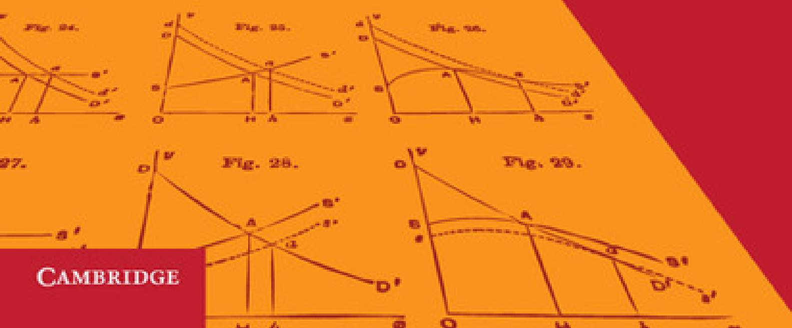
MARY S. MORGAN

THE WORLD IN THE MODEL

How Economists Work and Think



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How Economists Work and Think

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For Charles, and for Dori

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Modelling as a Method of Enquiry

PART I: CHANGING THE PRACTICE OF ECONOMIC SCIENCE

1. From Laws to Models, From Words to Objects

Two hundred years ago, political economy was overwhelmingly a verbal science, with questions, concepts, and a mode of reasoning all dependent on words. As a *science*, classical political economy of the eighteenth and early nineteenth centuries began with individuals, theorized their relations, and posited a few general laws that operated at a community level. One of the few laws that was expressed in mathematics was proposed by the Rev'd Thomas Robert Malthus, who claimed that the growth of population, driven by passions, increased in a way that would inevitably outstrip the more pedestrian growth of food supplies. So, he argued, there must also be checks at work in the world: the numbers of people were kept in check either through the vices of disease, famine, and war, or by virtue of celibacy or delayed marriage. While such laws might indeed have an iron grip on economic life, it was not thought easy to perceive these laws at work amongst the complicated changing events of everyday life. This created difficulties for the *art* of political economy, namely fashioning policy in line with an understanding of those scientific principles of political economy.[1]

Economics is now a very different kind of activity. From the late nineteenth century, economics gradually became a more technocratic, tool-based, science, using mathematics and statistics embedded in various kinds of analytical techniques.[2] By the late twentieth century, economics had become heavily dependent on a set of reasoning tools that economists now call ‘models’: small mathematical, statistical, graphical, diagrammatic, and even physical objects that can be manipulated in various different ways. Today, in the twenty-first century, if we go to an economics seminar, or read a learned scientific paper in that field, we find that economists write down some equations or maybe draw a diagram, and use those to develop solutions to their theoretical conundrums or to answer questions about the economic world. These manipulable objects are the practical starting point in economic research work: they are used for theorizing, providing hypotheses and designing laboratory experiments, they are an essential input into simulations, and they form the basis for much statistical work. Economics teaching is similarly bounded: students learn by working through a set of models: some portraying decisions by individuals and companies, others representing the behaviour of the whole economy, and for every level in between. The use of economic models has become habitual in government policy making, in trading on financial markets, in company decisions, and indeed, anywhere that economic decisions are made in a more technocratic than casual way. In economics, as in many other modern sciences, models have become endemic at every level.

The significance and radical nature of this change in economics is easily overlooked. The introduction of this new kind of scientific object – models – involved not just the adoption of new languages of expression into economics (such as algebra or geometry), but also the introduction of a new way of reasoning to economics. And having moved from a verbal to a model-based science, economists no longer depicted their knowledge in terms of a few general, though unseen, laws,

1 Nineteenth-century economists often used the term “principles” in the titles of their treatises on political economy. This term denoted both their theories and analysis of law-like elements in the economic system as well as the appropriate means of good governance (which might have an ethical, even moral, quality). For example, Malthus’ laws of population were almost laws of nature (they were based on individual instincts of passion and the need for food, empirical data on population growth, and hypothesized claims about likely growth of food output), while his policy arguments were designed around his understanding of these laws (for example, he was against social security schemes which, in the process of supporting the poor, would interfere with the natural checks on population growth operating within the system see Malthus, 1803).

2 For the twentieth-century development of economics into a tool-based science, see Morgan (2003a).

but expressed it in a multitude of more specific models. As models replaced more general principles and laws, so economists came to interpret the behaviour and phenomena they saw in the economic world directly in terms of those models.[3]

Despite the ubiquity of modelling in modern economics, it is not easy to say how this way of doing science works. Scientific models are not self-evident things, and it is not obvious how such research objects are made, nor how a scientist reasons with them, nor to what purpose. These difficulties of definition and understanding are exhibited in a most concrete fashion in an example that may well be the first such economic model in the history of the field.

The *Tableau Économique* is a wonderful numerical object: a cross between a table and a matrix, it presents an accounting portrait of the French economy (Figure 1.1). It shows the classes of people in the economy (farmers, manufacturers, and landowners) and has a zig-zag pattern of horizontal and diagonal lines between them with numbers on them indicating the amount of goods or money being transferred between the groups of people. It was invented in the late 1750s by François Quesnay, an economist, and physician in the court of Louis XV and thus at the centre of French political life in the mid-eighteenth century.[4] He treated the *Tableau* as a research object, using it to conduct various numerical exercises to explore the possibilities for the French economy to grow via agricultural investment and the subsequent circulation of the surplus created from Nature around the classes of people in the economy. In these exercises, various numbers for the agricultural surplus and the amounts circulated in the zig-zags were inserted, and then added downwards to determine whether such an economy would grow in a stable, balanced way, or if there was some lack of balance in the relations.

The *Tableau Économique*, as one of the earliest models in economics, makes a fine example to introduce a book on models, for it is one of the most celebrated in the history of economics. It can be regarded as the great-grandfather of models in many different economic traditions even while its own content and meaning remain somewhat mysterious. Two hundred and fifty years later, most modern economic models lack the decorative borders (and the dot-matrix qualities that make it look like a needlework sampler hanging on the wall), but are otherwise not so different. Models in economics are still mostly pen-and-paper objects depicting some aspect of the economy in a schematic, miniaturized, simplified, way. The most important point to note about this object, however, is that it was not simply a passive portrait of the economy; rather, it had the internal resources for Quesnay to

3 So, by the early twenty-first century, we find, for example, an account in which financial traders acting on models make markets behave like those models (demonstrating the performativity of economic models; see MacKenzie, 2006), and we find economists in newspaper columns explaining the phenomena of ordinary life by verbally reinterpreting those events as examples of these small worlds depicted in economic models (e.g., Harford, 2008, or Levitt and Dubner, 2005 and their columns in the *New York Times* and the London *Financial Times*). I return to this point in Chapter 10.

4 Examples (for there are several) of Quesnay's *Tableau* are found in Kuczynski and Meek (1972) and in Charles (2003) who discusses the development of the diagram.

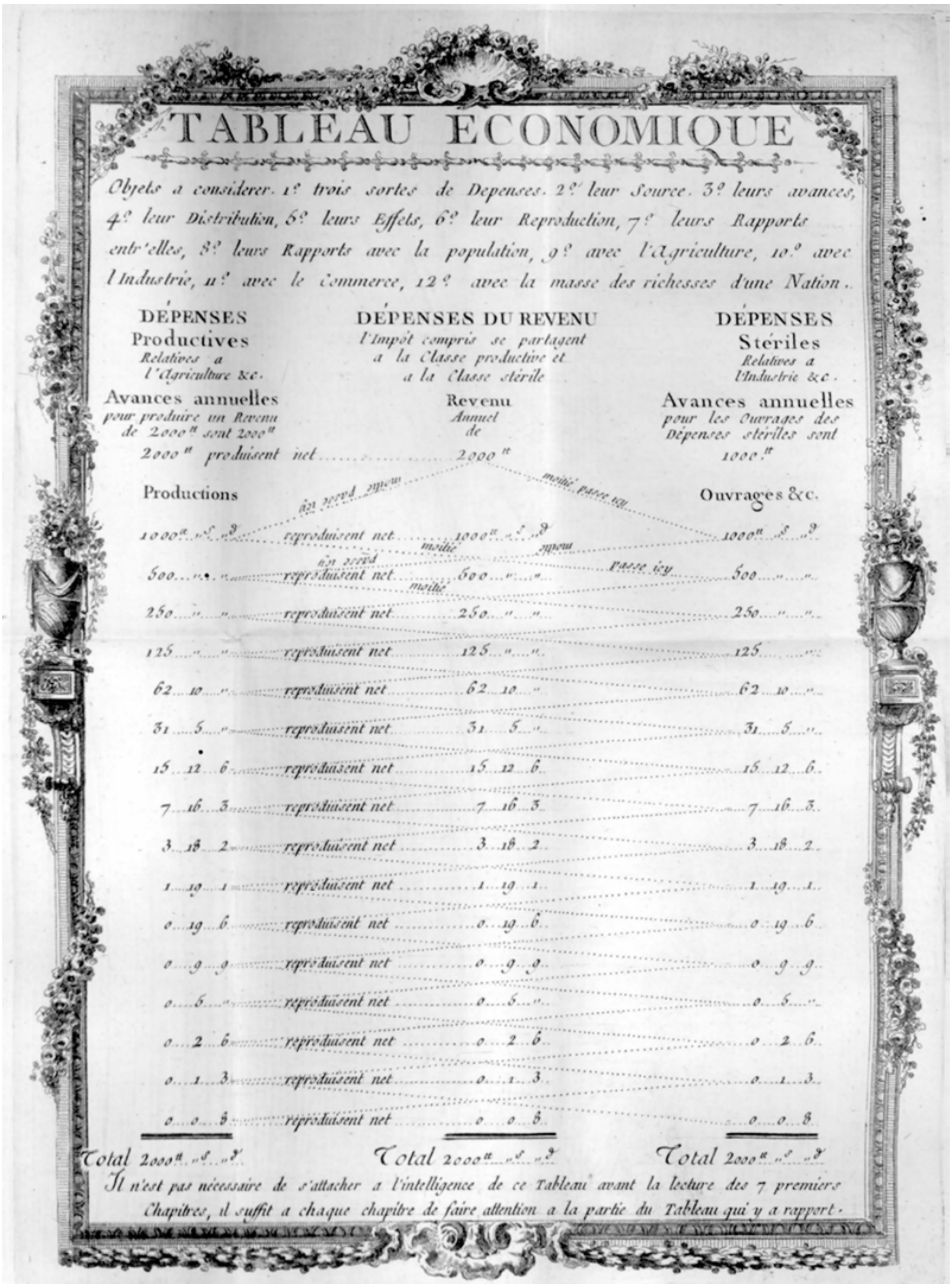


Figure 1.1. Quesnay's *Tableau Économique* (1767).

Source: Private collection. (Reproduced in Loïc Charles [2003] "The Visual History of the *Tableau Économique*". *European Journal of the History of Economic Thought*, 10:4, 527–50, 528.) Reproduced here with thanks to Loïc Charles.

investigate (by his arithmetic exercises) how such an economy as he depicted might work. It is this possibility for manipulation that turns such pictures into models for the economist.

It is also telling that Quesnay's contemporaries found the *Tableau* as difficult an object to interpret and use as do present-day economists. It is very hard for modern economists to understand how the different parts of the *Tableau* relate to each other, or to the economy he inhabited, and to reconstruct exactly how Quesnay reasoned using the object, without the evidence uncovered by historians to explain these things to us.[5] And if we think about how Quesnay might possibly have invented this research object, we can also appreciate that an imaginative and creative mind must have been at work. Such difficulties point to the cognitive and contingent aspects of models: they are objects that embed theoretical and empirical knowledge that later economists will not automatically be able to extract and articulate again, just as non-economists cannot read or use modern economic models without considerable training in the field.

Quesnay's *Tableau* is surely a special object, unique perhaps in its day, but its very specificity raises a number of questions that need answering. If such research objects are so specific to time and place, and if we need to know a great deal about their particularities to see how they work, then how can we characterize the scientific practice of modelling in a general way? This raises philosophical questions: 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?

The pioneer status of Quesnay's *Tableau* equally raises general historical questions. For while economists now find making and reasoning with such objects the natural way to do economics, we do not have a good account of how that happened, nor understand how it could make such a difference to economics as a science. Reasoning with models is a cognitive process by which economists acquire their knowledge and use it.[6] Sometime in the past, economists had to begin to think with such objects, and learn how to gain knowledge of economics with them, if later generations of economists were to come to reason easily with them and take it for granted as the method they should use.

That process of change: from economists reasoning with words to reasoning with models, is what this book is about. The historical and philosophical aspects of that change cannot be easily untangled. At the meta level, we can point to the considerable but gradual historical shift in the way economists reason, involving elements

5 For recent scholarship that investigates the likely sources of the *Tableau*, its various versions, and how it was used, see particularly Charles (2003) and Van den Berg (2002).

6 Nancy Nersessian (from her 1992 paper to most recent 2008 book) has been instrumental in connecting the literatures of cognitive science with that of the philosophy of scientific modelling. (See also, for examples of different approaches using this connection, papers by Gentner, by Vosniadou, and by Giere in Magnani and Nersessian, 2001.)

of both cognition and imagination that made a big difference to the epistemology of economics, that is, to how economists come to know things in economics. But to understand and appreciate fully the import of these changes, we need to look at the micro level, at the objects themselves. When we look at that level, we find we cannot understand how economists learn things from models without understanding how models are used, nor understand how they are used without understanding how they are built. But why a particular model is built, what questions it is designed to answer, and what uses it is put to, are historically contingent. History and philosophy cannot easily be pulled apart, and the cognitive and imaginative aspects of modelling prove equally sticky in figuring out how economists make and reason with models. These issues – philosophical and historical, involving elements of reasoning and imagination – are explored in the book through the investigation of a number of models of considerable significance, and long life, in the history of economics. It is by paying careful analytical attention to how these small objects are made and used in economics that we can understand the import of the big changes in economics. They provide the materials for both a naturalized philosophy of modelling in economics and a historical account of the naturalization of models in economics.[7]

2. The Naturalization of Modelling in Economics

Though the important historical and philosophical changes in economics are difficult to understand separately, a broad chronology for the historical development of modelling over the last 200 years can be outlined. There are three moments of time that are important. To begin with, we can find a few isolated examples of models in the late eighteenth and early nineteenth centuries and so call this period the prehistory. We then find, in the late nineteenth century, a first generation of modellers: a very few economists who regularly made and used such research objects. The second generation of modellers, the real developers of the method of models, emerged during the interwar period. Modelling then became widespread through economics only after the mid-twentieth century.

To make this history more concrete, and to get a real feeling for what these research objects are, I introduce a number of significant examples here. If we begin with the 'prehistory' of models, we find that not only does Quesnay's *Tableau Économique* exist as an object out of its time in the eighteenth century, but there

7 It is appropriate here to refer to three parallel investigations. Nersessian (2008) comes to the topic of 'model-based reasoning' from cognitive science and philosophy of science, and combines mental models, narratives, experiments, and reasoning in her account of the history of physics. Ursula Klein (2003) uses history and philosophy of science and semiotics to explore the nexus of paper tools, models, and experiments that created a shift of scientific reasoning and practice in chemistry (see also Klein, 2001). Their two accounts share many of the elements of my own project for economics, though we have put them together in somewhat different ways. Meli (2006), in another parallel, discusses how the science of seventeenth-century mechanics depended on reasoning with objects.

(a)

Now if no rent was paid for the land which yielded 180 quarters, when corn was at 4*l.* per quarter, the value of 10 quarters would be paid as rent when only 170 could be procured, which, at 4*l.* 4*s.* 8*d.* would be 42*l.* 7*s.* 6*d.*

20 qrs.	when 160 were produced, which at	£4	10	0	would be	£90	0	0
30 qrs.	150	4	16	0		144	0	0
40 qrs.	140	5	2	10		205	13	4

Corn rent[1] would increase in the proportion of {100, 200, 300, 400} and money rent in the proportion of {100, 212, 340, 485}

(b)

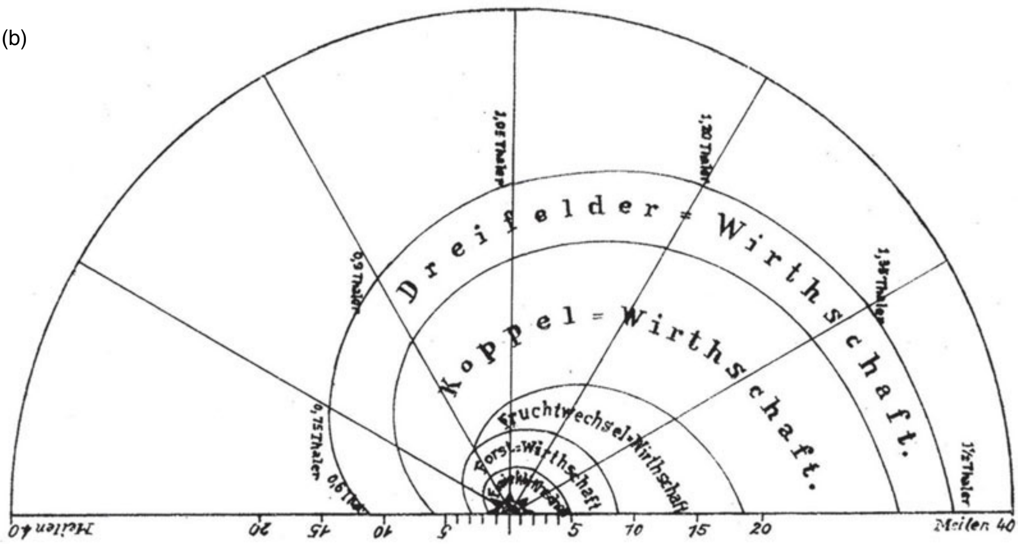


Figure 1.2. The Prehistory of Models.

(a) Ricardo's Farm Accounting (1821).

Source: Piero Sraffa: *The Works and Correspondence of David Ricardo*. Edited with the collaboration of M.H. Dobb, 1951–73. Cambridge: Cambridge University Press for The Royal Economic Society. Vol. I: *Principles of Political Economy & Taxation*, 1821, p. 84. Reproduced by permission of Liberty Fund Inc. on behalf of The Royal Economic Society.

(b) Von Thünen's Farming Diagram (1826).

Source: Johann Heinrich von Thünen, *Der isolierte Staat in Beziehung auf Landwirtschaft und Nationalökonomie*, Hamburg, 1826. Reprinted facsimile edition 1990. Berlin: Academie Verlag, p. 275.

are also very few further cases in the early nineteenth century. One is provided by a table of farm accounts developed by the English economist David Ricardo (1821) to explain how income gets distributed in the agricultural economy (one element of his table is shown in Figure 1.2a). Another is the diagram (in Figure 1.2b) of farm prices in relation to distance from towns, drawn by the German agriculturalist Johann von Thünen (1826), depicting an idealized abstract, landscape but with numbers drawn from his experience of farming at his own estate of Tellow.[8] These three objects – the *Tableau*, the accounting table, and the spatial diagram with numbers – each designed to show how the agricultural economy worked, jut out awkwardly from the sea of words that surround them in this early period of economic science.

In the late nineteenth century, we begin to see more regular occurrences of these objects we are calling models, but we may also notice that the few economists involved felt they had to justify their creation and usage of these odd research objects that they had invented to help them in their analysis. They did not yet have the concept or label of models and were indeed quite self-conscious about this activity. Three important examples epitomise this first generation of models and modellers and their understanding of the role of models. In 1879, the British economist Alfred Marshall began to draw little diagrams to explain more clearly how two countries trade with each other, in this case the curves depicting the offers of German iron for English cloth and vice versa as relative prices change (Figure 1.3a).[9] Marshall thought that such diagrams were useful if they could be illustrated with examples from economic life (and then he often presented them in his footnotes), but that if such pieces of mathematics were not useful, they should be burnt! In 1881, the Irish economist Francis Edgeworth outlined a somewhat different diagrammatic perspective on exchange relations (Figure 1.3b) to figure out the range of possible contracts that Robinson Crusoe might strike with Man Friday to gain his help in cultivating their island economy. Not being sure how to refer to this way of reasoning, he labelled his analysis with the diagram as offering a "representative particular" argument (see Chapter 3). In 1892, Irving Fisher, an American economist, designed and constructed a hydraulic mechanism to represent, explore, and so understand the workings of a mini-economy, one with only three goods and three consumers (Figure 1.3c).[10] He accompanied this work with an outright

8 Von Thünen's original contribution appeared in 1826; an English translation of part of the study became available in 1966, with a useful introduction. On different interpretations of his modelling project, see Judy Klein (2001, pp. 114–6), who reproduces his diagram and discusses it as a measuring device, and Mäki (2004), who analyses it as a theoretical model.

9 This was the first appearance of these curves in the history of economic theorizing about trade relations, on which Humphrey (1995, p. 41) comments: Marshall "by crystallizing, condensing and generalizing earlier insights into a powerful yet simple visual image" was able to create an object that made these relations "transparent". Marshall's 1879 diagrams and discussion were finally published in an edition of his early works edited by Whitaker in 1975, and this diagram provided the logo for the Charles Gide conference at which some parts of this paper were first presented. Marshall's views of mathematics are discussed by Weintraub (2002).

10 Fisher's thesis of 1891 was published in 1892 and republished in 1925, displaying a photograph of the mechanism in the frontispiece labelled "model of a mechanism".

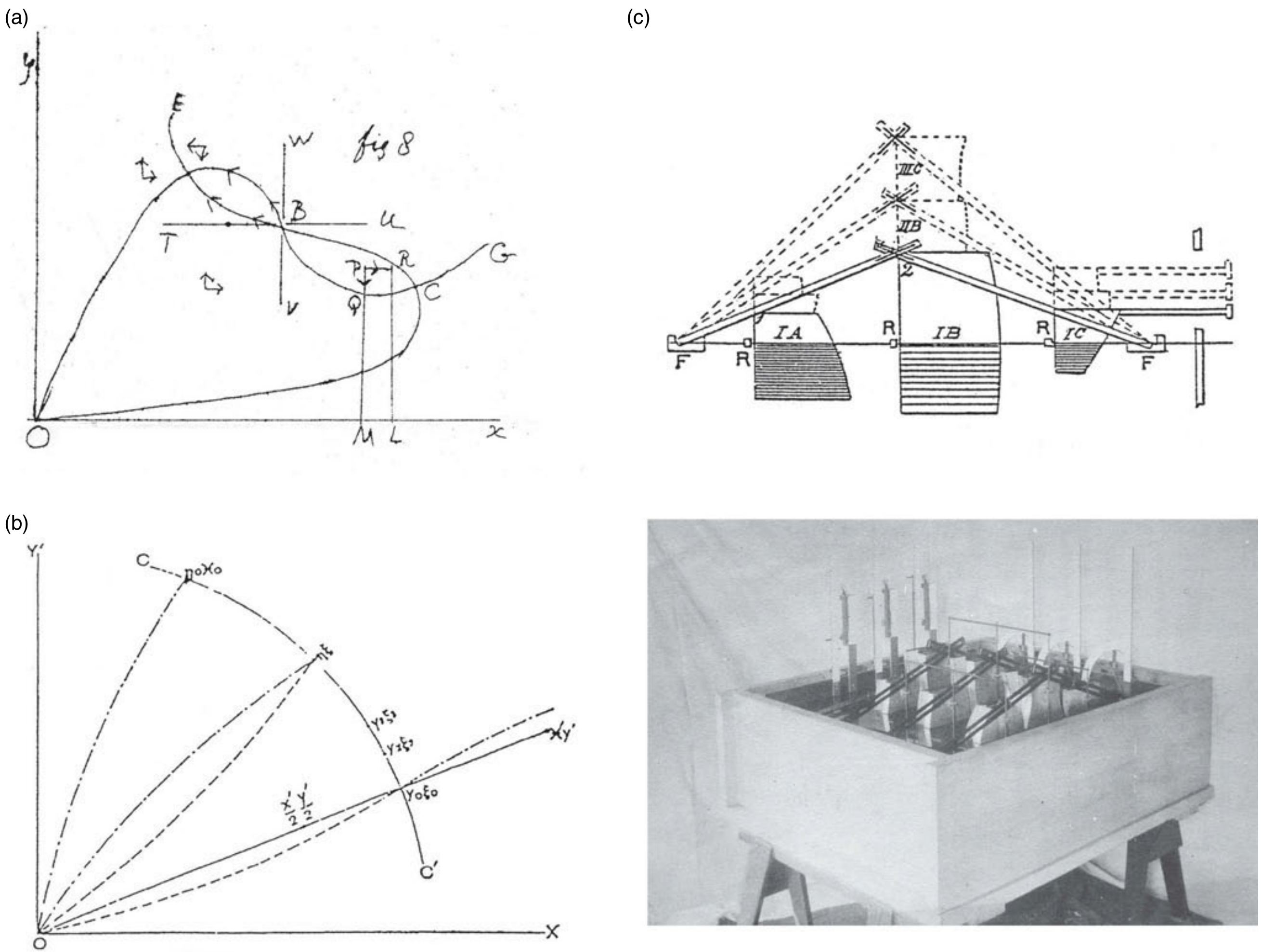


Figure 1.3. First-Generation Models.

(a) Marshall's Trade Diagram (1879).

Source: Alfred Marshall, "Pure Theory of Foreign Trade". Privately printed 1879, Figure 8, Marshall Library, Cambridge. (Reprinted, London: London School of Economics and Political Science Reprints of Scarce Tracts in Economics, No. 1, 1930). Reproduced with thanks to Marshall Library of Economics, Cambridge.

(b) Edgeworth's Exchange Diagram (1881).

Source: F. Y. Edgeworth, *Mathematical Psychics*. London: C. Kegan Paul & Co., 1881, Figure 1, p. 28.

(c) Fisher's Hydraulic Machine and Its Design (constructed 1893 from 1892 design).

Source: Irving Fisher, *Mathematical Investigation in the Theory of Value and Price*. Thesis of 1891/2. New Haven: Yale University Press, 1925. Frontispiece and Figure 9 on p. 39. Reproduced with permission from George Fisher.

defence of these research objects – mathematical, graphical, and real machines – that he designed and used for his economic analysis.

It seems reasonable to locate these three economists in the first real generation of model-makers, and their self-consciousness about their research objects as indicative of this moment of change. This late nineteenth century moment was noticed later on by Arthur Pigou in 1929, who cleverly understood the diagrams and equations we see in these examples as "tools", labelling Edgeworth as a "tool maker" and Marshall as a "tool maker and user". For Pigou, these objects were "pieces of analytic machinery", "thought-tools", or even "keystones".[11] And because economics is now dependent upon such research objects, all of these examples can today be understood as models, though, neither in the prehistory period, nor in this late nineteenth century moment, would economists have recognised them as such or used the label.

It was in the 1930s that economists really 'discovered' the idea of models. It was in that decade that these objects became conceptualized, gained the label 'model', and a fuller understanding of their usefulness developed. Two economists played an important role in this transformation, thus sparking the wider deployment of the label, notion, and usage of models in economic analysis. In 1933, in the depths of the Great Depression, the Norwegian economist Ragnar Frisch developed one of the first mathematical models of the business cycle. Because it had certain features, particularly the possibility to simulate a cyclical pattern, Frisch's "macro-dynamic system" created a new recipe for future business cycle models (see Boumans, 1999 and Chapter 6, this volume). As a recipe, it formed the basis for the first econometric model of a whole economy, built by the Dutch economist Jan Tinbergen in 1936 (1937), to see how to get The Netherlands out of the Depression. This object embedded a theory of the business cycle into the mathematical form, along with statistical information from the Dutch economy in the numbers (or parameters) of the equations. These two economists won the first Nobel Prize for Economics in 1969 for this model-based research; one of Tinbergen's model equations and a schema (from his slightly later US model of 1939) are shown in Figure 1.4a, while Frisch's model is shown later in Figure 1.6.

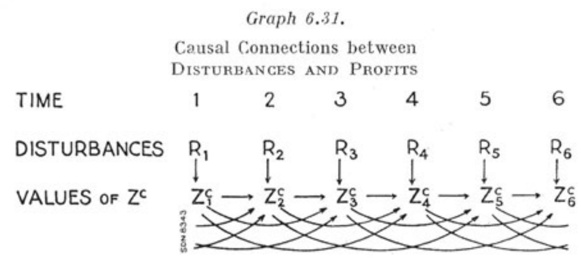
Tinbergen was also largely responsible for transferring the term 'model' in the early 1930s from physics, where it had usually referred to a material object, into economics to refer to the statistical and mathematical objects that he and Frisch were then developing.[12] So by the middle 1930s, the label 'model' had come into use,

11 See Pigou's lecture of 1929 (in his 1931), particularly pp. 2–8. Joan Robinson (1933) is more usually noted for introducing the notion of the "tool box of economics", but she was following Pigou, whose discussion, and prose, is more effective. Pigou's idea of tools was quite broad – it included not just models, but also the concurrent development of mathematical and statistical techniques. I return to the issue of "keystones" in Chapter 10.

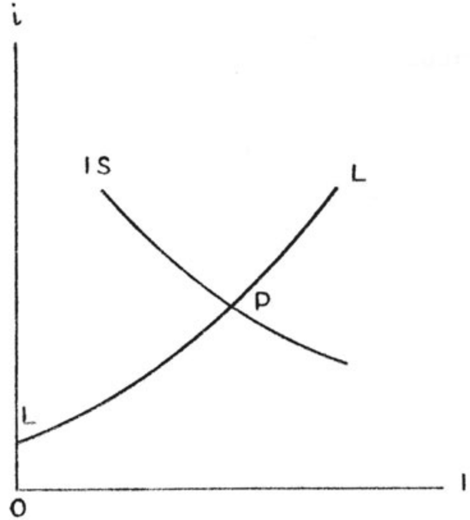
12 Ludwig Boltzmann had defined the term 'model' in the sense of a material object model, in what has become one of the classic articles on models in the 11th edition of the *Encyclopaedia Britannica* (1911). Boltzmann there provides a good view of nineteenth-century scientists' sense of the word. Boumans argues that it was Ehrenfest who probably broadened the scope to apply to mathematical objects, and since Tinbergen was his assistant in the mid-1920s, this is a likely route for the transfer of the term into economics (see Boumans, 2005, chapter 2) though there are also scattered uses of the term by other economists in the 1920s.

(a)

$$Z_t^c = e_1 Z_{t-1}^c + e_2 Z_{t-2}^c + e_3 Z_{t-3}^c + e_4 Z_{t-4}^c + (\mathrm{AU} + \mathrm{HO} + \mathrm{F} + \mathrm{R})_t \quad (6.31).$$



(b)



(c)

The national income at time t, Y_t, can be written as the sum of three components: (1) governmental expenditure, g_t, (2) consumption expenditure, C_t, and (3) induced private investment, I_t.

$$Y_t = g_t + C_t + I_t.$$

But according to the Hansen assumptions

$$C_t = \alpha Y_{t-1}$$

$$I_t = \beta[C_t - C_{t-1}] = \alpha\beta Y_{t-1} - \alpha\beta Y_{t-2}$$

and

$$g_t = 1.$$

Therefore, our national income can be rewritten

$$Y_t = 1 + \alpha[1+\beta]Y_{t-1} - \alpha\beta Y_{t-2}.$$

Figure 1.4. Second-Generation Models.

(a) Tinbergen's US Econometric Model: Equations and Causal-Time Process.

Source: Jan Tinbergen, *Business Cycles in the United States of America, 1991–1932*. League of Nations Publications, Series II, Economic and Financial, 1939 II.A 16; Equation 6.31 on p. 137 and graph 6.31 on p. 138. Reproduced with permission from Stichting Wetenschappelijke Nalatenschap Jan Tinbergen.

(b) Hick's IS-LL "Little Apparatus".

Source: J. R. Hicks, "Mr. Keynes and the 'Classics'; A Suggested Interpretation" *Econometrica*, 5: 2 (Apr., 1937), pp. 147–159; Figure 1, p. 153. Reproduced with permission from The Econometric Society.

(c) Samuelson's Keynesian Model.

Source: Paul A. Samuelson, "Interactions between the Multiplier Analysis and the Principle of Acceleration". *The Review of Economics and Statistics*, 21:2 (May, 1939), pp. 75–78; text and equations on p. 76. Reproduced with permission from MIT Press Journals.

though not everyone had noticed it.[13] For example, in 1937 John Hicks invented a "little apparatus" (p. 156), his IS-LL diagram (Figure 1.4b), to compare the workings of J. M. Keynes' new macroeconomics (of 1936) with that of the older classical system. In that same year, in another attempt to turn Keynes' theory into something more comprehensible, James Meade provided an eight-equation algebraic treatment, calling it a "A Simplified Model of Mr. Keynes' System", while soon after Paul Samuelson produced a smaller set of equations (seen in Figure 1.4c) to exemplify and explain the Keynesian relations, describing them as "a new model sequence" (1939, p. 75). All of these three 'Keynesian' models are discussed in Chapter 6.

Economists quickly broadened the scope of the label 'model' to refer to all kinds of mathematical and diagrammatic and material objects. But even then, models – as working objects and as a label – did not immediately and fully invade economics until a bit later. Only with the next new generation of economists – for whom both the label and the notion were unproblematic – did models cease to be special and became commonplace. Thus, William Baumol (1951) used the term as naturally as one might refer to a domestic weed when he referred to Harrod's (1939) small set of equations showing how an economy grows as "Mr. Harrod's Model", while Roy Harrod himself (of the same older generation as Hicks and Meade) still mused about the term as if it were some exotic imported plant:

> Many years after I had made certain formulations in the field of growth theory and after Professor Domar had made similar formulations, there began to be references to the "Harrod-Domar model". I found myself in the position of Le Bourgeois Gentilhomme who had been speaking prose all his life without knowing it. I had been fabricating "models" without knowing it. (Harrod, 1968, p. 173)

This brief history has enabled me to indicate the historical contours of when models were introduced and when modelling became the normal mode of reasoning in economists: from isolated examples in the prehistory, to a first generation of model-makers and users in the late nineteenth century, to a second generation who developed these research objects explicitly as models in the 1930s. It was this second generation who fully developed this "new practice" of modelling, as Marcel Boumans (2005) has justly labelled it. The label, the idea, and the use of models became the natural way to work for economists only in the period from the 1940s onwards.

Models are not easy objects either to define or, in general terms, describe, but those reproduced here, some of the most important models from the history

13 Nor was its meaning stable in the 1930s (for examples of its range, see Schumpeter, 1935). Although we see the term model taken up by those making models of Keynes' theoretical macroeconomic system (1936), it was not one of Keynes' terms (almost the only time he used it was in discussing Tinbergen's work, see Keynes, 1973, pp. 284–305). Keynes himself seemed to prefer the term "schema" or "schematism", which, as we will see later in Section 4.iii, has a slightly different connotation: it indicates an outline, rather than an apparatus that might be manipulated.

of economics, exemplify the sort of things that count as models in economics: either real objects, or pen-and-paper objects that are diagrammatic, algebraic, or arithmetic in form. Despite their variations in form, these objects share recognisable characteristics: each depicts, renders, denotes, or in some way provides, some kind of representation of ideas about some aspects of the economy. Yet, and this is a very important point to stress, these representations are not just pictures. Pictures of the economy go back a long way: we see shipbuilding in the eleventh century Bayeux Tapestry and building sites in the fifteenth century frescoes of the recently reopened hospital of Santa Maria della Scala in Siena. These depict the arrangements of labour and capital, show the technologies of the period, and so forth – but they are not models for the economist. As I pointed out earlier, Quesnay's *Tableau Économique* was not just a depiction of the economy but one that could be manipulated, and because it could be manipulated, it could be reasoned with. For economists it is the possibility to reason with the different kinds of representations shown in this chapter that makes them all into economic models.[14]

Hick's 1937 terminology of a 'little apparatus' nicely captures the manipulability of such research objects – they are working objects that can be played around with in various ways – even though his model is made only of paper and pen compared to the real apparatus of Fisher's earlier hydraulic model. These two examples have an obvious affinity with the material object models used centuries earlier. For example, models of the planetary system constructed out of papier maché and metal rods were used by scientists to explore the workings of the universe, while articulated wooden maquettes were made by architects to demonstrate the construction of their buildings. This comparison points to another critical point about models: they must be small enough in scale for their manipulation to be manageable in order that they can be used to enquire – indirectly – into the workings of those aspects of the economy depicted, just as those models of domes and the planetary systems were. It seems natural to take over this older sense of material models from the arts and sciences to understand the term 'model' that Tinbergen introduced into economics at that time: small-scale objects depicting aspects of the economy that can be analysed and manipulated in various ways.

But notice here how introducing this new kind of research object into economics brought along with it a new way of reasoning to that science, a method that economist-scientists now call simply 'modelling'. By the latter half of the twentieth century, mathematical modelling had become the preferred way of doing scientific and policy-making economics, and had come to inhabit a number of other domains where economists had left their mark in the scientific, public, and commercial realms. And, wherever statistical data were available, econometric modelling

14 The label *Tableau* is indicative that some tables of numbers may also have this manipulable quality, and so reasoning with them is also a possibility, for example, Leontief input–output tables both represent the economy and can be manipulated.

became the relevant way of working – although this book is not primarily about econometrics.[15] In other words, disciplinary arguments at all levels of economics came to hinge not just on the objects – models, but on economists' abilities to reason with them – modelling. Modelling had become *the* accepted mode of reasoning in economics in the sense that it became "the right way to reason.... what it is to reason rightly".[16]

3. Practical Reasoning Styles

This brings us to the question of reasoning method, for though we can discern some characteristics in common between those revered old models of the universe resting in our science museums and the modern mathematical models of the economy, it is perhaps not so obvious that economics shares a mode of reasoning with early astronomy.

3.i Modelling as a Style of Reasoning

Modelling is one of the six different "styles" of scientific thinking that Alistair Crombie distinguishes in his *Designed in the Mind: Western Visions of Science, Nature and Humankind*.[17] It is worth listing them all here – in the chronological order that they appeared in the history of the sciences.

15 The history of modelling in economics has been barely considered except in econometrics (on which see Morgan, 1990; Boumans, 1993, 2005; Qin, 1993; and Le Gall, 2007). The parallel literature on mathematical modelling – *qua* modelling – is less developed, but see Boumans (2005), who focusses on the 1920s–1930s in his discussion of it as a "new practice" in both statistical and mathematical terms. Solow (1997/2005) offers some suggestions about its takeoff in the 1950s and as a rare exception, compares the use of mathematics to the use of modelling to argue that economics is mainly a modelling discipline. Niehans (1990) recognises the "era of models" as a leitmotiv for the period since the 1930s (but does not say much about its history); and Colander (2000) portrays modelling as the "central attribute of modern economics" (p. 137). Most histories of twentieth-century economics allude to models, but the introduction of models, and their mode of argument, are largely taken for granted. Mirowski (2002) indirectly comes closest to dealing with this as an historical problem, but his questions are about ideas, theories, and techniques of economics in the context of the Cold War, rather than about modelling itself.

16 One of the peculiar signs of this acceptance (and it may be specific to economics compared to other scientific fields) is that economists rarely use the word *theory* nowadays, or if so, they use it interchangeably with *model* to such an extent that many economists find it difficult to distinguish between the two (see Goldfarb and Ratner, 2008). I return to the point in Chapter 10. The quote itself comes from Hacking (1992a, p. 10) and refers not to modelling in economics, but to a much broader claim about the nature of epistemic genres in scientific reasoning, discussed in the next section.

17 Crombie's claim – that there are basically six styles of scientific reasoning, first appeared in his paper of 1988 and the volumes of Crombie's massive three volumes: *Styles of Scientific Thinking in the European Traditions* (1994) were in draft in 1980. Thus, Hacking's review and further analysis (1992a) come after that first paper but predate Crombie's main publication of 1994.

1. Mathematical postulation and proof
2. Experiment
3. Hypothetical modelling
4. Taxonomy (the method of classification into natural kinds)
5. Statistical
6. Historical-genetic[18]

These categories label different 'styles' or 'epistemic genres' of scientific reasoning, that is, of ways of finding out about the world. They do not provide the kind of detailed descriptions in combination with a big picture analysis of how science goes on that we find in Ludwig Fleck's 'denkstil', Michel Foucault's 'epistemes', Thomas Kuhn's 'paradigms', nor Hans-Jorg Rheinberger's 'experimental systems'.[19] Rather, this set of categories provides a framework for historical epistemology in the sense that it enables the historian to track the changes in how scientists do their science. While modern economics barely makes it into Crombie's massive volumes, nor Ian Hacking's subsequent discussions, they provide the resources to understand how modelling as an epistemic style or genre came into economics and what kind of difference it made.[20]

According to Crombie (1994), modelling grew up in the field of early modern sciences and arts in the making of models of natural objects – and sprang from the desire to imitate nature, and in so doing to understand its workings. It had joint roots in natural philosophical investigations into the relationship of the Earth and the heavens (such as in astronomy) and in the craft skills found in the creation of objects such as imitation birds (singing, feathered, mechanical automata). Given these roots, Crombie labelled one of its characteristic features as involving "the construction of analogies" (1988, p. 11). Although there are good examples of analogical models in economics, analogical aspects no longer constitute a distinguishing feature of model-making in this field. It is therefore useful to broaden the canvas beyond analogies to see how the desire to understand Nature (or in the economists' case, the economy) through some form of imitation lies at the heart of modelling. And, just as there are many different genres and aims of representation in the arts, such scientific representations come in a variety of forms and disguises.

18 The "historical derivation of genetic development" is associated with evolutionary science. "Thinking in cases" is a seventh style added by Forrester (1996), as used for example in various branches of medicine and psychiatry. Karine Chemla (2003) has argued for an eight style – the algorithmic method. At first sight, none of these other styles seem to be connected to modelling, but as we shall see later in this book, the methods of taxonomy and classification, and the method of experiment, are both found in conjunction with the method of modelling in economics, while statistical reasoning is the basis for econometric modelling.

19 See Fleck (1935/1979), Foucault (1970), Kuhn (1962), and Rheinberger (1997).

20 As such, this account provides a parallel to Hacking's accounts for the development of the statistical style (1992b), for the experimental (laboratory) style (1992c) and for the taxonomic style (1993).

The history of modelling as a reasoning style in Crombie's account is built upon material object models, such as those in astronomy, and so we can continue to think of the planetary motion models of the Renaissance period as being exemplary for the idea of models and of how they are used for enquiry. They were built to represent the relationships – hypothesized by the early astronomers – between Earth and the heavens. They were carefully designed not just to present or illustrate known relationships but also to demonstrate those relations that scientists *supposed* to be true (their hypotheses) and thus to explain how the universe was thought to be arranged and to work. Those models that were manipulable (rather than with fixed parts) were particularly useful in enquiries into the hidden trajectories and contested relations of the heavenly bodies. It is this kind of physical activity of science in general that perhaps led Ian Hacking (1992a) to suggest that Crombie's style of "thinking" should be replaced by "reasoning".[21] Thus, we might rather think of each style as a generic kind of very practical reasoning, with different characteristics for each style.

We learn from Crombie that the adoption of any particular style of practical reasoning in any one field requires its own historical account. Take, as a parallel example to the introduction of modelling, the method of experiment. This grew up in the early modern period as a method of analysis and synthesis "to control [the method of mathematical] postulation and to explore by observation and measurement" (1994, Vol. I, p. 84). Crombie dates its main development from the thirteenth century and thereafter it took hold in various disciplines at different times and places. But typically those who would adopt a new style of practical reasoning for their science have to argue for it, as well as demonstrate its usefulness, for the acceptance of a new style generally institutes a *change* in reasoning style. This is one reason why the histories of the different sciences are so replete with arguments about how that science should be done. For example, Shapin and Schaffer (1985) analyse in detail how the method was fought over in the establishment of natural philosophy in seventeenth century England. To follow the example into economics: classroom experiments began there in the 1940s, though the activity was sufficiently limited that economists experienced their own battle for the acceptance of the experimental method within economics only in the period after 1970. Yet it is worth noting too, that in economics as in many modern sciences, the individual styles have begun to hybridize. Thus, even from the beginning of experimental work in economics in the 1940s, modelling informed those experimentalists' working hypotheses and models were found in their experimental designs (as we shall see in Chapter 7).

Finally, we can also take from both Crombie and Hacking that adopting a new reasoning style into a science does not come without significant consequences for its content. There are inevitably connections between style and content, and while

21 The practical aspects of this are important: for like Hacking, I find the term "reasoning" underrates the importance of the "manipulative hand and the attentive eye" (Hacking, 1992a, p. 4).

different sciences may rest on one or more of these styles of reasoning, that does not imply that any scientific system can rest on any style. For example, Quetelet's 'average man' of the mid-nineteenth century is a statistically defined concept and so unthinkable without the adoption of statistical reasoning. In economics now, it is almost impossible for economists to give an account of individual behaviour, or of the world economic crisis, which has not been defined in terms of their economic models and argued over using their model reasoning.

Any scientist's ability to reason in a chosen style is thus clearly dependent on the contingent history of that discipline, and whether that method is accepted within it. Yet, once more or less adopted within a discipline, a style, as Hacking says, becomes

> … a timeless canon of objectivity, a standard or model of what it is to be reasonable about this or that type of subject matter. We do not check to see whether mathematical proof or laboratory investigation or statistical 'studies' are the right way to reason: they have become (after fierce struggles) what it is to reason rightly, to be reasonable in this or that domain. (Hacking, 1992a, p. 10)

Once accepted by a group of scientists, a style of reasoning comes to seem natural to them, so natural that they do not question it. They neither question its historical origins, nor the objectivity of the knowledge gained from using the method, nor do they appeal to any outside or higher level for its justification. That is why, Hacking argues, once a style of reasoning is accepted in a community, reasoning rightly means to reason in that style.[22]

3.ii Modelling as a Reasoning Style in Economics

Although the broad historical contours of the appearance and spread of *models* in economics were outlined earlier (in Section 2), the processes by which *modelling* took hold as an independent style of practical reasoning are more hazy. There was, of course, no blank page in economics before modelling took over. Early economists used technical and conceptual terms (the terminology of their science), but reasoned with them in the modes of ordinary verbal argument. As modelling developed, it first partly overlayered and partly integrated with two other generic practices of scientific reasoning, namely mathematical ones in the late nineteenth century and then statistical ones in the 1920s and 1930s (in the form of econometrics). More recently it has become layered into the experimental and classificatory

22 Hacking even makes a stronger claim, arguing that a style becomes self-validating. For example, statistical reasoning is validated by arguments that are coherent within statistical thinking, not by ones from other styles of reasoning or some meta philosophical argument (see Hacking, 1992b, and for laboratory sciences, Hacking, 1992c). This all points to the relativity of scientific method and so the knowledge obtained by it, but it is not a radical relativity, for each of the styles is considered valid as a scientific method.

modes of reasoning (see Chapters 7 to 9). While modelling itself became deeply rooted in economics, so deeply rooted as to produce the overwhelmingly luxuriant growth that made it – in its various forms – the dominant mode of reasoning by the late twentieth century, it did so in forms that were either partly disguised or manifest in hybrids.

Treating the development of modelling as an epistemic genre – that is, as a practical mode of reasoning to gain knowledge about the economic world – does help to part the clouds that obscure the historical gaze. It reveals to us that mathematics grew up in two styles of reasoning in economics at more or less the same time in the late nineteenth century: the method of mathematical postulation and proof and the method of hypothetical modelling using mathematical models. We have already seen how the first generation of model-makers in the late nineteenth century generated a new practice of modelling, but by taking note of Crombie's categories, we can also see why it crept in unnoticed by historians who have paid more attention to the concurrent introduction of mathematical modes of arguing without distinguishing between two styles of reasoning both involving mathematics. It is fair to say, however, that recognising two distinct historical traditions in styles of scientific reasoning that both involved mathematical languages, and distinguishing between the method of hypothetical modelling versus that of postulation and proof, is not always easy. This knotty historical problem is further complicated by the fact that, as Weintraub (2002) has shown us, mathematics has its own self-image, one that changes in its relationship with the sciences. During that late nineteenth century time when these two mathematical modes of reasoning came into economics, mathematicians felt the need to have their work closely related to the sciences, though that relationship could be mediated in different ways, while for their part, economists of the time argued about the usefulness of mathematics as both a language and as a method.[23]

Nevertheless, we can contrast, as exemplars of these two reasoning styles in the late nineteenth century, Fisher's hydraulic/mechanical model of his three-commodity, three-person economy (pictured in Figure 1.3c) with the French economist, Leon Walras' 1874 mathematically described general equilibrium account for the whole economy. So, whilst Walras (amongst others) was busy introducing what might be recognised as mathematical language and the method of mathematical postulation and proof, we can also distinguish objects that we can call models, and a method of reasoning with them (including the use of mathematics), being developed by economists such as Fisher and Marshall. The fact that Fisher built his hydraulic model to represent Walras' ideas, and to figure out by exploring with that physical model the *process* by which the latter's mathematically postulated and proved general equilibrium might be arrived at, shows us the

23 Authorities on the history of mathematical reasoning in the history of economics are Roy Weintraub (see his 2002 and 2008); and Giorgio Israel (see particularly Ingrao and Israel, 1987/90, and Israel, 2002). For an insight into contemporary views, see Edgeworth (1889).

difference between them. The fact that both used mathematical ideas from physical systems demonstrates not only the closeness of mathematics and the sciences (but also shows how treacherous relying on analogies as indicators of reasoning styles can be). Individual economists worked with different styles of reasoning involving mathematics and the mathematical method, but as we should expect, their choices were locally determined, dependent on their own histories, times, and places and their own image of the role of mathematics in science.

Mathematics provides the languages of most modern economic model-making, and we know that economics became mathematized at the same time as it became a modelling science, but if we want the historical record to help us think about modelling, then we need to turn the terms around: in order to get at modelling in economics, we need to concentrate on the objects, on the models themselves rather than on their mathematics. Here, as we have already found, history matters whenever we are discussing any specific example of a model, for models are contingent, not timeless: we need history to understand why and how any particular model was built, how it was used, and what understanding economists gained from it. But to understand the development of modelling as an epistemic genre, we need to capture and explicate the generic qualities that we can find in the earlier models of Quesnay, and Ricardo, just as much as the twentieth century work of Frisch and Samuelson. To understand what is involved in this shift in economic science, a shift in how economists reason in economics and about the economy, we need to understand what constitutes the method of modelling in economics. Here, history begins to take second place: it provides the materials and examples for explanation, but we are instead concerned with philosophical questions about how models are made, about modelling as mode of reasoning, and about the nature of modelling as an epistemic genre.

PART II: MAKING MODELS, USING MODELS

4. Making Models to Reason With: Forms, Rules, and Resources

How do economists make models?[24] The process of model-making in economics has often been labelled one of "formalization", a term whose various meanings have so twisted and turned through the history of economics that I suggest we

24 The literature on the philosophy (or methodology) of modelling in economics has seen considerable attention in recent years, particularly since the formation of a specialist *Journal of Economic Methodology*. I have discussed the seminal contributions by various economists over the twentieth century alongside some philosophical reflections in Morgan, 2008/online, and surveyed the recent work in Morgan and Knuuttila (2012). Consequently, this chapter does not provide an additional survey: rather some elements are discussed in this chapter and others in the chapters that follow.

begin afresh with it.[25] I focus on two meanings of the term. First, if we think about its active form: 'to formalize', we imply to give form to, to shape, or to provide an outline of something. Second, 'formal' contrasts with 'informal', meaning lacking in exactness or in rules, whereas 'formal' implies something rule bound, following prescribed forms. Making models involves both senses: models give form to, in the sense of providing a more explicit or exact representation of our ideas about the world, and in creating those forms we make them subject to rules of conduct or manipulation. These two aspects of modelling – giving form to ideas and making them formally rule bound – are related, and if we understand how, we take a big step towards seeing how models provide the means for reasoning within economic science. I make use of some more examples of economic models to show how giving form to a model and making it subject to rules of reasoning go along together.

4.i Giving Form

All the models reproduced in this chapter – a small but representative sample from the history of the field – give representation to economists' vague ideas about the economy in various more exact forms: in diagrams, equations, pictures, and even in physical objects. How does this happen? Commentators have found a number of different ways to describe this process of 'giving form to' ideas, namely, as a process of recipe making, of visualizing, of idealizing, or of choosing analogies. It is helpful to see these four accounts as four different ways to understand how models get made rather than being either labels for different kinds of models or as terms used by the scientists/model-makers themselves. Nor are such accounts necessarily mutually exclusive in accounting for any particular model-building episode.[26]

25 It is indicative, for example, that at the end of the nineteenth century, the taxonomy of methods for economics given by W. E. Johnson in the Old Palgrave (the renowned dictionary of economics of 1894–6) contrasts "formal" with "narrative", although both categories fell under the term "descriptive economics"; meaning that they "describe the conceptions and facts with which the science deals". Formal methods were those which "analyse and classify" concepts and involved the "logical processes of definition and division". Both "Inductive" and "Deductive" methods fell on the other side of the taxonomy tree, under the title of "constructive" methods: those that "establish laws and uniformities" (Johnson, 1896, pp. 739–48). In contrast, most modern commentators align formal methods with mathematical methods, and thus with deduction. Some minority of economists continue to dispute the efficacy of 'formal methods' in economics, arguing that formalism is non-neutral (see Chick and Dow, 2001), or that it narrows and leaves out too much substantive content of importance compared to the verbal methods it supplanted (thus equating formal with a lack of substance), an argument that seems to hold both the language of mathematics, and the small-scale reasoning tools of modelling, equally at fault. Two recent debates about the meaning and content of 'formalism' are suggestive of the term's extraordinary range (see debate in *Methodus*, 1991, particularly contributions by McCloskey and Katzner, and in the *Economic Journal*, 1998, by Backhouse and Krugman).

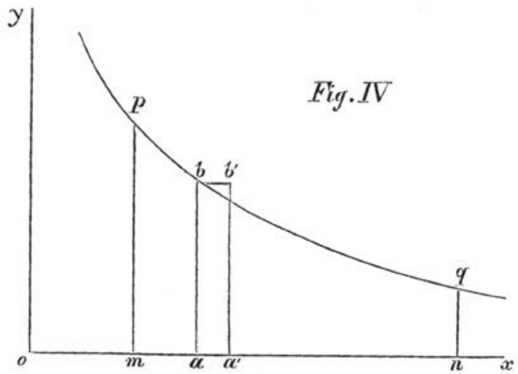
26 For example, as we find in Chapter 5, both Hesse's 1966 account of analogical modelling and Boumans' 1999 account of recipe-making help us understand the process of making the analogical Phillips-Newlyn hydraulic machine (see Morgan and Boumans, 2004).

The first of these four accounts of how models are made sees the process of giving form to ideas about the economy as analogous to recipe-making. Boumans' (1999) recipe notion embeds two ideas: economists choose the model's *ingredients* – their ideas, intuitions, and bits of knowledge of how the economy works – and then combine them together and fashion them to make something new. It is critical that this model-making involves processes of *integration*: mixing and shaping and baking the elements, 'cooking' them to form something whole that is not fully recognisable from the original elements (as in chemical synthesis). It may well be that the end product is not envisaged at the beginning, for *recipe-making* is a creative process (less so for *recipe-following*, which produces more reliable and known results). For example, Ricardo can be understood to have formed his model out of a set of little accounting tables (one of which is shown in Figure 1.2a): he integrated these elements together and reasoned with them until they emerged as the accounts of a model farm representing the agricultural economy of his day (as we shall see in Chapter 2). Hicks' IS-LL model provides another good example that can be well described as recipe-making: it was fashioned to make sense of Keynes' ideas about the macroeconomy by fitting together the simplified or basic elements and relations of the macroeconomy (see Figure 1.4b, and discussion in Chapter 6). Once synthesised, the new model recipe depicted certain macroeconomic relations in a new form (the IS/LM model) that proved flexible to many different interpretations and had a remarkably long life.

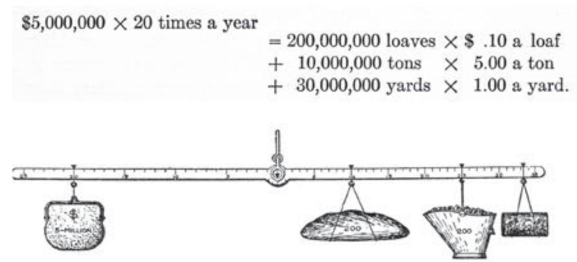
A second account of model-making derives from another comparison, this time drawing on the similarities between the practices of representation in arts and sciences and inspired by Nelson Goodman's work (1978). In Morgan, 2004, I argue that the activity of model-making requires *imagination* to hypothesize how the economy might work, and then the power and skill to *make an image* of that idea. For example, Edgeworth's first drawing (1881) of the relationships between Robinson Crusoe and Man Friday (Figure 1.3b) can be understood as his imagining and imaging the set of points on his graph where they might both be willing to make a bargain to help each other. This little diagram gradually evolved into the Edgeworth Box diagram in a process that was far from self-evident but depended on the processes of imagination and image making by a sequence of different economists, each of whom used this particular way of envisioning economic relations and portraying them into these little two-dimensional diagrammatic/mathematical forms (see the example by Bowley in Figure 1.5c). In this account (described in Chapter 3), modelling, understood as a way of giving form to economic intuitions, involves a kind of envisioning power.

A third account of model formation understands it as a process of 'idealization'. Philosophers of science have used this notion to explain the practices observed in mathematical modelling in physics (e.g., McMullin, 1985). Modelling there is portrayed as a process of picking out the relations of interest, and isolating them from the frictions and disturbances which interfere with their workings in the real world to give form to simpler, and 'ideal', world models (e.g., 'in an ideal world,

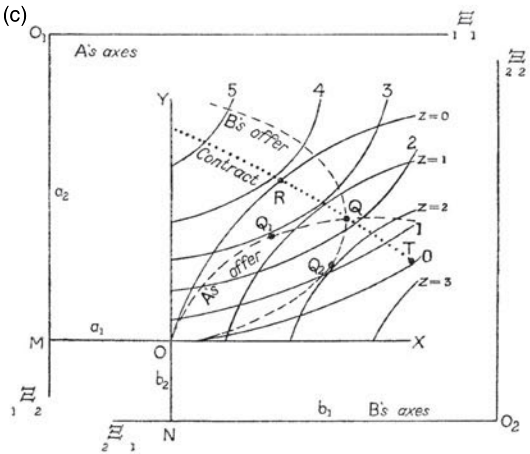
(a)



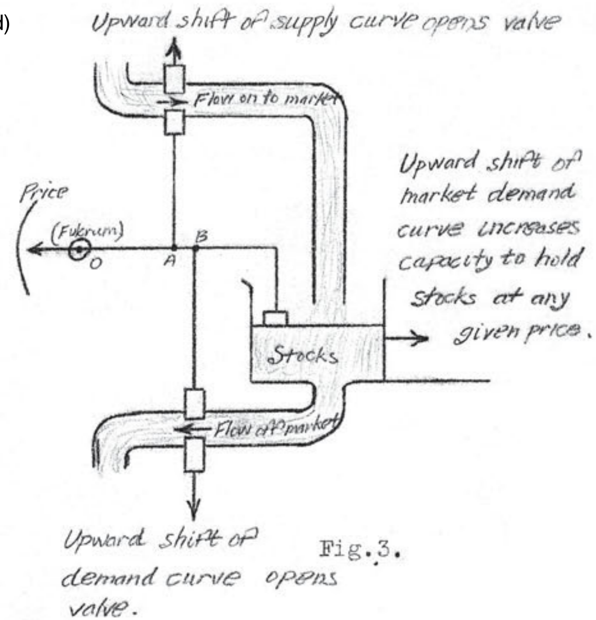
(b)



(c)



(d)



(e)

$$H: \begin{matrix} & \beta_1 & \beta_2 \\ \begin{matrix}\alpha_1 \\ \alpha_2\end{matrix} & \left[\begin{matrix} (5,\ 5) \\ (6,\ -4) \end{matrix}\right. & \left.\begin{matrix} (-4,\ 6) \\ (-3,\ -3) \end{matrix}\right]. \end{matrix}$$

Figure 1.5. Models: The Variety of Forms.

(a) Jevons' Utility Curve.

Source: William Stanley Jevons, *The Theory of Political Economy*, 1871. London: Macmillan & Co., Figure 4, p. 49.

(b) Fisher's Arithmetical and Mechanical Monetary Balance.

Source: Irving Fisher, *The Purchasing Power of Money*., New York: Macmillan, 1911; Arithmetic Balance, p. 18; Mechanical Balance, p. 21. Reproduced with permission from George Fisher.

(c) Bowley's Version of the Edgeworth Box.

Source: Arthur Lyon Bowley, *The Mathematical Groundwork of Economics, An Introductory Treatise*. Oxford: Clarendon Press, 1924; Figure 1, p. 5. Reproduced with permission from Oxford University Press.

(d) Phillips' Plumbing Diagram.

Source: Bill Phillips' undergraduate essay "Savings and Investment. Rate of Interest and Level of Income". Undated, 1948–9, p. 1, Figure 3. University of Leeds, Brotherton Library, Newlyn-Phillips Machine Archive.

(e) Luce and Raiffa's Game Matrix.

Source: Duncan R. Luce and Howard Raiffa (1957) *Games and Decisions*. New York: Wiley; matrix, p. 95. Reproduced with permission from Dover Publications.

there is no friction'). Such accounts have also been used to understand model formation in economics. Thus, Nancy Cartwright has used the term to discuss how economists made models to get at causal capacities in the economy while Uskali Mäki has used it to describe how economists isolate particular accounts (models) for theorizing purposes.[27] As an example here, Jevons' graphed economic man's experience of utility as dependent upon only two dimensions, its intensity and duration (Figure 1.5a). He did so because, by his own account, these were the two most salient elements in motivating mans' economic behaviour. This idealization enabled him to leave aside six other dimensions of utility that Bentham had suggested in an earlier verbal account. But this simplification also made it possible for Jevons to represent man's behaviour in making consumer choices into a form where he could treat the problem mathematically.[28] Such idealization processes of giving form to economic models can be described as the formation of ideal types (using Max Weber's account, 1904 and 1913) or even as a process of drawing out a caricature (see Chapter 4, and Morgan, 2006).

A fourth strand of literature, following Mary Hesse's (1966) work, argues that model-making, or giving form to a model, depends upon our cognitive abilities to recognise similarities and our creativity in exploring those similarities.[29] Scientists choose models on the basis of similarities seen in the form, structure, content or properties between two fields and investigate these similarities in a systematic way. For example, Fisher (1911) chose a mechanical balance as a model for his economic "equation of exchange" between money and goods because he recognised the similarity between the elements and their relations (see Figure 1.5b and Morgan, 1999). This ability to recognise similarities, and so to choose a form for a model, is only the first step, for it usually requires a lot of further work to fill out that form into a full model. In another example, Phillips drew the little plumbing diagram (Figure 1.5d) to help him to understand how the stocks and flows of a good interact in a market. With the collaboration of the monetary economist Walter Newlyn, the model grew into a large physical hydraulic machine of the economic system as a whole (see Figure 1.7 and Chapter 5).[30]

27 For example, von Thünen's model has been described (by Mäki, 2004) as arrived at by the process of "isolating" real-world aspects away for theoretical purposes, whereas it could also be understood as a process of "causal idealization" (in Cartwright's terms) since von Thünen used numerically based observations about his own farm in his model. On the general arguments on idealization in economics, see Cartwright (1989) and Mäki (1992); a survey with further references is provided in Morgan and Knuuttila (2012); Hamminga and De Marchi (1994) provide an important collection of earlier papers on idealization reviewed in Morgan (1996).

28 Historical work suggests that Jevons' gave form to his model not just through a process of idealization, but through an inspired transcription of ideas from several other fields and drew on his own working experiences and on his creativity as a scientist (see Maas, 2005).

29 See also Gentner (2001).

30 Marcel Boumans (in Morgan and Boumans, 2004) has described the move from such a metaphor to a model as a move from a vague to a more exact form of representation: from the one-dimensional representation of a metaphor to the two-dimensional analogical model, as in the little diagram by Phillips of a market as a plumbing arrangement of Figure 1.5d, or to fully formed three-dimensional model as in Fisher's built hydraulic machine of Figure 1.3d (see Chapter 5).

More recently, economists have themselves suggested that the point of modelling is not to *recognise* analogies, but to *create* them, rather as Fisher designed his analogical model of the gold standard mechanism in the late nineteenth century (see Morgan, 1997). For example, Robert Lucas argues his modelling of the business cycle creates “a mechanical, imitation economy”.[31] Robert Sugden has argued that modellers create “credible worlds”, where the credibility claim rests on some observed similarities in model outcomes, for example, between those of a checkerboard puzzle with the analogous pattern of segregated housing.[32] In seeking to capture not the workings of real economies but to mimic some aspect of it via an imagined analogous world, these practices of design take us back to one of the historical roots of modelling in the arts where craftsmen built mechanical birds that would ‘sing’ but did not suppose that birds were mechanical automata.

The activity of giving form to a model has been characterized in four different ways here, and exploring and analysing these different ways of thinking about model-making provide the subject matter of the next several chapters of this book. But in this chapter, I am less concerned with the differences in these accounts than with the things that they have in common. When we look at the examples of models presented in this chapter, it is not obvious what these general qualities of model-making might be. But certain points can be made, which, in part, arise from this very variety in the nature of the objects that get made.

To begin with, these accounts all understand the scientist-economist as acting in this process of model formation. It is obvious, but important to remember, that models are created by a knowing economist-scientist for a particular purpose. Whether the scientist is best described as making a new recipe, using his or her imagination and imaging powers, idealizing from some other account, or choosing between different analogies, the point is that models don’t make themselves.

Another shared feature is that, in making models, scientists form some kind of a representation of something in the economy. While the activity of creating a model can be described variously as representing, depicting, imagining, or imaging, more generous terms such as rendering or denoting, often seem equally pertinent and accurate as descriptions of the activity of model-making. The very different ways economist-scientists have of getting to their models, *and* the sheer variety of forms they have created, support this pluralistic language. The important point

31 Lucas (1980, p. 697); he most famously said of his business cycle models that “A good model, from this point of view, will not be exactly more ‘real’ than a poor one, but will provide better imitations”. (1980, p. 697), leading to a discussion of the artefactual character of the results of such modelling – see Hoover (1995) and Boumans (1997).

32 The notion of designed analogies or similarities is consistent with Sugden’s writings on how models are made and used in his discussions of the checkerboard and other examples (see his 2009 and 2002); see also Chapter 9 here. The development of ‘simulation’ in the 1960s as a way of using models shares a mimicking aim, but without necessarily sharing any particular view of the nature of models (see Chapter 8).

here is that whatever term is used should not unduly limit our understanding of what models are and how models work as a means of enquiry.[33]

These accounts of model-making also suggest that forming models is not driven by a logical process but rather involves the scientist's intuitive, imaginative, and creative qualities. When we look at the variety of objects displayed in this chapter, it would be difficult to see what, if any, such a logical process could possibly be that would cover all these instances. The importance of these creative qualities in the scientist's model-making activities may reflect the long-ago roots of scientific modelling in the decorative crafts. We found these evident in the *Tableau Économique*, but they remain in the delight that economists take in creating 'elegant' models.

Model-making is a skilled job. Perhaps it is not yet evident, but will become so in the chapters that follow, that learning how to portray elements in the economy, learning what will fit together, and how, in order to make the model work, are specialised talents using a tacit, craft-based, knowledge as much as an articulated, scientific, knowledge. It is not easy to pinpoint in any general way these skills of articulation and construction, or to see how economists acquire them except through apprenticeship. Perhaps, like Pigou, it suffices to note that some economists have considerable talent in model-making, and that these talents of the scientist-economist are recognised in the artefactual nature of the models that are made. Economists recognise these talents in terms of the qualities of the models themselves, where their term 'fruitfulness' indicates a model that is not just well put together and easy to use but easy to extend, generates interesting findings, new questions, and so forth. Economists' skills in articulating and crafting models, along with their imaginative and creative abilities, turn up in different ratios in different episodes of model-making, but they are all essential to the process of giving form to models.

33 Although philosophers of science tend towards using the terminology of 'representation', it is the subject of huge debate for the term raises a number of important and difficult problems. First, as discussed here: what is the process of representation? I am sympathetic to R. I. G. Hughes' (1997) argument (following Nelson Goodman) that 'denoting' is a better term for the activity of model-making than representing, for it makes clear that the models stands in relation to its economic system "as a symbol for it" and that while there is "no representation without denotation", denotation is "independent of resemblance" (1997, S330-1). Second, how is a representation defined? (Are models best thought of as maps, descriptions, structures, axiomatic systems, fictions, etc., or as artefacts with flexible representing relations?) The approach taken in this book, as in Morgan and Morrison (1999), is more concerned with how scientists use models than with an analysis of them as philosophical objects, so I use this awkward term representation as a descriptively useful one, without apology, and leave the philosophical problems for elsewhere (see Morgan and Knuuttila, 2012), and for others (addressed in recent volumes edited by Grüne-Yanoff, 2009 and by Suárez, 2008; see also Knuuttila, 2005). Third, what is the nature of the representing relation? The importance of this last lies in the view held by some philosophers that models have to represent the world accurately – for example, have a structural isomorphism to the world – in order for us to make truthful deductions about the world from them (for an early discussion in philosophy of social sciences, see Brodbeck, 1968). I reframe this as an inference problem later in this chapter (Section 5) and more fully later in the book.

4.ii Becoming Formal

Each of these four processes of understanding model-making: recipe-making, visualizing, idealizing and choosing an analogy, describes an act of giving form to ideas about the economy. But by representing the economy in a particular form, the economist-scientist at the same time creates an object that must obey certain rules – which brings us to our second sense of formal: meaning subject to rule and rigour in contrast to that sense of informal. Since in each particular case, these rules form the rules of reasoning with that model, they effectively determine the economist's valid manipulation or use of that model. Where do these rules come from? And, what kinds of rules are involved?

Rules for reasoning with a model come from two distinct aspects of the model. First, when an economist reasons with any model, he or she must obey certain reasoning rules according to the kind of the stuff that the model is made from, or language it is written in, or the format it has. So, these rules could be those of geometry or algebra, of mechanics or hydraulics, etc. depending on the model. Reasoning with Fisher's equation, for example, was subject to the rules of arithmetic; in contrast, reasoning with his mechanical balance model was subject to the rules of behaviour, and so manipulation, of mechanical balances (both in Figure 1.5b). Samuelson's equations model (in Figure 1.4c) can be manipulated following the formal rules for working with equations – either algebraically or arithmetically in a simulation (and he does both, as we shall see in Chapter 6). An important point about these kinds of rules are that they are given and fixed by the substance of the model, even where that model is a paper representation of a material model (as in Fisher's mechanical balance). They are 'formal' rules in the sense that they are not made them up each time the economist works with a particular model, rather, they come ready made from the form or language the modeller has chosen for that representation.

Second, and in contrast, allowable manipulations of the model are also determined and constrained by the economics subject matter represented in the model. For example, Samuelson's model of the macroeconomy must be manipulated in a certain order, not just because the economic relations have a certain time order (found in the equations' subscripts) but because of the implied causal links given by the economic content. In other cases, the characteristics and ambitions of model economic man are used to motivate how the resources of an economic model are used. For example, the reasoning in the Prisoner's Dilemma model is determined by the economists' view of how the economic model man will act in the world of the model. But – just as with the earlier *Tableau Économique* (in Figure 1.1) – the matrix of numbers depicting the Prisoner's Dilemma game in Figure 1.5e needs to be accompanied by a text account of the economic rules for the situations that the numbers represent before they can used in reasoning (see Chapter 9). These kinds of rules of manipulation don't come with the form, they come from the economic concepts and content that the model-maker uses in making the representation.

These two kinds of rules – the formal rules (those given by the form) and the economic rules (those given by the subject matter) – taken together provide the means of reasoning with a model.[34] For example, the little hydraulic diagram from Phillips is designed to work according to the hydraulics pictured, but is simultaneously subject to the rules of reasoning from the economic content enshrined in the arrangements of the parts: where demand and supply, and price and quantity, can be changed in particular ordered ways (Figure 1.5d). Usually the model form is designed so that these two different sets of reasoning rules will be complementary in the way that the model works. But sometimes, particularly with analogical models, they may turn out to be in conflict – as indeed, happened with Fisher's mechanical balance, where his economic rules of adjustment at first sight were at odds with the mechanical ones of the balance; Fisher found a way to resolve this dissonance by revising his economics (as we will see in Chapter 5). By the late twentieth century, these different sources of rules in a model might no longer be separately recognisable, for modern economics had reached the point where (just as McMullin [1985] noted in his discussion of physics) the concepts and arguments of economics are so thoroughly intertwined with, and even drenched in, the terms of their habitual mathematical expression that they can no longer be pulled apart. So modern economists looking at Jevons' graph of utility (Figure 1.5a), for example, will find it difficult to separate out the economic content from the mathematical argument he made with it.

For the purposes of our investigations of modelling, we can now appreciate how formalization here means that the economist-scientist both gives form to his or her ideas and simultaneously makes them rule bound in the model. The model is formed to represent their ideas about some aspects of the economic world, and their reasoning with the model is bound by the rules appropriate to that particular model – given by both its economic content and its language format. These two different sources of rules – from a model's format and from its subject content – determine and limit how each particular model can be used, and so, *constitute the kinds of right reasoning that are possible with that particular model*. So when we look at how an economist reasons with a model, we should expect to find some very specific reasoning rules being used. But what are these rules of reasoning to be used on?

4.iii Reasoning Resources

I argued earlier that representations become models only when they have the resources for manipulation: this unlocks the puzzle of how any particular model

34 We might describe these as syntactic and semantic rules – those that come from the format (or 'language' structure in which the model is formed) being syntax, and those that come from the economic meaning (the interpretation of the elements) being semantics. But this usage would not map onto traditional philosophy of science usage, where the 'syntactic versus semantic view of models' refer to different views of the relation of models to theories. A version of this explained for economists is found in Hausman (1992).

can be reasoned with.[35] Here we return to Frisch, who recall was one of the first to produce a mathematically expressed model of the economic system as a whole in an attempt to figure out how the elements of the economic system when put together could create business cycles in economic activity.[36]

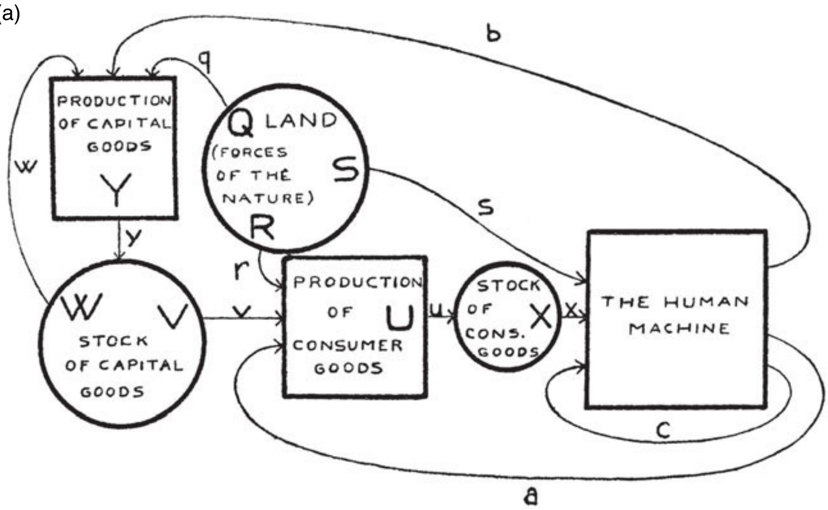
Frisch's first version of his model, in his now classic paper of 1933, was a schema of economic activity (shown in Figure 1.6a here, on which capital letters indicate stocks and lowercase letters indicate flows). It depicts his account of the main elements in the economy: some are "visualized as receptacles" (the circles) and "others may be visualized as machines that receive inputs and deliver outputs" (the squares) (Frisch, 1933, p. 173). He called it a *Tableau Économique*, surely a reference back to Quesnay's famous invention, and we can see that like that earlier example (Figure 1.1), Frisch visualized quite a complicated set of circular flows (indicated by arrows) around the elements in the system. Relative to Quesnay's *Tableau Économique* however, in which both numbers and ordering are specified in his 'table', Frisch's schema lacks the resources for the kind of model manipulation that Quesnay was able to do. Quesnay could use his numbers, and their ordering, to reason about the nature of the system he had depicted, and by playing around with these numbers, explore various different kinds of systematic behaviour in his model world and learn new things from so doing. Frisch's diagram shows the elements and their links, but these can be used only for a verbal description of the relations, and verbal reasoning about them, but not for more rewarding explorations that would tell him anything much about the behaviour of his system; indeed, without those arrows, the scheme could hardly be reasoned with at all. Frisch's schema has limited resources for verbal reasoning and none for numerical manipulation. It was, however, just a starting point.

From this scheme, Frisch developed a more simplified little mathematical model connecting y, the annual production of capital goods, with x, the annual production and consumption of consumer goods (there are no stocks held), and z_t, the amount of production going on at time t (Figure 1.6b). This little mathematical model of the economic system had the resources – of both mathematical and economic content – for Frisch to present it as a kind of machine: its form and content could, with certain manipulations, produce a dynamic pattern. This version of the model had sufficient resources for him to carry out simulations (by putting some parameter values in the equations) to show that the model could generate a cyclical pattern in productive economic activity. This was an important outcome, since one of Frisch's main reasons for making the model was to demonstrate that cyclical patterns could be generated by such a system of equations (see Chapter 6).

35 Amongst the older tradition of philosophical writings on models, Black (1962), mentions manipulation of models, but offers little in the way of discussion or analysis.

36 The story of Frisch's model has been told several times in the history of economics: Morgan (1990) concentrates on its place in the history of econometrics; Boumans (1999) on it as a new recipe in modelling the business cycle; and Louça (2007) on its analogical aspects.

(a)



(b)

$$(1) \qquad z_t = \int_{\tau=0}^{\infty} D_\tau y_{t-\tau} d\tau$$

$$(2) \qquad \dot{x} = c - \lambda(rx + sz)$$

$$(3) \qquad y = mx + \mu\dot{x}$$

Figure 1.6. The Reasoning Resources in Models.

(a) Frisch's *Tableau Économique*.

Source: Ragner Frisch, "Propagation Problems and Impulse Problems in Dynamic Economics". *Economic Essays in Honour of Gustav Cassel*, 1933. London: George Allen & Unwin Ltd. Figure 1, p. 174. Reproduced with permission from Ragner Frisch.

(b) Frisch's Macro-Dynamic System.

Source: Ragner Frisch, "Propagation Problems and Impulse Problems in Dynamic Economics". *Economic Essays in Honour of Gustav Cassel*, 1933. London: George Allen & Unwin Ltd., pp. 177 and 182. Reproduced with permission from Ragner Frisch.

In this example, the contrast between Frisch's *Tableau Économique* and his little mathematical model shows the importance of the presence of model resources that can be manipulated in order to make the object useful as a model. In the schema, there are resources that can be reasoned with, but they can not be manipulated in such a way that you gain any understanding about the possibilities for business cycles to occur from such reasoning. Recall that the rules for reasoning or manipulation come from the model in two distinct senses – from the format (or language) it has, and from the economic content. The schema has quite a lot of economic content, content that can even be reasoned with to some extent, but the format is that of a picture and pictures do not (generally) contain rules for their manipulation. The equations have less content in the sense that there are fewer elements and causal links, but the form (or language) of that content (equations) enables the use of a deductive mode of manipulation so that Frisch can reason mathematically about the nature of the business cycle with this version of his model.

These examples from Frisch enable us to understand not only how the reasoning rules come along with the particular model that is built, but also how necessary the resources are to provide materials to reason with. But this does not explain – in a more general way – how those model resources are used, nor to what purpose, though there are certainly hints in Frisch's example. I turn now to suggest a more general account of model reasoning.

5. Modelling as a Method of Enquiry: The World in the Model, Models of the World

It is easy enough to say that modelling constitutes an epistemic genre, but we still need to figure out how it functions as a way of doing economic science. Scott Gordon, in his history and philosophy of the social sciences, argues that "the purpose of any model is to serve as a tool or instrument of scientific investigation" (1991, p. 108).[37] This forms the starting point for my claim, in the latter half of the book, that economists use models to investigate two different domains: to enquire into the world of the model and to enquire into the world that the model represents.

Model-making – as we have already seen – is an activity of creating small worlds expressed in another medium. The economist represents his/her ideas about certain elements of the economy: the system as a whole, or people's economic behaviour, that they want to investigate or understand into other forms: into bits of mathematics, diagrams, machines, and even – sometimes – strictly defined verbal portraits. The models have certain qualities – they are smaller-scale, and it is supposed, simpler, than the real world, made of quite different materials, and their sense of representation, imitation, or similarity might be quite opaque.[38] I take up these awkward qualities of the way economists render their accounts of the world into models in Chapter 10, but for here, the point is rather that these representations – by design – contain economists' intuitions, or the things they already know, or both. That is, sometimes these small worlds in the model primarily represent speculations and theories about the economic world; the economist may be agnostic about how far they represent the workings of that world, or even deny that they do so at all (as we saw with Lucas), regarding them perhaps as parallel or imagined model worlds. At other times, models are created primarily to incorporate (in some form) features they already know, that is, to embody what the economist takes to be essential

37 Of course, I am not the first to see models as instruments of enquiry in the social sciences (arguably, Max Weber (1904, 1913) thought of his ideal types in this way – see Chapter 4), but few suggestions along these lines explore how such instruments work.

38 A nice parallel is found in the studies of geologists who built small boxes and filled them with different materials to see what happened when big physical shocks hit them as a simulation model for earthquakes (see Oreskes, 2007). On smallness see Chapter 10.

features of the relevant section of the world, how the parts relate, how the elements interact, and so forth, as with Frisch and Tinbergen. Most often, the 'world in the model' represents a combination of both economists' ideas and their knowledge.

These small objects, models, then have a stand-alone, autonomous, quality, that enables them to lead a potentially *double life* for, I argue, *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.[39] Model investigations offer economists the possibilities to speak both to their ideas and to their experience of the world at the same time, but characterizing such work as a method of enquiry, exploration, even discovery, still presents us with quite a puzzle. How do models provide such a method of enquiry that enables this double life to go on? My answer is that model reasoning, as a generic activity in economics, typically involves a *kind of experiment*.

Advancing the argument that appears later in the book, I suggest that we can characterize model reasoning as a kind of experiment in the following way. Models are made to address some particular purpose, and so working with a model typically begins with the economist asking a question related to that purpose. To answer the question, the economist makes an assumption that fixes something in the model, or changes something in the model, that is, in the diagrams or equations, or other material, that the model is made in. He or she then investigates the effect of that assumption, or change in the model, by manipulating the resources of the model in a model experiment to demonstrate an answer. That demonstration is deductively made, for it uses the reasoning rules given in the language format and in the carefully specified economic content of the model. The process of demonstration itself prompts a narrative about the economic content. This combination of *questions*, *experimental demonstrations*, and *narrative answers* forms the way in which the economist explores a particular model (see Morgan 2002 and Chapter 6). From experimenting on the model, economists investigate and come to understand, in the first instance, only the world of the model. How such experimental investigations into the model might also provide some understanding about the world that the model represents is a messier problem that I return to shortly.

Let me begin with the easy part of this double life of models: *models as objects to enquire into*. Economists investigate the world in the model using this mode of experiment to understand their economic ideas or theories. This seems odd: since they created that little world in the model, wouldn't they already understand it? Not

39 The ways that models function in these two domains in economics is not well accounted for by the standard views in philosophy of science that have tended to worry about the definition of models and to treat them either as mini-versions of theories or as efficient descriptions of data from the world. As we will find in the chapters that follow, the diagrammatic models of the Edgeworth Box, Ricardo's arithmetic chains, and Samuelson's mathematical model of the Keynesian system all function as independent forms: they embody ideas and knowledge about the economy, but are themselves neither theories or data descriptions. In Morrison and Morgan (1999), we argued such construction was responsible for the observed practical autonomy of models that enabled them to mediate between the mathematics of theory and the empirics of observation (see Chapter 2).

so, for if ideas about the world can be expressed very simply, economists don't need a model to think with. But as soon as they abstract two or three characteristics of economic man together, or isolate two or three hypothesized relationships from the economy at once, it becomes difficult to reason about what happens when they are combined. That is why economists create models in the first place, and why they need this kind of experimental approach in order to answer questions about this small person or world in the model.

Investigating the world in the model through such experimental means is the way that economists explore their theories and intuitions.[40] By asking questions and making such investigations, they understand the implications of their intuitions, explore the limits of economic behaviour that their models imply, codify and classify the various different outcomes that some more general theory might overlook, and are prompted to develop new hypotheses about the behaviour of the elements represented in the model. For example, Samuelson wanted to know the effect of increasing government expenditure. He found by his experiments on the little mathematical model in his 1939 paper that the model could generate cyclical behaviour, explosive growth, or gradual decline in the elements of the model, according to the numerical parameters he inserted into their relations. These model explorations provided some surprising answers about certain aspects of the Keynesian account of the world as well as generating more understanding about the various extant theories of business cycles.

The second part of this double life of models is the way that economists use *models as objects to enquire with*, for it is clear, from the way economists work, that the small person or world in the model also serves as an object to investigate the aspect of the real people or real world that it is taken to represent. This aspect of model work is much more difficult to characterize than the way economists use models to investigate their ideas and theories.

Philosophers have problems at this point, and for good reasons. Their justly sceptical argument goes as follows. If the model is an accurate representation – in some way – of the relevant parts of the economic world or of economic man's behaviour, and if those elements can be treated in isolation, then it might be that the results gained from model experiments can be applied directly and unambiguously to the world, and give truthful statements and valid explanations about those things in the world.[41] These 'ifs' are big ones – for how does the economist know if they have an accurate model of the world? Or, that it can be treated in isolation? It is this ignorance that creates philosophers' worries about modelling, and,

40 Crombie assumed some kind of a one-for-one relationship: that "a model embodies a theory" (1994, Vol. II, p1087), and on this basis, that the method of models offered "a characteristically effective scientific combination of theoretical and experimental exploration." This is certainly a useful hint about experiments (which he does not expand), but the account of how models are formed in this chapter, and various examples discussed in Chapters 2–6 suggest that the relationships between theories and models are varied and not easy to characterise.

41 See, for a recent discussion, Cartwright (2009).

most especially, their concern about the status of the representation involved. But of course, it is just such problems – and this same lack of knowledge – that lead economists, like scientists in other fields, to adopt modelling as a mode of investigation in the first place!

It may help to clarify my account of modelling as a double method of enquiry in economics if we compare it with two of the other reasoning styles mentioned earlier: the method of mathematical postulation and proof and the method of laboratory experiment.

If we portray mathematical modelling as a version of the method of mathematical postulation and proof, then we could say that economists *postulate* the economic world in the model and so could quite reasonably expect to make mathematical truths about that world in the model. This account works well for *enquiries into* the world of the model: models can indeed be truth-makers about that restricted and mathematical small world. But as economists recognise, these are not truths that they can transport unconditionally to the world that the model represents. Economists (just like their astronomer forebears) understand that a model stands in for their economic universe to enable them to explore certain properties of that world represented in the model. But whether they can come to valid conclusions about the behaviour of their actual economic universe is a much more difficult problem, as they know themselves.

If we make the alternative comparison with laboratory experiments, we get an idea of how economists use a model as an object to *enquire with*. In this way of understanding modelling as an epistemic genre, economists *hypothesize* how the world is when they represent it in the model, and then experiment with that world or person in the model to see how it behaves. Then the important question of whether the results of the experiment on the model can then be transferred to the world that the model represents can be considered an inference problem. So, by treating model enquiries as a form of experiment, the question of how this mode of reasoning connects models to the world switches from a truth-making problem to an inference problem, though no less difficult to answer.[42] This is why I suggest that we view modelling as a method of investigation and enquiry more akin to the method of experiment than to the method of postulation and proof.

Of course, model experiments in economics are usually pen-and-paper, calculator, or computer, experiments on a model world or an analogical world (such as an hydraulic machine), not laboratory experiments on the real world. This has implications for the inferences that can be made. There are two issues here: one is the form of the inference arguments, and the other is the power of the inferences that can be made.

42 Others have suggested that the model-world relation might be thought of in inferential terms, but without seriously considering the nature of the inference in practical terms, or whether the inferential relation lies in the original construction of the model, or rather in its subsequent relation back to the world (see for example Suárez, 2004 and Woody, 2004; and the essays in Grüne-Yanoff, 2009).

Inference arguments from model experiments are informal: when economists talk of 'testing their models' (having already assured themselves of their internal mathematical qualities and coherence) they are interested in judging the usefulness of their model experiments by comparing the behaviour of the model world to that of the real world in a kind of matching or bench-marking process. They may compare the model experimental behaviour of their thin model of economic man with the behaviour of real economic people, or surmise how a particular policy change instituted in a model compares with the equivalent actual policy in the world. A characteristic feature of these informal inference arguments from economic models is that they often involve narratives in making inferential or explanatory accounts that serve to link results *from* the experiment made into the world in the model *to* events in the world that the model represents (discussed in various ways in Chapters 6 to 9).[43]

These informal comparisons made from model experiments to the world clearly lack the formal decision rules based on probability measures found in statistical inference, and that are used to validate and make inferences from econometric models. But it is worth remembering that inferences made from laboratory experiments also lack formal decision rules. Laboratory scientists, like modellers, depend upon both tacit and articulated knowledge in making sense of their experimental findings and judging their relevance within the laboratory.[44] And, like model work, laboratory scientists face the same question of whether their experimental results can form the basis for inference beyond the laboratory, namely the problem of external validity.[45]

But in another respect, clearly, the experiments made on models are different from the experiments made in the laboratory, and the inferences that can be made differ in principle. This has nothing to do with the formality or informality of the inference argument, but rather, as I argue in Chapter 7, it is because model experiments are less powerful as an epistemic genre. It does make a difference to the power and scope of inference that the model experiment is one carried out on a pen-and-paper representation, that is, on the world in the model, not on the world itself. While model experiments may *surprise* the economist with unexpected results, laboratory experiments may *confound* the economist-scientist by producing results that are not only unexpected but potentially unexplainable given existing knowledge.[46]

Let us look briefly at a more complicated example to see how the model is both an object to enquire *into* and an object to enquire *with*, holding these notions of questions, deductive experiments using the resources of the model, and informal inferences, in mind. The Phillips-Newlyn Machine (shown in Figure 1.7 and

43 See Morgan (2001, 2007).

44 It is precisely this difficulty that has led Deborah Mayo to advance her framework for making inferences from experiments (see her 1996), which recognises that such inferences depend on the knowledge of the scientist in making relevant pre- and post-experimental judgements.

45 See Chapters 7 and 8, and Guala (2005, chapter 7).

46 See discussion in Morgan (2003b, 2005).

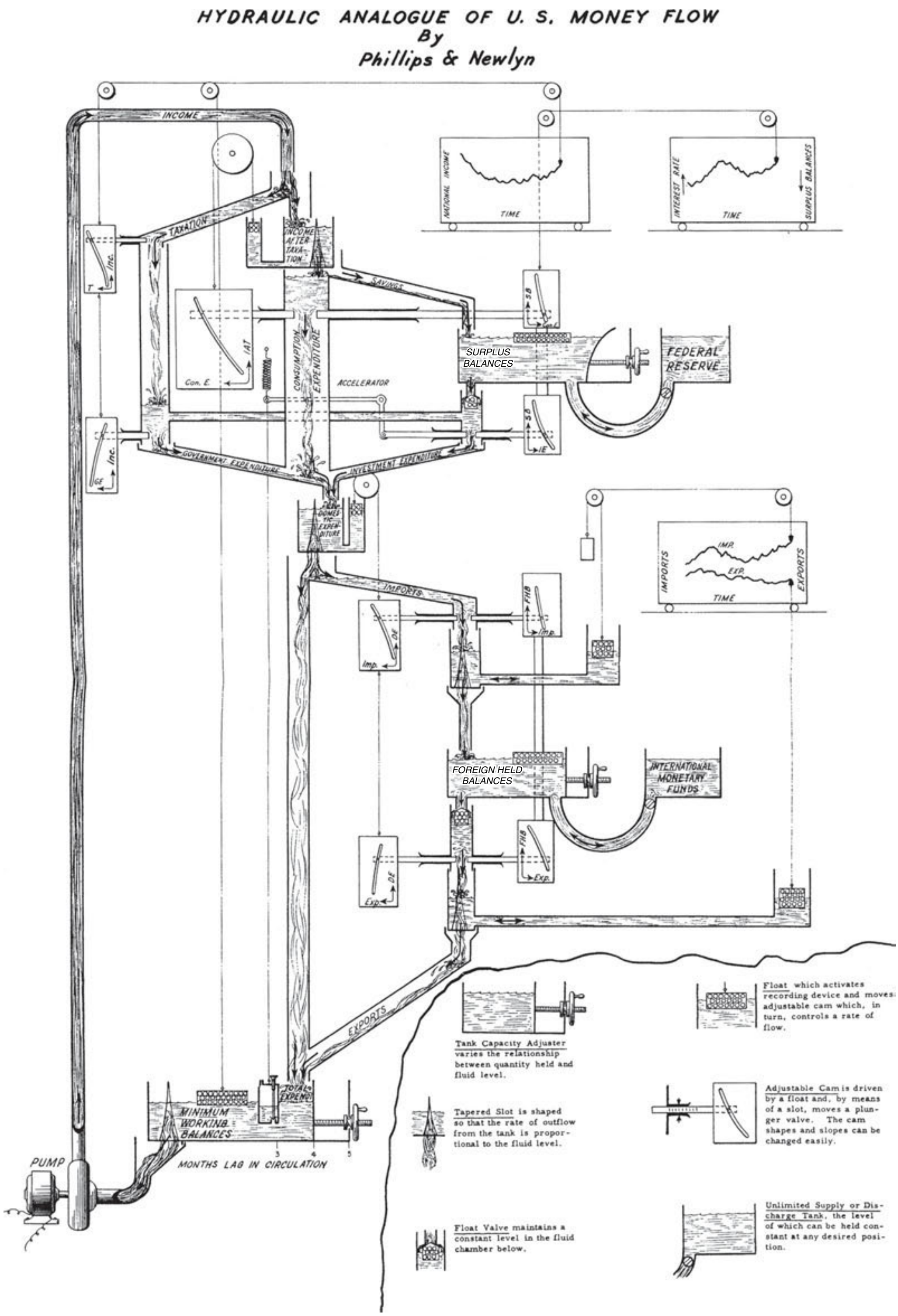


Figure 1.7. The Phillips-Newlyn Hydraulic Machine.

Source: The James Meade Archive, Box 16/3, BLPES Archives, LSE. Reproduced with permission from the estate of James Meade.

discussed fully in Chapter 5) is a big apparatus – a real hydraulic model – of which we can see here only a drawing. The physical model itself operates according to the language rules of hydraulics, with the flow of water around the machine controlled by physical valves. But the overall form and parts of the of the machine are designed to imitate the stocks and flows of money (red water) around an economy, and the behavioural functions of the economic relations are drawn into the small rectangular "slides" that can be seen on the drawing; these in their turn control the opening and closing of the valves in the hydraulic system. Despite its complexity, and even without knowing what these economic relations are, we can see how the rules of form (hydraulics) and content (monetary macroeconomics) are instantiated in the Machine.

The next point to see is how the Machine's resources are reasoned with in an experimental mode of investigations by using the rules of language and content. The economist sets up the model to answer a particular question, such as: What will happen if I increase the money in this system by increasing the liquid in the "money tank" fed by "the central bank" (at the top right)? This is the experimental intervention (or manipulation) into the world of the model. The pump circulates this increased liquid through the machine, the valves control the flows according to the economic relations ascribed in the model, and the model demonstration churns out a set of outcomes of this experiment: the effects of this change in the amount of money on the income in the economy is automatically charted in one of the top right-hand corner graphs.

The Machine model has tremendous resources: it can be set up to answer any number of questions – and thus associated model experiments. With some of these questions the economist can *enquire into* abstruse points in economic theory, for example, as to whether the interest rate is determined by the stock or flow of investment funds. Such questions and experiments about the world in the model make demonstrations that enable those theories to be compared with each other. And once economists have discovered how their world in the model works, they use this knowledge to generate further questions about those theories. Another set of questions are prompted by different historical or current situations that turn up such as financial crises or great depressions. These deliver experimental outcomes for the world in the model that economists will compare with the events that they observe in the world. That is, with these questions, economists *enquire with* the model into the world that the model represents. Economists may come to explain or reinterpret or find a new understanding about some aspects of the real-world behaviour through these experimental means.[47] That is how, by experimenting with the model, economists can gain understanding and provide explanations of *how the*

47 Economists also use this model-generated knowledge to teach others their insights, for example, economists used the Phillips-Newlyn Machine to demonstrate and explain the UK Government policy changes (an experiment with the Machine screened by the BBC and visible now on a video in the London Science Museum next to the Machine).

economic world in the model works and use these in an informal way to *reflect on the workings of the real economy* that the model is taken to represent (see Morgan and Boumans 2004, and Chapter 5).

So, modelling as a style of reasoning in economics works as a method of enquiry comprising probing questions, manipulations to provide demonstrations that are both deductive and experimental, and informal inference arguments involving elements of narrative that offer explanatory or interpretative services. These characteristics are explored in a nutshell format for Ricardo's model farming experiments in the next chapter. And, with a wider gaze, these characteristics of the style of practical reasoning of modelling are explored in different ways, and at much greater depth, in the second half of the book.

6. Conclusion

Reasoning with models enables economists to enquire directly into their theories or ideas about the world, and enables them to enquire indirectly into the nature of the economic world. They reason about the small world in the model and reason about the big economic world with the model; they reason about the thin economic man in the model and reason about real people with the model man. Yet, critically, these two spaces of exploration are not always clearly demarcated: in working with models economists often simultaneously investigate the world in the model and the world their model represents. In this sense, reasoning with economic models is like reasoning with astronomical models. Those models exemplified astronomers' theories about the arrangements of the heavens, and could be used to explore the full implications about those ideas at the very same time as being used to offer explanations or accounts for particular observed events or patterns in the behaviour of the heavenly bodies. Economic models, like those models of the planetary system, are objects to enquire into and argue over, but *at the same time* ones to take to the world and explore it to gain understanding, insight, or explanations from doing so.

The comparison between astronomical models and economic models that has woven its way through this chapter is not just an heuristic comparison that helps us see how economists use models, but reminds us that the modelling style of reasoning has an illustrious history. Indeed, the scientific revolution of the sixteenth and seventeenth centuries was not just one of content, but of styles of reasoning. Modelling has been portrayed as the working method of Galileo no less, and continues to be prevalent in modern natural sciences.[48] Despite this ancestry, economists are not quite sure that the method has a credible scientific respectability. Models are relatively small and simple compared to the economic world, they are made of different materials, and cannot well be applied directly to that world. Even so, like those

48 Hacking, for example, recognises it as the basic method of "cosmology and cognitive science – none other than the chief modern instances of the Galilean style...." (Hacking, 1992a, p. 7).

models of the universe of earlier days, economic models may capture the heart of the problems that economists seek to understand. Modelling is not an easy way to find truths about the economy, but rather a practical form of reasoning for economists, a method of exploration, of enquiry, into both their ideas and their world. That is the thesis of this book.

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References

Backhouse, Roger E. (1998) "If Mathematics Is Informal, Then Perhaps We Should Accept that Economics Must Be Informal Too". *Economic Journal*, 108:451, 1848–58.

Baumol, William (1951) *Economic Dynamics.* New York: Macmillan.

Black, Max (1962) *Models and Metaphors*. Ithaca, NY: Cornell University Press.

Boltzmann, Ludwig (1911) "Models". In *Encyclopaedia Britannica* (11th ed, pp. 638–40). Cambridge: Cambridge University Press.

Boumans, Marcel (1993) "Paul Ehrenfest and Jan Tinbergen: A Case of Limited Physics Transfer". In Neil De Marchi (ed), *Non-Natural Social Science: Reflecting on the Enterprise of More Heat than Light* (pp. 131–56). Annual Supplement to *History of Political Economy*, Vol. 25. Durham, NC: Duke University Press.

(1997) "Lucas and Artificial Worlds". In John B. Davis, D. Wade Hands, and Uskali Mäki (eds), *New Economics and Its History* (pp. 63–88). Annual Supplement to *History of Political Economy*, Vol. 29. Durham, NC: Duke University Press.

(1999) "Built-In Justification". In Mary S. Morgan and Margaret Morrison (eds), *Models as Mediators* (pp. 66–96). Cambridge: Cambridge University Press.

(2005) *How Economists Model the World to Numbers*. London: Routledge.

Brodbeck, May 1968 [1959] "Models, Meaning and Theories". In May Brodbeck (ed), *Readings in the Philosophy of the Social Sciences* (pp. 579–601). New York: Macmillan.

Cartwright, Nancy (1989) *Nature's Capacities and Their Measurement.* Oxford: Clarendon Press.

(2009) "If No Capacities, Then No Credible Worlds. But Can Models Reveal Capacities?" In Till Grüne-Yanoff (ed), *Economic Models as Credible Worlds or Isolating Tools?* Special Issue of *Erkenntnis* 70:45–58.

Charles, Loïc (2003) "The Visual History of the Tableau Économique". *European Journal of the History of Economic Thought*, 10:4, 527–50.

Chemla, Karine (2003) "Generality above Abstraction: The General Expressed in Terms of the Paradigmatic in Mathematics in Ancient China". *Science in Context*, 16:3, 413–58.

Chick, Victoria and Sheila Dow (2001) "Formalism, Logic and Reality: A Keynesian Analysis". *Cambridge Journal of Economics*, 25:6, 705–22.

Colander, David (2000) "The Death of Neoclassical Economics". *Journal of the History of Economic Thought*, 22:2, 127–43.

Crombie, Alistair C. (1988) "Designed in the Mind: Western Visions of Science, Nature and Humankind". *History of Science*, 26, 1–12.

(1994) *Styles of Scientific Thinking in the European Traditions*, Vols. I–III. London: Duckworth.

Edgeworth, Francis Y. (1881) *Mathematical Psychics.* London: Kegan Paul.

(1889) "Opening Presidential Address", Section F: Economic Science and Statistics, *Nature*, Sept. 19, 496–509.

Fisher, Irving (1892/1925) *Mathematical Investigations in the Theory of Value and Prices* (thesis of 1891). New Haven: Yale University Press.

(1911) *The Purchasing Power of Money.* New York: Macmillan.

Fleck, Ludwik (1935; 1979 translation) *Genesis and Development of a Scientific Fact* (translated by F. Bradley and T. J. Trenn). Chicago: University of Chicago Press.

Forrester, John (1996) "If *p*, Then What? Thinking in Cases". *History of the Human Sciences*, 9:3, 1–25.

Foucault, Michel (1970) *The Order of Things: An Archaeology of the Human Sciences.* New York: Random House.

Frisch, Ragnar (1933) "Propagation Problems and Impulse Problems in Dynamic Economics". In *Economic Essays in Honour of Gustav Cassel* (pp. 171–205). London: Allen & Unwin.

Gentner, Dedre (2001) *Analogy in Scientific Discovery: The Case of Johannes Kepler*. In Magnani and Nersessian, 2001, pp. 21–40.

Giere, Ronald (2001) "Models as Parts of Distributed Cognitive Systems". In Lorenzo Magnani and Nancy J. Nersessian (eds), *Model-Based Reasoning: Science, Technology, Values* (pp. 227–42). New York: Kluwer Academic/Plenum Press.

Goldfarb, Robert S. and Jon Ratner (2008) "'Theory' and 'Models': Terminology Through the Looking Glass". *Econ Journal Watch*, 5:1, 91–108.

Goodman, Nelson (1978) *Ways of Worldmaking.* Indianapolis: Hackett.

Gordon, Scott (1991) *The History and Philosophy of Social Science.* New York: Routledge.

Grüne-Yanoff, Till (2009) [ed] *Economic Models as Credible Worlds or Isolating Tools?* Special Issue of *Erkenntnis*, 70:1.

Guala, Francesco (2005) *The Methodology of Experimental Economics.* Cambridge: Cambridge University Press.

Hacking, Ian (1992a) "'Style' for Historians and Philosophers". *Studies in the History and Philosophy of Science*, 23:1, 1–20.

(1992b) "Statistical Language, Statistical Truth and Statistical Reason: The Self-Authentification of a Style of Scientific Reasoning". In Ernan McMullin (ed), *The Social Dimensions of Science* (pp. 130–57). Notre Dame, IN: University of Notre Dame Press.

(1992c) "The Self-Vindication of the Laboratory Sciences". In Andrew Pickering (ed), *Science as Practice and Culture* (pp. 29–64). Chicago: University of Chicago Press.

(1993) "Working in a New World: The Taxonomic Solution". In Paul Howich (ed), *World Changes: Thomas Kuhn and the Nature of Science* (pp. 275–310). Cambridge, MA: MIT Press.

Hamminga, Bert and Neil De Marchi (1994) [eds] *Idealization in Economics*. Amsterdam: Rodopi.

Harford, Tim (2008) *The Logic of Life*. London: Little, Brown.

Harrod, Roy (1939) "An Essay in Dynamic Theory". *Economic Journal*, 49:193, 14–33.

(1968) "What Is a Model?" In N. Wolfe (ed), *Value, Capital and Growth* (pp. 173–91). Edinburgh: Edinburgh University Press.

Hausman, Daniel M. (1992) *The Inexact and Separate Science of Economics.* Cambridge: Cambridge University Press.

Hesse, Mary (1966) *Models and Analogies in Science*. Notre Dame, IN: University of Notre Dame Press.

Hicks, John, R. (1937) "Mr. Keynes and the 'Classics': A Suggested Interpretation". *Econometrica*, 5, 147–59.

Hoover, Kevin D. (1995) "Facts and Artifacts: Calibration and the Empirical Assessment of Real-Business-Cycle Models". *Oxford Economic Papers*, 47:1, 24–44.

Hughes, R. I. G. (1997) "Models and Representation". *Philosophy of Science,* 64:S325–36.

Humphrey, Thomas, M. (1995) "When Geometry Emerged: Some Neglected Early Contributions to Offer-Curve Analysis". *Economic Quarterly*, 81:2, 39–73.

Ingrao, Bruna and Giorgio Israel (1987) *The Invisible Hand*, English edition (1990). Cambridge, MA: MIT Press.

Israel, Giorgio (2002) "The Two Faces of Mathematical Modelling: Objectivism vs Subjectivism, Simplicity vs Complexity". In P. Cerria, P. Fregiglio and C. Pellegrini (eds), *The Application of Mathematics to the Sciences of Nature* (pp. 233–). New York: Kluwer.

Jevons, William St. (1871) *The Theory of Political Economy.* London: Macmillan.

Johnson, W. E. (1896) "Method of Political Economy". In R. H. Inglis Palgrave (ed), *Dictionary of Political Economy*, Vol. II (pp. 739–48). London: Macmillan.

Katzner, Donald W. (1991) "In Defense of Formalization in Economics". *Methodus*, 3:1, 17–24.

Keynes, John M. (1936) *The General Theory of Employment*, *Interest and Money*. London: Macmillan.

(1973) *The Collected Writings of John Maynard Keynes*, Vol. XIV, ed D. Moggridge. London: Macmillan.

Klein, Judy L. (2001) "Reflections from the Age of Economic Measurement". In Judy L. Klein and Mary S. Morgan (eds), *The Age of Economic Measurement* (pp. 111–36). Annual Supplement to *History of Political Economy*, Vol. 33. Durham, NC: Duke University Press.

Klein, Ursula (2001) [ed] *Tools and Modes of Representation in the Laboratory Sciences*. Boston Studies in the Philosophy of Science. Dordrecht: Kluwer.

(2003) *Experiments, Models, Paper Tools: Cultures of Organic Chemistry in the Nineteenth Century*. Stanford, CA: Stanford University Press.

Knuuttila, Tarja (2005) "Models, Representation, and Mediation". *Philosophy of Science*, 72, 1260–71.

Krugman, Paul (1998) "Two Cheers for Formalism". *Economic Journal*, 108:451, 1829–36.

Kuczynski, M. and Ronald Meek (1972) *Quesnay's Tableau Économique*. London: Macmillan.

Kuhn, Thomas (1962) *The Structure of Scientific Revolutions.* Chicago: University of Chicago Press.

Le Gall, Philippe (2007) *A History of Econometrics in France.* London: Routledge.
Leontief, Wassily W. (1946) "The Pure Theory of the Guaranteed Annual Wage Contract". *Journal of Political Economy*, 54, 76–79.
Levitt, Steven D. and Stephen J. Dubner (2005) *Freakonomics.* New York: Harper Perennial.
Louça, Francisco (2007) *The Years of High Econometrics*. London: Routledge.
Lucas, Robert E. (1980) "Methods and Problems in Business Cycle Theory". *Journal of Money*, *Credit and Banking*, 12, 696–715.
Luce R. Duncan and Howard Raiffa (1957) *Games and Decisions.* New York: Wiley.
Maas, Harro (2005) *William Stanley Jevons and the Making of Modern Economics*. Cambridge: Cambridge University Press.
MacKenzie, Donald A. (2006) *An Engine, Not a Camera: How Financial Models Shape Markets* Cambridge, MA: MIT Press.
Magnani, Lorenzo and Nancy J. Nersessian (2001) [eds] *Model-Based Reasoning: Science, Technology, Values*. New York: Kluwer Academic/Plenum Press.
Mäki, Uskali (1992) "On the Method of Isolation in Economics". In Craig Dilworth (ed), *Idealization IV: Intelligibility in Science* (pp. 317–51). Amsterdam: Rodopi.
(2004) "Realism and the Nature of Theory: A Lesson from J. H. von Thünen for Economists and Geographers". *Environment and Planning A*, 36, 1719–36.
Malthus, Thomas R. (1803) "An Essay on the Principle of Population". Everyman edition, 1914, reprinted 1982. London: Dent.
Marshall, Alfred (1879) "The Pure Theory of Foreign Trade". In John K. Whitaker [ed] (1975), *The Early Economic Writings of Alfred Marshall, 1867–1890*, Vol. 2, Part III.5. London: Macmillan for the Royal Economic Society.
Mayo, Deborah (1996) *Error and the Growth of Experimental Knowledge.* Chicago: University of Chicago Press.
McCloskey, D. N. (1991) "Economic Science: A Search Through the Hyperspace of Assumptions". *Methodus*, 3:1, 6–16.
McMullin, Ernan (1985) "Galilean Idealization." *Studies in History and Philosophy of Science*, 16:3, 247–73.
Meade, James E. (1937) "A Simplified Model of Mr. Keynes' System". *Review of Economic Studies*, 4:2, 98–107.
Meli, Domenico Bertoloni (2006) *Thinking with Objects: The Transformation of Mechanics in the Seventeenth Century.* Baltimore: Johns Hopkins University Press.
Mirowski, Philip (2002) *Machine Dreams: Economics Becomes a Cyborg Science.* Cambridge: Cambridge University Press.
Morgan, Mary S. (1990) *The History of Econometric Ideas.* Cambridge: Cambridge University Press.
(1996) "Idealization and Modelling" (A Review Essay). *Journal of Economic Methodology*, 3:1, 131–8.
(1997) "The Technology of Analogical Models: Irving Fisher's Monetary Worlds". *Philosophy of Science*, 64, S304–14.
(1999) "Learning from Models". In Mary S. Morgan and Margaret Morrison (eds), *Models as Mediators: Perspectives on Natural and Social Sciences* (pp. 347–88). Cambridge: Cambridge University Press.
(2001) "Models, Stories and the Economic World". *Journal of Economic Methodology*, 8(3), 361–84.
(2002) "Model Experiments and Models in Experiments". In Lorenzo Magnani and Nancy Nersessian (eds), *Model-Based Reasoning: Science, Technology, Values* (pp. 41–58). Dordrecht: Kluwer Academic/Plenum Press.

(2003a) "Economics". In T. Porter and D. Ross (eds), *The Cambridge History of Science*, Vol. 7: *The Modern Social Sciences* (pp. 275–305). Cambridge: Cambridge University Press.

(2003b) "Experiments Without Material Intervention: Model Experiments, Virtual Experiments and Virtually Experiments". In H. Radder (ed), *The Philosophy of Scientific Experimentation* (pp. 216–35). Pittsburgh: University of Pittsburgh Press.

(2004) "Imagination and Imaging in Economic Model-building". *Philosophy of Science* (Proceedings of the 2002 Biennial Meeting of the Philosophy of Science Association), 71:5, 753–66.

(2005) "Experiments versus Models: New Phenomena, Inference and Surprise". *Journal of Economic Methodology*, 12:2, 317–29.

(2006) "Economic Man as Model Man: Ideal Types, Idealization and Caricatures". *Journal of the History of Economic Thought*, 28:1, 1–27.

(2007) "The Curious Case of the Prisoner's Dilemma: Model Situation? Exemplary Narrative?" In A. Creager, M. Norton Wise, and E. Lunbeck, *Science Without Laws: Model Systems, Cases, Exemplary Narratives* (pp. 157–85). Durham, NC: Duke University Press.

(2008/online) "Models". In S. N. Durlauf and L. E. Blume (eds), *The New Palgrave Dictionary of Economics*, 2nd ed. London: Palgrave Macmillan http://www.dictionaryofeconomics.com/dictionary

Morgan, Mary S. and Marcel Boumans (2004) "Secrets Hidden by Two-Dimensionality: The Economy as a Hydraulic Machine". In Soraya de Chadarevian and Nicholas Hopwood (eds), *Models: The Third Dimension of Science* (pp. 369–401). Stanford, CA: Stanford University Press.

Morgan, Mary S. and Tarja Knuuttila (2012) "Models and Modelling in Economics"; forthcoming in Uskali Mäki (ed), *Handbook of the Philosophy of Economics* (one volume of the *Handbook of the Philosophy of Science*. General Editors: Dov Gabbay, Paul Thagard, and John Woods). Amsterdam: Elsevier/North-Holland. Available at: http://papers.sspn.com/sol3/papers.cfm?abstract_id=1499975.

Morgan, Mary S. and Margaret Morrison (1999) [eds] *Models as Mediators: Perspectives on Natural and Social Science*. Cambridge: Cambridge University Press.

Morrison, Margaret and Mary S. Morgan (1999) "Models as Mediating Instruments". In Mary S. Morgan and Margaret Morrison (eds), *Models as Mediators: Perspectives on Natural and Social Science* (pp. 10–37). Cambridge: Cambridge University Press.

Nersessian, Nancy J. (1992) "In the Theoretician's Laboratory: Thought Experimenting as Mental Modelling". *PSA Proceedings of the Biennial Meeting of the Philosophy of Science Association*, 2, 291–301.

(2008) *Creating Scientific Concepts*. Cambridge, MA: MIT Press.

Niehans, Jürg (1990) *A History of Economic Theory*. Baltimore: Johns Hopkins University Press.

Oreskes, Naomi (2007) "From Scaling to Simulation: Changing Meanings and Ambitions of Models in the Earth Sciences". In Angela Creager, M. Norton Wise, and Elizabeth Lunbeck (eds), *Science Without Laws: Model Systems, Cases, Exemplary Narratives* (pp. 93–124). Durham, NC: Duke University Press.

Pigou, Arthur C. (1931) "The Function of Economic Analysis". In Arthur C. Pigou and Dennis H. Robertson, *Economic Essays and Addresses* (pp. 1–19). London: P. S. King & Son.

Qin, Duo (1993) *The Formation of Econometrics.* Oxford: Clarendon.

Rheinberger, Hans-Jörg (1997) *Towards a History of Epistemic Things*. Stanford, CA: Stanford University Press.

Ricardo, David (1821) *The Principles of Political Economy and Taxation* (3 editions: 1817, 1819 and 1821; 1821 reprinted in Piero Sraffa and Maurice H. Dobb (eds), Vol. 1:

Collected Works and Correspondence of David Ricardo (1951). Cambridge: Cambridge University Press.

Robinson, Joan (1933) *The Economics of Imperfect Competition.* London: Macmillan.

Samuelson, Paul A. (1939) "Interactions between the Multiplier Analysis and the Principle of Acceleration". *Review of Economics and Statistics*, 21, 75–78.

Schumpeter, Joseph A. (1935) "The Analysis of Economic Change". *Review of Economic Statistics*, 17:4, 2–10.

Shapin, Steven and Simon Schaffer (1985) *Leviathan and the Air-Pump*. Princeton: Princeton University Press.

Solow, Robert M. (1997) "How Did Economics Get That Way and What Way Did It Get?" *Daedalus*, 126:1, 39–58 (reprinted fall 2005, *Daedalus*).

Suárez, Mauricio (2004) "An Inferential Conception of Scientific Representation." *Philosophy of Science* (Proceedings of the 2002 Biennial Meeting of the Philosophy of Science Association), 71:5, 767–79.

(2008) [ed] *Fictions in Science: Philosophical Essays on Modeling and Idealization*. New York and London: Routledge.

Sugden, R. (2002) "The Status of Theoretical Models in Economics". In U. Mäki (ed), *Fact and Fiction in Economics: Models, Realism and Social Construction* (pp. 107–36). Cambridge: Cambridge University Press.

(2009) "Credible Worlds, Capacities and Mechanisms". In Till Grüne-Yanoff (ed), *Economic Models as Credible Worlds or Ioslating Tools?* Special Issue of *Erkenntnis*, 70:1, 3–27.

Tinbergen, Jan (1937) *An Econometric Approach to Business Cycle Problems.* Paris: Hermann & Cie.

Van den Berg, Richard (2002) "Contemporary Responses to the Tableau Économique". In S. Boehm, C. Gehrke, H. D. Kurz, and R. Sturn (eds), *Is There Progress in Economics?* (pp. 295–316). Cheltenham: Edward Elgar.

Von Thünen, Johann Heinrich ([1826]1966) *Von Thünen's Isolated State* (English translation of *Der isolierte Staat*, 1966, translated by Carla M. Wartenberg; ed Peter Hall). Oxford: Pergamon Press.

Vosniadou, Stella (2001) "Mental Models in Conceptual Development". In Lorenzo Magnani and Nancy J. Nersessian (eds), *Model-Based Reasoning: Science, Technology, Values* (pp. 353–68). New York: Kluwer Academic/Plenum Press.

Walras, Leon (1874/1954) *Elements of Pure Economics*, translated by William Jaffé. London: Allen and Unwin for the American Economic Association and Royal Economic Society.

Weber, Max (1904) "Objectivity in Social Science and Social Policy". In *The Methodology of the Social Sciences* translated and edited by Edward A. Shils and Henry A. Finch, 1949 (pp. 49–112). New York: Free Press.

(1913) *The Theory of Social and Economic Organisations* (translated by A. M. Henderson and Talcott Parsons, Part I of *Wirtshaft und Gesellschaft*, 1947). New York: Free Press.

Weintraub, E. Roy (2002) *How Economics Became a Mathematical Science*. Durham, NC: Duke University Press.

(2008) "Mathematics and Economics". In Steven Durlauf and Lawrence Blume (eds), *The New Palgrave Dictionary of Economics*, 2nd ed. London: Macmillan; Available online at http://www.dictionaryofeconomics.com/article?id=pde2008_M000372&goto=M&result_number=2269.

Woody, Andrea (2004) "More Telltale Signs: What Attention to Representation Reveals about Scientific Explanation". *Philosophy of Science* (Proceedings of the 2002 Biennial Meeting of the Philosophy of Science Association), 71:5, 780–93.