

This book on modelling in economics is a noteworthy instance of integrated history and philosophy of science (HPS). Mary S. Morgan begins by telling us that “[s]cience is messy”, that she will “eschew smoothness” and that her book is “a series of historical case studies through which the philosophical commentary runs” (xv). These statements might lead the reader to expect a much more fragmented book than what Morgan actually offers. With respect to the history, the case studies fit in a broader narrative about the “naturalization of modelling in economics” (sec. 1.2). With respect to the philosophy, the singular to “commentary” above is crucial: the philosophical commentary runs through the whole book instead of being fragmented into more or less independent commentaries for each case study. In the end, what Morgan wants to offer is “a broader account of how economics became, and works, as a modelling science” (xv). The case-based method is simply the most effective way, according to Morgan, to get detailed clues for the broader account.

Indeed, the greatest strength of this book is in the breadth and depth of the historical material. In 400 pages, it covers early instances of models – Quesnay's *Tableau économique* (ch. 1), Ricardo's model farm (ch. 2) – a plethora of well-known models – the Edgeworth Box (ch. 3), the rational agent (ch. 4), the Newlyn-Phillips Machine (ch. 5), the macroeconomic models of Frisch, Meade, Samuelson and Hicks (ch. 6), the supply and demand model (ch. 7), the prisoner's dilemma (ch. 9) – and some less-known models used in simulations (ch. 8). The historical material is thus broad. It is also studied in depth: the book describes the models, and the model makers, with a considerable amount of detail. Morgan's expert handling of case studies makes this book a great resource for historical information on models and the rise of modelling in economics.

The “philosophical commentary” is also enlightening in many respects, though Morgan's willingness to “eschew smoothness” sometimes lapses into imprecision, and even verges on confusion. In what follows I will discuss, in turn, three questions of philosophical import to which the book contributes: (i) What are models? (ii) How are they built? And, (iii) How do they function as tools of investigation?

(i) What are models?

Though Morgan thinks that there are questions “more fruitful” than “What are models?” (xvi), she must use an implicit definition of models in order to circumscribe her object of study. She emphasizes two conditions that do a fine job of roughly delimiting an object: denotation and manipulation. Economic models stand for (parts of) the economy (24-25) and the model user can modify parts of the model and note the implications in some of its properties (5, 27-28). When models are defined in this way, Morgan is undoubtedly right that “[o]ver the last hundred years, models and modelling became the primary way of doing economic science.” (379)

Not all types of economic models are considered in Morgan's book. It is only in a late footnote that the restricted focus is stated clearly: “Statistical or econometric models […] are not the subject of this book” (388fn). Econometric models meet the two conditions of denotation and manipulation and they hold a third property: “models are applied to data from the world, and are validated with statistical

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1 Unless indicated, the references in parenthesis are to the book being reviewed.

2 In fact, Morgan uses most often the term ‘representation’, though she recognizes Hughes's (1997, S330) point that what is crucial is the ‘stand for’ relation and not a resemblance relation that is usually associated with representation (25fn).
The best way to capture the specificity of the models covered in this book is by a negative property: they are not subject to this systematic testing using real-world data. With these models, “a looser ‘casual’ mode of connection of the model to the world goes on and the criteria of validity are similarly loose.” Morgan often calls this set “mathematical models”, but this term does not do justice to the variety of objects she discusses. Most clearly, the Newlyn-Phillips Machine (ch. 5) is not a mathematical object; it is made of water tanks and tubes.

It is disappointing that the author of the celebrated The History of Econometric Ideas (Morgan 1990) does not thoroughly discuss, in this more recent book, the distinction between econometric and non-econometric models and the ways the two types of models relate. These relations are especially crucial when we are interested, as Morgan is, in the ways by which models might make us learn about the systems they denote. No doubt Morgan has opinions on the relations between econometric and non-econometric models, as is attested by quite a few footnotes in the book (293fn, 320fn, 334fn, 388fn). My point is that a book that purports to provide an account of economics as a modelling science should not systematically push econometric models into footnotes. The resulting picture passes over important aspects of contemporary economics as a modelling science.

(ii) How are models built?

The chapters of the book that focus on how models are built (chaps. 2-5) are fascinating. Morgan's account of this process is one of formalization in two senses (pp. 19-27). Formalization, as opposed to leaving something informal, is to make an object “rule bound” (20). A model comes with a set of reasoning rules that “determine the economist's valid manipulation or use of that model” (26). Some rules are implications of “the stuff that the model is made from” (26) – i.e., if the model is algebraic, the rules of algebra apply. Other rules come from the economic interpretation of the model: given the narrative accompanying the object, some elements of the model must be manipulated first, others not at all.

Most interestingly, formalization is to give form, to provide a definite shape to an object. What Morgan achieves through her case studies is a demonstration that qualitatively different processes can give form to a model. She discusses four ways, which might be understood as ideal types combining to various degrees in concrete cases of model building. Simplifying, the key differentiating element among these four ways is the locus of the initial knowledge.

1. A modeller might believe she knows disparate elements of the target system. In this case, model-building amounts to combining these elements into a single object (see ch. 2 using Ricardo's model farm).

2. A modeller might have little initial knowledge apart from broad, differentiating characteristics of the target system and vague ideas about its inner workings. In this case, imagination is required to add elements to an initially bare structure (see ch. 3 using the Edgeworth box).

3. A modeller might have quite detailed knowledge of the target system, the only problem being that the system is too complex for thinking through the implications of manipulating it. In this case, modelling works by abstraction and idealization that generate a manageable caricature (see ch. 4 using rational economic man).

4. Initial knowledge might be of another system. Model building could then proceed by thinking about the target system in analogy to this familiar system (see ch. 5 using the Newlin-Phillips Machine).

Morgan's case-based approach shows its strength in the discussion of these four ways of building models: reading through chapters two to five, we get a clear impression of the peculiarities of each process.
Models as tools of investigation

The second half of the book moves from model building to model use. It is meant to answer my last question: how do models function as tools of investigation? Morgan works with a simple and useful distinction between a model and the system it denotes. (She often uses the terms “model world” and “real world” to mark this distinction—as I shall argue below, this is less helpful.) One handy point she makes regards the centrality of narration in model use. Once a model is built, questions can be asked about the model and answers to these questions can be derived by manipulating it. This process is accompanied by an ineliminable narration, which makes sense in economic terms of the question, the process of demonstration and the answer (ch. 6).

Demonstration through manipulation driven by questions about the system under study makes modelling a sort of experimentation (31). What does the economist learn through this experimentation? Most obviously, she learns about properties of the model. To put this point in terms of a standard account of propositional learning, the model user reaches true and justified beliefs about propositions regarding the model. For instance, by manipulating the payoffs in the matrix of the prisoner's dilemma, the model user finds out that the unique Nash equilibrium remains (defect, defect) if and only if two sets of inequalities among payoffs hold in this model (349). Similarly, a drug test on mice in a laboratory makes us reliably (yet fallibly) know propositions of the form: ‘this drug causes an average increase in life expectancy of \( x \) days in this sample of mice.’ An obvious question is whether and how this learning about the experimental system can reliably make us learn about other systems – e.g., some real situations of strategic interactions or the effect of the drug in a population of human beings. Morgan is right to point out that this question is about the issue of external validity, an issue that modelling faces along with all other experiments (282).

Before discussing Morgan's account of how models might make us learn about the systems they denote, let me take up an aspect of her account that is most imprecise. Many times in this book, the reader stumbles on words related to ‘theory’ and the author even states that “models work as theorizing instruments” (379, my emphasis). Unfortunately, I have to repeat a complaint voiced by Guala and Psillos (2001, 290) in their book review of Models as Mediators, a 1999 book edited by Morgan and Margaret Morrison: “we are not told what theories are and how they are different from models.” It is perplexing that, thirteen years later, Morgan relies again on the distinction without explicating it, while she notes that, “in practical terms, economists have collapsed the distinction between ‘theories’ and ‘models’ ” (394). Should we all follow suit and collapse the distinction? But if there is no distinction, the above claim about the function of models boils down to the uninformative statement ‘models work as modelling instruments’.

Perhaps we should avoid the general term ‘theory’ and simply be more precise about the various outputs of modelling. One such output that the book's case studies highlight is conceptual novelty (see esp. sec. 7.2). It is one thing to learn propositions about a system, it is another to learn concepts that can then be used to categorize different states of a system – i.e., conceptual learning. Much of the vocabulary used in contemporary economics has been developed or at least refined through modelling. There is thus no doubt that conceptual learning has taken place through modelling. Another output that Morgan stresses at times is the generation of conjectures (or hypotheses) about the denoted system (see esp. pp. 387-88). In a strict reading of hypothesis generation, the activity producing the hypothesis, here modelling, is no evidence for the hypothesis, it is only a cause of it being entertained.

Beyond conceptual learning and hypothesis generation, it is undeniable that economists make
inferential use of their models: they use propositions about their models as evidence for propositions about the denoted systems. The epistemological issue is whether they should. Morgan's way of approaching the issue is sound: modelling is experimenting. From this starting point, one might expect that she will deflate the epistemological issue by arguing that the inference from beliefs about the model system to beliefs about the denoted system follows the same fallible logic as any such movement from experimental system to target system. However, Morgan follows a different line in which she tries to drive a wedge between the inferential potential of models and other experimental systems (ch. 7). In the end, she claims, it all boils down to ontological differences:

It is because real experiments are made of the same stuff as the world that their epistemological power is greater: inference back to the world is likely to be easier and more convincing than for the case of model experiments where there is no shared stuff, no shared ontology of things and materials. (287, my emphasis)

Morgan intends this claim to apply both to research on social and natural systems and wants it to hold about a general difference between mathematical models and “experiments directly in the material of the natural or social worlds” (287fn). As such, it is false. Some mathematical models of physical systems have a higher epistemological power than lab experiments. For instance, although we should not be too confident about our current climate models, it would be careless to drop them and rather base our forecasts of global mean surface temperature in 2050 on experiments ran on a small-scale ‘real’ atmosphere created in a lab. Having ‘real’ molecules and ‘real’ light does not make the lab experiment a better inferential base than our climate models when our target system is the global atmosphere. There is indeed an ontological difference: mathematical models, in climate science and in economics, are not material while our systems of interest are. But this ontological difference does not imply the epistemological consequence that Morgan identifies.

Perhaps Morgan's faulty conclusion here sheds light on the limitation of her case-based method. The actual experiments that she discusses in chapter 7 – e.g., classroom simulations of markets – seem intuitively to be more reliable as evidence for propositions about our target systems than the models she discusses – i.e., standard supply and demand models. There is, however, a great leap from granting that experiments come out ahead in this case to drawing Morgan's general lesson. But Morgan's conceptual framework might also be to blame. She relies extensively on the distinction between the model world and the real world, instead of talking about an experimental system and a denoted system. Since lab experiments fall on the ‘real’ side of the model/real divide, they seem to be necessarily closer to the denoted system than models. But that might not be the case because, when it comes to the reliability of the evidential source, the fact that the source is material is not decisive.

The poverty of the model/real distinction combines with another conceptual imprecision in the book to generate a confusing, if not confused, argument. The imprecision is that Morgan often writes about knowledge, explanation and inference without specifying knowledge and explanation of what and inference to which propositions. The problematic argument is that scientists can be confounded by an experimental result, but they can only be surprised by a model result (293-96). Indeed, anyone having experience with modelling will recognize that some results of manipulating the system can clash with our prior beliefs about this model system. The same possibility exists with any experimental system. When Morgan says that experimental results from, say, lab experiments can do more than surprise – they can confound – she seems to mean that they are “unexplainable given the existing body of knowledge” (296). What is the difference with models? Surely, we can be unable, at first, to explain

3 See Frigg and Reiss (2008, 608–10) for a more detailed argument, with further examples, against this position of Morgan.
why some surprising truth about the model is the case. Contra Morgan, models can confound too. The only difference is that, with a model system, we can easily go back to our model and manipulate it again and again until we learn other properties of it providing the sought-for explanation for the result. In contrast, there are serious “limits on experimental manipulability” (295) for most lab experiments. Why our mice reacted in a specific way to a drug might remain unexplainable for long. Morgan recognizes this point – which highlights an epistemic advantage of models – but she nevertheless concludes, after an obscure argument (293-96), that lab experiments are epistemically superior. If only she had specified that, for both model experiments and lab experiments, the surprising truth is about the experimental system and not about the target system, the non-sequitur nature of her argument would have been blatant.

In sum, Morgan's *The World in the Model* is a great resource for historical material on models and the rise of modelling in economics. It also significantly contributes to our philosophical understanding, especially in the discussion of the various ways models are built. Yet, there are quite a few elements that Morgan leaves imprecise: how econometric modelling interacts with non-econometric models in the modelling science of economics, how we should understand the relationship between modelling and theorizing in economics, and what knowledge claims she is discussing at various junctures. These imprecisions are particularly damaging to her account of how models function as tools of investigation. Nonetheless, this book is a great scholarly achievement, and takes up like none before the challenging project of offering a detailed “account of how economics became, and works, as a modelling science” (xv).

François Claveau

*Université du Québec à Montréal, Canada*

REFERENCES


