

Models and the phenomena they depict are non-symmetrically isomorphic.[30] There is a similarity of structure but not an identity. It is due to this very fact of having the properties of positive, negative, and neutral analogies (especially the latter) that allows for the growth of theories. Mary B. Hesse puts it thus:

> The theoretical model carries with it what has been called 'open texture' or 'surplus meaning', derived from the familiar system. The theoretical model conveys associations and implications that are not completely specifiable and may be transfered by analogy to the explanandum; further developments and modifications of the explanatory theory may be therefore suggested by the theoretical model. Because the theoretical model is richer than the explanandum, it imparts conceptual relations not present in the empirical relations alone.[31]

Assumption III: A MODEL HAS THE CHARACTERISTICS OF A FORMAL ANALOGY.

In a formal analgy, there is a one-to-one correspondence or mapping thus giving rise, in the 'positivist tradition', to identities. But this is surely not the case. The analogy relation is one of similarity, even limited similarity. Let us look at the concept of transitivity, by the way of illustration.

Models based on analogies with familiar situations are not transitive, for if $\frac{a}{b} :: \frac{c}{d}$ and $\frac{c}{d} :: \frac{e}{f}$, there may not be any analogy or similarity relation between $\frac{a}{b}$ and $\frac{e}{f}$. The features of the model are <u>selectively similar</u> (the positive analogy) but not <u>entirely similar</u> (the negative analogy) and indeed, some aspects require further exploration to discover whether they 'carry over' or not (the neutral analogy).

Let us now summarize some of these criticisms. The 'positivistic tradition', by stressing mathematical isomorphism, places excessive prominence on formal analogies and neglects substantative or material

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