

MODELLING AS A METHOD OF ENQUIRY

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Forthcoming as Chapter 1 in

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Chapter 1: Modelling as a Method of Enquiry¹

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Part I: Changing the Practice of Economic Science

1. From laws to models, from words to objects

Two hundred years ago, political economy was primarily a verbal science, with questions, concepts and a mode of reasoning all dependent on words. As a *science*, classical political economy of the eighteenth century and early nineteenth century posited a few general laws, whose workings and outcomes were often difficult to perceive clearly amongst the complicated changing events of everyday life. One of the few laws that was expressed in numbers was due to the Rev'd Thomas Robert Malthus, who posited a law of population in which the growth of people, driven by their passions, increased in a way that would inevitably outstrip the more pedestrian growth of food supplies. Given such laws, he argued that there must be checks at work in the world: the numbers of people must be held down either through disease,

¹ Elements of this chapter were written for an address "Forms and Tools in 20th Century Economics" to the Association Charles Gide September 1999 in Paris, at their meeting on the theme "Modèles formels et théorie économique: histoire, analyse, épistémologiques". I thank Annie Cot and the Scientific Organising Committee of that society for their invitation, memorable to me for the imposing room in the Sorbonne in which it took place, and for the fact that I shared the platform with a Nobel Prize winner about whom this chapter is partly about - Gerard Debreu (CHECK). The argument was subsequently presented at Duke University in February 2000. More recent elements of the chapter have been developed for talks to development specialists at Bonn (ZEF), at Rotterdam (econometrics seminar), at the University of Oslo (on the 75th Anniversary of their Institute of Economics), at the Summer Institute for History of Economic Thought at George Mason University, at Les Treilles, France (a workshop on style in science), at the University of Toronto (HPS seminar) and at Chung Cheng University, Taiwan (conference on Models and Evolution in Economics). I am grateful to those who commented on all these occasions, to Charles Baden-Fuller for commenting so carefully on various drafts, and to Sheldon Steed for research assistance.

famine, and war, or by virtue of celibacy or delayed marriage. While the laws were thought to have an iron grip on economic life, it was not easy to see those laws at work, although the checks might be evident. This created difficulties for the *art* of political economy, whose aim was to fashion policy advice around an understanding of those few general scientific laws.²

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.³ And 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 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 manipulatable objects are the practical starting point in economic research work: they are used for theorizing, provide hypotheses and design constraints for laboratory experiments, are an essential input into simulations, and 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, indeed, anywhere that economic decisions are made in a more technocratic than casual way. As in many other modern sciences, models have become endemic at every level in economics.

The significance and radical nature of this change in economics is easily overlooked. The introduction of this new kind of scientific object - namely models - involved not just the adoption of new languages of expression into economics (such as arithmetic or

² For example, Malthus’ laws of population were based on the aggregation of individual behaviour and lead to his policy arguments against social security schemes which, in the process of supporting the poor, would interfere with these checks on population growth, see Malthus, 1803.

³ For the twentieth century development of economics into a tool-based science, see Morgan, 2003.

geometry), but of introducing 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, but unseen, laws, but expressed it in a multitude of more specific models. As models replaced laws, so economists came to interpret the behaviour and phenomena they saw in the economic world directly in terms of those models.⁴

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 lays claim to being the first such research object in economics.

Diagram: Quesnay's *Tableau Economique*

The *Tableau Economique* is a really wonderful numerical object: a cross between a table and a matrix, it presents an accounting portrait of the French economy. 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 giving the amount of goods or money being transferred between the groups of people. It was invented in the 1760s by François Quesnay, economist, and physician in the court of Louis XV and thus at the centre of French political life in the mid-eighteenth century.⁵ 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

⁴ So, by the early twenty first century, for example, we find 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 re-interpreting those events as examples of these small worlds depicted in economic models (for example, Harford, 2008, or his weekly column in the *Financial Times*). I return to this point in Chapter 10.

⁵ Examples (for there are several) of Quesnay's *Tableau* are found in Kuczynski and Meek (1972) and in Charles (2003).

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 Economique*, 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 this was not simply a passive portrait of the economy, rather, it had the internal resources for Quesnay to 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.⁶ 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 which 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 very special object, unique perhaps in its day, but its very specificity raises for us 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 lot about their particularities to see how they work, then how can we characterise the scientific

⁶ For recent scholarship which investigates the likely sources of the *tableau*, its various versions, and how it was used, see particularly Charles (2003) and Van den Berg (2002).

practice of modelling in a general way. This raises philosophical questions: How do economists construct 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 characterise 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.⁷ Sometime in the past, economists had to begin to think with such objects, learn how to use them, and 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: the historical and philosophical changes from reasoning with words to reasoning with models, is what this book is about. Yet these historical and philosophical aspects cannot be easily untangled. At the meta level, we can point to the considerable but gradual historical shift in the way economists reason, a shift involving elements 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

⁷ 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.)

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.⁸

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 pre-history. 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 in 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 “pre-history” of models, we find that not only does Quesnay’s *Tableau Economique* exist as an object out of its time in the eighteenth century, but there are 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. Another is the diagram of farm prices in relation to distance from towns, drawn by the German agriculturalist Johann von Thünen (1826) depicting an idealised abstract, landscape but with numbers drawn

⁸ It is appropriate here to refer to two 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.

from his experience of farming at his own estate of Tellow.⁹ These three objects - the *Tableau*, the accounting tables, 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.

Set I: Pre-History of Models: von Thunen's diagram and Ricardo's tables

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 those few economists involved felt they had to justify their construction and usage of such odd research objects 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.¹⁰ Marshall thought that such diagrams were useful if they could be illustrated with examples from economic life, and then he often presented them in the footnotes; but if such pieces of mathematics were not useful, they should be burnt! In 1881, the Irish economist Francis Edgeworth outlined a different perspective on exchange relations 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 a offering a “representative particular” argument (see Chapter 3). In 1892, Irving Fisher, an American economist, designed

⁹ 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.

¹⁰ This was the first appearance of these curves in the history of economic theorizing about trade relations, in which Humphrey (1995, p 41) argues: 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 the some parts of this paper was first presented. Marshall's views of mathematics are discussed by Weintraub (2002).

and constructed an hydraulic mechanism to represent, explore, and so understand the workings of a mini-economy, one with only three goods and three consumers.¹¹ He accompanied this work with an outright defence of these research objects: mathematical, graphical and real machines, that he designed and used for his economic analysis.

Set II: 1st Generation of Models: Marshall's trade diagram and Fisher's hydraulic machine

It seems reasonable to locate these three economists in the first real generation of model builders, and their self-consciousness about their research objects as indicative of this moment of change. This late nineteenth century moment was noticed a little 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”.¹² Because economics is now dependent upon such research objects, all these examples can now be understood as models, though, neither in the pre-history period, nor in this late nineteenth century moment, would economists have recognised them as such nor 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 conceptualised, gained the label ‘model’, and a full understanding of their usefulness developed. Two economists played an important role in this transformation, thus sparking their wider deployment in economic analysis. In 1933, in the depths of the Great Depression, the Norwegian economist Ragnar Frisch was responsible for developing 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, as a recipe, it formed the basis for

¹¹ 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”.

¹² See Pigou, 1931 (a lecture of 1929), 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 the concurrent development of mathematical and statistical techniques. I return to the issue of “keystones” in Chapter 10.

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.

Set III: 2nd Generation Models: Hicks's IS-LL diagram, Samuelson's equations Frisch's macro-dynamic equations scheme and Tinbergen's Dutch model

Tinbergen was also responsible for transferring the term 'model' in the early 1930s from physics, where it had usually referred to a physical object, into economics, to refer to the statistical and mathematical objects that he and Frisch were then developing.¹³ So by the middle 1930s, the label 'model' had come into use, though not everyone had noticed it.¹⁴ For example, John Hicks in 1937, invented a "little apparatus" (p 156), his IS-LL diagram, to compare the workings of J.M. Keynes's new macroeconomics (of 1936) with that of the older classical system. In that same year, in another attempt to turn Keynes's theory into something more comprehensible, James Meade provided an 8-equation algebraic treatment, calling it a 'A Simplified Model of Mr Keynes' System', while soon after Paul Samuelson produced a smaller set of equations to exemplify and explain the Keynesian relations, describing it as "a new model sequence" (1939, p 75). (Two of these three examples are shown in Set III.)

¹³ Ludwig Boltzmann had defined the term 'model' in the sense of a physical 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 the likely route for the transfer of the term into economics (see Boumans 2005, Chapter 2).

¹⁴ 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 macro-economic 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, p 284-305). Keynes himself seemed to prefer the term "schema" or "schematism" which, as we will see later, has a slightly different connotation: it indicates an outline, a map, rather than an apparatus which might be manipulated.

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 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 small set of equations (1939) 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 was 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, p173.)

This brief history has enabled me to indicate the historical contours of when modelling was introduced and when it became the normal mode of reasoning in economists: from isolated examples in the pre-history, 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 in the period from the 1940s onwards.

This brief history has also introduced some of the most important models from the history of economics and so allows me to give a better sense of what a model is. Models are not easy objects either to define or to generally describe, but those reproduced here exemplify the sort of things that count as models in economics: either real, or pen-and-paper, objects - the latter being diagrammatic or mathematical. Despite their variations in form, these objects share recognisable characteristics: each depicts, renders, denotes, or in some way provides some kind of representation of our 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 11th century Bayeux Tapestry and building sites

in the 15th century frescoes of the recently re-opened 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 for Quesnay's *Tableau Economique*, there we found not just a picture but a depiction of the economy 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.¹⁵

Hick's 1937 terminology of a 'little apparatus' nicely captures the manipulability of such research objects - that they can be played around with in various ways - even though his model is only made of paper and pen compared to the real apparatus of Fisher's earlier hydraulic model. These two examples have an obvious affinity with the physical 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 constructed 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 planets 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 call simply "modelling". By the later half of the twentieth century, such 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 became the relevant

¹⁵ The label *Tableau* is indicative that some tables of numbers may also have this manipulatable quality, and so reasoning with them is also a possibility, for example, Leontief input-output tables both represent the economy and can be manipulated.

way of working - although this book is not primarily about econometrics.¹⁶ 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".¹⁷

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,

¹⁶ The history of modelling in economics has been barely considered except in econometrics (on which see Morgan, 1990, Boumans, 1993 and 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-30s in his discussion of it as a new practice in both statistical and mathematical terms. Solow (1997/2005) offers some suggestions about its take-off in the 1950s and as a rare exception, compares these mathematics to 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 says not 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.

¹⁷ 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 for example, Goldfarb and Ratner, 2008). If pressed, they might suggest that a theory is more general than a model, or that it is less preliminary, less speculative, than a model. 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.

Nature and Humankind”.¹⁸ It is worth listing them all here - in the chronological order that they appeared in the history of the sciences.

1. Mathematical postulation and proof
2. Experiment
3. Hypothetical modelling
4. Taxonomy (the method of classification into natural kinds)
5. Statistical
6. Historical-genetic¹⁹

As practical styles of reasoning, these categories - of modelling, or statistical analysis, or experimental work - do not provide the kind of detailed description 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’.²⁰ Rather, these categories provide a framework for historical epistemology in the sense that they enable 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, this literature does provide us with certain resources to understand our specific story about how modelling as an epistemic genre came into economics and what kind of difference it made.²¹

¹⁸ 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) actually predated Crombie’s main publication.

¹⁹ 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.

²⁰ See Fleck (1935/1979), Foucault (1970), Kuhn (1962) and Rheinberger (1997).

²¹ 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 (1993).

According to Crombie, modelling grew up in the field of early modern sciences and arts - in the making of automata: mechanical 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 earth and the heavens and in the craft skills found in the construction 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), and, although we have good examples of analogical models in economics, analogical aspects no longer constitute a distinguishing feature of model construction in economics. 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.

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 inquiry. They were built to represent the relationships - hypothesized by the early astronomers - between the earth and the heavens. They were carefully designed not just to present or illustrate known relationships but 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 which were manipulatable (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 presumably lead Ian Hacking (1992a) to suggest that Crombie’s style of “thinking” should be replaced by “reasoning”.²² 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

²² The practical aspects of this are important: for like Hacking, even this term “reasoning” seems underrates the importance of the “manipulative hand and the attentive eye” (Hacking, 1992a, p 4).

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. And, to follow the example into economics: classroom experiments began there in the 1940s, though the activity was sufficiently limited that economists only experienced their own battle for the acceptance of the experimental method within economics in the period after 1970. Yet it is worth our noting too, that in economics, as in many modern sciences, the individual styles have begun to hybridise. 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 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. Similarly 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 p10) 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.²³

3.ii Modelling as a Reasoning Style in Economics

For economics, the contours of this historical move into modelling and its widespread acceptance have already been limned, yet its history as a style of practical reasoning is not a simple one. There was of course no blank page in economics before modelling took over. Early economists used technical and conceptual terms (the terminology of a science), but reasoned with them in the modes of ordinary verbal argument. As modelling developed, it first partly over-layered 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 modes of reasoning (see Chapters 7 and 9 respectively). 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 we can think of as either 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

²³ 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.

of hypothetical modelling using mathematical models. We have already seen how the first generation of model builders in the late nineteenth century generated a new practise 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, but 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.²⁴

Nevertheless, we can reasonably contrast, as exemplars of these two reasoning styles in the late nineteenth century, Fisher's hydraulic/mechanical model of his 3-commodity, 3-person economy (pictured above) 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 which we can call models, and method of reasoning with them, being developed by economists such as Fisher and Marshall. The fact that Fisher built his hydraulic model to represent Walras's ideas, and to figure out by exploring with his model the *process* by which the latter's mathematically postulated and proved general equilibrium might be arrived at, shows us the 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

²⁴ Further discussion about the history of mathematical reasoning and analogical modelling in economics are contained in Weintraub (2002) and Ingrao and Israel (1987/90).

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 building, and we know that economics became mathematized while it also 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 our attention on the objects, the models themselves, rather than on their mathematics. Here, as we have already found, history matters whenever we are discussing any particular example of a model, for models are contingent not timeless: we need history to understand how any particular model was built, how it was used, and what understanding economists gained from it. But what we really need to do is to capture and explicate the generic qualities that we can find at work in the earlier models of Quesnay, and Ricardo, just as much as the twentieth century work of Frisch and Samuelson. In order 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 Models²⁵

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

How do economists make models? 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 begin afresh

²⁵ 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 Knuuttila and Morgan (2008). Consequently, this chapter does not provide an additional survey: rather some elements are discussed in this chapter and others in the chapters that follow.

with it.²⁶ 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 when we create 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 with in economic science. I make use of some more examples of economic models (reproduced here) 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 our 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’ our ideas, namely as a process of recipe making, of visualizing, of idealizing, or of choosing analogies. These four

²⁶ 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 which “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).

accounts offer four different ways to understand how models get made. (They are not labels for different kinds of models.) And which account is most relevant or useful in helping us to make sense of any particular model-making experience can not be pre-judged.²⁷

The first of these four accounts of how models are made sees the process of giving form to our ideas as analogous to recipe making. Boumans' (1999) recipe notion embeds two ideas: economists choose the model's ingredients - their ideas, intuitions, 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 was shown earlier): he integrated these elements together and reasoned with them till they emerged as the accounts of a model farm representing the agricultural economy of his day (as we shall see in the next 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's ideas about the macroeconomy by fitting together the simplified or basic elements and relations of the macroeconomy (see diagram earlier, and discussion in Chapter 6). Once synthesized, the new model recipe depicted certain macro-economic relations in a new form (the IS-LM model) that proved flexible to many different interpretations and had a remarkably long life.

An alternative account of model making derives from another comparison, this time drawing on the similarities between the practises of representation in arts and sciences that draws inspiration from 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

²⁷ Nor are such accounts necessarily mutually exclusive in accounting for any particular model-building episode. 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).

example, Edgeworth's first drawing (1881) of the relationships between Robinson Crusoe and Man Friday 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 which was far from self-evident but depended on the processes of imagination and image making by a sequence of different economists, each of whom had a particular way of envisioning economic relations and portraying them into these little 2-D diagrammatic/mathematical forms (see example by Leontief below and as described in Chapter 3). In this account, 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 the mathematical modelling in physics (for example, 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 (eg "in an ideal world, there is no friction"). Such accounts also supply a way to understand model formation in economics. Thus, Nancy Cartwright has used the term to discuss how economists build 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.²⁸ As an example here, Jevons' graphed economic man's experience of utility as dependent upon only two dimensions, its intensity and duration (reproduced below). He did so because, by his own account these were the two most salient elements in motivating man's 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

²⁸ 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 Knuuttila and Morgan (2008); Hamminga & De Marchi (1994) provide an important collection of earlier papers reviewed in Morgan 1996.

problem mathematically.²⁹ Such idealization processes of giving form to economic models can even be described using Max Weber's account (1904 and 1913) of the formation of ideal types, or even of caricatures, in the social sciences (see Chapter 4, and Morgan, 2006).

Set IV Jevons' 1871 diagram of utility, Fisher's 1911 balance, Phillips' 1947 plumbing diagram, Leontief Box 1946, Luce and Raiffa 1957 prisoner's dilemma matrix

Another 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.³⁰ 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. In choosing a mechanical balance as a model for the equation of exchange between money and goods (diagram shown), for example, Fisher (1911) recognised the similarity between the mechanical balance and the economic subject matter in his arithmetic "equation of exchange" (see Morgan, 1999). This ability to recognise similarities, and so to chose 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. For example, Phillips drew the little plumbing diagram (above) 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 Chapter 5).³¹

More recently, economists have themselves suggested that the point of modelling is

²⁹ 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 that drew on his own working experiences and on his creativity as a scientist (see Maas, 2005).

³⁰ See also Gentner (2001).

³¹ 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 1-D representation of a metaphor to the 2-D analogical model, as in the little diagram by Phillips of a market as a plumbing arrangement, or to fully formed 3-D model as in Fisher's built hydraulic machine (see Chapter 5).

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”.³² Sugden has argued that modellers create “credible worlds”, where the credibility rests on some observed similarities in model outcomes, for example, between those of a checkerboard puzzle and segregated housing.³³ In seeking to capture not the workings of real economies but to mimic some aspect of it, 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. In effect, models are imagined worlds but ones which are designed to provide imitations.

The activity of giving form to a model has been characterised in a number of 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 those 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-building 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.

First, 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 constructed by some knowing economist-scientist for some particular purpose. Whether the scientist is best described as making a new recipe, using their imagination and imaging powers, idealizing from some other account, or choosing between different analogies: the point is that models don’t make themselves.

The second point is that forming models is not driven by a logical process but rather

³² 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.

³³ 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.

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 Economique*, but they remain in the delight that economists take in creating 'elegant' models.

Third, model-making is a skilled job: 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, nor 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. These abilities in articulating and crafting models, along with their imaginative and creative ones (above), turn up in different ratios in different episodes of model making, but they are all essential to the process of giving form to models.

Fourth, in making models, scientists form some kind of a representation of something in the economy. While the activity of writing or drawing a model can be described variously as: representing, depicting, imitating or imaging; other 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 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.³⁴

³⁴ 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'

4.ii Becoming Formal

Each of these processes of understanding model making, building, choosing, visualizing - call it what you will - 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 our 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, s/he 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. Samuelson's equations model shown earlier can be manipulated following the formal rules given 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

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" (p S331) denotation is " independent of resemblance" (1997, S330). 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 Knuuttila and Morgan (2008), 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 discuss this question of inference briefly later in this chapter and more fully in later chapters.

representation of a material model (as in Fisher's mechanical balance). They are if you like, '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. In the prisoner's dilemma model (just as with Quesnay's *Tableau Economique*) the matrix of numbers needs to be accompanied by a text account of the economic rules for the situations that the table represents before any particular case of reasoning can be done with it (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-builder 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.³⁵ 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. 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

³⁵ 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).

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, 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 Resources for reasoning with

I argued earlier that representations only become models when they have the resources for manipulation: this forms the third element in model building which unlocks the puzzle of how any particular model can be reasoned with.³⁶ Here we return to the earlier example from 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

³⁶ 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.

business cycles in economic activity.³⁷

Frisch's first version of his model, in his now classic paper of 1933, was a schema of economic activity (shown in his Figure 1 here, on which capital letters indicate stocks and small 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) (1933, p 173). He called it a *Tableau Economique*, surely a reference back to Quesnay's famous invention, and we can see that like that earlier example, Frisch visualized quite a complicated set of circular flows (indicated by arrows) around the elements in the system. Relative to Quesnay's *TE* 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 only be used 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.

Diagram: Frisch's *Tableau Economique*

From this scheme, Frisch developed a more simplified little mathematical model connecting y , the annual production of capital goods, with x , the annual production of consumer goods, and z_t , the amount of production going on at time t (shown earlier). 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, produced 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

³⁷ 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 econometric; Boumans (1999) on it as a new recipe in modelling the business cycle; and Louça (2007) on its analogical aspects.

a cyclical pattern in all three elements of productive economic activity over time. 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).

In this example, the contrast between Frisch's *Tableau Economique* 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 much understanding about the business cycle 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 contrasting 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.

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).³⁸

³⁸ Of course, I am not the first to see models as instruments of enquiry in the social sciences (arguably, Max Weber (1904 and 1913), thought of his ideal types in this way - see this book Chapter 4), but few suggestions along these lines explore how such instruments work.

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 in another form. 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 they make are smaller-scale, and it is supposed, simpler, than the real world, made of quite different materials, and whose quality of representation, imitation or similarity might be quite opaque.³⁹ These representations - by construction - may 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 and 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 time, models are primarily constructed to incorporate (in some form) features they already know, that is, to embody what the economist takes to be essential 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 that other world, the real world economy.⁴⁰ Model investigations offer economists the

³⁹ A nice parallel is found in the studies of geologists who built small boxes and filled them with different materials - sand or wax - to see what happened when big physical shocks hit them as a simulation model for earthquakes (see Oreskes, 2007).

⁴⁰ 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

possibilities to speak both to their ideas and to their experience of the world at the same time, but characterising 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 characterise 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. In order to answer the question, the economist makes an assumption that fixes something in the model, or changes something in the model, ie in the diagrams or equations, or other material that the model is made in. S/he then investigates the effect of that assumption, or change in the model, by using the manipulatable resources of the model 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. These *questions, experiments, demonstrations* and *answers* are the way in which the economist explores a particular model (see Morgan 2002 and Chapter 6). From their experiments 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 also provide some understanding about the world that the model represents is a messier problem that I return to in a minute.

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 - they made the model, they surely already understand that little world they created? Not 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

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.

combined. That is why they create models in the first place. So, much model reasoning consists of answering questions by investigating this small person or world in the model with the kind of experimental approach outlined above.

Investigating the world in the model through such experimental means is the way that economists explore their theories and intuitions. By asking questions and making such investigations, they understand the implications of their intuitions, explore the limits of economic behaviour that their models imply, and 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 and found by his experiments in his 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.⁴¹

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 characterise 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

⁴¹ 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.

model experiments can be applied directly and unambiguously to the world, and give truthful statements and valid explanations about those things in the world.⁴² 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, most especially, their concern about the status of the representation involved. But of course, it was just such questions, and this same lack of knowledge, that lead economists, as scientists in other fields, to adopt modelling as a mode of investigation in the first place!

It may help to clarify this account of modelling as a double method of enquiry 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 form of the method of mathematical postulation and proof, then we can say that economists *postulate* the economic world in the model and so can 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 be truth-makers about that restricted and mathematical world picture. 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

⁴² See, for a recent example, Cartwright, 2009.

less difficult to answer.⁴³ 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.

Inference arguments from model experiments are informal: when economists talk of testing their models they are interested in the usefulness of their model experiments and judge them 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 Chapters 6 and 9).⁴⁴

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 used to validate and make inferences from econometric models. But it is worth remembering that so, equally, do inferences made from laboratory experiments. Laboratory scientists must also depend upon both tacit and articulated knowledge in making sense of their experimental findings and judging their relevance within the

⁴³ Others have suggested that the model-world relation might be thought of in inferential terms, but with debates over the nature of such inference and whether the inferential relation lies in the original construction of the model, or in its subsequent relation back to the world rather (see for example Suárez, 2004 and Woody, 2004; and the essays in Grüne-Yanoff, 2009).

⁴⁴ See Morgan 2001 and 2007.

laboratory.⁴⁵ And, like model work, laboratory scientists too face the question of whether those experimental results found within their laboratories can form the basis for inference beyond the laboratory, namely the problem of external validity.⁴⁶

But in another respect, clearly, the experiments made on models are different from the experiments made in the laboratory and the nature of 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 which are not only unexpected but potentially unexplainable given existing knowledge.⁴⁷

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 Newlyn-Phillips Machine (discussed at length in Chapter 5, and shown here) was 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 flowing around the machine controlled by physical valves. But the overall form and parts of the of the machine were 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” which 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

⁴⁵ It is precisely this difficulty that has lead 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.

⁴⁶ See Chapters 7 and 8, and Guala, 2005, Chapter 7.

⁴⁷ See discussion in Morgan 2003a, and 2005.

(hydraulics) and content (monetary macroeconomics) are instantiated in the Machine.

Picture of Newlyn-Phillips 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 arrangements, and the model demonstration churns out a set of outcomes of this experiment: the results of a change in the amount of money on the economy's income are 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 the economist 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 re-interpret or find a new understanding about some aspects about the real world behaviour through these experimental means.⁴⁸ By experimenting with the model, economists can gain understanding and provide explanations of *how the economic world in the model works* and use these in an informal way to *reflect on the workings of the real economy*

⁴⁸ Economists also use this model generated knowledge to teach others their insights, for example, economists used the Philips-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.

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 enable 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, and so their explorations with the model often go on in a manner which simultaneously investigates 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 which helps us see how economists use models, but keeps reminding 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.⁴⁹ Despite this ancestry, economists are not fully 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 models of the universe of earlier days, economic models may still 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.

⁴⁹ 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).

References

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

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.

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

Boltzmann, Ludwig (1911) “Models” in *Encyclopaedia Britannica*, 11th Edition (Cambridge: Cambridge University Press), pp 638-640.

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*, (Durham, NC: Duke University Press).

Boumans, Marcel (1997) “Lucas and Artificial Worlds” in *New economics and Its History*, John B. Davis, D. Wade Hands and Uskali Mäki, eds, *History of Political Economy*, Annual Supplement to Vol 29 (Durham: Duke University Press), pp 63-88.

Boumans, Marcel (1999) “Built-In Justification” in Mary S. Morgan and Margaret Morrison (eds) *Models as Mediators* (Cambridge: Cambridge University Press), pp 66-96.

Boumans, Marcel. (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*, (Macmillan, New York), pp. 579-601.

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

Cartwright, Nancy (2009) “If no capacities, then no credible worlds. But can models reveal capacities?” in Grüne-Yanoff, ed, 2009, pp 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 & 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, pp 127-143.

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

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

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

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

Fisher, Irving (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* (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 Magnani & Nersessian, 2001, pp 227-42.

Goldfarb, Robert S. & 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 Publishing Co.).
- Gordon, Scott (1991) *The History and Philosophy of Social Science* (New York: Routledge).
- Grüne-Yanoff, Till (2009) *Economic Models as Credible Worlds or Isolating Tools?* Ed, 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.
- Hacking, Ian (1992b) “Statistical Language, Statistical Truth and Statistical Reason: The Self-Authentication of a Style of Scientific Reasoning” in Ernan McMullin (ed), *The Social Dimensions of Science* (Notre Dame: University of Notre Dame Press) pp 130-157.
- Hacking, Ian (1992c) “The Self-Vindication of the Laboratory Sciences” in Andrew Pickering (ed) *Science as Practice and Culture* (Chicago: University of Chicago Press), pp 29-64.
- Hacking, Ian (1993) “Working in a New World: The Taxonomic Solution” in Paul Howich (ed), *World Changes: Thomas Kuhn and the Nature of Science* (Cambridge, Mass: MIT Press) pp 275-310.
- 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.
- Harrod, Roy (1968) “What is a Model?” in *Value, Capital and Growth*, ed N. Wolfe (Edinburgh: Edinburgh University Press) pp 173-191.
- 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: University of Notre Dame Press).

Hicks, John, R. (1937) “Mr. Keynes and the ‘Classics’; A Suggested Interpretation” *Econometrica*, 5, 147-159.

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

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

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

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

Johnson, W.E. (1896) “Method of Political Economy” in *Dictionary of Political Economy*, Vol II, edited by R.H. Inglis Palgrave (London : Macmillan) pp 739-48.

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).

Keynes, John M. (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 & Mary S. Morgan *The Age of Economic Measurement*, Annual Supplement to Vol 33, *History of Political Economy* (Durham: 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).

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

Knuuttila, Tarja (2005), “Models, Representation, and Mediation”, *Philosophy of Science* 72: 1260-1271.

Knuuttila, Tarja and Mary S. Morgan (2008) “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).
- Krugman, Paul (1998) “Two Cheers for Formalism” *Economic Journal*, 108:451, 1829-36.
- Kuczynski, M and Ronald Meek (1972) *Quesnay’s Tableau Economique* (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.
- 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).
- McCloskey, D.N. (1991) “Economic Science: A Search Through the Hyperspace of Assumptions” *Methodus*, 3:1, 6-16.
- MacKenzie, Donald A. (2006) *An Engine, Not a Camera: How Financial Models Shape Markets* (Cambridge, Mass.: MIT Press).
- McMullin, Ernan (1985) “Galilean Idealization”, *Studies in History and Philosophy of Science*, 16:3, 247-73.
- Maas, Harro (2005) *William Stanley Jevons and the Making of Modern Economics*, Cambridge: Cambridge University Press.
- Magnani, Lorenzo & Nancy J. Nersessian (2001) *Model-Based Reasoning: Science Technology, Values* (New York: Kluwer Academic/Plenum Publishers).
- Mäki, Uskali. (1992). On the method of isolation in economics. In Craig Dilworth (Ed.). *Idealization IV: Intelligibility in Science*. (pp. 317-351). Amsterdam: Rodopi.
- Mäki, Uskali (2004), “Realism and the nature of theory: A lesson from J.H. von Thünen for economists and geographers”, *Environment and Planning A*, 36, 1719-

1736.

Malthus, Thomas R (1803) *An Essay on the Principle of Population* in Everyman edition, 1914, reprinted 1982 (London: Dent).

Marshall, Alfred (1879) "The Pure Theory of Foreign Trade" in *The early Economic Writings of Alfred Marshall, 1867-1890, Vol 2, Part III.5* edited John K. Whitaker (London: Macmillan for the Royal Economic Society).

Mayo, Deborah (1996) *Error and the Growth of Experimental Knowledge* (Chicago: University of Chicago Press).

Meade, James E. (1937) "A Simplified Model of Mr. Keynes' System', *Review of Economic Studies* 4:2: 98-107.

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).

Morgan, Mary S. (1996) "Idealization and Modelling" (A Review Essay) *Journal of Economic Methodology* 3:1, 131-8.

Morgan, Mary S. (1997) "The Technology of Analogical Models: Irving Fisher's Monetary Worlds" *Philosophy of Science* 64, S304-314.

Morgan, Mary S. (1999) "Learning from Models" (in Morgan and Morrison, 1999, pp 347-88).

Morgan, Mary S. (2001), "Models, stories and the economic world, *Journal of Economic Methodology*, 8(3) 361-84 (also in Mäki, 2002, pp 178-201).

Morgan, Mary S. (2002) "Model experiments and models in experiments" in *Model-Based Reasoning: Science, Technology, Values* Lorenzo Magnani and Nancy Nersessian (eds) (Dordrecht: Kluwer Academic/Plenum Press), pp 41-58.

Morgan, Mary S. (2003) "Economics" in *The Cambridge History of Science, Volume 7: The Modern Social Sciences*, eds: T. Porter and D. Ross (Cambridge: Cambridge University Press), pp 275-305.

Morgan Mary S. (2003a) "Experiments Without Material Intervention: Model Experiments, Virtual Experiments and Virtually Experiments" in *The Philosophy of*

Scientific Experimentation ed H. Radder, University of Pittsburgh Press, pp 216-35.

Morgan, Mary S. (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.

Morgan, Mary S. (2005) “Experiments Versus Models: New Phenomena, Inference and Surprise” *Journal of Economic Methodology*, 12:2, 317-29.

Morgan, Mary S. (2006) “Economic Man as Model Man: Ideal types, Idealization and Caricatures” *Journal of the History of Economic Thought* 28:1, 1-27.

Morgan, Mary S. (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*, (Duke University Press) pp 157-85.

Morgan, Mary S, (2008/online) “Models” in *The New Palgrave Dictionary of Economics*, 2nd edition, eds: S.N. Durlauf and L.E. Blume (Palgrave Macmillan; <http://www.dictionaryofeconomics.com/dictionary>)

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

Morgan, Mary S. & Margaret Morrison (ed.) (1999) *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 Morgan and Morrison (1999), pp. 10-37.

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*, Vol 2, 291-301.

Nersessian, Nancy J. (2008) *Creating Scientific Concepts* (Cambridge, Mass.: MIT Press).

Niehans, Jürg (1990) *A History of Economic Theory*, Baltimore: Johns Hopkins 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*, (Duke University Press) pp 93-124.

Pigou, Arthur C. (1931) “The Function of Economic Analysis” in *Economic Essays and Addresses* by Arthur C. Pigou and Dennis H. Robertson (London, P.S. King & Son), pp1-19.

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

Ricardo, David (1821) *The Principles of Political Economy and Taxation* (3 editions:1817, 1819 and 1821; 1821 reprinted in Vol 1, *Collected Works and Correspondence of David Ricardo*, eds Piero Sraffa and Maurice H. Dobb, 1951 (Cambridge: Cambridge University Press).

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

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

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, same journal).

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, 767-779.

Suárez, Mauricio (2008) ed: *Fictions in Science: Philosophical Essays on Modeling and Idealization* (New York & 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* (Cambridge: Cambridge University Press).

Sugden, R. (2009) “Credible Worlds, Capacities and Mechanisms” in Grüne-Yanoff, (2009) ; 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 *Is the Progress in Economics?* S. Boehm, C. Gehrke, H.D. Kurz and R. Sturn, eds, (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 Magnani & Nersessian, 2001, pp 353-68.

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, Edward A. Shils and Henry A. Finch (1949) New York: Free Press.

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

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

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, 780-793.