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Scientific Models

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Abstract
This contribution provides an assessment of the epistemological role of scientific models. The prevalent view that all scientific models are representations of (part of) the world is rejected. This view points to a unified way of resolving epistemic issues for scientific models. The emerging consensus in philosophy of science that models have many different epistemic roles in science is presented and defended.

The role of models in science has been a focus of philosophical discussion for at least a century.[1] In what follows, I provide some examples of scientific models, introduce some of the relevant philosophical discussion about models and then focus in on issues arising from a specific view about what models do: all scientific models are representations of parts of the world. I side with an emerging consensus that this unified view is not tenable. Scientific models play a number of different epistemological roles in scientific inquiry and as a result, philosophical inquiry about models should be pursued in a number of different directions.

Scientists attempt to produce knowledge about the world via numerous methods and philosophers of science attempt to clarify the various methods and, in some cases, defend a unifying underlying method that accounts for them all. This latter task became almost an end in itself during the heyday of logical empiricism. Almost all philosophers of science at this time shared a view of science that included the following components: scientific theories are collections of sentences (including sentences characterizing scientific laws), theories provide explanations and the hypotheses of the relevant theories are subject to test and can be either confirmed or exposed to disconfirmation. Epistemological projects for philosophers of science included developing accounts of confirmation, explanation, and prediction for the sentences or sets of sentences that make up hypotheses or theories. The logical empiricist view began to break down for various reasons, not least of which was the increase in criticisms of various aspects of the view that were generated from careful case studies in the history of science. One central component of the view that was a perennial focus of attack was the idea that theories are sets of sentences and the related view that sentences are the relevant units of epistemic appraisal in science.

Several alternate units of epistemic appraisal were proposed, including Kuhn's (1970) paradigms, Lakatos (1970) research programs and Laudan's (1977) research traditions. Models were proposed as a focus of attention, not as an alternate to theories but as an alternate to sentences. The idea was that scientists do not trade in sentences but rather in models. Nelson Goodman had this to say about models:

> Few terms are used in popular and scientific discourse more promiscuously than ''model''. A model is something to be admired or emulated, a pattern, a case in point, a type, a prototype, a specimen, a mock-up, a mathematical description – almost anything from a naked blonde to a

quadratic equation – and may bear to what it models almost any relation of symbolization (1976: 171).

He went on to say: ‘scientists and philosophers […] have been forced to fret at some length about the nature and function of *models*’ (1976: 171; italics in original). For quite some time philosophers have been more or less agreed on what sentences do, how they work and how their truth conditions are spelled out, or at least we know how to disagree about the specifics of these issues. Models posed a new set of problems. Everyone agreed that scientists use them to ply their trade but there was very little agreement about what they are or what role they play in the epistemic appraisal business.

Goodman is right, there are lots of applications of the term ‘model’ in the sciences. Before we get a sense of the range of types of models used by scientists an important clarification is in order: there is a distinct philosophical view that there are many different types of models in science, which reflects the empirical fact about scientific practice that there are many types of models (see, e.g. Downes 1992). This can be confusing when we confront the models literature in philosophy of science. Philosophers of science characterize models in a number of ways. At one extreme, there are those who think that models are only equations and the set of curves that those equations characterize (see, e.g. Forster 2004). At another extreme, there are those who think that anything, including organisms like rats and stuffed animals in a museum, count as models (see, e.g. Griesemer 1990; Downes 1992; Plutynski 2001). If one commits to a view that models can only be a particular type of thing, such as certain kinds of mathematical structures, a number of important consequences follow. What I pursue for much of the rest of this article are epistemological consequences connected to the representational capacities of models but another important consequence has to do with the demarcation of the sciences (Cf. Cohen and Callender 2006).

If you define scientific models only as certain mathematical structures, then science is the practice that involves the use and application of these kinds of models. Some bite this bullet but there are troubling consequences for doing so: Darwin’s work on the foundations of evolutionary theory proceeded without the use of a single mathematical model. As a result, this work cannot count as scientific on this model-based definition of science. Those who think that it was not until the New Synthesis, with its mathematical machinery, that evolutionary biology became a science may be comfortable with this conclusion but perhaps not with the conclusion that much of molecular biology is not science either. Crucial work in molecular biology is carried out by working with models of mechanisms such as gene duplication and transcription that rely on pictorial representations of idealized molecules and no mathematics. Unless you detach your account of models from your account of what a science is, these kinds of consequences are inevitable.

So what are some examples of models in science? Many are mathematical, for example, in evolutionary biology and ecology there are hundreds, perhaps thousands of mathematical models of various dynamic processes. Models of disease spread, the action of selection on a particular trait, or the interaction between various species of organisms in an area of rain forest are distinct in virtue of the mathematical machinery that goes into each of them. How to generate the appropriate type of mathematical model for a specific set of phenomena is an important skill for the biologist (Otto and Day 2007). For many purposes in molecular biology, it is sufficient to model a given allele as a linear string of nucleotides and model its role in the cell by use of diagrams that reveal operations upon the nucleotide string. Similarly, many of the central biochemical processes of cells are modeled with complex diagrams, for example, standard models of the Kreb cycle are diagrams. Scientists debate the adequacy of types and styles of modeling in a specific context. For example, adaptive landscapes in evolutionary biology have been modeled both as diagrams and mathematically and the relative utility of each style of modeling is actively debated. Finally, as indicated above, individual organisms can be models. We model numerous human genetic and physiological processes in rats and mice.

Philosophers efforts to spell out an account of models have thus far tended to emphasize their representational role.[2] Here is Teller's summary of the predicament:

> I take the stand that, in principle, anything can be a model, and that what makes a thing a model is the fact that it is regarded or used as a representation of something by the model users. Thus in saying what a model is the weight is shifted to the problem of understanding the nature of representation (2001: 397).[3]

On a view like this, models stand in some representational relation to a world system or the observable aspects of that system. This view comes along with a system of epistemic appraisal centered around the fit of the model with the world or the observed world. Rather than being true or false, models are claimed to be isomorphic with the system they represent, or similar to that system in various respects and degrees.

In developing his influential version of this view, Giere (1988, 1996, 1999a,b, 2004) reminds us that any definition of models that philosophers of science give should be constrained by what scientists take models to be. An important part of the philosopher of science's job, according to Giere, is to characterize actual scientific practice. His account of a model keeps this constraint on philosophy of science front and center, for him models are the idealized systems discussed in scientific text books (Giere 1988: 78–80). Such idealized systems can be the kind of systems that satisfy specific sets of equations, such as a simple harmonic oscillator in mechanics, but may also be idealized systems such as models of sea floor spreading in geology, which do not satisfy specific sets of equations and in fact are not even presented as equations.

Continuing the emphasis on representation, Giere provides an account of scientific theories, which are collections or families of models that represent the world. Giere takes the relevant representation relation to be one of similarity. He takes the relation between pictures and their objects and between models and the real systems they stand for to be analogous, each being types of similarity relation.

Van Fraassen (2008) keeps the emphasis on representation in his recent work but holds that the key to understanding how models represent is to understand how they are used. According to Van Fraassen, there is nothing in any given model that gives it its representational capacity, rather the use it is put to in a practice is the key to understanding its representational role. This view leaves Van Fraassen with a very liberal view of what models are or can be but also has some interesting consequences. Anything can be taken to represent anything else on Van Fraassen's view but the choice of our representations is guided by some constraining considerations. For example, resemblance can be selected as the 'vehicle of representation' and is clearly what is driving the use of scale models as representations, according to Van Fraassen. Although Van Fraassen's view has liberal implications, he focuses most of his attention on mathematical models in physics. There is also another interesting consequence of his view, which is that mental representations do not represent, because they are not used by anybody to do so. A further implication is that there are no 'natural representations', representations are artifacts that we produce and use to stand for states of affairs and, sometimes unobservable, situations.

Van Fraassen explicitly resists providing an exhaustive analysis of representation. For example, although he allows a role for similarity in our understanding of representation, he does not believe that there is any treatment of similarity that could provide a reductive analysis of representation. Giere and others are more optimistic about such analyses, thinking that problems with earlier similarity based accounts of the way in which scientific models represent, can be overcome by appeal to more powerful and sophisticated analytic techniques. For example, Weisberg (forthcoming) is developing a formal account of similarity for models that is designed to give them their epistemic punch back.

The representationalist view of science is attractive. Our examination of scientific practice tells us that scientists trade in models and if the epistemic value of these objects can be cashed out in terms of some type of representation relation, e.g. similarity, then we have the makings of a nice unified account of science. On this account science is successful to the extent that scientists produce models of the world that are similar to their objects, or observable aspects of those objects, in relevant respects and degrees.

Given that this is an attractive view of science, why not settle on it? Unfortunately, there are lots of reasons for not settling on this view. The logical empiricist consensus faced many challenges and the model-based view of science[4] that proposes to replace the consensus faces its challenges too. In what follows I lay out some of these challenges and outline some responses to them. I do this in order to strengthen and perhaps redirect the model-based view of science, rather than to advocate overthrowing it and returning to the logical empiricist consensus. Scientists use models in many aspects of their work and philosophers of science need to be able to explain how this work advances scientific knowledge.

Let us focus for the moment on the model-based view of science and include, along with Giere, that models represent world systems and that the representation relation between models and those systems is one of similarity. A question should arise for the view: can similarity based accounts of representation carry enough epistemic weight for philosophy of science (Cf. Downes 2009)? There are a number of ways to spell out this question and, as a result, spell out problems for the model-based view. One is to focus on the notion of similarity itself. As many have pointed out (Goodman 1976; Downes 1992, 2009; Teller 2001; Suarez 2003) similarity based accounts of representation have all manner of problems.[5] If one expands the range of what we take models to be, the ways in which they can be similar to their objects are many and varied. Also, while pictures and scale models fit reasonably well with a similarity account of representation, mathematical models do not. Mathematical models share with sentences of natural language a certain arbitrariness with respect to the objects they are taken to represent. The productive nature of all symbolic systems stems in part from the fact that we can 'take' any arbitrary symbol to stand for a part of a world system.[6]

Imagine all the problems for similarity based accounts of representation were solved, would the model-based view then provide an adequate epistemology for science? No, at least not according to Cartwright (1983), Teller (2001), and Wimsatt (2007), among others. The complaint here is that the role of models in science is by no means exhausted by representation. When Suarez (2004), Nersessian (1999), or Downes (2009) talk of the various inferential roles that models can play, they imply that there is a far more expansive epistemic role that models can play in forwarding scientific work than mere representation. Scientists use models to do many things and even, according to Wimsatt, use 'false models as means to truer theories' (2007). Pursuing Wimsatt's approach a little further illustrates this point.

Wimsatt wants to explain the usefulness of false models in science.[7] He first explains in what ways models can be false. This can be due to local applicability, idealization, incompleteness, misdescription of interactions, being a totally wrong-headed picture of nature or failing to describe or predict the data. For example, if a model is locally applicable, it may not be generalizable at all. Alternately, if it is highly idealized it may have no local applicability (Cf. Cartwright 1983). Scientists choose their models in order to learn something from them.[8] For example, oversimplified models can be starting points in a 'series of models of increasing complexity and realism' or two false models can define the extremes of a continuum of models upon which better, more accurate models fall (Wimsatt 2007; : 104). The point Wimsatt makes is that in many, many cases of model building the aim is not to accurately represent the structure of world. For example, we learn a great deal about the basics of population genetics from simple two allele models and biologists began their understanding of the complex dynamical systems of changing gene frequencies by working with such models. The simple two allele models accurately represent almost no real world system. A further example in this vein is R. A. Fisher's three sex model. The model was not put forward as a representation of any real world system but rather to assess aspects of the dynamics of population genetics.[9] No account of fit, accuracy or similarity is appropriate in the assessment of the epistemic work that such models do and yet such work is one of the keys to advancing our knowledge of the world.

One of the aspirations of philosophers of science who championed the model-based view as the alternative to logical empiricist philosophy of science was to produce a new unified view of science. The attractive picture associated with Giere appears to bring us back to what Teller (2001) calls the 'Perfect Model Model' of science. The new version is that all theories are collections of models and models represent the world via a relation of similarity with their objects. We have seen here some of the reasons that this approach will not work: the problems associated with similarity accounts of representation and the arguments that models also serve non-representational roles in science, such as those illustrated by Wimsatt. The further problem for this version of the view is that not all science proceeds via the use of models and here I mean models in the widest possible sense. Unifying views of science are appealing, because they cut down in the variety of epistemological work we have to do. If all scientists are in the business of confirming hypotheses, then all we need is a good confirmation theory and we have our epistemology for science. Likewise, if all science involves the use of models to represent the world, then our account of representation more or less solves our epistemological problems. Scientists do use models, both in a representational capacity and in the ways that Wimsatt says that they do but they also attempt to understand the natural and social world in lots of other ways and, as a result, provide lots more opportunities for epistemology. Hypothesis testing is very much part of the business of science and the way in which hypotheses are tested should be and is a focus for epistemology. The assumptions built into statistical packages can have an effect on scientists' hypothesis choice and unearthing these assumptions and understanding their role in hypothesis choice is important work for epistemologists. A reasonable, deflationary model-based view of science is just one part of a broad based approach to scientific practice that attempts to clarify, understand and perhaps even justify the numerous methods that scientists use to gain knowledge about the world.

What we have here is by no means an exhaustive account of the work on models in philosophy of science. There a few much more comprehensive introductions to the topic, the most up to date and helpful of which is Frigg and Hartmann's (2009). Cohen and Callender (2006) provide a nice overview of problems of representation as they relate to models in science and Godfrey-Smith (2006) presents and defends a deflationary model-based view that fits well with biological practice. Weisberg (2007b) makes a very good case for a middle ground view of modeling that fits between the extremes of only mathematical equations on one end and objects such as dead animals on the other. Much interesting work is currently underway on models as fictions (Fine 2009; Godfrey-Smith 2009), trade-offs between the epistemic virtues of models (Matthewson and Weisberg forthcoming), models and idealization (Weisberg 2007a; Thomson-Jones forthcoming a,b) among other topics. Wendy Parker is currently developing an approach to assessing the epistemic virtues of models that avoids emphasis on testing models themselves, calling for a focus on the 'adequacy-to-purpose' of models (Parker forthcoming a,b). This approach shows promise for assessing scientific models that are not presented as straightforward representations. Finally, philosophers continue to pursue issues of modeling via a focus on specific sciences. For example, Elisabeth Lloyd (2009; Lloyd 2010) and Parker (2009) examine climate science via the various complex modeling techniques scientists in this interdisciplinary field appeal to. What all this work illustrates is perhaps the emergence of a consensus that models can be many different things and play differing roles in scientists' work. While a unified view of the epistemic role of models is not likely in the offing, a much better understanding of the varied epistemic roles that models play is.

Short Biography

Stephen M. Downes research interests are in philosophy of biology, specifically in biological approaches to explaining human behavior. He also has broader interests in philosophy of biology as well as an interest in the role of models in science. His work is published in journals such as *Philosophy of Science*, *Biology and Philosophy*, *Studies in History and Philosophy of Science*, and *Perspectives on Science* and in various anthologies, such as Wiley-Blackwell's *Contemporary Debates in Philosophy of Biology*. Downes held a 1 year position at the University of Cincinnati and a Post-Doctoral Fellowship at Northwestern University before taking up his appointment at the University of Utah where he is now Professor of Philosophy and Department Chair. He has a BA (Hons.) Degree in Philosophy from the University of Manchester, an MA in Philosophy from the University of Warwick and a PhD in Science and Technology Studies from Virginia Polytechnic Institute.

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Notes

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[1] See, e.g. Boltzmann (1902/1974) and Vaihinger (1911).

[2] Such accounts are often traced back to an article by Boltzmann (1902/1974).

[3] Solving this problem is what Cohen and Callender call answering the constitution question: 'What constitutes the representational relation between a model and the world?' (2006: 8).

[4] The view I have outlined so far is often referred to as the semantic view of theories. I prefer Peter Godfrey-Smith's (2006) terminology: model-based view of science. Godfrey-Smith correctly points out that the semantic view is committed to being a view for all scientific theories and the sense of model used is the same as or close to the sense of model in logic (Cf. Downes 1992). Model-based views of science need not be committed to either of these assumptions (see also Teller 2001).

[5] Van Fraassen wants to retain notions of similarity or resemblance but has no aim to provide an account of representation as similarity or as reducible to similarity. There is a brief but helpful discussion of Van Fraassen on this topic in Thomson-Jones (forthcoming a,b) review of Van Fraassen's book.

[6] As noted above, Weisberg (manuscript) is developing a formal approach to similarity in an attempt to deal with exactly these problems.
[7] 'False model' is Wimsatt's locution, which I retain here. Ideally this would not be the terminology used, given that the issue for the representationalist is finding an appropriate relation for models that does much of what truth does for sentences; 'not very similar model' does not have much of a ring to it.
[8] Fine (2009) traces an early precursor of this idea to Vaihinger (1911), who understood that scientists used a model not necessarily as a representation but as 'a mental expedient, an instrument of thought used to help navigate the world at large' (2009: 118).
[9] Cf. Weisberg (2007b).

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