their behalf. It is only a short step, then, from voluntary euthanasia (self-inflicted or authorized), to directed euthanasia administered to a patient who has given no authorization, to involuntary euthanasia conducted as part of a social policy. As social policy, it would give society or its representatives the authority to eliminate all those who might be considered too 'ill' to function normally any longer.

4. Arguments from Analogy

Analogies add richness to our language. Often they are used for poetic or picturesque purposes, but they are also used to clarify our claims or as a basis for conclusions. An analogy draws a partial similarity between two different things by identifying similar features they both possess. A neurosurgeon delivering a lecture on the structure of the human brain might introduce her lecture by saying that 'The brain is like a highly efficient and compact computer' and then organize her lecture around specific similarities.

As long as no conclusion is drawn from the comparison between the brain and a computer, we do not have an argument. It is an analogy, but one used simply for elucidation. When the comparison is used as a basis for some conclusion we then have an argument from analogy: the drawing of the conclusion that two things are analogous in a certain respect because they are analogous in one or more other ways. If on the basis of similarities that characterize both the human brain and the computer and from the fact that computers are machines, the neurosurgeon concluded that 'Humans are just complicated machines', we would then have an argument from analogy.

We call the two things compared in an analogy 'analogues'. For purposes of exhibiting the structure of and discussing analogical arguments, we can label the analogues X and Y. Those respects in which X and Y are said to be alike can be represented as p, q, r, and so on. Each of these letters represents a statement that is true of both X and Y. Since Y is like X in possessing the qualities p, q, r, . . . we conclude that it possesses some additional quality z that we know X possesses. Schematically, the argument can be depicted as follows:

X—p, q, r, . . . z.
Y—p, q, r, . . .
Therefore, Y—z.

The analogues do not have to be single entities. One or the other or both may be groups of things, in which case the form of the argument may look like this:

X, W, R, S—p, q, r, . . . z.
Y—p, q, r, . . .
Therefore, Y—z.

We analyse analogical reasoning in terms of the similarities between the analogues. In the extreme case, the analogues, X and Y, will be identical. In that event, the conclusion necessarily follows, for any property X has will be shared by Y. If X and Y are identical twins, for example, then finding out something about X's genetic make-up allows us to conclude the same of Y. Other instances where the conclusion of an argument from analogy is guaranteed by its premises are cases where the properties that X and Y share entail z. Two arguments, X and Y, let us say, have the same structure; hence if one is valid, the other one must also be valid. But the cases we usually consider are cases where X and Y are not identical and where the conclusion that X has property z is only probable, given the premises of the argument.

Such analogies are often used in science. Medical research provides ample examples in which discoveries about the effect that particular substances have on rats or other mammals are used as a basis for conclusions about the effects they will have on humans. Because there would be disadvantages to carrying out initial tests on humans, medical researchers use a species with a physiological system analogous to that of humans and conclude that humans would probably be similarly affected.

A convincing argument from analogy must enumerate real and not just apparent similarities between the analogues. Furthermore, the similarities must be relevant to the conclusion, and not incidental or irrelevant features. Often this will mean that particular aspects of an object are relevant in some contexts and not in others. If you want to buy a new car and decide to buy one similar to a friend's, then you must ask yourself what aspects of her car attract you. If it is style and appearance, then a comparable car will be one with similar shape, interior, and perhaps colour. If it is dependability that matters most to you, then you would focus instead on such things as engine design and size.

Considerations such as these lead us to define a good argument from analogy as:

> An argument that supports a conclusion about Y by pointing out (i) that it is true of X, and (ii) that Y is similar in sufficient relevant respects, and not relevantly dissimilar.

A good argument from analogy will have premises that establish that the conclusion does hold of the first analogue and that the analogues are similar in ways that are relevant to the conclusion, and it will not overlook any relevant dissimilarities.

Given these criteria, a good argument *against* an argument from analogy will show either that the purported conclusion does not hold of the first analogue or that the two things being compared are not analogous and should not be considered similarly. To construct a good argument along these lines, we must show that the criteria for a good argument from analogy cannot be met

or are not met in a specific argument. If someone argues, for example, that human evil is like the thorns of a rose, and therefore necessary while not detracting from the overall beauty, we can reply that the comparison between humans and roses is simply too far-fetched and unsupported to provide evidence for the conclusion. In the process we construct an argument from disanalogy.

One of the best-known examples of an argument from analogy in the history of philosophy is the 'argument from design', which states that given that the universe exhibits a particular order, predictability, and design, it is reasonable to infer from this the existence of a designer. This designer is, of course, God, and the argument from design is one of the traditional proofs for the existence of God. David Hume, the eighteenth-century Scottish philosopher, provides an interesting discussion of this argument in his *Dialogues Concerning Natural Religion*. His argument takes place in the context of a dialogue between several participants, which allows Hume both to present the argument from analogy and then to criticize it.

> Look round the world . . . you will find it to be nothing but one great machine, subdivided into an infinite number of lesser machines, which again admit of subdivisions to a degree beyond what human sense and faculties can trace and explain. All these various machines, and even their most minute parts, are adjusted to each other with an accuracy which ravishes into admiration all men who have ever contemplated them. The curious adapting of means to ends, throughout all nature, resembles exactly, though it much exceeds, the productions of human contrivance—of human design, thought, wisdom, and intelligence. Since therefore the effects resemble each other, we are led to infer, by all the rules of analogy, that the causes also resemble, and that the Author of nature is somewhat similar to the mind of man, though possessed of much larger faculties, proportioned to the grandeur of the work which he has executed.

The world is compared to one large machine, made up of innumerable smaller machines, all of which are purposeful, ordered, and precise. The manner in which everything in nature appears to happen for a reason, to fulfil a particular purpose, suggests the product of a specific design, akin to human design but far superior. This resemblance in the origins or causes suggests that the Author of nature is analogous to the human mind, though on a much greater scale, appropriate to the larger scale of creation as a whole. Hume's particular modification here is to argue less for the existence of God as for the nature of God: we infer the nature and mind of God by analogy with our nature and mind, only extended to divine proportions.

Hume next proceeds to criticize the analogy, in the voice of another participant in the dialogue, Philo, who proclaims it to be a very weak analogy. Suggesting that analogies weaken the moment we shift the terms of reference,

Philo states that when we see a house, we conclude that it had an architect or builder, because this is the kind of effect we have observed to result from that kind of cause. However, he continues:

> Surely you will not affirm that the universe bears such a resemblance to a house that we can with the same certainty infer a similar cause, or that the analogy is here entire and perfect. The dissimilitude is so striking that the utmost you can here pretend to is a guess, a conjecture, a presumption concerning a similar cause.

Philo's point is that the dissimilarities between the universe and house are so great that they threaten to outweigh the similarities and the analogy breaks down; therefore the attempt to infer, on the basis of one thing, a similar cause in the other—or any cause at all in the case of a proof of God's existence—is fruitless. In fact, what we have is a guess, not a fully convincing argument.

Hume's discussion goes on to elaborate the ways in which the universe and a house differ, much of it rooted in the basic tenet of his philosophy, that nothing we do not know from experience can be proven or accepted, and while we know from experience where houses come from, we cannot say the same about the universe. Much more could be said about this discussion, but our purpose is to illustrate a basic and renowned philosophical example of an argument from analogy and its attempted counter-argument.

A more contemporary example of an argument from analogy comes from a letter to the *Toronto Star* (27 Apr. 1983) and centres on a definition argued from analogy:

> Whether or not a fetus is a human being is a matter of personal opinion but nobody can deny that forcing a woman to carry and give birth to a child against her will is an act of enslavement. Consider: someone approaches you and demands to be hooked up to your life support system for nine months, on the grounds that this is necessary for survival. It would be an unselfish gesture to comply, but you have every right to refuse. After all, it's your body—isn't it?

Using the method of diagramming we introduced in Chapter 2, we can diagram the argument as follows:

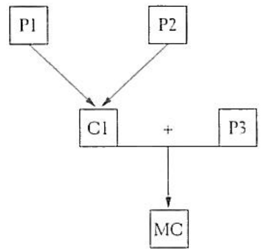
P1 = In forcing a woman to carry and give birth to a child and in forcing you to allow someone to be hooked up to your life support system, it is one's own body that is being used.

P2 = In both cases, the use of one's body is necessary to ensure survival of the person or the fetus.

C1 = Forcing a woman to carry and give birth to a child against a woman's will is like forcing you to allow someone to be hooked up to your life support system for nine months.

P3 = In the second case, you would have the right to refuse to comply (it would be an act of enslavement to force you to comply).

MC = A woman has every right to refuse to carry a child (it would be an act of enslavement to force her to carry it).



At first glance, this argument may strike you as plausible. One of the analogues appears somewhat fanciful, but there are grounds for comparing these two situations because, allowing that the fetus is a person, they both involve the dependence of one person on another. There are, however, at least two major dissimilarities that have been omitted, and once they have been introduced, MC, and therefore the whole argument, becomes completely unacceptable.

Should someone approach you and demand to be hooked to your life support system, it would be quite reasonable for you to point out that you are in no way responsible for that individual's predicament and to require some justification as to why that demand should be made specifically of you. In the case of a mother carrying a child, she usually bears some responsibility for her situation, and the justification for the demand implicitly made by the fetus is quite unlike that in the other analogue. There the person approaching you comes from the outside and, presumably, already has some autonomous existence. But in the case of a pregnancy, the fetus has developed from within and has had no antecedent existence. MC will not, therefore, be acceptable to any reasonable audience. If the two situations are to be analogous, these dissimilarities would have to be eradicated. If we can eradicate them (imagine, for the moment, that you are responsible for the predicament of the person who needs to be hooked up to your life support system), it is no longer obvious that it would be wrong to force you to support the sufferer in question.

This example illustrates the importance of being very clear about what counts as a relevant dissimilarity in any argument from analogy we construct.

Weaknesses in analogical arguments invariably arise because we overlook some relevant dissimilarity when drawing our conclusion. While the addition of relevant similarities strengthens an argument from analogy, the presence of major dissimilarities irretrievably weakens it.

Box 11.5 Arguments from Analogy
Arguments from analogy are founded on claims that individuals or states of affairs are analogous. A good argument from analogy supports a conclusion about Y by pointing out (i) that it is true of some X, and (ii) that Y is similar in sufficient relevant respects and not relevantly dissimilar.

5. Appeals to Precedent

Morality and law require consistency. Similar cases must be dealt with in a similar way and, hence, we may appeal to precedents to establish that a particular situation should be treated in a particular way. If analogous cases were treated this way in the past, then justice demands that they be treated in the same way in the present and future. If two householders are allowed to add an addition to their house, this sets a precedent for other people in the neighbourhood to do likewise.

When we argue by appealing to a precedent we are arguing by analogy. The key question is whether the particular case that is said to be a precedent is analogous to the other cases with which we compare it. The underlying principle is one of justice. Consistency demands that a new situation, similar to a preceding one, be treated as the preceding one was treated. To resolve a dispute containing an appeal to precedent, we must decide whether the cases in question really are analogous. In particular, we must determine whether there are any relevant dissimilarities between the case at hand and the precedent with which it is compared.

Appeals to precedent can take a negative or a positive form. Sometimes we argue negatively that some action will set an undesirable precedent, allowing actions that are unacceptable. A professor may argue that it would be unfair to accept a late paper from one student because he or she must then accept late papers from other students in similar situations. In the present context, the important point is that this argument depends on the claim that the actions of other students will be analogous to those of the student in question and cannot morally or legally be separated.

We can also use appeals to precedent to argue that a given case should be treated in a particular way because it will establish a commendable precedent. We might argue, for instance, that we should prosecute a particular industrial polluter and not forgive a first offence since consistency would then demand that we forgive other first offenders.

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

3. Practical Reasoning Styles

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

3.i Modelling as a Style of Reasoning

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

3.ii Modelling as a Reasoning Style in Economics

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

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

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

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

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

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

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

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

PART II: MAKING MODELS, USING MODELS

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

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

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

use of a deductive mode of manipulation so that Frisch can reason mathematically about the nature of the business cycle with this version of his model.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

43 See Morgan (2001, 2007).
44 It is precisely this difficulty that has led Deborah Mayo to advance her framework for making inferences from experiments (see her 1996), which recognises that such inferences depend on the knowledge of the scientist in making relevant pre- and post-experimental judgements.
45 See Chapters 7 and 8, and Guala (2005, chapter 7).
46 See discussion in Morgan (2003b, 2005).

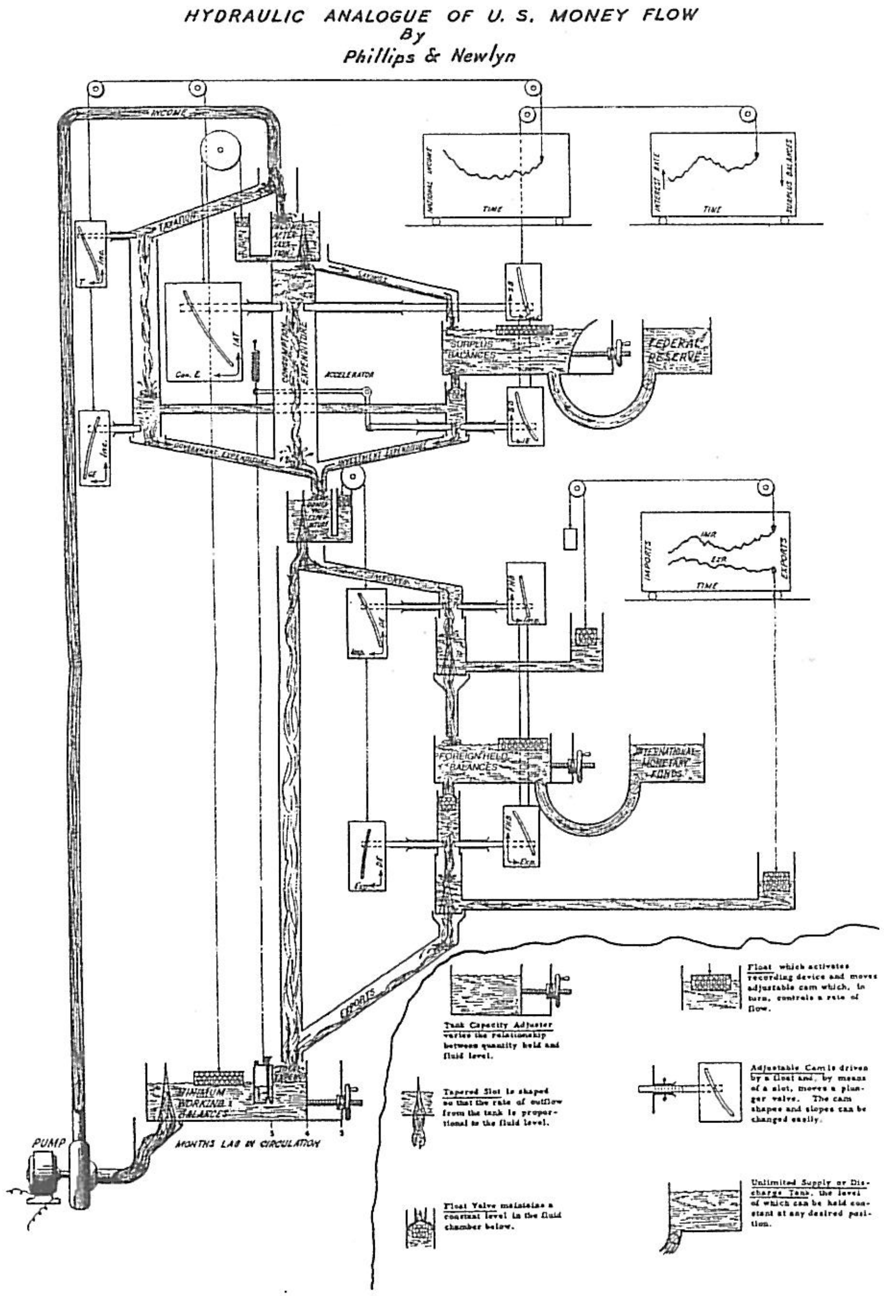


Figure 1.7. The Phillips-Newlyn Hydraulic Machine.

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

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

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

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

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

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

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

6. Conclusion

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

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

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

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

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