

**A World of Models: Review of Mary S. Morgan,  
“The World in the Model: How Economists Work and Think”<sup>1</sup>**

by Itzhak Gilboa

**1. The aim of the book**

Mary Morgan’s new book is a fantastic resource for economists, philosophers of economics, and anyone who has an interest in the way economic research is conducted. Starting with François Quesnay’s *Tableau Economique*, dating back to 1767, and ending with modern-day game theory and computer simulations, the book seems to cover practically all the ways that economists ever conceived of “models.” These include simple diagrams such as Jevons’s utility curve or Edgeworth’s box, as well as actual constructions that were supposed to capture the workings of the economy, such as the Newlyn-Phillips machine. Models are conceived of as representations and narratives, analogies and metaphors, as well as worlds in their own right. Alongside the standard view of models in science, as approximations or idealized representations of reality, models can also be stories or parables, which help scientists think about reality. Thus Morgan puts a special emphasis on the *world in the model* – the fact that models have an existence of their own, and that modeling has become the thing that economists do – and the *model in the world* – the way models affect the world, sometimes being explicitly used in economic activity, such as in the pricing of options or in the design of auctions. Importantly, Morgan also highlights four different ways in which economists come to generate models: recipe-making, involving the integration of ingredients; representing, requiring imagination and making images; idealizing; and recognizing similarities.

Morgan starts her book by the assertion “Science is messy.” She argues that historians often attempt to shape the history they describe into a comprehensible narrative, one that has a beginning and an end, a point, perhaps even a moral.

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<sup>1</sup> I wish to thank Caterina Marchionni for many helpful comments.

By contrast, Morgan promises not to do any of that. She proclaims that her goal is to present a series of historical case studies. Surely, the cases will be annotated and interpreted, viewed in perspective and astutely analyzed. But these cases are not guaranteed to constitute a nice story that can be neatly summarized. Morgan warns us at the outset that such simplifications will not be found in the book.

The promise is kept. Throughout the book, Morgan manages to resist the temptation to put history in order, shape it up or trim it down to fit a mode that makes sense. Some readers might be frustrated by the absence of clear categorization of cases, delineation of periods, or identification of trends of development. But they should be reminded of the book's goal: to present illuminating case-studies.

## **2. Digression and background**

Much of (non-metaphysical) philosophy can be viewed as a social science: it deals with phenomena that occur in the human mind.<sup>2</sup> Questions such as, how do and how should we define "good" or "beautiful"; questions about our faith and the way we do science; questions about knowledge and language – are all about human mental activities. As such, these questions are often similar to the questions addressed by psychology and sociology.

The main distinction between these disciplines is that philosophy tends to be more normative (asking "ought" questions) whereas psychology and sociology – more descriptive (dealing with "is" questions). For example, social psychology might deal with ethical judgments actually made by people, whereas philosophy of ethics would deal with the way such judgments ought to be made. Psychology might document fallacies in reasoning, whereas philosophy would study how reasoning should be conducted. One might suggest clarifying our terms so that "psychology" and perhaps also "sociology" refer to purely descriptive questions,

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<sup>2</sup> This claim can be turned on its head to be used as a definition of metaphysics, dealing with philosophical questions that cannot, even in the final analysis, be translated into questions about how people do or should think.

whereas their normative counterparts are part of “philosophy.” For example, in the second half of the 20<sup>th</sup> century the philosophy of science shifted its emphasis from normative to descriptive questions. Indeed, this shift was often described as a change from “philosophy of science” to “sociology of science.”<sup>3</sup>

In any event, if we set aside the descriptive/normative distinction, branches of philosophy and social science do not seem to be that different. In particular, philosophy of science is particularly close to social sciences. It is therefore tempting for a social scientist to think of philosophy of economics along similar lines to the way one thinks about economics. Economic history offers a set of case-studies, which theorists attempt to organize and classify. Similarly, the history of economic thought describes cases, as does Morgan in her book, which a theorist might attempt to categorize and characterize.

Being an economic theorist, I have been conditioned (and possibly also self-selected) to prefer elegance over accuracy; insight over detail. Thus, when I read Morgan’s book, I find it hard to resist offering a few ways in which one may classify the use of models in economics over the years. True to the tradition of economic theory, the classifications I suggest should be viewed only as potential ways of organizing the case studies. The possible classes discussed are not supposed to be part of a grand theory.<sup>4</sup>

### **3. Models**

The term “model” is widely used in science, art, engineering, architecture, and everyday life. With no attempt to exhaust the possible senses of the word, one may distinguish among the following three notions:<sup>5</sup>

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<sup>3</sup> Clearly, the institutional distinction between philosophy and social science – as defined by the scope of academic departments, journals, and the like – is not that neat.

<sup>4</sup> In this sense, the philosophy of economics is at times more case-based than rule-based – as is the case with economics itself. See Section 4 below.

<sup>5</sup> In the following, I refer to the ways models are used, or intended to be used, as opposed to the ways models are created, as discussed by Morgan. That is, Morgan’s four categories refer to the way economic researchers think in generating models, whereas this section deals with the way people, such as applied economists, are presumed to think when applying these models.

1. An example to be copied/ imitated In this sense, a specimen is considered to be sufficiently successful to be replicated. This is probably the meaning used in the first appearances of “models” in ancient Greece (though Greek has several different words that are translated into “model”). According to this view, the model is, in some sense, superior to its copies. In modern language we have this meaning in mind when we say that a person can serve as a role model to young children, or when we say that one country has fashioned its legal system after the model of another’s.
2. A representation, or approximation of the “real thing” This is the usage of models in much of science and engineering, as well as in architecture. In this sense, the model is not real, and it often lacks important aspects of the “real thing.” For example, a model in architecture might be a maquette, which is smaller than the building one intends to build, and lacks most of the details that the real thing would have. An engineer who uses a wind tunnel to test the design of an airplane may hope that her model doesn’t miss any important details, but, again, the model is smaller, and less important than the real device one is planning to construct. In science a mathematical model involves many abstractions of reality, in the hope that the details dropped from the model are less essential than those represented in it. In all of these usages, the model is, in a sense, inferior to the reality it models. This might also be the usage of “models” in the visual arts in previous centuries: a painter who paints the Madonna in a biblical scene may hire a “model” to sit and be drawn, where the model is a mere representation of the “real thing.”
3. A test of consistency This is the meaning of a “model” in formal logic: one starts with propositions, and in order to prove that a set of axioms is not self-contradictory, one proves that the axioms have a “model”: a mathematical entity to which the propositions can be mapped. If the axioms are shown to hold in the model, their consistency is proved.

It is an interesting phenomenon that a model may switch meaning over years of usage. For instance, consider fashion models. The profession probably originated in the mid-19<sup>th</sup> century, and the models – women whose job was to show to potential customers what certain cloths might look like on them – were not highly regarded. They were probably only a representation of the real thing, namely, the customer who might actually buy and wear the outfit under consideration. Yet, today one observes the phenomenon of “top-models”, who are cultural icons, exemplary specimen to be imitated by others. Thus, the fashion industry has started with models in sense (2) and, to some degree, switched to sense (1).

The term “model” in economics might have also gained more than one meaning over recent decades. The standard view of science is along the lines of definition (2): a model is an approximation or a representation of economic reality; it need not be an exact copy of reality, for such a copy would provide no new insights; rather, it should abstract from some of the less important details to focus on the major ones; it can be used to make calculations that would, hopefully, be good predictions of economic reality, and so forth. However, when one observes modern economic theory, one is often surprised at how unrealistic the models are. In my youth, I used to raise such concerns, and often I received the reply, “Well, the assumptions are indeed unrealistic; yet, there is some value in testing whether one can explain a certain phenomenon with our standard assumptions.” That is, the defense of economic models that was suggested to me was along the lines of definition (3): ask not, are the assumptions a good approximation of reality; ask, are the assumptions consistent with certain phenomena one observes.

Morgan’s book does not focus on the role of models as mere tests of consistency. Indeed, it is possible that this definition of models is not a very significant development in the history of economics at large. Yet, economists and philosophers of economics may use Morgan’s historical perspective and ask, when and how did economic models start having the third meaning, on top of the

second? Is this practice sustainable? Is it to be frowned upon? Does it happen in other sciences?

#### **4. Case-based and rule-based reasoning in economics**

There seems to be a consensus that there is something unique, not to say bizarre, about the use of models in economics these days. The models use assumptions that are inaccurate, to say the least. Sixty years ago, Milton Friedman already felt the need to defend economic models by saying that the veracity of the assumptions is not of great import (Friedman, 1953). While Friedman's main defense was that the theory makes good predictions, many would argue that economics has not been tremendously successful at generating quantitative predictions. Moreover, much of modern economic theory seems rather abstract, and often models are presented without quite saying what they are modeling, that is, what is their scope of applicability. Yet, economists feel that they learn a lot from these models, and the puzzle is, what and how do economists learn from such models? Should they be trusted that such models are truly useful, despite appearances?

Gibbard and Varian (1978) argued that models need not be paintings or drawings of reality; rather, models may also be caricatures thereof. (See also Section 4.7 in Morgan's book.) Thus, they claimed, models can be useful even when their discrepancy with reality is intentionally exaggerated. Cartwright (1998, 2009) argues that models are used to identify capacities. Sugden (2000, 2009) pointed out that economic models are often published without too many hints about their scope of applicability. He argued that they are useful because they are "credible worlds," which allow one to draw conclusions about reality by means of induction. In particular, Sugden, as well as Morgan (see Chapter 9) suggest that one use of models is as sources of analogies. Gilboa, Postlewaite, Samuelson, and Schmeidler (2013) relate this notion to case-based reasoning in psychology and artificial intelligence, as well as to data-based methods (such as kernel estimation and nearest-neighbor techniques) in statistics and machine learning.

They argue that several puzzles in the sociology of economics can be explained by the view that models are used as tools for case-based, rather than rule-based reasoning. That is, economists who propose models do not purport to find general regularities, but only to report on theoretical cases that they have analyzed, and that may bear some similarity to real economic problems. The common practice is to offer such models without saying how the reader should judge their similarity to real-life problems. Such models come without a “user’s manual” that may help the reader in applying them in an algorithmic way. As a result, one may argue that the development of such models doesn’t qualify as “science” in the sense of generating refutable predictions: as long as the similarity function remains unspecified, the models can be viewed only as setting the stage for subsequent scientific predictions. Developing such model is a “pre-scientific” activity.

Morgan refers to case-based reasoning (using these very words), and suggests (in Chapter 9) the terms “typical cases” and “exemplary narratives.” Indeed, Gilboa et al. may be viewed as offering a formal model of this type of scientific reasoning, relating it to models in psychology and to formal techniques of statistical learning. However, it is not clear when and how the practice of case-based pre-scientific reasoning developed in economics. Is Edgeworth’s Box an example of case-based reasoning? And, if so, can the same be said of a physical model of a ball rolling on a frictionless one-dimensional surface? Does economics differ from other sciences in this respect?

## **5. Economics: science and critique**

Among academics as well as among laypeople, the standard view of economics is that it is a science that should be able, first and foremost, to make predictions. Providing explanations is important, but typically one expects a scientific discipline to do more than explain facts ex-post.<sup>6</sup> Many view economics also as a normative science, and therefore expect it to provide recommendations – but

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<sup>6</sup> The meaning of explanation and the distinction between explanations and descriptions will not be discussed here.

these also rely on predictions. Whichever way we look at it, predictions are essential to science. However, the success of economics in generating predictions is mixed. Clearly, there are many instances in which economic analysis yields qualitative predictions, providing robust insights that allow us to predict trends, compare economic systems, and so forth. Yet, economics is not considered to be a successful science when quantitative predictions are concerned.

There is, however, another view of economics, by which it can have other successes: it is a field of enquiry whose goal is to critique reasoning about economic phenomena.<sup>7</sup> For instance, suppose that a politician intends to raise taxes on consumption of a good, and calculates the predicted revenues based on the new tax rate and the quantities consumed. An economist might say, “Well, you’re not taking into account the new equilibrium that would emerge as a result of the new tax rate: once the good is more expensive, you should expect it to be consumed at a lower level, and the total effect on tax revenue is not clear.” In this type of situation the economist might provide useful advice without necessarily making any quantitative prediction. Her job in this case is mostly to examine and critique someone else’s reasoning.

Critiquing arguments and reasoning can take many shapes and forms. Pure logic is a critique of impure reason: sometimes, an argument is simply logically flawed. Alternatively, empirical work can also be useful for critique. For example, if someone argues that “It is well-known that higher growth rates are accompanied by higher levels of inequality,” a relevant comment might be “well, I know the theory, but I’ve read many empirical papers on this problem and I can’t say that the bottom-line conclusion is very obvious.”

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<sup>7</sup> Casual observation suggests that very few economists are willing to subscribe to the view I present here. Most economists would certainly agree that economics can be successful in critiquing reasoning, but they would stress that economics is successful also as a much more ambitious endeavor, in particular in explaining phenomena, providing a paradigm to think about them, and even in generating predictions.



In this sense, empirical work is useful for critique in the same way that history is. Indeed, the analogy between economic and historical facts can sometimes become an identity. If, for instance, someone comes up with a great new idea that we abolish private property, and suggest that everyone work as hard as they can and consume as little as they need, we would hope that someone – an economist or a historian – would be able to raise their hand shyly and say, “Oh, I seem to recall that this has been tried before and the results were not quite in line with expectations.” In this case the study of history, or of economic history, is useful not necessarily because it generates quantitative predictions, but because it allows one to find weaknesses in certain arguments.

History is a respectable academic discipline. It is commonly believed that it is essential to study history, though it is not a science in the sense of generating quantitative predictions. Economics gets quite a bit of criticism regarding its ability to make predictions – criticism that history is spared. It is possible that, were economics to position itself among the “critique disciplines,” it would be considered as successful as is history.

When economics is viewed as a science, models are expected to help in generating predictions. But when it is viewed as a critiquing discipline, their role is quite different: it is to test whether other predictions make sense. Morgan’s book describes a history of models, all of which could be used – and most of which have been used – as tools of critique. The historical perspective raises the question, has the balance between prediction and critique changed over the past decades?<sup>8</sup>

## **6. Conclusion**

Morgan offers us a fantastic, erudite, and enlightening historical analysis of the use of models in economics. She makes a conscious decision to stay away from over-arching theories, and analyzes cases as such. Her rich book invites

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<sup>8</sup> Observe, again, that the focus of my question is on the way models can, or should be used, rather than on the way economists come up with models.

philosophers and economists to ask when, how, why, and to what extent have models begun to (i) be consistency tests, and not only approximations or representations of reality; (ii) serve as tools for case-based, and not only rule-based reasoning; and (iii) provide the conceptual basis for critique, and not only prediction, as well as to ask whether these developments are specific to economics, and to what extent they are desirable.

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