How Models Are Used to Represent Reality

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Most recent philosophical thought about the scientific representation of the world has focused on dyadic relationships between language-like entities and the world, particularly the semantic relationships of reference and truth. Drawing inspiration from diverse sources, I argue that we should focus on the pragmatic activity of *representing*, so that the basic representational relationship has the form: Scientists use models to represent aspects of the world for specific purposes. Leaving aside the terms "law" and "theory," I distinguish principles, specific conditions, models, hypotheses, and generalizations. I argue that scientists use designated similarities between models and aspects of the world to form both hypotheses and generalizations.

1. Introduction. Within the philosophy of science, it has typically been assumed that the fundamental representational resources are linguistic, mathematics being understood as a kind of language. Following practice in the foundations of logic and mathematics, it has then been assumed that the language of science has a syntax, a semantics, and, finally, a pragmatics. While syntax is deemed important, semantics, which includes the basic notions of reference and truth, has received the most attention. Much of the debate regarding scientific realism, for example, has been conducted in terms of the reference of theoretical terms and the truth of theoretical hypotheses. Pragmatics has been largely a catchall for whatever is left over, but seldom systematically investigated. I now think that this way of conceiving representation in science has things upside down. Some recent work on the nature of natural languages suggests that language is primarily a cultural achievement (Clark 1997; Tomasello 1999). It is, if you will, a cultural artifact. Learning a language is learning to be a member of a culture with its history and mores. Insofar as it makes sense to talk about levels here, this all takes place at the level of pragmatics. Syntax

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and semantics seem to be emergent features of language use that only become visible, so to speak, in the study of written languages.[1]

I wish to carry over these latter ideas to the more specialized context of scientific cultures. On this way of thinking, the scientific practices of representing the world are fundamentally pragmatic. If we wish to understand these practices, we should not begin with the language itself, but with the scientific practices in which the language is used.

2. Representing. The focus on language as an object in itself carries with it the assumption that our focus should be on *representation*, understood as a two-place relationship between linguistic entities and the world. Shifting the focus to scientific practice suggests that we should begin with the activity of *representing,*[2] which, if thought of as a relationship at all, should have several more places. One place, of course, goes to the agents, the scientists who do the representing. Since scientists are *intentional* agents with goals and purposes, I propose explicitly to provide a space for purposes in my understanding of representational practices in science. So we are looking at a relationship with roughly the following form:

S uses *X* to represent *W* for purposes *P.*

Here *S* can be an individual scientist, a scientific group, or a larger scientific community. *W* is an aspect of the real world. So, more informally, the relationship to be investigated has the form: Scientists use *X* to represent some aspect of the world for specific purposes. The question is, "What are the values of the variable, *X*?"

Focusing on scientific practice, one quickly realizes that *X* can be many things, for example, words, equations, diagrams, graphs, photographs, and, increasingly, computer-generated images. Here, however, I wish to focus on the traditional medium of scientific representation, the scientific *theory*.[3]

3. Theories. The assumption that scientific theories are sets of statements goes along with the view that scientific representation is to be understood as a two-place relationship between statements and the world. A focus

1. I have been told by linguists that only languages for which there is a written counterpart have a word for "word." In languages that are only spoken, words are apparently not even a recognized category. This is in agreement with the well-known fact that graphical representations of the sound of spoken languages reveal no obvious breaks at what we recognize as words, but just more or less continuous spiking.

2. This is in line with the suggestion made by Hacking (1983) two decades ago.

3. I have examined the use of pictures, graphs, and diagrams as representational media in Giere 1996.

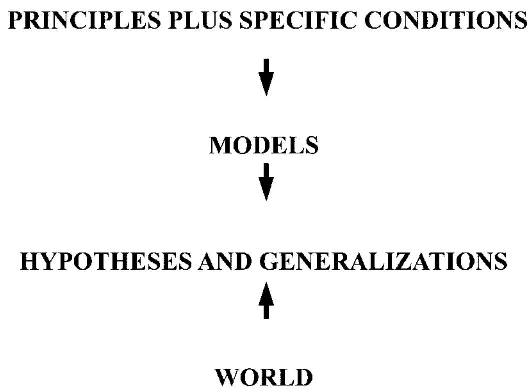

Figure 1

on the activity of representing fits more comfortably with a model-based understanding of scientific theories. Figure 1 provides an abstract picture of such a view of theories.

On this picture, scientists generate models using principles and specific conditions.[4] The attempt to apply models to the world generates hypotheses about the fit of specific models to particular things in the world, hypotheses that may be generalized across previously designated classes of objects.

3.1. Principles. In some sciences, models are constructed according to explicitly formulated principles. Physics is especially rich in such principles: Newton's principles of mechanics, Maxwell's principles of electrodynamics, the principles of thermodynamics, the principles of relativity, and the principles of quantum mechanics. But evolutionary biology also has its principle of natural selection and economics boasts various equilibrium principles.

What I am here calling principles have often been interpreted by sci-

4. This picture applies only to mature theories, such as quantum mechanics or evolutionary biology, in which there are recognized principles. As has been emphasized by Cartwright (1999) and Morgan and Morrison (1999), much science is done using principles drawn from many different areas or using no principles at all. This makes for a less tight connection between principles and models. Also, the arrow down to models from principles and specific conditions definitely does not indicate deduction. Again, as emphasized by Cartwright and others, constructing models may be a very complex activity that frequently includes a variety of approximations and simplifications.

entists and empiricist philosophers as empirical laws, that is, generalizations that are both universal and true. My view (Giere 1988, 1999), which I share with Nancy Cartwright (1983, 1999), Paul Teller (2001) and some others, is that, if understood as universal generalizations, the resulting statements are either vacuously true, or else false and known to be so. The remaining problem is how otherwise to characterize these principles.

I think it is best not to regard principles themselves as vehicles for making empirical claims. Newton's three laws of motion, for example, refer to quantities called force and mass, and relate these to quantities previously well-understood: position, velocity, and acceleration. But they do not themselves tell us in more specific terms what might count as a force or a mass. So we do not know where in the world to look to see whether or not the laws apply. One can give a similar account of the evolutionary principles of variation, selection, and transmission.

If we insist on regarding principles as genuine statements, we have to find something that they describe, something to which they refer. The best candidate I know for this role would be a highly abstract object, an object that, by definition, exhibits all and only the characteristics specified in the principles. So the principles are true of this abstract object, though in a fairly trivial way.[5]

More important is how the principles function in representational practice. Their function, I think, is to act as general templates for the construction of more specific abstract objects that I would call "models." Thus, to the principles one adds what I am here calling "specific conditions," the result being a more specific, but still abstract object. To take a canonical example, adding the condition that $F = -kx$ yields a general model for a simple harmonic oscillator, where x is the displacement from an equilibrium position. With this model we are still some distance from any empirical claims. This model could be applied, for example, to a pendulum with a small amplitude, a mass hanging from a spring, the end of a cantilevered beam, or a diatomic molecule. But even specifying that x is the displacement of a mass on a spring does not get us to an empirical application. We still have only an abstract model of a mass on a spring. To get down to an actual empirical claim we must designate a particular real mass on a spring. Only then can it be empirically determined whether

5. Thus, in the old debate whether Newton's laws should be regarded as empirical claims or definitions, I am closer to those who argued for the definitional point of view. Mainly, however, the difference becomes unimportant. What matters is the function of principles in the construction of models that may be used in making empirical claims. The principles help shape and constrain the structure of these models.

the motion of that real mass on that particular spring agrees with the motion calculated for the abstract mass in the model.

3.2. Theories and Laws. In my picture of scientific theories, there is no element explicitly designated as being "The Theory" or as being "A Law." This is because the terms "theory" and "law" are used quite broadly both in scientific practice and in metalevel discussions about the sciences. Their use typically fails to distinguish elements that I think should be distinguished if one is to have a sound metaunderstanding of scientific practice. Thus, for example, references to "evolutionary theory" may often be understood as referring to what I am calling "the principles of evolutionary theory." Now I regard these principles as defining a quite abstract object and not as directly referring to anything in the world. But just about everyone would insist that evolutionary theory is an empirical theory. In my terms, this means that some specific evolutionary models structured according to evolutionary principles have been successfully applied to real populations. So, from my point of view, the term "theory" is used not only ambiguously, but in contradictory ways.

As I understand them, it is part of the job of a naturalistic philosophy of science and science studies more generally to construct what would ordinarily be called a "theory of science."[6] This pretty much requires some regimentation in the usage of existing terms as well as the introduction of some new concepts. It is obviously desirable to follow widely accepted usage as much as possible. Thus I appropriated the term "principle" for things that within the sciences themselves are often referred to as "principles." But compromises are necessary, and so, as noted above, I prefer to leave the term "theory" as it is and not appropriate it for my own account of scientific practice.

The same holds for the term *law*. What is commonly called Newton's second law of motion, for example, is for me a central principle of classical mechanics. The so-called law of the pendulum, on the other hand, is an explicit part of the characterization of a much more specific, though still abstract, model of the simple pendulum. Indeed, in many accounts, the law of the pendulum would be regarded as merely an empirical generalization. So, here again, I prefer to leave the term *law* as it is and use a more precise vocabulary in my own account of science.

4. Models. At first sight, the things that are commonly called models seem to form a quite heterogeneous class including physical models, scale

6. Here I am using "theory" with its ordinary, ambiguous meaning. I do not think that it is useful at this stage to attempt the reflexive move of casting my own metatheory in a form designed for mature sciences in the physical and biological sciences.

models, analogue models, and mathematical models, just to name a few. Thus we have Watson's original tin and cardboard model of DNA, Rutherford's solar system model of atoms, the Bohr model of the atom, and the de Sitter model of spacetime. There are also equilibrium models in economics and drift models in evolutionary biology. I think it is possible to understand models in a way that usefully encompasses much of this heterogeneity.

Following the general schema of Figure 1, models in advanced sciences such as physics and biology should be abstract objects constructed in conformity with appropriate general principles and specific conditions.[7] One might think of them as artful specifications of the very abstract models defined by the principles. What is special about models is that they are designed so that elements of the model can be identified with features of the real world. This is what makes it possible to use models to *represent* aspects of the world. So here, finally, we have a candidate for the X in the general scheme for representation with which we started: Scientists use *models* to represent aspects of the world for various purposes. On this view, it is models that are the primary (though by no means the only) representational tools in the sciences.

4.1. Similarity. How do scientists use models to represent aspects of the world? What is it about models that makes it possible to use them in this way? One way, perhaps the most important way, but probably not the only way, is by exploiting *similarities* between a model and that aspect of the world it is being used to represent. Note that I am not saying that the model itself represents an aspect of the world because it is similar to that aspect. There is no such representational relationship.[8] Anything is similar to anything else in countless respects, but not anything represents anything else. It is not the model that is doing the representing; it is the scientist using the model who is doing the representing. One way scientists do this is by picking out some specific features of the model that are then

7. I take abstract entities to be human constructions, the ability to create such constructions being made possible by symbolic artifacts such as language and mathematics. But abstract models are definitely not to be identified with linguistic entities such as words or equations. Any particular abstract model can be characterized in many different ways. Nor should abstract models be thought of as merely formal. They are created already interpreted. To take a homey example, we all know how to plan a trip to a supermarket, including making a shopping list. Such a planned trip is an abstract entity. I would even call the plan a model of a trip, a model that might never apply to anything if, for example, an emergency prevents the trip from taking place. Most abstract scientific models are much more complex, of course, but, as abstract entities, they should not be regarded as any more mysterious than a planned shopping trip.

8. This point has been very effectively argued by Mauricio Suárez (2003).

claimed to be similar to features of the designated real system to some (perhaps fairly loosely indicated) degree of fit. It is the existence of the specified similarities that makes possible the use of the model to represent the real system in this way. Thus, in the example of the mass hanging from a spring, using the mathematical characterization of the model, one can calculate the period of the oscillation as a function of k/m. Knowing the value of this parameter for the real spring system, one can then determine how close the measured value of the period is to the value calculated for the model.[9]

I wish to emphasize that representing aspects of real systems in this way does not require the existence of an objective measure of similarity between the model and the real system. Nor does the lack of such an objective measure introduce an undesirable amount of relativity in claims of similarity between the model and the real system. Claims about features of the world remain as objective as they ever were.

It is now clear that the above account of using abstract models to represent real systems applies as well to physical models. To take an obvious example, it was particular similarities in physical structure that made possible Watson's use of his tin and cardboard model to represent the structure of DNA. He clearly was not saying that DNA is similar to his model with respect to being composed of tin and cardboard. Part of being able to use a model to represent some aspect of the world is being able to pick out the relevantly similar features. Another part of using a model to represent something is having some reasonable idea of how good a fit might be expected. The angles in Watson's model used to represent bonding angles in DNA were not exactly the bonding angles later determined for samples of DNA. But no one doubted they were close enough to conclude that DNA has a double helical structure. Moreover, the angles in the model were somewhat adjustable, and so could be made better to fit the angles in DNA that were more precisely determined by later experiments.

4.2. Laws and Generalizations. As noted above, some statements called "laws of nature" function more like lower-level generalizations than grand principles. These abound in physics; Hooke's law, Snell's law, and Galileo's law of the pendulum being traditional examples. The prevalence of

9. It is, of course, always possible to describe this situation as one of merely determining the truth, or truth-likeness, of a statement about the expected period of the bouncing mass. There is no way of ruling out such interpretations. I think my way of understanding the situation is superior overall, but cannot directly argue for that position here. I can only present what I take to be desirable features my position for the consideration of the reader.

such laws goes down as one moves up Comte's hierarchy through chemistry, biology, and psychology to the social sciences. Even in physics, however, the simple statements of such laws cannot be both universal and true. There are always known restrictions and exceptions. The question is what to make of this situation.

One solution is not to take these law statements at face value, but to regard them either as being tacitly supplemented with embedded ceteris paribus clauses or as being accompanied by separate qualifications. A problem with this sort of solution, from my point of view, is that it requires being definite about something that is decidedly indefinite, and so the resulting package ends up being incomplete. Alternatively, in trying to be indefinite, this approach is likely to end up making laws vacuous, claiming, in effect, that the law holds except where it does not.

A better solution, I think, is to keep the simple law statements, but understand them as part of the characterization of an abstract model and thus being true of the model. The required qualifications, then, concern only the range of application of the model. One need only indicate, tacitly or explicitly, where it applies or not, and to what degree of exactness. One might wish to claim, for example, that a whole class of previously identified mass-spring systems can be represented using the same type of model. And this could be tested directly by measuring periods of randomly selected members of the class. Of course we now know enough about such systems that direct tests are no longer necessary.

5. Purposes. Thus far I have assumed that models are being used for the general purpose of learning what something is like. Watson used his model simply to represent the physical structure of DNA. His goal at that time was to discover this structure. Of course he also had other, longer-term goals, such as understanding the mechanisms of inheritance. But these goals required that one first have a good model of the physical structure. That is the goal reached in 1953.

Models are also used for more specific purposes.[10] Here is an example that has been used by both Margaret Morrison (1999) and Paul Teller (2001). If one is investigating diffusion or Brownian motion, one models water as a collection of molecules. However, if one's concern is the behavior of water flowing through pipes, the best-fitting models are those

10. Here I should acknowledge, as has been urged by a number of students of the scientific enterprise (Morgan and Morrison 1999), that scientists use models for all sorts of purposes other than representing the world. I do think, however, that representing the world is a very important function of models and is often presupposed in discussions of other roles for models. So a focus on the role of models in representing is well justified.

that treat water as a continuous fluid. Thus, the type of model one uses to represent water depends on the kind of problem one faces. Note that there is no conflict in saying that scientists use continuous fluid models to represent water for the purpose of studying fluid flow and also use molecular models for the purpose of representing water for the study of Brownian motion.

Of course, one wants to ask, "But what is water really?" And the expected answer is, "Molecules." But the overall superiority of molecular models is easy to justify because there is a clear asymmetry in favor of a molecular perspective. That is, from within a molecular framework one can, in principle, explain how a macroscopic fluid made up of microscopic molecules could be fitted very well within a framework based on principles regarding continuous fluids. We just don't know how to construct molecular models of macroscopic fluids, and maybe we never will. On the other hand, there is no way to construct models using continuous fluid principles to model Brownian motion. So we can say that the world is such that there is, in principle, a molecular model for all of the many manifestations of water. This is as close as we can come to saying what water "really" is. In practice, there are many manifestations of water that are most usefully modeled within other frameworks.

6. Realism. Thus far I have made no distinctions among elements of a model that might be identified with aspects of the real world. Any element might be so designated. In this respect, the account given so far is realist as opposed to empiricist in the sense that claimed similarities between models and the world are restricted to those aspects of the world that are in some sense "observable" (van Fraassen 1980, 1989). In general, I think that the distinction between what is observable or not by ordinary humans is not of fundamental importance in any theory of science. It is only necessary that humans can observe enough to practice science, which is not in doubt. Here I will only consider three examples illustrating a range of realist claims.

The first example concerns a very old question about the interpretation of classical gravitation. Newton's principles of mechanics were originally formulated in terms of forces acting instantaneously at a distance. Skeptics suggested, not unreasonably, that these forces seemed rather occult. One attempt to reduce the intellectual discomfort of embracing action at a distance was to introduce the notion of a *field* of force. Thus, on the original interpretation, the earth exerts a force at a distance, GMm/r^2, on the moon. On the field interpretation, associated with the earth there is a gravitational potential of GM/r. Any body of mass m in this potential experiences a force of $-GMm/r^2$. Now it is tempting to ask: In addition

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to forces, are there also really fields in nature? Or, are there maybe really only fields?

On my view, these are not legitimate scientific questions. The principles of gravitational field theory are no more the kind of things that can be empirically true or false than the principles of Newtonian mechanics. They can both only be more or less fruitful at guiding the construction of well-fitting models. In this case, field principles are just as fruitful as Newtonian principles because they generate exactly the same set of models. One cannot directly test principles by empirical means. One can only test the fit to the world of particular models that incorporate the principles. Thus, empirically, there is no basis for preferring one set of these principles over the other. So my account puts some limits on realist claims.

My second example is the discovery of the double helix. In this case, the data consisted of such things as the Chargaff rules for the relative prevalence of the four basic nucleotides in DNA (the results of chemical analysis) and Rosalind Franklin's X-ray diffraction photographs of DNA, from which one could calculate various parameters of a supposed double helix. The conclusion, of course, was that DNA strongly resembled Watson and Crick's physical model, with nucleotides in very specific relative positions partly determined by chemical boding angles. None of these things were then in any sense directly observable. I don't see how any adequate theory of science can question the legitimacy of these conclusions. Here realism prevails.

Now imagine two models of the overall spacetime structure of the universe that differ only in regions outside our light cone. According to our best current theories, no causal interaction with the supposed physical counterparts of the differing structures is possible. So no differences are even in principle physically detectable. We can also imagine that the two models are equally endowed with any supposed superempirical virtues such as simplicity or unity. Here I am strongly inclined to say that there can be no scientific basis for claiming that one model better fits the overall structure of the universe. Again, we have a limit on realist claims.[11]

7. Conclusion. Most recent philosophical thought about the scientific representation of the world has focused on dyadic relationships between language-like entities and the world, particularly semantic relationships, but also evidentiary relationships. I have said nothing about evidentiary relationships in this paper, but in other works I have argued that these should be thought of in terms of human decisions to accept or reject

11. The view expressed here is exactly like van Fraassen's (1980), if one replaces his restriction to humanly observable features of the world with a restriction to what is in principle detectable by any means compatible with our best physical theories.

hypotheses in light of various interests (Giere 1988, ch. 6; 1996). Here I have argued that scientific representation should be thought of in the same general way, that is, in terms of the use of models by scientists to represent aspects of the world for various purposes. My hope is that by *not* abstracting away from the activity of doing science one may achieve a better understanding of the nature of modern science.

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