

7-1-2014

The World in the Model and the Model in the World: Review of *The World in the Model* by Mary Morgan

John B. Davis

Marquette University, john.davis@marquette.edu

Essay Review of The World in the Model and the Model in the World

John B. Davis

*Department of Economics, Marquette University
Milwaukee, WI*

Mary Morgan's new book on models in economics represents the culmination of more than a decade of research on the subject. She is certainly one of the most expert and accomplished researchers on the nature and role of models in economics, and this book provides an extensive review of the practice of modelling in economics across the history of the field. Her *Models as Mediators*, edited volume with Margaret Morrison ([1999](#)), is an influential collection of papers on how models function in modern science. So, readers in the history and philosophy of science interested in modelling in economics should find this book a valuable resource. Whether it answers their questions about models and resolves debates about the practice of modelling in economics is another issue. Morgan delimits some of those concerns in defining her project in a particular way. She speaks as an historian of economics first and foremost, and largely defers more weighty

philosophical questions to others. She has two main reasons for doing so.

The first is methodological. In her estimation, adequate historical narrative is, as she puts it, 'messy'. Historians often pursue linear accounts in the interest of providing a clear and cohesive understanding of their subject matter. But this comes at the expense of suppressing events and developments that do not fit their general story, and for Morgan, this undermines the integrity of that story. Important details are left out, and differences between cases get suppressed. Thus, *The World in the Model* is meant to be a heterogeneous collection of historical case studies each of which stands in its own right as an exercise in modelling in economics. "I have long described [the book] as a kind of travel guide: I present, as three-star tourist sites, some of the best known, and historically significant, models in economics, and use each as the basis upon which to fashion a philosophical commentary about the nature of modern economics" (xv). That philosophical commentary comes in the form of a collection of insights but not as a large philosophical argument about what models are. The book "does not try to give a definition of models—but it does discuss the qualities that make them useful in a science" (*ibid.*).

The second reason Morgan puts history before philosophy is that her vision of her subject is of "a history of the naturalization of modelling in economics and a naturalized philosophy of science for economics" (*ibid.*). Thus, if *The World in the Model* does not offer a large philosophical argument about what models are, it certainly takes a distinct and strong philosophical position in regard to the interpretation of the history of economics as a science. Note that, as she puts it above, the history of economics with respect to modelling is itself a history of naturalizing its subject, with the outcome being a naturalized history. The tradition she sees herself contesting is that which emphasizes normative standards for good scientific explanation in economics and charts the history of economics in terms of progress in realizing those standards. Morgan's view, in contrast, is that the history of economics has increasingly made the model a natural object in the sense of an historical product, so that we should only study models in a naturalized philosophy of science for economics. Her position, therefore, is not just that history is 'messy' and we risk

getting it wrong when we overlook its richness; it is that this history is becoming messier in and of itself, and this determines that we must study it with a naturalized philosophy of science.

This outcome was not, however, an inevitable state of affairs. Modelling as a reasoning style in economics is a relatively recent historical development. Though there are precursors of models in the modern (1700 onward) history of economics—François Quesnay's late 1750s *Tableau Économique* is one of the earliest—economists only first referred to models in the 1930s and began then to make modelling their main method of investigation—with Jan Tinbergen (a physicist by training) transferring the term 'model' into the field from physics. In the earlier history, economics was a verbal science of broad generalizations, a few laws, and much reasoning about economic relationships. In contrast, the twentieth-century rise of modelling as a practice has been built around the construction and use of relatively small, compact mathematical objects subject to ready manipulation. Their ascendancy displaced (or obscured) both verbal reasoning in economics and general principles and laws as the main object of scientific achievement. The lesser scale of models, their chameleon nature made possible by manipulation of their assumptions, and their mathematical form has left economics with a disparate set of conceptual episodes, much like different natural species, a conceptual space in which taxonomy takes precedence over philosophy.

In this naturalized world, the main 'philosophical' questions have a pragmatic character: "How do economists create such research objects? What exactly is involved in scientific reasoning with such objects? How does working with such objects tell us anything about the world? That is: How should we characterize the making, using, and learning from models as a way of doing science?" (5). Wade Hands in his *Reflection without Rules* gives us a good sense of how questions of this kind have changed from the sorts of questions that philosophers and methodologists of economics previously asked. Traditionally, they engaged in a quite normative applied philosophy of science which borrowed and applied established arguments from natural science to economics—an "off-the-shelf view of scientific philosophy". In its place, we find an inquiry without broad rules in which "our views about the epistemic order are ... inexorably intertwined with our views about the economic order" (Hands [2001](#), 7).

In keeping with this, Morgan nonetheless makes important philosophical claims about what modelling involves. She takes modelling to be a distinct 'epistemic genre' or style of reasoning which has acquired its own specific form in economics, and uses the ambiguity of the term 'formalization' (a central concern in the development of postwar economics) to explain modelling as a two-sided activity. One meaning of 'to formalize' is to give something form or shape. But 'formal' also contrasts with 'informal' and refers to something being rule bound. These two aspects of modelling thus correspond to two interconnected aspects of models, their making and their use, which constitute the two respective halves of the book. Thus: "*models function both as objects to enquire into and as objects to enquire with.* That is, they are objects for investigation in their own right, and they help the economist-scientist investigate the real-world economy" (31).

This conception leads Morgan to one of her principal claims. Anticipating that some might say a dual view of models confuses our understanding of what models are, she gives a succinct characterization of the generic activity that model reasoning involves, at least in economics: using a model involves experiments. Models in themselves are not experiments but the use of them offers experiments: "model reasoning ... involves a kind of experiment" (*ibid.*). More fully, modelling is a two-sided practice of systematically experimenting with ideas and their ultimate relevance. Consider models as objects to enquire *into*. Economists have a variety of theoretical ideas and conceptions, and they experimentally (imaginatively?) assemble them together in abbreviated and disciplined form in their models. But what the effects of these combinations or these experiments are only gets determined in their modelling. This reverberates back onto economists' theoretical intuitions and leads to new models or experiments. The results can range from surprising to meaningless. So, overall model making is an activity, process, or practice that needs to be seen as ever ongoing. Consider models then as objects to enquire *with*. While scepticism about what models tell us about the real world is fair, compacting the 'world' or the 'person' into a small conceptual space clearly offers opportunities for inferences about real people and the real world. At the same time, what we believe we find in the real world is surely influenced by the model 'world' we use as a means of investigation.

But how we justify scientific inference about the world is hardly a new problem, and while we may reach agreements about what makes for valid inference, agreements about particular inferences will always be subject to dispute. Thus, we can only continue to try to make inferences about what our models tell us, that is, continue to experiment with their fit.

Morgan accordingly does not deny there are serious philosophical issues associated with how the 'world in the model' relates to the 'model in the world'. She does say that modelling brings together two sides of one practice of investigation which as an ongoing activity is by nature experimental. Science never ends despite the fact that it has often been seen as a progressive series of outcomes. If we rather see it as open-ended process, we get a better idea of why models are two-sided experiments.

This account of Morgan's book, however, leaves unaddressed an important position she takes in regard to models in economics with respect to the meaning of formalization in economics. Among economists, formalization is more controversial than it might seem to non-economists. On the one hand, there is little reason to expect any less recourse to formal modelling and mathematical reasoning in the field or any significant return to the verbal forms of explanation that dominated the subject prior to 1950. On the other hand, economists also exhibit some ambivalence and uncertainty about what this transition in forms of reasoning involves, and whether it might leave economics less able to articulate its theoretical motivations and grounds for economic policy.

Morgan's intervention in this regard is to emphasize the story in the model. For her, narratives matter (217ff). This may sound quaint and old-fashioned, or somehow at cross-purposes to the impulse to formalization in economics, but it might also rather be seen as an important insight about the nature of modelling. After all, what drives the practice itself? Why do economists create models in the first place? Essentially, it is because they have questions about their theories and the world they want to answer, and the process of answering their questions and then asking new questions is intrinsic to creating and re-creating models. But nowhere in the mechanics of model making or model using does this vocabulary of asking and answering questions

appear. We take it for granted that this lies behind science. But Morgan sees it as only sensible that we bring what underlies modelling out into the light of day, otherwise we really fail to understand why economists model what they model.

Note that there is a bit of a divide between philosophers of science/economics and methodologists of economics on this subject. The former look for the philosophy of science issues in economics and see economics as a means, just like other sciences, for getting at philosophical issues, which is the end. The latter rather look to philosophy as a means for getting at issues in economics, which is the end. That is, the two views are diametrically opposed with regard to order of importance and strategy of investigation.

Morgan, then, comes down on the side of economic methodologists on this issue because she believes that you must know what questions bother economists—irrespective of whether those questions refer to or can be translated into fundamental questions of philosophy—if you are to understand economics. So her insistence that one must see the stories in the model and try to extract the stories economists are trying to tell represents a conviction about the nature of economic knowledge. This also underlies her commitment to a naturalized philosophy of science for economics. Naturalization in philosophy of science in its basic meaning is associated with the idea that the forms science takes are best explained naturalistically or as if they were altogether natural species. But naturalization for Hands (2001) in economics concerns the intertwining of the epistemic order and the economic order. The pursuit of economic knowledge flows from the problems of economics, however mundane or important they may be. Morgan delivers this message when she tells us narratives matter.

The mechanics of the making and using of models in economics is therefore only the visible part of the practice of modelling. The overlooked part involves tacit understanding and offline talk by economists (McCloskey 1994) that frames their activities and output. Morgan demonstrates this admirably in her sixth chapter (“Questions and Stories: Capturing the Heart of Matters”) where she brings this conversation and story-telling to the surface in the history of macroeconomic theory. On the using side of models, she says: “How

do economists use models?' is, in one sense, easy to answer: They ask questions with them and tell stories! Or more exactly: They ask questions, use the resources of the model to demonstrate something, and tell stories in the process" (217–218).

But then this means that a popular reputation of economics—that it has become a formalized science and that ordinary language is a thing of the past—is mistaken. Rather what has occurred is that the informal side of economics has become even more informal as conversation rather than text, while the formal side of economics has become even more formal in its increased scope as the exclusive text that economists employ. Yet both sides of science remain albeit in their changed forms.

There are important scientific issues here that can be easy to miss. One is that the limitations of making inferences from models *per se* to the world are misconceived if the background narrative of questions and answers involved in their making and use is overlooked. How? If we have weak grounds for saying the model world taken on its own is sufficient for 'addressing' the real world, when we add an account of what questions the model was meant to answer, we generate further grounds for saying whether or not the model world is a credible or plausible means of 'addressing' the real world. I say 'addressing' because the usual term 'representing' gets the relation between the model world and the real-world wrong. On Morgan's view, models are created objects—or 'working objects' (Daston and Galison [1992](#)). They are things one does things with or instruments, not pictures or replicas of the real world.

Another issue is the identity of science. With regard to economics, there has long been a distinction between the science of economics and the art of economics (Keynes [1891](#)), where the latter was typically seen as a lesser, applied activity, value-laden and often idiosyncratic, and derivative of pure science. But this distinction, which arguably exists in other sciences as well, does not stand up well in the light of Morgan's book. Indeed, she offers a significantly different and thoughtfully developed vision of science that merits reflection. This vision is cloaked in her demurrals about *The World in the Model* being essentially a history of a heterogeneous collection of case studies. But

those case studies tell a story which philosophers and historians of science and economics will find valuable and informative.

Acknowledgments

Thanks for comments to Theodore Arabatzis, Marcel Boumans and Wade Hands.

References

- Daston, L., and P. Galison. 1992. The image of objectivity. *Representations* 40: 81–128.
- Hands, D.W. 2001. *Reflection without rules: Economic methodology and contemporary science theory*. Cambridge: Cambridge University Press.
- Keynes, J.N. 1891. *The scope and method of political economy*. London: Macmillan.
- McCloskey, D.N. 1994. *Knowledge and persuasion in economics*. Cambridge: Cambridge University Press.