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Models, stories and the economic world

Mary S. Morgan[1]

Abstract Stories form an integral part of models. An economic model can not be fully characterized simply by knowing its structure: the model can only be completely described when we know how it works and what it can do. This activity of manipulating a model requires a narrative device, such as a question, which sets off a story told with the model. The structure or system portrayed in the model constrains and shapes the stories that can be told, but without stories showing how the structure works, we cannot tell what might happen in specific cases. Without these narrative elements, we cannot apply model-structures directly onto the facts of the economic world, nor demonstrate outcomes about the hypothetical world represented in the model. Thus, stories are not simply devices of persuasion, but constitute an important part of the identity of a model.

Keywords: models, stories, questions, narrative explanation, models as structure, models as metaphors

1 MODELS, STORIES AND THE ECONOMIC WORLD

It is conventional amongst philosophers of science to think of models in their relation to theory, and thus to concentrate on the model-theory relationship. Here, I want to concentrate on the relation of models to the world. In this paper, I claim that the way models help us to describe and understand the economic world in which we live is by telling stories about the world. That story may be a story about the real world (past, present or future), or it may be a story about the hypothetical world portrayed in the model: the relationship of the story to the model structure is the same. Modelling involves a style of scientific thinking in which the argument is structured by the model, but in which the application is achieved via a narrative prompted by an external fact, an imagined event or question to be answered. Economists use their economic models to explain or to understand the facts of the world by telling stories about how those facts might have arisen. The stories are neither 'merely heuristic' nor 'just rhetoric' but an essential part of the way models are labelled and used.

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2 THE STORY SO FAR

There are two accounts in which stories figure prominently in the methodological literature on models in economics. First, chronologically, we have the account by Gibbard and Varian (1978). They claim, in one of the few philosophical accounts of models in economics, that stories are an integral part of the way models work. Their account is relevant for my purposes because they set out to deal with the models of economic theorists (not econometricians), and concentrate on 'descriptive' models which 'attempt to describe, *in some sense*, economic reality' (p. 665) rather than on 'ideal' models. They liken such descriptive models to realistic drawings which help us to gain greater understanding of the thing depicted. On their definition:

> A model . . . is a *story* with a specified *structure*. The *structure* is given by the logical and mathematical form of a set of postulates, the *assumptions* of the model. The structure forms an uninterpreted system, . . . Although the term 'model' is often applied to a structure alone, we shall use it in another sense. In economists' use of models, there is always an element of interpretation: the models always tells a story.
>
> (Gibbard and Varian 1978: 666)

This sounds very promising until we read on and find that the interpretation necessary for an applied economic model is rather narrow. It consists of telling us what the symbols in the model refer to in the world (firms, consumers and the like):

> If we think of the structure as containing uninterpreted predicates, quantifiers, and the like, we can think of the story as telling what kind of extension each predicate has and what kind of domain each quantifier has. The story may be vague . . . the structure itself must be specified with the precision needed for mathematical reasoning.
>
> (Gibbard and Varian 1978: 666–7)

It is not clear what 'the story may be vague' means, nor that the interpretation necessarily involves a connected sequence of events which could be described as story telling. They also make the more conventional point that they derive theorems from the postulates and this tells us things about the structure that we did not know before, but there is no mention of stories in this context either.

Remarkably then, having introduced the story motif, they more or less ignore it. Two other elements of their argument can be brought together to suggest how we might proceed to make their account into one in which stories do matter.

One element is their claim that 'all economic models . . . pose(s) a question of the form 'What would happen if such and such were the case?' in such a way that it can be answered deductively' (p. 668). This characterization makes

models into some kind of counterfactual. They are counterfactuals in that they begin, as they say, with assumptions, which are known to be unrealistic – counter to the facts. A prominent usage of counterfactuals in economics has been in economic history where they work by taking the known/agreed facts, assuming that they did not hold, and exploring the implications in an empirical way – spinning out alternative possible facts or false histories (as in the classic example of Fogel and the railroads). In this kind of counterfactual, the exercise is empirical. In the Gibbard and Varian case, the exercise starts from assumptions, which we take to be true (when we know they are not), and 'deductions' not 'facts' follow from assumptions. Thus, the question posed can only be answered deductively. But this does not necessarily require any stories, for deductions do not necessarily have a narrative structure. And if the models provide only deductive answers to the question, and the assumptions are thought to be unrealistic, how then do models relate to the world?

The relevant part of Gibbard and Varian's paper which might answer this question comes in their discussion of 'The fit of models to the world'. Remember that they are concerned with the application of theoretical models, not empirical models, and here they suggest that one of the ways that economists apply models to situations is 'casually':

> The goal of casual application is to explain aspects of the world that can be noticed without explicit techniques of measurement. In some cases, an aspect of the world (such as price dispersal, housing segregation, and the like) is noticed, and certain aspects of the micro-situation are thought perhaps to explain it; a model is then constructed to provide the explanation. In other cases, an aspect of the micro-world is noticed, and a model is used to investigate the kinds of effects such a factor could be expected to have.
>
> (Gibbard and Varian 1978: 672)

So their applied models relate to casually observed facts about the economy and the function of the model is to provide an explanation for that casually observed fact. But how?

According to Gibbard and Varian then, models pose a question in such a way that it can be answered deductively, and are used to explain our casual observations about the world. We might assume that the model question 'What would happen if . . . ?' can only be answered by a story. But why should this be so? We might be tempted to read into their account of how models explain, that they do this by telling a story about the observed phenomena. But why should models explain in this way? I agree with Gibbard and Varian when they say you need stories, but it is not yet clear why their account of modelling requires stories or exactly what they mean when they say that a model is a 'story with a specified structure'. The gaps in the Gibbard and Varian account are that they don't explain how the story is told from the

model: they don't deal with the relationship of the story to the structure, nor how such stories might be deductive.

The second account of why we need stories in economics is by Dierdre McCloskey. In her original account of stories (1990b) (there are one or two wrinkles that creep in later), models are metaphors, and stories are something entirely different. Metaphors and stories provide two different ways of understanding things, two ways of answering the question 'Why?':

> When a biologist is asked to explain why the moulting glands of a crab are located just as they are he has two possibilities. Either he can call on a model – a metaphor – of rationality inside the crab, explaining that locating them just *there* will maximise the efficiency of the glands in operation; or he can tell a story, of how crabs with badly located glands will fail to survive. If he is lucky with the modelling he will discover a mathematical model with analytical solutions. If he is lucky with the storytelling he will discover a true history of some maladapted variety of crabs, showing that it is dying out. Metaphors and stories, models and histories, are the two ways of answering 'why'.
>
> (1990b: 61)

She goes on to suggest that metaphorical and narrative explanations answer each other. In other words, the unsatisfactory elements of the answer provided by the first mode prompts an explanation in the second mode. 'A story answers a model. Likewise a model answers a story. The modes can, of course, mix . . . ' (1990b: 61) but it is not clear how, rather they seem to follow each other in sequence, each answering the holes in the other's account. She also suggests that metaphors and stories each play a critical (criticizing?) role versus the other (1990a: vii). It is not entirely clear to me how this works. It might mean they should provide balance for each other, or perhaps that they should work against each other, or in separate spheres?

Later in her accounts, metaphors and stories come to feature as two of the four necessary elements of 'good' economics, along with facts and logic (see particularly 1994: 62), though the separateness of metaphors and stories remain the norm for economics. There are two ways in which she sees them occasionally combining. The two may come together in the form of allegory and though the allegory is a form, which meets with approval, it is an unusual form to find in economics: 'Any rich account of a real economy is going to be allegorical, requiring its stories and metaphors to coexist. The point is that they coexist uneasily' (1990a: 94). Do they co-exist uneasily because they answer 'why' questions in different format? Yes, for in an allegory 'in which both must function, they can contradict each other' (1990a: 94). Allegories, in which they combine, are a good thing to find in economics, but it is not clear why they are better than the two separate elements, nor indeed how they can ever combine in an explanatory framework to answer the question 'Why?' given that they provide different types of explanation.

Stories also come together with models in counterfactuals, and here the co-existence is more clearly recognized as being necessarily part of the mode of argument. But, McCloskey argues the inherent contradictions between the two elements persist. The vaguer the model the better the story can fit into the historical world, while the more exact the model, the more absurd the history becomes. Counterfactuals are about impossible historical worlds, but they are not purely fantasy constructions since they begin with the factual world and face limitations in the 'counter' claims made. They might, in the end, expect us unduly to suspend our disbelief, but always they have some point at which they remained glued to the world.

There is also one particular form of a model – the differential equation – for which McCloskey recognizes a 'similarity between the most technical scientific reasoning and the most humanistic literary reasoning' (1991: 25). But in this account, the form is characterized as a 'mixed' one, standing between 'the pure . . . metaphor and pure . . . story' (p. 22). This is to argue for a special case in which the story is 'thematized' and the metaphor 'dynamized' whereas, as I argue below, it is in the *use* of models (ie metaphors) that one typically tells stories, and that story telling is generic to the use of models, not dependent on whether the model structure is static or dynamic.

Some might think that since the rhetoric approach is largely concerned with how economists argue and persuade, it doesn't necessarily connect with the facts of the economy. But we would be mistaken in thinking these are absent in McCloskey's account. Metaphors and stories play different roles, but both connect, as we have seen, with economists' explanations of the world. Metaphors are static and forward-looking, stories are historical and time-based. The stories in McCloskey's account are of economies at specific times and places; mostly, she takes stories as histories which means that they are typically about actual economies, not about imaginary economies, in which benevolent dictators might rule over perfectly rational, far-sighted individuals. The examples of metaphors (models) that McCloskey gives us often involve hypothetical economies with clearly imagined, but not necessarily idealized, properties. Sometimes they are about worlds which we might easily live in, a world in which taxes are changed, pension plans are made etc. In other words, the examples of models in McCloskey's account are often like the examples of applied economic models discussed by Gibbard and Varian, relying on *ad hoc or casual observations*.

It is probably evident from this discussion that, while I am sympathetic to both the Gibbard and Varian and the McCloskey accounts in certain respects, neither of them provides an account which explains why stories are important for models or tells us exactly how they relate. I claim that an economic model can not be accurately characterized as either a metaphor or a structure, for both labels fail to fully describe models and prevent us from fully understanding how models function. Rather, our ability to relate our models to the world and use models to understand the world depends upon narrative

devices as well as upon structures or metaphors. Model usage requires either structures (or metaphors) and stories – they are complementary and they relate in a particular way. These claims and my characterization of model usage require me to explore two aspects of modelling. First, in what particular way are stories an integral part of modelling? That is, How do stories relate to the mathematics of a model? Second, How do stories figure in relating our models to the world?

3 WHY MODELS NEED STORIES

Defining models either as metaphors or as structures is equally incomplete for the same reason. Economists' use of models involves not just a metaphor or a structure but nor are models just stories. In practical terms, models and stories go hand in hand. I agree with Gibbard and Varian when they say that a model is 'a story with a specified structure', but the story is not wholly given by the structure. The structure constrains and shapes the stories that can be told with a model, but the structure itself, like the metaphor in McCloskey's account, cannot do the work expected of a model on its own. Using a model necessarily involves both.

To portray models as passive objects in economics misses the point of modelling. As Margie Morrison and I have argued elsewhere, scientific models help us to learn things mainly by being used (see Morrison and Morgan 1999). A similar point is made in the DDI account (denote, demonstrate, interpret) of how models function by R.I.G. Hughes (1997). His account is very general, and is compatible with my discussions here. But even in the DDI account, there is something missing. As Hughes argues: 'To be predictive, a science must provide representations that have a dynamic of this kind [provided by mathematics] built into them. That is one reason why mathematical models are the norm in physics. Their internal dynamic is supplied – at least in part – by the deductive resources of the mathematics they employ' (p. 332). Hughes' account is general: the deductive resources of the model need not be mathematical – the model could be a material one. Nor need a mathematical model have any particular form, it could be geometric (Hughes' example) and it might, but need not, involve a dynamic form (McCloskey's special case). Models of all sorts involve resources that allow for manipulation: this is what Hughes calls the 'internal dynamic' of the model.

But what supplies the other part of the dynamic, and how does the demonstration begin? My answer to the Hughes conundrum is that models have to be 'questioned' to make use of their 'internal dynamic', and answering the question using the 'deductive resources' of the model typically involves storytelling.[2] How does the story start? The story is suggested by a question, or by an ad hoc observation, which needs accounting for, or by a supposed change to some term in the model, or by the modification of an assumption.

This 'question' begins the work done by the model: some term or element in the model is set or altered to represent this arrangement, possibly there is a sequence of changes, and finally there is a new outcome.

This process of putting the model to work to answer a question amounts to the 'demonstration' in Hughes' account and to the 'successful application' of the model in Gibbard and Varian's account. Structures or metaphors need questions and stories because it is only in this combination that models can be active participants in the production of knowledge. Story-telling occurs because it is only by using structures to answer questions that models help us to find out things we did not know or understand before, such as, 'What happens if . . . ?' or 'How does it happen that . . . ?'. Gibbard and Varian are right then, that we use the model to answer questions, but the 'model' itself does not pose the question as Gibbard and Varian suggest. We choose and pose the questions, and use the mathematics or other resources of the metaphor or structure to help us answer them. Our questions are the 'external dynamic' which enables us to make use of the 'internal dynamic' of the structure noted by Hughes.

The simplest example of a model in economics and one which can be interpreted in both the McCloskey mode and Gibbard and Varian mode is the supply and demand curve model. This commonest of economic models is a representation, in two dimensional space, on which economists draw one downward sloping curve which they label 'Demand' and one upward sloping curve which they label 'Supply'. The model crosses the micro-macro divide, for sometimes this is presented as a market-level analysis and the model represents the outcome of the behaviour of firms and consumers. Othertimes we find it as a macro-level model, depicting the total or aggregate supply and demand curves for the economy. This model can easily be interpreted as a 'structure' in the Gibbard and Varian sense (though there might be many arguments about exactly how it is derived from a set of postulates or theoretical assumptions) while the model has been interpreted as a metaphor by Klamer and Leonard (1993).

So what happens next in this classroom example? Typically, the intersection point is noted as the point at which demand for the good is equal to its supply. Maybe this is also an equilibrium point. 'Solving' the model for its market clearing point, the point of intersection is the first step, but all such a first step does is check out that certain properties (thought desirable by economists) hold in the model. The model is then ready for its real use which is to provide the answers to a number of typical questions: What happens if, at a given price, the amount supplied is greater than that demanded? What happens if there is a sudden increase in demand? What happens if consumers' incomes increase? What happens if there is technical change in the production of the good? Each question may also be motivated by reference to some ad hoc observation about the economic world, or some hypothetical situation which can easily be imagined to be real.

For example, let us take the question 'What happens if consumers' incomes increase?' Recall that consumers' incomes were not even on the original diagram. But they were one of the hidden *ceteris paribus* assumptions, which can now be taken out of the pound (see Hausman 1990). If incomes increase, it is conventional to show this by shifting the demand curve to the right on the diagram: increased demand at all prices, and this results in an initial rise in price and quantity to the new intersection point. The question creates a change in something inside the model, which creates a new outcome. But the sequence of changes may not end here, indeed in both the Smithian verbal description and the Marshallian account, there may be a reaction by the supplying industry to the higher market price, so that in the longer run, supply increases and the supply curve is also depicted as shifting to the right. What is the new outcome? Well that depends on the relevant shifts and the exact slope of the curves. But the story told will also depend on whether the economist thinks they are in a Walrasian or Marshallian world, that is, by convention, whether it is quantities or prices or which adjust to changes. To provide an answer to such questions in this genre of models typically requires some change or movement up or down the curves (from the starting intersection point) and/or shifting of the curves in the space.

Economists might argue that this well-used method of comparative statics is merely the comparison of static solution points, and does not involve any kind of a narrative.[3] But their protestations tend not to accord with their practice. Economists typically do fill in the gaps in such a way when using this model. They do so because using the model involves answering questions, and the structure cannot demonstrate these answers by sheer deductive logic or unadulterated mathematics without the prompt given by the question that sets off the changes. The question gives us the starting point; and there may be many possible ones using the constraints of the structure. Each time one of these questions is asked, the economist gives an account which begins with the situation proposed in the question and is taken to be a question about the world defined by the economic model. The elements in the model have to be mentally or physically shifted around on the diagram, or the algebra has to be manipulated and solved through, to suggest an answer or demonstrate outcomes. This is not just because economic models are 'paper tools' (see Klein 1999), such manipulation may equally be the case with material models (think, for example, of some of the early planetary motion models). Nor is it because our example uses the method of comparative statics. Even with models where the system solves itself (so to speak), as in certain kinds of dynamic mathematical models or the hydraulic Phillips machine (see Morgan and Boumans 1998), each time the scientist asks a question, the model has to be properly calibrated and set, going to answer the relevant question. Models may require more or less manipulation to provide demonstrations, but they don't manipulate (or solve) themselves, nor will they do so in the absence of an external dynamic.

What are the characteristics of the way these questions-and-answers work that allows me to characterize them as narratives. First, typically these sequences of changes, prompted to answer the question, have the structure of a narrative. The question proposes an event, which changes something in the model, which suggests that something else happens (and maybe another round of changes result) and then we arrive at a final outcome (different from the starting point) or a new 'solution' point. Secondly, the elements in the interpretation are related, with implied, rather than overt, causal connections. Thirdly, whenever we ask and answer these questions, we tend to bring in the interpretative level and discuss the changes in the elements in terms of the things in the world we have represented in our model.

These first two characteristics relate directly to certain peculiarities of narrative modes of reasoning which seem particular apt for our discussion of story-telling in connection with economic models, apt in as much as they fit the prejudices and the tensions we may experience, as economists, in working with models. Narratives are ambiguous about causality and necessity in a way which is entirely natural to most economists, who prefer to avoid making direct causal claims: '. . . narrative does not demonstrate the necessity of events but makes them intelligible by unfolding the story which connects their significance.' (Mink 1970: 545 summarizing Gallie on historical narrative). Narratives involve chronological sequences, but at the same time are often ambiguous about whether elements in the story are connected by logical or time relations, and again this suits economists' preference: 'the mainspring of narrative is precisely the confusion of consecutive and consequence, what comes *after* being read in narrative as what is *caused by*' Barthes (1982: 265–6). The tension for economists is that economic models of the type discussed here, models used in theorizing, should seem to be governed solely by deductive or theoretical modes of arguing, yet when we make use of them we make use of another logic – the logic of narrative.[4] When we look at the practices of economists, particularly in seminars rather than in written papers, we see how this tension, which stems from the fact that modelling involves both deductive and narrative modes of argument, may sometimes be resolved. I will return to this point, and the place of interpretation, in the final section of this paper.

In this section, I have argued that using a model means using it to answer questions and this involves telling stories. Thus, the identity of the model is not only given by the structure (or the metaphor), but also the questions we can ask and the stories we can tell with it. These in turn are constrained and shaped by the structure (or metaphor): we can only ask questions and tell stories about terms and relations that are represented in the structure, and only within the range allowed by the mathematics or materials of the structure. Nevertheless, the structure does not determine the questions, and it is only by asking questions and telling stories that we explore and demonstrate the full range of features and outcomes compatible with the

structure (i.e. stories which are consistent with, and use the resources of, the 'dynamic' of the model, to use Hughes' term). So, Gibbard and Varian are right when they say that modelling involves questions, structures and stories. I have shown how these three elements fit together: it is the question we choose or the prompting real or imagined event in the economy (the external dynamic) which sets off our story with the structure, and the story is deductive because it uses the logic of the mathematics or materials (the internal dynamic) of the model to answer the question. In terms of the McCloskey account, we have neither a separation, nor uneasy co-existence of model and story – each answering 'Why?' questions in different ways, but instead a compatible interdependence of the two, metaphors and stories, both answering the question 'What happens if . . . ?'

This section relied upon a classroom or textbook example, and its explanation was somewhat laboured. Dan Hausman (1990) considers a series of good examples of modelling using this diagram. His discussions support my claims, in the sense that he makes his points about how economists explain by describing a set of their narrative sequences as they use the model – a point to which I return later. Let me now turn to professional-level economics for a more sophisticated case.

4 MODELLING KEYNES' GENERAL THEORY

In a period when the majority of the economics profession did not indulge in economic modelling, the immediate reactions to the publication of Keynes' *General Theory* are somewhat startling. Though modelling was not a standard method, and not all such attempts were self-described as 'models', Darity and Young (1995) have rightly referred to the various algebraic and geometric constructions built to try to understand and make sense of the *General Theory* as 'models purporting to represent Keynes's message'. Their survey of these models (translated into common format, and with modern modelling terminology) discusses eight papers, reviews or responses which appeared in print in the period 1936–38. The purpose of these model-building attempts was to try and capture the essence of the Keynesian theory and, in some cases, to provide a representation of it which would allow comparison with other systems, particularly the classical theory. The most famous of these attempts was the one by Hicks, which gave us the IS/LM diagram. I prefer to concentrate on the one by Meade, as it exhibits the more typical characteristics of model building (both good and bad) and model usage in economics. His work can be more easily characterized in terms of the Gibbard and Varian account of a model as a structure than in the McCloskey account of a model as a metaphor.[5]

The stated object of Meade's paper (1937), evident in his title, was 'to construct a simple model of the economic system discussed in Mr. Keynes' *General Theory* . . . in order to illustrate:

(i) the conditions necessary for equilibrium;
(ii) the conditions necessary for stability of equilibrium; and
(iii) the effect on employment of changes in certain variables.

(Meade 1937: 98)

The order of Meade's paper is as follows. First, we find seven assumptions about specific elements in the economy (p. 98) and these are followed (p. 99) by a list of the eight conditions under which an economy meeting the seven initial assumptions will be in equilibrium. These verbal conditions 'construct' eight 'relationships', which are mirrored in the mathematical model given in his Appendix 1.[6] So far there are no stories: there is a structure, and it has been built to satisfy certain requirements: that the volume of employment can be determined in the short-run equilibrium.[7] But where did that requirement come from? It did not come from the structure of the model, but rather from Meade's interpretation of the main contribution of Keynes' book and an understanding of the main policy question of the day.

Meade's paper continues in stereotypical modelling form (for economics) by considering next the stability of the equilibrium:

> The system is in short-period equilibrium when these eight relationships are satisfied. But is this equilibrium stable? Suppose that the money wage-rate and the proportion of income saved remain constant, but that there is an accidental increase in total expenditure on commodities . . .
>
> (Meade 1937: 100)

We can note two things about the argument. First of all its form: suppose something changes, other things being held constant, what will be the effects? The treatment is verbal: Meade's argument involves a narrative sequence of connected events traced through the various relationships in the model. It ends with the particular equilibrium condition that comes out of the tracing process (also demonstrated formally in the appendix with the mathematical model). He then repeats the same type of narrative argument with various different banking policies. Each exploration depends on its own story using the model. What he is doing is working out the equilibrium conditions for stability, given various different events taken from the world that might occur with such a system.[8]

The main point of the modelling is yet to come. The really good stories about events which might happen in the world of the 1930s occur in part III of his article. We have already noticed that the demand for labour is a critical criteria for Meade. As before, his working of the model is verbal, tracing through effects of changing one thing in the model while holding others constant to see what effect such changes have on all the intervening elements, as well as on the 'short-period demand for labour'. He works through four cases to see the effect on employment of a reduction in interest rates, an increase in money supply, a reduction in money wage rates and a reduction in

the proportion of income saved. Once again we have the same method, basically the method of comparative statics but involving a complex eight-equation system made up of equations involving differential terms. Once again, causality is implied in the order in which the tracing process is followed, and the interpretation also allows consideration of whether the links that occur are plausible ones. In all cases his tracing process provides the narrative to accompany the resources of the mathematical model (under assumptions of stable equilibrium worked out in the previous section). The narrative begins with the starting point and the question, and depends on the order that the model structure is solved to reach outcomes: of course the narrative doesn't alter these mathematical resources, it uses them.

We can see that in using his model, Meade required both a structure (the eight equations) and to tell stories. The stories were shaped by the mathematics of the structure and constrained by it, but not fully determined by it. The way the question was asked, the objects of interest, what else was held constant and what allowed to vary, and the order of solution: all these affected the way any particular story was told. The decision what to change and the description of what happened was told in terms of an economic story in which the effect on the demand for labour (the point of the 1930s problems) was assessed in its own terms but also for various other impacts. The stories enabled him to tell not just the outcomes, but also something of the processes by which the results were arrived at, and the side effects involved. As Barthes wrote (in a different context): 'meaning is not 'at the end' of the narrative, it runs across it' (1982: 259). Typically, the questions Meade answered were not 'Why?' questions, but, given the model, 'What happens if?', and 'How does it happen?' questions, questions which require attention not only to final outcomes but to the intervening path to them.

But why were so many questions and stories necessary to Meade? Understanding the possibilities of any structure, how it can be used and applied, means gaining knowledge of its flexibility (and limitations) to tell a range of stories. Since this relative flexibility is indeed an important characteristic of models, it provides support for my stronger claim, namely that we cannot fully identify a model just by its mathematical structure, even with an interpretation of its terms. We need also to know what kind of stories it can be used to tell. For further discussion of this point, I turn to two other model-building examples of the time, one of Samuelson's earliest papers (1939) and, briefly, the paper by Hicks that introduced the famous IS/LM model.

Samuelson (1939) used model simulations on Hansen's model of a Keynesian type system to explore the nature of the multiplier and accelerator in conjunction in answering the question: 'What happens if government expenditure increases?'. He first used arithmetical simulations taking both into account using a verbal discussion (without even giving the reader the mathematical structure). Starting with an investment of government spending, he traced out sequences of quantitative outcomes over time. These

outcomes varied according to the values chosen for the parameters in the two relations. That is the structure did not alter, but the simulations showed that the stories, which attended them, altered as the parameter values altered. Some of these simulations showed little change in national income over the time period, some produced explosive cycles in national output and some produced exponential increases in outputs as a result of a very small increase in government expenditure. As Samuelson noted:

> By this time, the investigator is inclined to feel somewhat disorganized. A variety of qualitatively different results emerge in a seemingly capricious manner from minor changes in hypotheses. Worse than this, how can we be sure that for still different selected values of our coefficients new and stronger types of behaviour will not emerge? Is it not even possible that if Table 2 [the simulation results] were extended to cover more periods, new types of behaviour might result for these selected coefficients?
>
> (Samuelson 1939: 76)

> Fortunately, these questions can be given a definite negative answer. Arithmetical methods cannot do so since we cannot try all possible values of the coefficients nor compute the endless terms in each sequence. Nevertheless, comparatively simple algebraic analysis can be applied which will yield all possible qualitative types of behaviour and enable us to unify our results.
>
> (Samuelson 1939: 76)

But such analytical methods could not be done in abstract, they required an algebraic model which Samuelson duly provided. Using this, he solved for the different roots of the equation system (a two-period difference equation, non-linear in parameters, but not variables) to show the effects of different combinations of the parameters on the behaviour of national income in terms of periodicity, damping factors and effectiveness of pump-priming funds.

Samuelson claimed that the generality (meaning the full range) of these new stories compared to previous analyses was useful:

> Contrary to the impression commonly held, mathematical methods properly employed, far from making economic theory more abstract, actually serve as a powerful liberating device enabling the entertainment and analysis of ever more realistic and complicated hypotheses.
>
> (Samuelson 1939: 78)

By the use of analytical solution methods, he was able to take account of joint variation in both multiplier and accelerator parameters and to map how these varied together over the full range. He was also able to show how some rather bizarre results came from what seemed to be simple and plausible assumptions about policy options.

Here we have examples of story telling, not in conjunction with comparative static methods of analysis, but in conjunction with simulation methods and with analytical solution methods. Yet, as before, both methods of model usage involved some kind of story-telling. The simulation, like the comparative statics, required some initial external input (the question about government expenditure) to get the system working, and thereafter each story developed with the simulation over time. The algebraic solution allowed him to classify these various stories according to a certain criterion, namely the qualitative behaviour of national income in relation to the original stimulus. Both methods, whether they dealt with hypothetical numbers or general characteristics, told stories about the behaviour of the national income according to the variation in behavioural parameters of the model. The specific details of these different stories were not known in advance, but came from the way the model was calibrated and simulated to answer questions about the model economy. There was only one model structure being used, constructed to combine the multiplier and accelerator relations, one question being asked – and then it was used to tell various different stories according to varying values in the relations.

The Hicks (1937) example is rather well-known amongst economists for introducing the celebrated IS/LM model of the Keynesian system, which Hicks called at the time a 'little apparatus'. Though I would hesitate to depict Hicks' reasoning in this article as providing a paradigmatic example of modelling, it is nevertheless the case that he constructed the model in order to compare the stories that could be told, within the same graphic structure, about both the Keynesian and the classical system of theory. By the 1970s, it became even more widely used, but knowing that an IS/LM structure is being used did not determine what kind of a macroeconomic story was being told.

A much more general case of this nature can be given by the example of the Edgeworth–Bowley box diagram. This box has been used for the last 100 years to tell stories about consumers in exchange situations, about firms and production decisions, about countries and trade policy, about welfare questions and so forth (see the survey paper by Humphrey 1996). The structure remains the same, the interpretation of the elements differs, and the stories that are told alter. Knowing that a piece of economics uses an Edgeworth–Bowley box diagram does not even enable you to predict the domain of the economic story it will be used to tell.

5 HOW MODELS CONNECT TO THE WORLD

How do models connect to the world? The kind of reflective, but non-analytical, accounts that economists occasionally give of their own practice recognize that story telling is somehow involved in this. For example, Krugman, in autobiographical mode, noted:

> The models I wrote down that winter and spring were incomplete, if one demanded of them that they specify exactly who produced what. And yet they told meaningful stories.
>
> (Krugman 1993: 26)

McCloskey made the following observation on the rhetorical practice of economists:

> Economists, especially theorists, are for ever spinning 'parables' or telling 'stories'. The word 'story' has in fact come to have a technical meaning in economics, though usually spoken in seminars rather than written in papers. It means an extended example of the reasoning underlying the mathematics, often a simplified version of the situation in the real world that the mathematics is meant to characterize . . . Here the story is the modifier, the mathematics the subject.
>
> (McCloskey 1983: 505)

This observation suggests two *simultaneous* roles for stories: they are an 'extended example of the reasoning underlying the mathematics', and *at the same time* a 'simplified version of the situation in the real world that the mathematics is meant to characterize'. I like McCloskey's rather loose description[9] because it points to the way stories relate the mathematics to the world and comes very close to what I have been arguing above about the relation between the story and the mathematics.

McCloskey and Hughes are both right that there are two links between models and the world. First, we characterize something about the world in our mathematics. Then we use that mathematical characterization to answer questions relevant to the world, and in doing so we tell stories which link back to the world. (McCloskey portrays these two links as happening together; Hughes sees the latter link as two separate steps of demonstration and interpretation.) The first link is about how models are built. This, in turn, involves deeper questions about the nature of representation or denotation used (see Morrison and Morgan 1999 and Hughes 1997). How models are built also brings in the major, but well-known, problem of the realism of assumptions. Certainly one way in which models might connect to the world is via their assumptions: if the assumptions on which they are constructed are realistic to the world, we might find them useful tools to learn about the world. Economists recognize that most economic models are not built on realistic assumptions. And, although it makes sense to ask if the assumptions of a model are realistic to the world, it doesn't seem sensible to dwell on this as the *sole* aspect of realistic correspondence. As Gibbard and Varian characterize the practice of economists, 'successful application' means using the model to produce outcomes that connect with the world (and is not dependent upon the realism of the assumptions). We might interpret this as the stereotyped instrumentalist position – worry about the predictions, forget about the

assumptions. But this would be a misinterpretation of Gibbard and Varian. Let me set aside the onerous problems of representation and the realism of assumptions, and concentrate on the second link, on how stories link our mathematics to the world.

Telling stories about the world is one way of explaining the world to ourselves. Gibbard and Varian suppose that economists apply models to the world in a rather casual way to explain facts about the world (but without being able to say exactly where stories fitted in to this application). McCloskey supposed, in her more formal analysis of the rhetoric of economics, that stories were one way of answering questions and providing explanations about the world (though remember in her account, this was different from explaining using metaphors (models). This conjunction of 'stories' and 'explanations' in discussions of model usage can also be found in Hausman's (1990) account. Because he wants to demonstrate how the demand and supply diagram is used to answer questions about events, which have happened in the world, by using such simple models, he puts the model to work. As soon as he does this, he cannot avoid story telling with the model. The ability of such model-usage to explain something about the world in such cases is recognized by Hausman as follows: 'But the simple supply and demand explanation surely captures *the heart of the story*' (Hausman 1990: 169). Mäki, in support of this statement, continues: ' "The heart of the story" may be taken as akin to a potentially true account of the isolated essence or the primary determinants and their causal mechanisms involved in the process. "The actual story," on the other hand, is a more comprehensive account which encompasses many secondary factors as well' (Mäki 1992: 344). It is important to note that in Hausman's statement, it is the simple supply and demand *explanation* which captures the heart of the story, not that the simple supply and demand *diagram*. The diagram on its own with interpretative labels explains nothing (or at least very little). As I have argued already, it must be questioned and put to work, it must be used to explore the (causal) mechanisms through a series of narratives in order to offer any explanations.[10]

Explanations are often characterized as answers to the question 'Why?'. Our cases of model usage suggest a wider version of the question, something like 'What happens if . . . ', and in which explanation and story telling go together in the answering process. In portraying stories as answers to questions and in linking this to explanation, we are pointing to the use of narrative explanation in economics: that is, we use narrative as a cognitive tool, a tool by which we explain something or come to understand something about the world. Apart from the rhetoric accounts of McCloskey, there seems to have been little reflection within the methodology of economics about this cognitive role of narrative and its explanatory power, in contrast to the discussions in philosophy of history or literature. In the philosophy of history, narratives are sometimes taken to provide a 'causal-genetic' account while

others portray them as entailing some loose version of the D–N covering law mode of explanation or as an alternative type of rational action explanation.[11] In the philosophy of literature, perhaps the most salient discussion of the cognitive role of stories in comparison with other modes of explanation is by Mink:

> narrative is a primary cognitive instrument – an instrument rivalled, in fact, only by theory and by metaphor as irreducible ways of making the flux of experience comprehensible. Narrative form as it is exhibited in both history and fiction is particularly important as a rival to theoretical explanation or understanding.
>
> (Mink 1978: 131)

Mink's determination to present these modes of explanation as mutually exclusive might seem to make my account of model usage problematic, both because I presented metaphors and stories as complementary and because I suggested models in science might use both deductive and narrative reasoning (as discussed in section 3). But just because we can recognize these differences between cognitive tools does not mean that they cannot be used in complementary ways.

One way to understand this complementarity is to remember that models relate to the world in two ways. In *building* the model, we try to represent the situation in the world in such a way that we incorporate our general theoretical claims or hypotheses about the world.[12] Cartwright's (1983) simulacrum account of models in physics suggests that getting a good model consists of choosing the right theoretical equation to fit the prepared description of the phenomenon, so that 'To explain a phenomenon is to find a model that fits it into the basic framework of a the theory . . . ' (Cartwright 1983: 152). In this relationship of model to theories and laws, finding or constructing such a model seems to give us access to a scientific explanation for the phenomenon. But models also relate to the world via their interpretations, where I have argued we tell stories in answering questions and in doing so we make points about concrete cases (specific or typical, but not necessarily real) which have, or which might have, occurred in the world. That is, when we *use* economic models, we do so to relate the general (theoretical or law like) claims back to the specifics of the world. But what kind of explanation does story-telling give of that specific case? And how is it different from scientific explanation? Further attention to the force of narrative explanation is needed to understand the cognitive role of model-based story telling.

In asking how we come to comprehend a set of objects, Mink contrasts the theoretical mode of natural science with the configurational mode of history. In the scientific mode, we understand how objects are related by all being instances of the same thing (with its covering law view of explanation, which he suggests is 'powerful but thin'). In the configurational mode of history (the one relevant for the concept of a story), we comprehend objects as 'elements in

a single and concrete complex of relations' not by constructing a theory about the elements or events, nor by relating them to a concept, but by showing 'how it [the object] belongs to a particular configuration of events like a part to a jigsaw puzzle' (Mink 1970: 551).

Although Mink paints these as alternatives in terms of comprehension and cognitive domain, once again they may well prove complementary in practice. A very nice example is given by van Fraassen (1988: 136–7) in which he asks why a certain tower must be so high that it casts a shadow at a particular spot. The length of the shadow is explained by the laws of physics: the scientific explanation; but in explaining the height of the tower he reverts to narrative explanation giving us a choice of stories about why the tower was built just so high. The latter explanations are case specific, and would hold nowhere else, while the former are general and are supposed to hold everywhere. The two explanations, scientific and narrative, are clearly complementary.

The same seems to hold in using economic models. To the extent that we make use of general theoretical claims we have embodied in the structure of the model, then we make use of theoretical (scientific) explanation, but when we use the model to discuss specific cases, we also rely on the complementary explanatory power of narrative. Thus, to go back to the simple example, the theoretical explanation says that when price is high, demand is low because it is a case of the law of demand. A powerful, but thin, explanation, because it uses the power of the law, but explains nothing of the detail. In telling stories with the model, we use it to explain the specifics of why coffee prices are high in 1976 (to take Hausman's example). In Meade's case, the structure of the model incorporates a version of the Keynesian theory, but when he uses the model to create narratives, he is describing specific cases (relevant for the 1930s experience) which might occur to understand how they would work out in the model economy. It is because of the dual nature of a model's relationship with the world that in using models we can call on the explanatory power of more than one mode of argument: the theoretical and narrative forms.

Models are mixed instruments. In model-based story telling, the relationships between the elements of the models are covered by the economic theories and incorporate the logic of whatever mathematics they are expressed in. But where to start the tale, which questions are interesting and relevant, and even the order of solving the model is somewhat open – the user has to make sensible choices in order to tell meaningful stories, stories which are plausible and interesting about the world. In economic model usage, the logic of story telling has to be combined with the logic of the theory and the mathematics, but it is the narrative which connects with the specifics of the world, and in which the configurational mode of explanation and cognition dominates.

It is probably because of this potential double nature of models that we have difficulty characterising their usage – namely that in the model-theory relationship, the model is an exemplar for a general claim (see Cartwright 1993), so in applying the model, the scientific explanation form is called upon,

but in the model-world relationship, the model is applied to the specific case and this is associated with the narrative form of explanation. The configurational (narrative) mode may be used in close conjunction with the theoretical mode, as for example, in the way the supply and demand curve model is used (see the examples in Hausman). This is in comparison with the Van Fraassen example, where the two modes are more clearly separate but complementary to each other. It is no wonder that a certain confusion results when the narrative and scientific modes seem to point in different directions, as with Samuelson's 'capricious' narratives resulting from apparently plausible scientific explanations.[13]

There is, however, more to be said about the cognitive nature of narrative explanation. Economists' unwillingness to recognize the role of narratives except in informal conversation and informal writings, may be because narrative is often misunderstood when viewed from the scientist's viewpoint; it seems only to answer 'and then what happened' questions and gives no understanding beyond that. But as philosophers of the humanities know, narratives offer more than chronicles, narratives always give something more than a listing of the order of events, they provide some kind of an account of the relationship of events. Narrative helps us to understand the world, in part, as Mink suggests, because narrative sits between theory and the world in terms of a generalizing device.

> On the one hand, there are all the occurrences of the world – at least all that we may directly experience or inferentially know about – in their concrete particularity. On the other is an ideally theoretical understanding of those occurrences that would treat each as nothing other than a replicable instance of a systematically interconnected set of generalizations. But between these extremes, narrative is the form in which we make comprehensible the many successful interrelationships that are comprised by a career.
>
> (Mink 1978: 132)

So the characteristic kind of understanding that comes from narrative is the phenomenon of grasping things together at this intervening level between complete and exhaustive detail and complete generalization. This is not just a question of grasping a typical temporal sequence, or of seeing how parts relate to each other or to the whole (though both these may be involved), but that in narrative we gain the possibility of grasping the whole rather than the parts: 'That grasping together a complex sequence of inference is possible is attested to by mathematicians, who commonly are able to see a demonstration as a whole rather than as merely a sequence of rule-governed transformations' (Mink 1970: 548). Bruna Ingrao, in a 1998 paper on the role of novels, claims something similar: that what novels teach us about is complexity: a complexity involving both the essential and the details, as well as the importance of time, chance and necessity in events.[14]

It seems that what is being said here is that stories allow you to grasp a certain degree of complexity in the whole, whereas the common argument about models in economics is that they are required because the detailed real world is too complex to understand and so we need to simplify it. These points are not necessarily inconsistent. In constructing models, one of the processes involved is that of simplification. The hypothetical world constructed in our models is simpler (we imagine) than the real world. This creates the difficulty that when we have built the model, satisfied ourselves about the model, we still do so only in terms of that simplified world in the model. But, though simplification is part of the construction, the suggestion we might take from the literature on narrative as a cognitive instrument is that in using the model to tell stories about the world, we are able to grasp not only the model as a whole, but we are also in some way trying to re-grasp the complexity of the real whole world and the typical elements in it. Samuelson's model of the world was very simple, but by telling stories with the model, he was able to picture the full range of outcomes compatible with the structure. Meade's model was more complex, but he too used the narratives to explore the full range of behaviour compatible with his structure. This ability to take hold again of the complex world relies upon our explorations with the model. These explorations, answering questions and telling stories, are the means by which we explore the limits and full behavioural characteristics of the model we have built, in which we learn all the possible processes and outcomes compatible with the structure, and indeed, which questions it can answer and which not. Thus, in considering how we learn things by using models in economics, I would reverse the claims made by Haydon White for history. White wrote that 'We understand the specific story being told about the facts when we identify the generic story-type of which the particular story is an instantiation' (1975: 58). This suggests that we only understand the model when we understand the more general theory it embodies. I think, on the contrary, we only fully understand our model when we have identified all the specific stories that it can encompass or tell about the world.

6 CONCLUSION

In the context of a complex world, models are an accepted way of representing the economic world in a simpler way so that we can think about its features. By asking questions and then manipulating models, we are able to tell ourselves stories about the hypothetical world portrayed in the model. But these stories also help us to explain the real world, for it is these stories, an integral part of modelling, rather than the typically unrealistic assumptions from economic theory, which make the stronger connections to the real world. We expect these stories to be related to our questions about the world, our typical experience of the world, and to the kinds of events we find in our world. We don't expect them to be exactly true to any particular events of the

world. If they were, they wouldn't be stories. But nevertheless, they connect by the questions that prompt the story, by the fact that the events they portray correlate to events in the world, and by the congruence of outcomes. It is by telling stories about the economy that we most effectively connect our models to the facts of the world.

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NOTES

1 This paper was written at Nuffield College, Oxford during my period in residence as the Norman Chester Senior Research Fellow, Fall 1997, for presentation at a conference in Erasmus University, Rotterdam, November 1997. It circulated in revised form as a University of Amsterdam working paper in 1999 and is to be reprinted in *Fact or Fiction: Perspectives on Realism and Economics*, edited by U. Mäki (Cambridge University Press, 2002). I thank the Warden, Fellows and Students at Nuffield for their hospitality and many discussions of the role of models and my NAKE students (classes of 1997 and 1999) for pertinent questions when I taught the topic. I have benefited from discussion by participants at the Rotterdam conference, as well as at subsequent seminars at Nuffield College, Oxford, at Groningen, at the WRR (Dutch Scientific Council for Government Policy in The Hague) and at the HES meeting in Montreal. I thank particularly Ben Gales, Roger Backhouse, Margie Morrison, Harro Maas and Nancy Cartwright for their comments.

2 I should distinguish here the role and importance of stories *in model construction* compared with *in model usage*. A very nicely observed case, of the role of the story in the *construction* of the 'MIT Bag Model' of quark confinement, is reported by Stephan Hartmann (1999).

3 This issue was raised by the Rotterdam conference audience and is answered in this paragraph, and discussed further in section 5 of the paper in terms of the forms of explanation involved in model usage.

4 Indeed, if one views narrative from the viewpoint of science, 'narrative would be a systematic application of the logical fallacy' (Barthes 1982: 266). This may be why economists find it difficult to give a convincing account of the role of models, for economists want models to work in the mode of scientific reasoning, even while they work in part with a narrative reasoning. To some extent the problem is a shared one: some scholars of narrative in the literary genre insist on the chronology of narrative, others on the logical relations involved.

5 I have given an extensive account of the use of analogical, or metaphorical models elsewhere (see Morgan 1999) in which my claims about the relation of narrative to metaphorical elements are easily demonstrated. Irving Fisher's mechanical balance model for the equation of exchange had to be manipulated to demonstrate old stories about the quantity theory and to tell new stories about the processes of adjustment. His hydraulic model of the monetary system was used to tell stories about how Gresham's Law worked, as well as to demonstrate the possibility of bimetallism.

6 This is an article from 1937, so it is perhaps not surprising that both in stating the assumptions, and in describing the conditions for the simple economy to be in equilibrium, the verbal form is used and he only gives the mathematical model in the appendix.

7 A feature of modelling, non-specific to this type of macro, or comparative static, modelling, is that solutions must meet certain conditions, so that we often find a rather uninteresting first usage of the model (checking that such conditions hold) before the model is used to answer more interesting questions.

8 Somewhat disarmingly (and perhaps not a feature of later modelling) he concludes this section 'It is of course possible that in the real world the system is unstable' (p. 102) but he continues with the assumption of stability (ie that the conditions do hold) because of the difficulty of analysing the impact on volume of employment in his next section without such an assumption.

9 Similarly with Mäki (1992) who suggests that 'a *model* provides a more or less rigorous and skeletal representation of the relations within an isolated field, while a *story* attached to the model is a looser and thicker commentary . . . ', (p. 330). I find both statements somewhat 'loose' in the same kind of way as the account by Gibbard and Varian. McCloskey's account, though highly suggestive, implies that the mathematics and the situation it is taken to represent both have a narrative form, without being able to tell us why this is so. Mäki does not explain how a 'thicker' story relates to and is attached to a skeletal representation.

10 A parallel example is the way that chemical formulae were first used with short narrative devices to explain the details of how chemical reactions actually took place: see Klein (1999).

11 The classic comparison is found in Hempel (1962). Special attention to narrative explanation is found in Ankersmit, (particularly his summary paper of 1986 and his 1988), Dray (1989), Roth (1988 and 1989) and the essays in Ankersmit and Kellner (1995).

12 Hausman (1992) and Giere (1988) suggest that models are sets of statements and a theory is a hypothesis that the model applies to the world. This suggests that theoretical claims are only relevant in model usage. This does not seem to be quite how economists use the term 'models', in the sense that certain theoretical claims or resources are built into the model though that is not all that is in the model – see Boumans (1999), and to this extent economists' practice may be more compatible with Cartwright's 1983 definition for physics than with those of Hausman for economics.

13 The second confusion which sometimes follows from the conflation of the science and narrative mode is the tendency to treat the narrative form like a hypothesis in science (Mink 1978:145), but a narrative cannot be confirmed or disconfirmed, it can be plausible or implausible, satisfying or not, meaningful or not, insightful or not. This probably relates to the misunderstandings which arise in discussions about 'testing' mathematical economic models, and re-inforces the stance taken by Hausman (1992) that one can only test theories not models.

14 She indicates the possibility of rethinking our economic models in terms of the form, character and actions characteristic of events in novels.

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