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What if? Models, fact and fiction in economics

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Abstract: Economists build models to understand the economy, but to outsiders these often seem to be imagined or fictional worlds, accounts that seem closer to those of science fiction than to matters of science. Such a judgement underrates the importance of fictional elements and the imagination in the way economists make and use their models. Paying attention to the 'what-if' questions that economists ask when they use their models reveals how they create the keys that enable them to translate between their imaginary model worlds and the real economic world we all live in.

Keywords: economists, economic models, fictional worlds, 'what-if' questions.

I FICTION IN SCIENCE

Economists use models as a way to understand the world. But their ways of working with models often appear quite strange, and even worthy of suspicion to commentators, because those worlds that economists picture in their models seem to be imaginary worlds, not the one we live in. This fictional quality of economists' models makes them interesting for an historian and philosopher of economics. Are economists' models fictions (in which case how do they become so)? Or is it that economic models function as fictions? If they are fictions, are they a form of science fiction, or are economists creating fictions in the service of science? And most important, how do economists reason with these imagined worlds—with their seemingly fictional qualities—to understand the real world?

The idea that we might—in general terms—think of scientific models as fictions has recently become fashionable in the philosophy of science, particularly amongst younger scholars in that field.[1] One direction in this literature takes heart from the

[1] Three recent collections show the breadth and depth of this initiative—those edited by Suárez (2009), by Frigg & Hunter (2010) and by Wood (2010); individual works of particular relevance to this essay are Frigg (2010), Toon (2010, 2012), and Suarez (2010).

philosophical discussions of fiction itself, their idea is that we can think of the models of science as props in a game of make believe.[2] So, these philosophers argue, rather than make a model to represent or describe the world we live in, scientists prescribe, or imagine, a world which is pictured in the form of a model—in equations or diagrams or even made into a machine. The idea of models as fictions seems particularly apt for the kinds of model that are popular in economic theorising.

If models are props in a game of make believe, we can use the term 'playing games with the model' as a short-hand description for how economists work with their models—typically by asking 'what-if' questions of their models and telling answering stories about their imagined worlds. By playing these games, economists figure out how each of their imagined worlds works, explore its limitations, and see how far they can stretch its possibilities. As in all games of make-believe, there are rules for playing the game. When economists play games with their models for their own scientific purposes, those rules come from two different sources (see Morgan, 2012: chapter 1). One source of rules comes with the medium or language of the model (whether it is made up of equations or diagrams or is an hydraulic model). The other source of rules is the economics subject knowledge which acts both as a constraint on, and a prompt for questions about, the kinds of things that are imagined to happen within that world in the model. Constraints might be factual ones: for instance, economists will rule out a solution within a mathematic model that requires them to imagine that people live negative lives, that is, imagining people might live a minus number of years not a positive number of years—this is a fiction too far.[3] Prompts might be theoretical hunches about how economic society might work, for example, about the wealth transfers between generations. The rules set by the kind of medium that the model is made in (geometry, algebra, arithmetic, etc.) determine the kinds of manipulations that are allowable but it is the economists' subject knowledge that determines which manipulations are relevant and of interest.

Many of these games seem not to have immediate or obvious relations to any concrete events or characteristics of the economic world, they are often theory-making or theory-exploring games. Playing games with the model according to those rules makes truths, but only about the imagined world in the model not about the real world. So the idea of models as fictions, and game playing in using those fictions, raise puzzling questions about how such games with imagined worlds relate to factual kinds

[2] The usual starting point is Kendall Walton (1990), but equally relevant is the distinction he draws in 1993 between fictions that are a means to understand the props (e.g. model airplanes) and those that focus attention on the make-believe that props stimulate (e.g. a hobby horse).

[3] See Hausman's investigation (1994) of the overlapping generations model in economics which reveals some of these fictional aspects.

of economic knowledge, and how such models could be used to gain insight into the real economic world.

The notion of scientific models as fictions held by philosophers of science, and economists' own usage of their imagined world models, prompts a number of questions. The first question to address is what kind of imaginings are going on here? If we treat economic models as fictions, we might wonder: do economists prescribe a world of science fiction, or do they rather offer imaginative accounts of the real world that we live in? The difference may be difficult to draw even if we have some sense that science fiction is different from a scientist's imaginative account. Leila, the 3-year old daughter of a friend, for several weeks saw dragons everywhere: every corner that she went round might contain a dragon; and if a cobweb grabbed her hair 'it's a dragon mummy'. Even though for a 3-year old, dragons might be pretty real, perhaps there is a difference compared to the classic example used in philosophy of fictions which concerns the taking of tree stumps (as props) to be bears, so that children playing a game about bears in the forest, whether they are hunting them or running away from them, will regard tree stumps as bears and behave accordingly.

My instinct is to put the dragons example into the category of science fiction, and using tree stumps in the forest as props for bears as being rather an imaginative account of the world (particularly relevant for children in America, who grow up with bears in their forests). If so, it may seem as if facts only come into the second kind of games, those of bears (factual stumps in the forest) versus those of dragons. But this dependence on factual props may not provide a useful dividing line when one looks at the practices of science. So, the second question to be raised is how do facts and fictions interrelate in the construction and use of models: are they used together, do the facts limit the fictions, or are they embedded inside the fictions or prescribed imaginings?

Third, following on from the second point, I question whether labelling models as 'fictions' is not too narrow and restrictive a way to think about economic models. *Artefacts* maybe a more helpful term for what they are and how they are used. Both terms refer to something human made, but artefact points also towards the tool aspect of modelling. This does not mean that imagination (and its props) is not part of the modelling recipe, indeed, imagination remains a critical ingredient, as one of the roots of the term artefact hints. I will return to this particular idea later, after a discussion of the place of imagination in economic modelling.

II SCIENCE FICTION VS IMAGINATIVE FICTIONS IN ECONOMICS

A good place to begin questions about imagination and economic science is in the restaurant at the end of the universe in Douglas Adams's *Hitchhiker's Guide to the*

Galaxy. Ford, one of the main characters, sees an old friend Hotblack Desiato sitting at a table, minded by some heavies. Ford tries to speak to his friend who does not react, and after several attempts to get a response, he is pushed away by the minders who tell him that Hotblack Desiato is 'spending a year dead for tax purposes' (1986/92: 225).[4] This is a wonderful idea, just the kind of idea that an imaginative economist might have written into their model, one that escapes the factual constraints of life, but only by one, admittedly critical, step! Let me compare this with another example, from Francis Edgeworth, a famous Irish economist of the late 19th century, in a book called *Mathematical Psychics* which in some respects is not so unlike *The Hitchhiker's Guide to the Galaxy*. Edgeworth begins his investigation into the far mysterious areas of economic life with a discussion of the nature of happiness or 'atoms of pleasure' and their properties:

> *Atoms of pleasure* are not easy to distinguish and discern, more continuous than sand, more discrete than liquid . . . We cannot *count* the golden sands of life, we cannot *number* the 'innumerable smile' of the seas of love, but we seem to be capable of observing that there is here a *greater*, there a *less*, multitude of pleasure-units, mass of happiness; and that is enough. (1881: 8–9)

That is enough at least for Edgeworth to begin his mathematical investigations into how exchange relations are conducted:

> To gather up and fix our thoughts, let us imagine a simple case—Robinson Crusoe contracting with Friday. The *articles* of contract: wages to be given by the white, labour to be given by the black. Let Robinson Crusoe = X. Represent y, the labour given by Friday, by a horizontal line measured *northward* from an assumed point, and measure x, the remuneration given by Crusoe from the same point along an *eastward* line. Then any point between these lines represents a contract. (1881: 28)

This passage leads into the first drawing of a very famous model in economics that came to be called the Edgeworth Box, a prosaic looking object, but one that takes some explanation, and which was developed over the following 60 years as a wonderfully flexible theorising tool. The point here is not to understand the model, but to recognise the same imagination and structure of argument going on in both of these sites. For Edgeworth, imagining the exchange relations between Crusoe and Friday, and making an image of those imagined relations, is an act of prescribing the world in the model. By the same token, imagination is clearly driving the *Hitchhiker's Guide to the Galaxy* example. And the latter seems almost more real than the Crusoe–

[4] The first elements of the *Guide* were radio scripts broadcast in 1978; the first full set of adventures (if that is the right word) were put together by 1986 (according to the introductory notes by Adams to the 1986 volume).

Friday affair, after all, more people 'play dead' by leaving their country for a year for tax purposes, than are wrecked or abandoned on isolated islands.

It is not easy to tell what we should be concentrating on here, and what might be the relevant differences. One might think that anyone called Hotblack Desiato must be a fictional character. But in fact Adams borrowed the name from a real estate agent in London, and during a period of house hunting, years ago, I would regularly meet Mr Hotblack, but never Mr Desiato. There were various stories circulating about his absence; perhaps he had merely gone away for a year for tax purposes, perhaps he never existed. Robinson Crusoe is an even more interesting case if one tries to distinguish fiction and fact. *Robinson Crusoe* is understood as a novel written by Daniel Defoe (1719), but it was written as if it were a news account of something that had happened in his day—so we are already in a fiction/fact ambiguity. If it is a novel, Robinson Crusoe must be imagined, but the character and part of the story were probably based on a Scottish sailor called Alexander Selkirk who was abandoned on an island 700 km off Chile in 1704. He was picked up in 1709 by a passing ship, and his landing back in Britain became a big news item in London before Defoe wrote Robinson Crusoe. So, Adams's character Hotblack Desiato and Defoe's Robinson Crusoe each have an opaque status between facts and fictions.

What matters here for thinking about economics? Is it that the people and their actions are fictional, or that their situations are fictional, or that the interrelations between people or groups and other elements in the economy are fictional, or that the stories told about them are fictional? For most economists, model building usually involves some 'as if' assumptions about behaviour. Just as children may imagine tree stumps as bears, economists imagine people in their models as always rational (according to economists' ideas of rationality). Fictional 'as if' assumptions about the behaviour of 'agents' (individuals, firms, organisations etc.) are typical in the building of economic models and have been the subject of extensive discussion by philosophers of economics ever since Milton Friedman's seminal 1953 discussion of them. But in considering the *use* of models (rather than their *construction*), those 'as if' questions are less important, than the 'what-if' questions used when economists play games with their models, and it is the 'what-if' fictional *usages* of models that are the focus here. For such 'what-if' questions, situation descriptions are often important: for example, the prisoner's dilemma situation prompts lots of what-if accounts (see Morgan, 2012: chapter 9). Other stories may rely more heavily on the time relations of events, or interrelations between groups of economic actors, proposed in the model. Imaginative stories prompted by the what-if questions that economists like to ask about their model worlds are where we see economists playing their games of make-believe.

Once again, *Hitchhiker's Guide* has a neat example—in a what-if story about the planet Magrathea in one of the *Guide's* entries. A long time ago (where all stories start), when many men became rich, they also became dissatisfied with the planets they lived in . . .

> either the climate wasn't quite right in the later part of the afternoon, or the day was half an hour too long, or the sea was exactly the wrong shade of pink.
>
> And thus were created the conditions for a staggering new form of specialist industry: custom-made luxury planet building. The home of this industry was the planet Magrathea, where [new planets were designed and made] … to meet the exacting standards that the Galaxy's richest men naturally came to expect.
>
> But so successful was this venture that Magrathea itself soon became the richest planet of all time and the rest of the Galaxy was reduced to abject poverty. And so the system broke down, the Empire collapsed, and a long sullen silence settled over a billion hungry worlds, disturbed only by the pen scratchings of scholars as they laboured into the night over smug little treatises on the value of a planned political economy. (1986/92: 87)

The what-if question—what happens if people get very rich—prompts the answer worked out in a story (with a distinctly moral flavour): some people get very rich, a new luxury industry starts, that industry is so successful that the distribution of income becomes even more unequal, and so forth until the system breaks down.[5]

Exploring economists' what-if questions reveals why, and when, the fictional qualities of economic models matter to the ways that economists use them. There is no one way for economists to play these games. Three examples explored in the rest of the paper must suffice to give a sense of their variety. In the first example, an economist imagined an entirely fictional USA in order to work out something factual about that economy: perhaps the most famous use of a counterfactual experiment relying on models in economic history by Robert Fogel. The second case involves a very standard model in economics, namely the supply and demand model, in which fictional elements are used to define what facts are economically interesting and the possibilities of locating matching facts in the world by economic historical work, by econometrics and in experimental economics. The final example involves a splicing of facts into fiction in the creation and usage in the Newlyn–Phillips machine, a model which imagines the economy as an hydraulic system. In each case, we find combinations of fact and fictions, and what-if questions at work.

[5] *The Hitchhiker's Guide* invites historiographical speculations: textual evidence from his final comment suggests that perhaps Douglas Adams recognised these imaginative accounts given by economists, and made fun of them, and perhaps Thomas Pikkety (2013) had read this story.

III WHAT IF . . . THERE WERE NO RAILROADS?

In the economic history of the first half of the 20th century, railroads were considered to be absolutely indispensable to the US economy, not just to its growth but in determining its development: where people settled, what occupations they followed, how cities and regions grew up, and what kind of economy each region turned out to be. A salient example of this general thesis is offered by an historian Leland Jenks who (in 1944) wrote about the railroad as an invention with three domains of impact. The first domain was as the idea of a new transportation system, with economic effects even before it was built as towns, communities, and industries located in the proposed path of the railroad and agricultural areas were opened up ahead of time. Second, it was effective as a construction enterprise: building the railways created a demand for raw materials, new labourers, and industries, with knock-on effects on wages, migration, and so forth. And, as a result of those demands, there were both consumer multiplier effects on local communities, and, from the effects of financing the railroads, on banking and finance. Only third, did Jenks countenance the effect of the railroad as a producer of new transport services with new characteristics, for instance refrigerated trucks (which allowed agriculture to specialise in different goods for urban centres), and with added speed which affected the costs of many other industries and created new supply chains. In addition, the railway companies formed new large organisations, which, according to Alfred Chandler, were the locus for the development of managerial capitalism. In other words, the railroads' tentacles were felt everywhere in economic life.

Leland Jenks's article is narrative in form and economic analytical in argument—it swings along in fine style to cover a huge terrain. In effect he made use of his historical imagination to piece together an impressive narrative of the highly complex pattern of the geographical and economic interrelations of American development. But it is clear that each individual cause–effect link in Jenks's account needed to be separately evidenced and this became a very popular area of research for American economic historians in the period. By the mid 1960s, Jenks's style had begun to look increasingly old fashioned in both technological and evidential terms compared to the explosion of cliometric and statistical studies of railroads in that decade. Yet what was more important than this change of technical style was the change in starting point, which brings us to Robert Fogel, who radically questioned the standard view. Fogel (1964) set out to assess the railroad's dispensability (not its indispensability). He began with the counterfactual: What if there had been no railroad in the US in 1890? Asking the question this way around immediately moved his economics into the world of fiction, and for Fogel, this involved an elaborate re-imagining of US economic history.

Of course there are lots of different ways of setting up a counterfactual to challenge

Jenks's ideas; and equally there are many different ways in which the 19th-century US economy could be re-imagined without the railroads. Fogel attacked just one little strand of Jenks's many different elements by specifying his counterfactual world very narrowly: he considered only how life would be different in transporting, by water and cart from farm to final shipment point, only four agricultural products—corn, wheat, pork and beef. He reasoned that the cost of such imagined transportation compared to the cost of moving these goods by railroads in 1890 would give a starting measure of the effect of the railroads on the American economy. Even just to figure out what those transport costs would have been in his imagined world proved an immense data task because there were 4.5 million farms sending goods to regional shipment points (the intraregional shipments), and then on by interregional shipment to final market points. For the comparison of transport costs for the latter interregional transport, he largely replaced railroads with the alternatives of canals and rivers, which would have been cheaper than railroads, but slower, and with winter closures. The social savings in 1890—the difference between not having and having the railroads—for this interregional part of the transport system, amounted to only a very tiny proportion of the total income of the economy. The more problematic bit of the imaginary world, and so of his calculations, was to figure out the alternative cost of all the transport from the farms to the main regional shipping points by water and road. Including these intraregional costs (along with the interregional costs) for those four main farm commodities still only saved 2.8 per cent of total economic income. That is, if America had no railroads, they would have been 2.8 per cent less well off as an economy in 1890 than they were, which is very small (perhaps only 1 year's growth).

Fogel had made a huge factual effort within his imagined world to prove that the railroads didn't make much difference to the real world, but he thought his figure of 2.8 per cent was still an overestimate because if you took away the railroads in 1890, you must have them taken away over the previous decades, so people wouldn't have settled where they settled and wouldn't be shipping stuff from where they shipped it to the markets—they would probably have settled somewhere else, and perhaps would not even be keeping pigs and cattle but growing crops, or even living in cities.

This prompted Fogel to an even greater imaginative effort. Before the railroads came along in mid century, canals had been built and rivers improved such that 76 per cent of the cultivatable land used in 1890 could still have been reachable without the railroads. He argued that if there hadn't been any railroads then there would have been further development of those existing transport systems. Fogel imagined and designed 5,022 miles of new canals and waterways that he claimed could have been built to fill in many of these spaces, leaving only 7 per cent of good land unreachable. Figure 1 shows the new waterways that he imagined would have been built to fill in

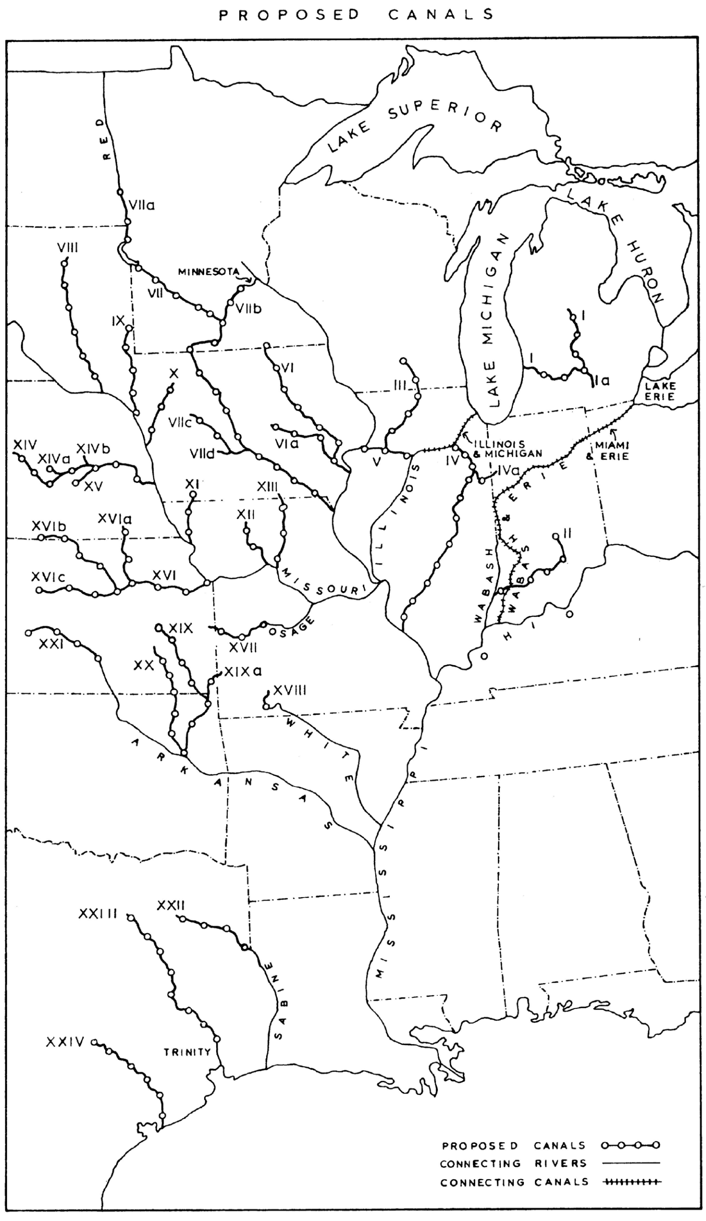


Figure 1. Robert Fogel's Map of Imagined New Waterways.
Source: R.W. Fogel (1964), *Railroads and American Economic Growth*, (Baltimore, Johns Hopkins University Press), p. 93.

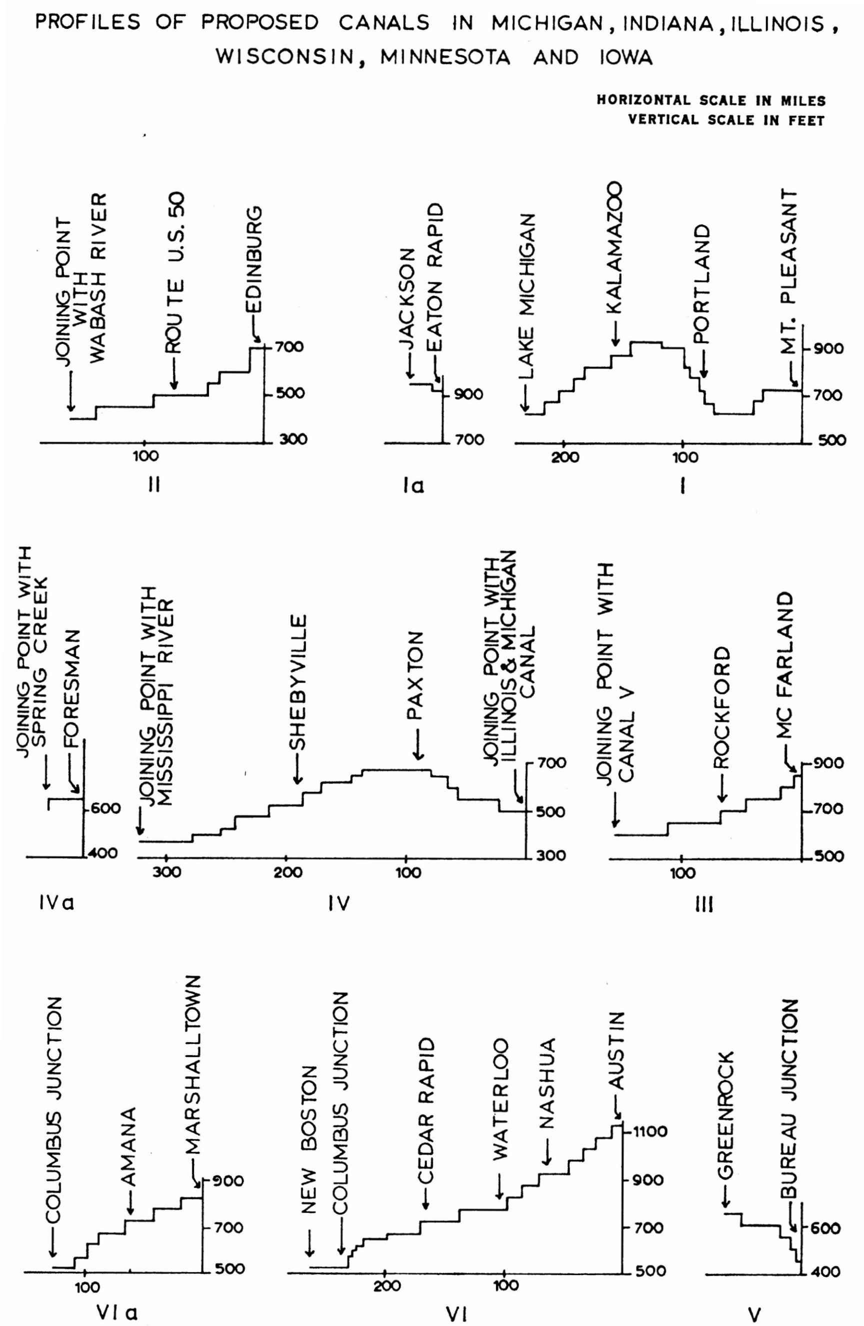


Figure 2. Robert Fogel's Profiles of Imagined Canals.
Source: R.W. Fogel (1964), *Railroads and American Economic Growth*, (Baltimore, Johns Hopkins University Press), p. 101.

much of that space, and Figure 2 shows one set of his canal designs. (There are in fact 5 pages of new canals designed to fill in his map of new waterways.) As can be seen, each canal path, its length and all the locks are planned; some of them are very long (several hundred miles) and some have more rises and falls than those canals that were built in the US economy before the railroads came along. Each canal was costed for construction and usage in order to measure properly the transport costs for his chosen goods in this newly designed counterfactual world. Having these additional waterways in his imagined economy meant that his calculations of the social savings of having railroads versus not having them dropped to only 1.5 per cent of GDP—peanuts. One of the reviews of Fogel's work captured the outcome in a nutshell: 'The iron horse was the paper tiger of the nineteenth century' (Rubin, 1967: 230).

Economists and historians who were sympathetic to Fogel's project and those of other 'new economic historians' (or cliometricians, as they came to be called) found the number rather small, but Fogel's calculations were so grounded in all those facts that it was hard to disagree with his numbers. 'Fogel's techniques are less striking than his use of imagination and a detailed knowledge of, and scrupulous regard for, the facts' (Williamson, 1965: 111). Most of the complaints from his sympathisers were that he hadn't covered enough of the agricultural goods let alone the industrial ones (raw and manufactured). Adding non-agricultural freight brought Fogel's saving calculation up to 4.7 per cent, still only a couple of year's growth; but he still hadn't covered passengers. Others thought that he had chosen the wrong date to make the calculations, and that transport services were not the most important domain for assessing the effects of the railroads on American development. Jenks would probably have agreed with this last point, for transport of agricultural produce was only one strand in his tapestry of effects, and one he thought not very important.

Fogel was criticised by other economic historians for using facts constructed from models rather than ones that were 'found' in the archive, or that could have in principle have been found in the archive but had got 'lost', or even ones which could have been interpolated in between other facts. Indeed, although Fogel's book was extremely heavy with facts, it was not quite clear where the facts stopped and the fictions began. One historian recalled his feeling of drowning in Fogel's facts when reading his book—and then suddenly, out the blue, there were 'new' facts about the cost of transport in the absence of railways—but where did they come from?[6] Still other reviewers were more overtly critical, describing Fogel's work as 'science fiction' (Erickson, 1966: 107), 'quasi-history', based on 'a figment' rather than on 'an hypothesis' (Redlich, 1965: 486, 484), and an 'imaginary journey into what might have been' (Kirkland, 1967: 1494). For this last group of historians, there was a fundamental difference between the facts

[6] In personal discussion at University of Pennsylvania seminar.

that could have been there and the ones that never were there, between theoretically underpinned quantitative economic history (such as Albert Fishlow's contemporary studies of the railroad's impact) versus model-builders and counterfactualists, that is, between those who dealt in facts and those who dealt in fictions.

Where indeed were these models? Rereading Fogel now it its pretty hard to see them as they slide in rather quietly and early in his account. He had used a 'linear programming model'—a relatively new form of model-basis for calculating the most efficient use of resources, one that had been developed in solving the logistical problems of the Second World War.[7] The use of this model to calculate the most efficient use of transport available for the 1890 economy with, and without, railroads meant that there were fictions on both sides of the social saving measurement. This is because the linear programming model calculated what would have been the most efficient use (at lowest cost) of the railroads in 1890 (not the actual cost of transporting goods by rail in 1890), and compared this cost with the imagined transport by his replacement waterways, also most efficiently used. In other words, they were both imagined economies, they were both model-based calculations, providing reason for Fogel to argue that his social savings numbers must be overestimates.

So the battles over the status of facts and fictions in this counterfactual were not just old versus new economic history, nor just a narrative with analytical economics (of the kind offered by Jenks) versus lots of data collection and counting. Rather this was also an argument within quantitative economic history between those who were theoretically informed but held steadfast to actual data in their statistical work and the model builders and counterfactualists who did not necessarily want to be held by what actually happened but by what could or should have happened in the most efficiently working economy that could be imagined. Fogel's world was a fictional world and one that depended, in the final moment, upon fictional data.

There are many layers of imagining and fictions going on here. Nevertheless they are all very grounded in facts compared to another counterfactual advanced by R. Preston McAfee (later a coeditor of the prestigious *American Economic Review*), who asked: What would have happened by 2000 if Columbus had not discovered America but fell off the edge of the flat earth? This was clearly a light hearted take-off piece of the counterfactualist project and began with Fogel's work:

> . . . if the railroads did not exist in 1890, they probably do not exist now, for there is no record of them being invented in the last ninety-two years. Non-existent railroads are unsafe to use. (1983: 735)

[7] The technique was rather new—developed by Leonid Kantorovich in Russia in 1939 (for war-time resource planning) and by George Dantzig and John von Neumann in the USA (see Klein, 2001: 128–33).

While using non-existent railways may seem less daunting than falling off the edge of a flat earth, McAfee's counterfactual has very few facts indeed to ground it in any account of American economic development. In contrast to Fogel's heavily quantitative and fact-driven work that produced his imaginative account of the economic world, McAfee's counterfactual is a piece of science fiction.

Since Fogel and McAfee both deal with imagined events and do so within imagined worlds, why does it seem reasonable to regard one as science fiction and the other as an imaginative account of the world we live in? Fogel's imagined world is more plausibly our world in the sense argued by Geoffrey Hawthorn's writings about counterfactuals and the conditions he found necessary for creating 'plausible worlds' (as he calls them, 1991). He argues that in order to create a sensible counterfactual you need first to choose a sensible node point—as in Fogel's case 1890, when most of the railroads had been pretty much filled in. Second, you have to keep as much as possible the same rather than changing everything; that means you shouldn't choose a counterfactual that requires you completely to rewind the past and to rewind the future—as McAfee does. Third, counterfactuals, as Hawthorn reminds us, don't start with theories and models but start with some fact-like situation counter to the actual facts, and only bring in models and theories when they are needed to help generate those counterfacts. McAfee breaks these conditions and Fogel follows them, which offers a useful way to validate the distinction between McAfee's science fiction and Fogel's imaginative but plausible reconstruction.

In another comparison, one could say that both Jenks and Fogel give an imaginative description of the world but they use their imagination and facts at different points of analysis. If we recall Jenks's big canvas account, he relies on broad facts, not heaps of little facts (data points), and the ability to imagine all the interconnections between them. Fogel imagines the details of only a small part of Jenks's canvas, but this in turn requires him to imagine the whole transport landscape in a different way, an exercise that relies heavily on factual information. Both Fogel and Jenks keep their imagination firmly grounded in facts, both offer plausible world accounts, but facts and fictions intersect in very different ways in their two accounts.

IV WHAT IF . . . PLAYING GAMES WITH THE LAWS OF SUPPLY AND DEMAND

Philosophers of science, when they think about models, worry about the problem of representation: How do we know when a model offers a good representation of the part of the world that is being modelled? For the scientist, this is the problem of assessing the similarity of their model of the world to the world that we live in and know. The second major problem for the philosopher of science is to wonder about scientists' incipient claims that their models explain things in the world. How do economists use their models to do more than say 'this is a picture of the world' but use it to explain things in the world, and perhaps even change things in the world via economic policy? In the philosophical account of models as fictions, the suggested solutions to the problem of representation, and thence perhaps to explanation, relies on 'keys' (see Frigg, 2010; Bokulich, 2012; and earlier Goffman, 1974) that translate elements of the prescribed fictions on to the facts of the world. We can think of these as map keys. Drawing a map requires one to key in the things in the world onto the map. Typically, elements in the world are represented on the map with conventional symbols: maps in Britain have beer mugs where there are pubs and round red dots where there are stations (which isn't quite so obvious). These conventional symbols act as keys to tell us what we are going to find when we look at the map without knowing the territory, and just as important, where we will find them when we are in the territory (assuming we know how to read the map and its symbols). So keys as a matching device work both ways—from the world to the map, and from the map back to our actions in the world.

Having elements keyed onto a map helps us to learn things about the world from the map—in particularly about the relationships of things to each other in that world. Keying on a map enables the user to make inferences, for example: where the nearest railway station is in relation to a pub. This is what was happing in Fogel's counterfactual, where each set of real world facts and counterfacts from the imagined worlds created from the linear programming models could be compared. Because he could make the comparisons (between transport by rail or by water, alternatively imagined), he could also make the measurements, and inferences could be made from those comparisons. In his imagined counterfactual world, each set of objects and activities can be easily compared with the world actually happening in 1890 because the factual keys are there.

But unlike maps, keys don't necessarily work both ways for models. The first great difficulty with the notion of 'keys' as a solution to the fiction–fact translation for models is that it relies on the scientist first knowing an awful lot about the world to make the model, just as it does for someone to draw a map, that is to make a good geographical representation of the world and key the relevant social and environmental symbols that depict the elements in the right places. As with any scientist, the economist does not necessarily understand enough about how the world works to build their models as accurately as a map. This is indeed the reason they need to use their imagination to build models—to try out their ideas about how the economic world might be, and how it might work with their what-if questions. Because models are not full enough or accurate enough representations of the world, economists' what-if discussions enable inferences to be made within the model about the things that are keyed there, but do not necessarily enable us to infer back to the world that the model represents.

The second problem is that, in many cases of models, it is not obvious that there are factual equivalents to those imagined elements, for model elements are often concept-based not fact-based, so that keys chosen to translate from models to things in the world might be quite misleading. Even if there are not missing or misleading elements, ‘props’ may not be easy to ‘reverse’, as required if the model elements are to be translated back for understanding the world or for policy action in the world. Models are not the same things as maps. And key fashioning clearly requires more than just prescribing that a stump is a bear in a game of make believe. Indeed, it may be reasonable to argue that the task of scientific modelling is *both* to create models and to fashion the keys for using them.

Creating the means and processes of translation from fictions to facts can be well illustrated in the ways that economists build and use their supply and demand models. The supply and demand model—how economists imagine market relations—is probably the most recognisable diagram of economics for people who are not economists: an early example from Marcel Lenoir (1913) is given in Figure 3. The

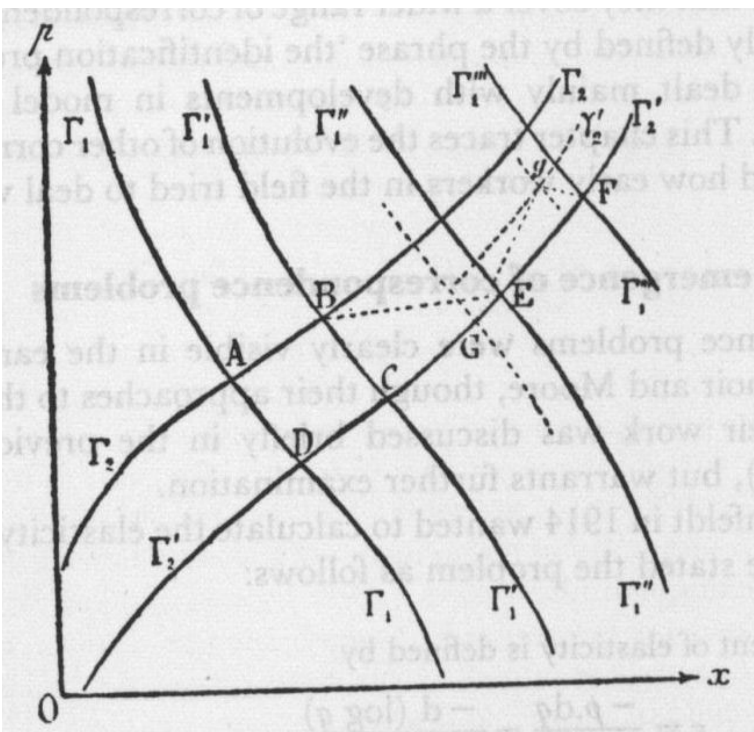


Figure 3. Marcel Lenoir’s Supply and Demand Diagram.
Source: M. Lenoir (1913), *Etudes sur la Formation et le Mouvement des Prix*, (Paris, Giard et Brière), p. 58, fig. 44.

vertical axis measures prices and the horizontal one quantities; supply curves (usually) slope upwards to the right and demand ones upwards to the left. Each curve represents how economists imagine or hypothesise what a set of consumers and producers imagine they would buy or sell at the different prices in the market place (and these diagrams also show that these curves shift). So there's a double level of imagination going on here. The demand curve is not such a strange thing to imagine because people know, from their own introspection, how they might respond to prices: if prices are higher or lower, they might buy less or more of the good, which is surely why the diagram is easy to explain. But there are serious problems here. This introspection is for the individual, and these curves model the market. And while casual observations of markets may support these diagrams, the diagrams themselves were not initially based on any direct observations of supply and demand curves because those things are not there to be seen in the market. Rather, these diagrams are constructed objects on paper that economists use to describe market relations and they believe that different kinds of markets might require different shaped curves. Notice also that there is (usually) no scale, these diagrams have this abstract quality because they are conceptual constructs—another element of imagination. These now standard items of economics were first drawn in the mid-19th century (though they were imagined before that) and they are used to ask a whole range of what-if questions using different technologies of investigation.

Fashioning the keys for the demand and supply curve diagrams took place between 1860 or so and 1960. It involved four different kinds of research work to understand what kinds of supply and demand facts might be expected from theory and might be found in data. These activities involved: (1) experimenting with different versions of this abstract conceptual model; (2) defining what interesting economic facts implied by the model might or might not exist in the world; (3) fitting statistical facts from markets to the fictional curves; and (4) using the models in experimental designs for the first classroom experiments done in economics. Starting from a simple, abstract, imagined model, these projects have not only explored what kinds of keys are possible, and fashioned some usable ones, for the model, but also generated lots of different kinds of interesting findings.[8]

(1) Although Marshall (1930 [1890]) was not the first to draw these supply and demand diagrams (see Humphrey, 1992), he did develop both the diagram and its standard usage in model experiments. Figure 4 shows a block of six of his diagrams, where we can see that the supply curve has different shapes—horizontal, upward (the normal case) and wavy. We can also see that in the top three, the demand curve is shifting upwards (perhaps because there is an increase in income for consumers) and

[8] Discussion of examples (1) and (4) is drawn from Morgan (2012: chapter 7).

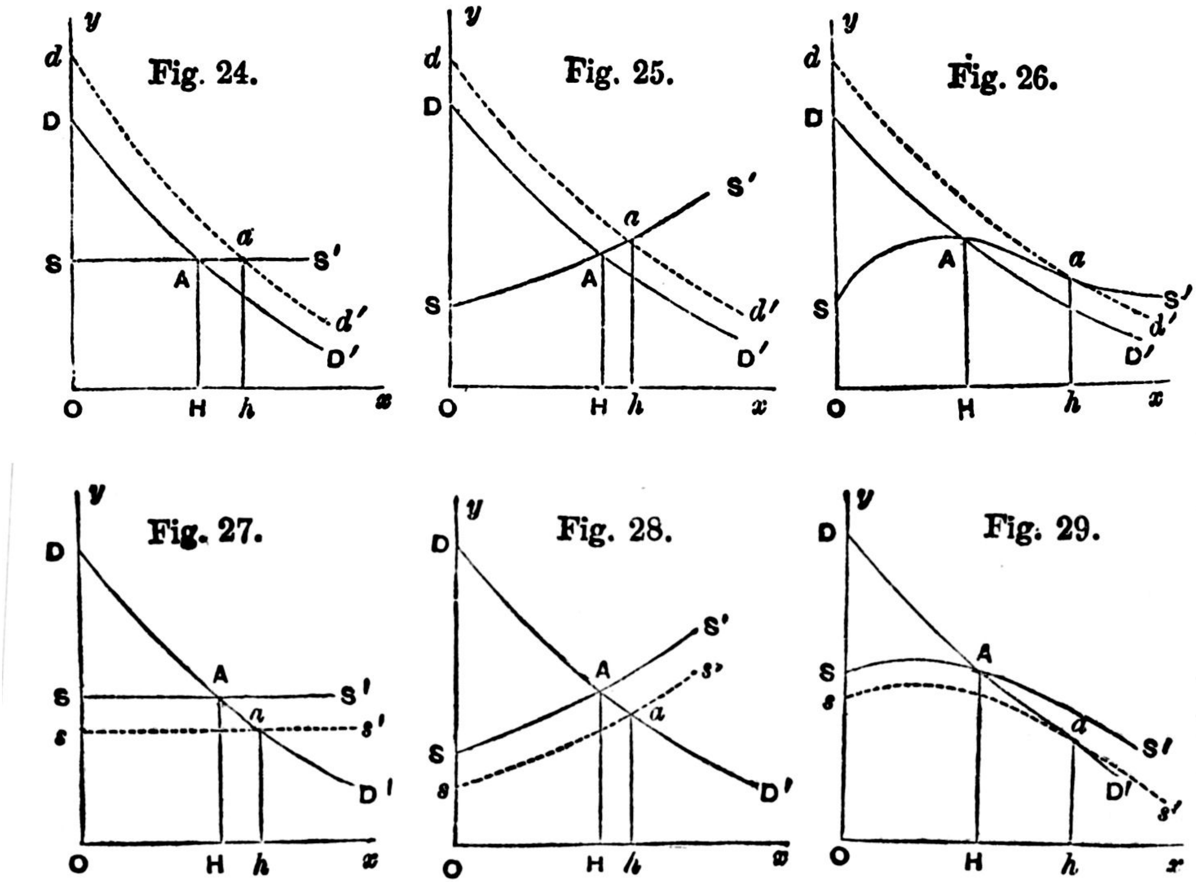


Figure 4. Alfred Marshall's Multiple Supply and Demand Diagrams.
Source: A. Marshall (1890), *Principles of Economics* Book V, Chapter XIII, Figures 24–6, note 1, p. 464; and Figures 27–9, note 1, p. 466. Reproduced with acknowledgement to Marshall Library of Economics.

in the bottom three, supply is shifting downwards perhaps because of a technical change which makes production cheaper at all levels of output). Marshall developed the method of using these little models of supply and demand in 'what-if' experiments: such as what happens if the supply or demand curve rises or falls, or what happens if a tax is put on the price of the good. (For example, in the top left-hand one, where the supply curve is horizontal, he asked what would happen to the price and quantity if the demand curve shifted slightly upwards. Clearly, the quantity along the horizontal axis would increase but the price would not change.) Some of these what-if experiments were simple, others (such as the far right pair) had outcomes which were less obvious, and some quickly took him into quite big questions about ethics and distribution. As already noted: this diagram is an imagined set of relations about how people as a group are imagined to behave, not a map of what was already known and already tied down. Marshall used these what-if experiments as a testing ground for theories about supply and demand, and so each of these little diagrams offers a site of different experiments about what we might expect to see happening if each one offers a good picture of the world. Already it is clear that one diagram is not enough, supply and demand relations of the economic world are quite varied in Marshall's imagination, which means that there must be several keys depending on the circumstances of the industry.

(2) This imaginative quality of the supply and demand diagram was problematic for those such as John Clapham, an economic historian, who argued that these diagrams were only 'mental furniture' for the economist and could not be made applicable to things in the world. He was interested in real industries, real goods, real prices, real quantities, and complained that whereas he would expect to go into a hat factory and find boxes of hats on the shelves, these conceptual devices of Marshall were like so many 'empty boxes' on the 'shelves of his [an economist's] mind' (1922: 305). They were labelled with constant return industries (the horizontal supply curve), increasing return industries (the supply curve falls with more produced) and decreasing return industries (the supply curve rises with more produced). Not only were these boxes mental constructs, but he argued that no keys were possible to take you to anything in the world: thus those boxes are empty. They are empty, Clapham argued, because it is very difficult to get any kind of information that would give you these supply curves as observational or factual things, based on the point that the supply curve refers to an industry not a single producer or firm. He gave four reasons. First, there is no such thing as a standard commodity (in manufacturing rather than in raw materials) and so no well-defined industry: 'a standard hat is not a mathematical concept' (Clapham, 1922: 311). Second, there are no existing statistical keys which would tell which real firms, if they could be collected together into industries, were examples of any of these idealised returns to scale curves. Third, it was impossible—in

practical ways—to separate out increases in efficiency due to increase in scale from those due to technical change, or 'to separate out the effects of organisation from those of invention' (313), so independent ways of measuring returns to scale were not viable. Finally, even if the economists could turn their conceptual boxes into actual boxes (by solving these measurability and classificatory problems), it is not clear what the practical use of such actual boxes could be. Thus these model boxes are empty as a matter of principle, and you can not fill them up, or fashion any keys, to enable these curves to refer to objects in the world.

Taking up the challenge, Arthur Pigou (1922), one of the leading figures following Marshall, agreed that these little model diagrams did indeed exist as boxes on the shelves of the economist's mind—they are the economists' mental furniture: they provide the 'intellectual machinery' (461) that helps them work things out analytically (rather than realistically). As with Marshall, the logical machinery of these various arrangements of things in the world enabled the economist to 'disentangle and analyse the causes by which the values of different things are determined' (460). For Pigou, not only are these boxes, containing different shaped curves and labelled with different returns to scale, in economists' minds, but if you opened any box, you would find even smaller boxes fitting inside which offer further subdivisions. In other words, there are classes of different kinds of things in the industrial world—as Marshall's diagrams show—but there are also sub-kinds or classes within these. Each box can be opened for what-if reasoning using the logical machinery inside it. Boxes do not have different kinds of goods in them as Clapham hoped, they have different kinds of industries with different market behaviours, as conceptualised into models by these imaginative economists. The keys had not yet been fashioned to match realistic markets, but the investigation of the basic model and its proliferation into kinds and sub-kinds revealed that, rather than any uniform or master key, many different keys might well be needed.

(3) In the 1920s and 1930s, economists made a determined effort to find statistical facts to match these supply and demand mathematical and diagrammatic models, that is to make the curves themselves measurable and factual. Although Clapham had pointed to the absence of data for manufactured industry goods, there were quite good data on market prices and quantities for agricultural goods, and for some industrial raw products. These are the markets where such measuring initiatives were made, for this was—above all—a keying project, matching facts and fictions together. But whether such curves were measurable proved not to be a simple question of finding data, but of the specific patterns of stability and variability in the behaviour of the buyers and sellers in the market. As we can see from the earlier Figure 3, the abstract model suggests supply and demand curves are both liable to shift around because of other factors that affect either demand or supply or both. The implication of this was explored by the economist Philip Green Wright (father of the famous

geneticist Sewall Wright) who was one of the first to work on this problem as a statistical problem. His charts (1929), found in Figure 5, point both to the problem and to a new kind of machinery developed to fashion statistical keys. He asked: What if just the demand curve shifts in the market place and nothing else changes? Then those shifts will trace out the data points on the supply curve (see his Chart I) and the statistical matching will measure the supply curve. Vice-versa: What if just the supply curve shifts and nothing else changes? Then the statistical matching will trace out and measure the demand curve (his Chart II). So, under certain behaviour conditions in the world, the supply or demand curve could be recovered from the market statistics and measured, in these two cases—simple statistical keys will work. But what happens if both curves are shifting from some other reason over time—then the outcome will be as in his Chart III. There, the curve traced out by the statistics of supply and demand is neither the supply nor the demand curve, but simply a trace of changes in prices and quantities over time. A simple statistical key will not do for whatever this final statistical curve was, it was not something imagined in the original abstract model.

This problem of figuring out what relations could be keyed to the world became known as 'the identification problem'. The specific problem here—of identifying the supply and demand relations when market curves were each subject to lots of different influences—was the first exemplar of this generic problem. The point is that just having statistical data does not make the keys that unlock the problem of turning those imagined models of markets into factual ones. Creating such keys that would reveal the factual relations in the world involves some imaginative work in solving identification problems, and econometricians (as such statistical economists came to be known) developed some very nifty technical solutions to these very knotty problems, and this continues to be a vibrant research area in econometrics.[9]

Sometimes the identification problem is irrelevant because the market does not even exist in a form that would create any such data. Thus, we find economists in the 1950s trying to measure market quantities to fit into a wider computation of national income, for which an accounting of all the goods produced in a country is needed. If much of the material produced is subsistence, or is exchanged in barter, then it is problematic to measure the production of those goods accurately via any kind of limited market statistics. In such cases, the original imagined supply and demand model does not seem to apply, yet economists developed processes of indirectly deriving measurements of exchange quantities even where those markets did not exist. In other words, 'factual curves' were produced even when the market itself was only an imagined and never a real market. This solution is akin to Fogel's use of the linear

[9] Wright was also the first to use what has come to be called 'instrumental variables' as a solution, using a variable that acts as an instrument so that the relationship of real interest can be measured accurately. Discussion of the identification issue with this example (3) is drawn from Morgan (1990: chapter 6).

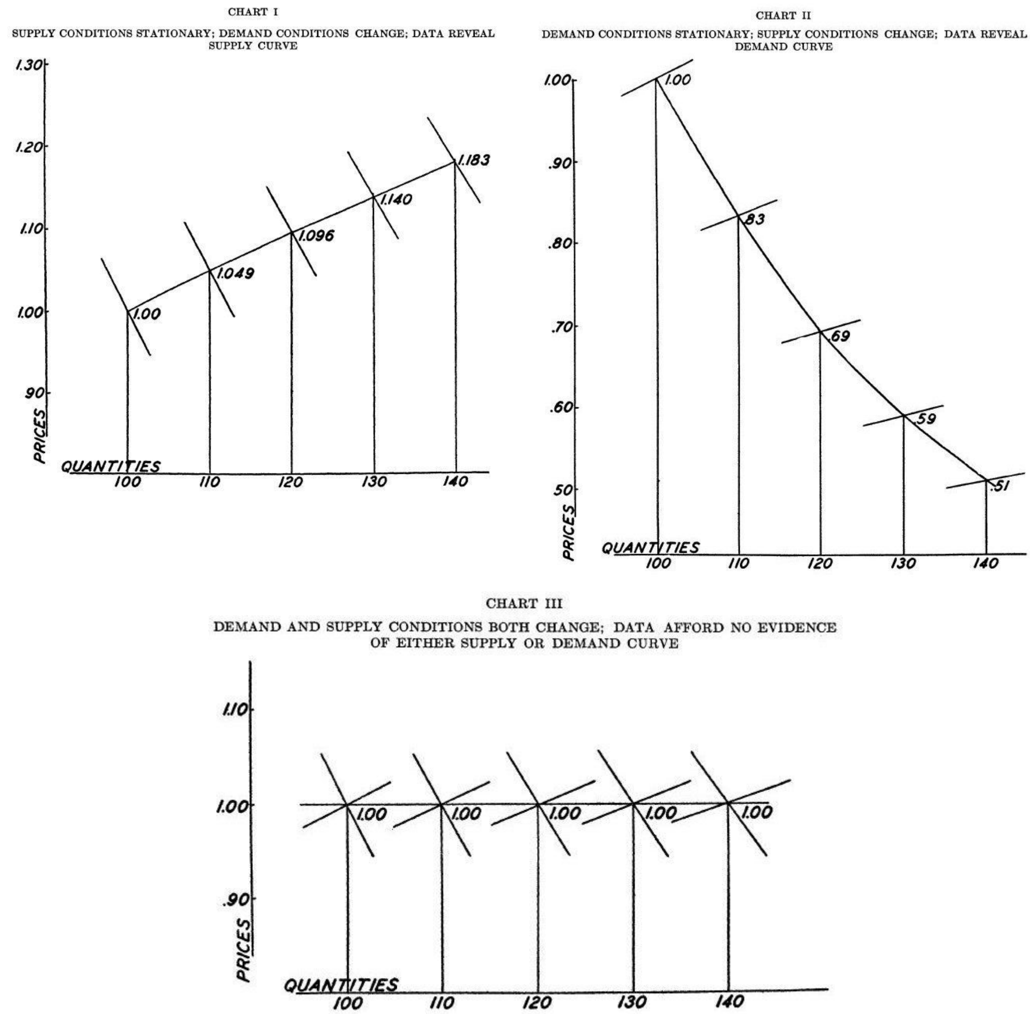


Figure 5. Philip Green Wright's Identification Diagrams.
Source: P.G. Wright (1929), 'Review of Henry Schultz: *Statistical Laws of Demand and Supply*', *Journal of the American Statistical Association*, 24: 207–15. Charts I, II and III, pp. 211–13. Reprinted by permission of the publisher, Taylor & Francis Ltd.

programming model to create facts for his imagined US economy of 1890, with similar responses from the critics, as we see in this reaction from a team asked to create data for the missing market for Nigeria in the 1950s:

> Where goods and services are not marketed it is possible to go as far as asking what they might be worth if they were. To take the further step of inventing functional relationships such as demand and supply curves, or even appearing to invent such relationships, seems to us unwarranted . . . Where complex economic transactions do not exist, little purpose is served by making them appear to do so. (Prest, Stewart & Lardner, 1953)

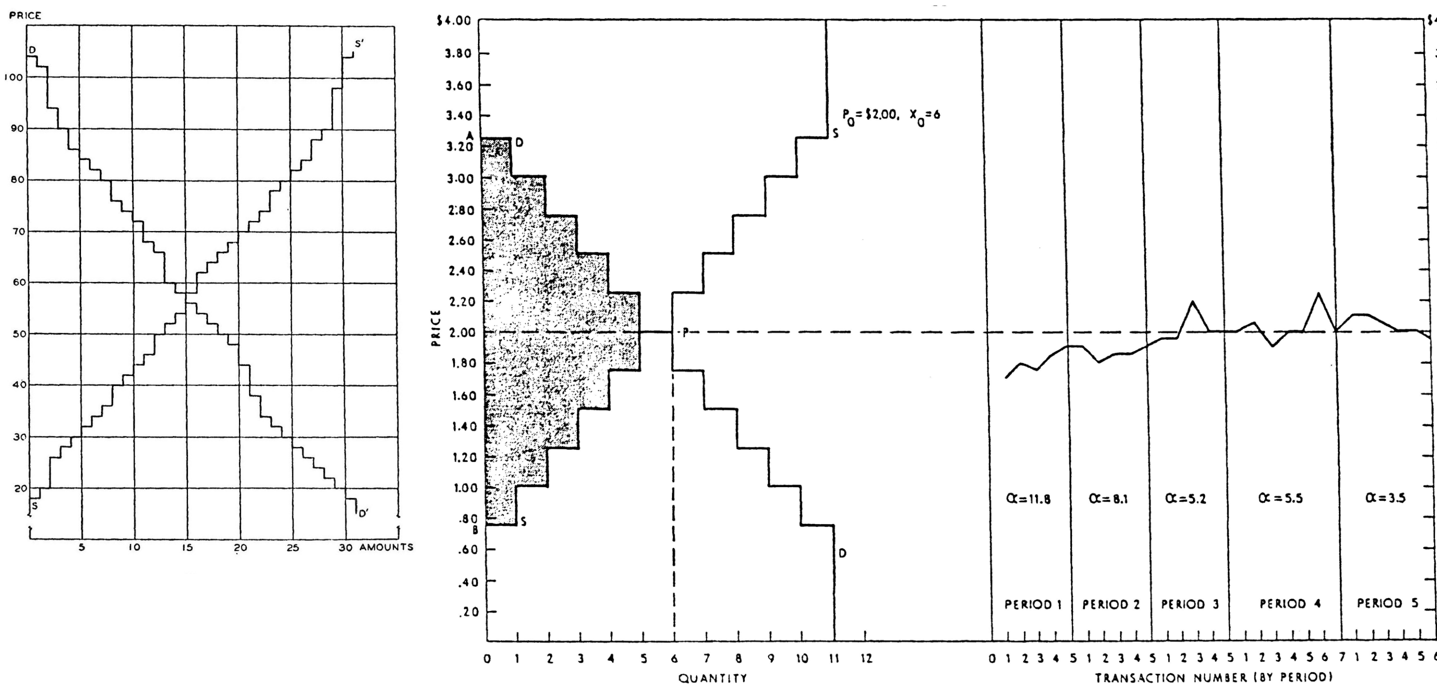


Figure 6. Edward Chamberlin's (left) and Vernon Smith's (right) Experiments with Market Models.
Source: E. Chamberlin (1948), 'An Experimental Imperfect Market', *Journal of Political Economy*, 56: 95–108; Figure 1, p. 97; V. Smith (1962), 'An Experimental Study of Competitive Market Behavior', *Journal of Political Economy*, 70: 111–37, Chart 1, p. 113. Reprinted by permission of the publisher, University of Chicago Press.

(4) Another mode of keying models to factual materials began with the first ever classroom experiments in economics. Edward Chamberlin in 1948 and Vernon Smith in 1962 developed the process of using the imagined supply and demand model to create a model market in the classroom. Chamberlin's stated aim was to 'observe a real model'—notice he did not seek to observe a real market, but to observe the actions of real people in the imagined model market of economics. The tradition he established was to draw out a set of demand and supply schedules with numbers attached and give to each member of the class a card telling them if they were buyers or sellers, and what their 'reservation price' was (the price above or below which they would not trade). There were rules of trading to govern how the model market worked, and in the graphs (Figure 6) we can see recorded examples from both Chamberlin's and Smith's model markets. Factual keys to the objects of the model (the demand and supply prices and quantities of the two curves) had already been created as part of the experimental design and its protocols, while the classroom experiment was undertaken to fashion outcome keys to the intersection points of the curve. Chamberlin's outcomes were not quite as predicted by the model, but Smith soon discovered how the rules of trading in the protocols (such as one period of exchange or more, open-outcry or individual contracting) could be varied to obtain the 'correct' outcomes for the classroom market as predicted by the model. None of these keys—created in the design or outcome of the experiment—were initially related to real world markets. Only later did economists draw on these kinds of results and their findings about trading rules to design real world markets that worked according to their models.

V WHAT IF . . . THE ECONOMY IS AN HYDRAULIC MACHINE

Thinking about models as fictions and the ways in which models involve fictional elements has proved fruitful. It has given insight into the importance of imagination in model making, pointed to the myriad ways in which the fictional and factual intersect in economists' modelling activities, and to the difficulties of translations between factual and fictional elements. But the presence of these factual elements suggests that we might consider another label, and might indeed prefer to consider scientific models as *artefacts* (Knuuttila, 2011). This notion of models as artefacts is not to be understood here as being in opposition to that of models as fictions, but rather to offer a more generous notion. The new label still implies that models embed or require some kind of imagination to make and use. And fictional elements are not ruled out, for the label has the advantage of openness: model inputs are unrestricted, models can contain both factual and fictional things, intuition, analogies, indeed, all sorts of stuff: there is no fixed or single recipe for making a scientific model, and lots of ingredients are possible (see Boumans, 1999). The notion of artefacts accurately refers to the fact that not only are the models that economists use human-made but that they are material objects, even if only pen and paper objects, or written into machine code. That material quality of models is itself an important characteristic, for it this which enables them to function as a technology of enquiry (see Morrison & Morgan, 1999). In Morgan (2012), I argued that this technology of enquiry is manifest in the way models are used as experimentable objects answering what-if questions—both to extend the imaginative reach of the modeller and to offer materials that *might*, with difficulty, be keyed to events in the world. And while understanding models to be artefacts does not solve the classic philosophical problems of representation and inference, it does hold open some of the possibilities that the idea of models of fictions particularly seems to close off, namely about how answering what-if questions with models can be informative about the world.

To show how this artefactual understanding of models is rewarding while at the same time building on the ideas developed while thinking about models as fictions, the final model to be discussed is an hydraulic machine. It was built just after the Second World War to understand the macroeconomic system, and as it is a machine, it is difficult to refer to this model as a fiction, but certainly the notion of artefact fits it perfectly well. J.M. Keynes imagined that the aggregate economy needed to be treated as a system with its own behavioural laws. But Keynes was no modeller, he really wasn't. Rather, his *General Theory* (1936) gave a primarily verbal account of the aggregate economy in a book that many of his contemporaries found opaque. The young economists of his day immediately set out to write down some little mathematical or diagrammatical models in order to understand exactly what Keynes was trying to say, and whether—indeed—he was saying anything new.[10] These arguments continued into the post-war period and provide the context for one of the most unusual models in the history of economics, built in the late 1940s, to model the macroeconomic system as an hydraulic machine.[11]

The machine is constructed from a combination of observations, facts and fictions

[10] For economists, the most famous of these is the ISLM diagram, drawn by Hicks (1937) in an explicit attempt to figure out whether Keynes was saying anything new, and it became the standard model to understand Keynes's economics. Two other models are particularly relevant to the keying discussion. One is J. Tinbergen's (1937) fitting of statistics to a Keynesian model of the Dutch economy in the 1930s in order to see how to get out of the great depression (see Morgan, 1990: chapter 4). The other is P. Samuelson's (1939) simulation model that he used to explore what kind of policies would have what effects in a Keynesian model, and found that almost anything can happen, in other words, providing some very implausible stories (see Morgan, 2012: chapter 6).

[11] The tradition of thinking of money as liquid goes way back in the history of economics and there are designs for, and built, hydraulic machines to be found in the late 19th century, particularly in the work of Irving Fisher (see Morgan, 1997).

all intertwined together. There are keys that translate both the elements built into the model and the outcomes from experiments with the model back to the world. Although the artefact looks like a rather low-budget piece of schoolboy imagination, it functions as a carefully crafted and subtly imaginative account of the economic world.

The machine is normally known as the Phillips Machine, but it is more accurate to call it the Newlyn–Phillips Machine because it was jointly built by Bill Phillips and Walter Newlyn, and I want to write Newlyn back into the history.[12] These two economists can be seen here (Figure 7) with two generations of the machine. The one on the left is the first prototype machine, the Mark 1 machine, with Newlyn leaning over it. Its inauguration was reported by the *Yorkshire Evening Post*, because this machine was commissioned by Leeds University where Newlyn was working, and now stands in the entrance to their Business School building. Phillips on the right is shown with the second generation, Mark 2, machine (which might perhaps be labelled the Phillips–Newlyn machine). It is about 7 ft by 5 ft (slightly bigger than the prototype), and stands in the computation gallery of the London Science Museum (and is possibly its only exhibit from economic science). It has been restored, and a video display shows one of the last times it was used when televised by the BBC for a budget report in the 1990s.

These two inventors, Walter Newlyn and Bill Phillips, both left school with no qualifications. They made their way through the depressed 1930s world, to fight in the Second World War. Newlyn was an expert in communications equipment, was evacuated in the last days from Dunkirk and then served in India. Phillips worked in the air force on aircraft systems. He was taken prisoner of war by the Japanese and there are some wonderful stories about clandestine radio systems, which he managed to keep going, and about the Japanese failing to understand why the lights dimmed every evening (because Phillips had managed to figure out how the soldiers could get themselves hot tea) (see Blyth, 1975; Leeson, 2000). They returned from the war, and as they had both done some evening courses at the University of London before the war, they went to LSE as returning veterans and became friends there.

The traditional story, as I have said, has written Newlyn out and it is very important to bring him back in because of his experience in the 1930s. He started off in the 1930s as a very junior clerk to Darlings, the big Australian grain merchants and he worked his way up to be their chartering clerk on the Baltic exchange. The Baltic exchange then was not just an exchange for grain trading it was also the major exchange for trading freight. If you wanted to carry something around the world in a freighter you would come and make a contract in the Baltic exchange. Newlyn wasn't the trader; he

[12] A full version of the short history of the machine that follows draws on Morgan (2012: chapter 5). See also Morgan & Boumans (2004).

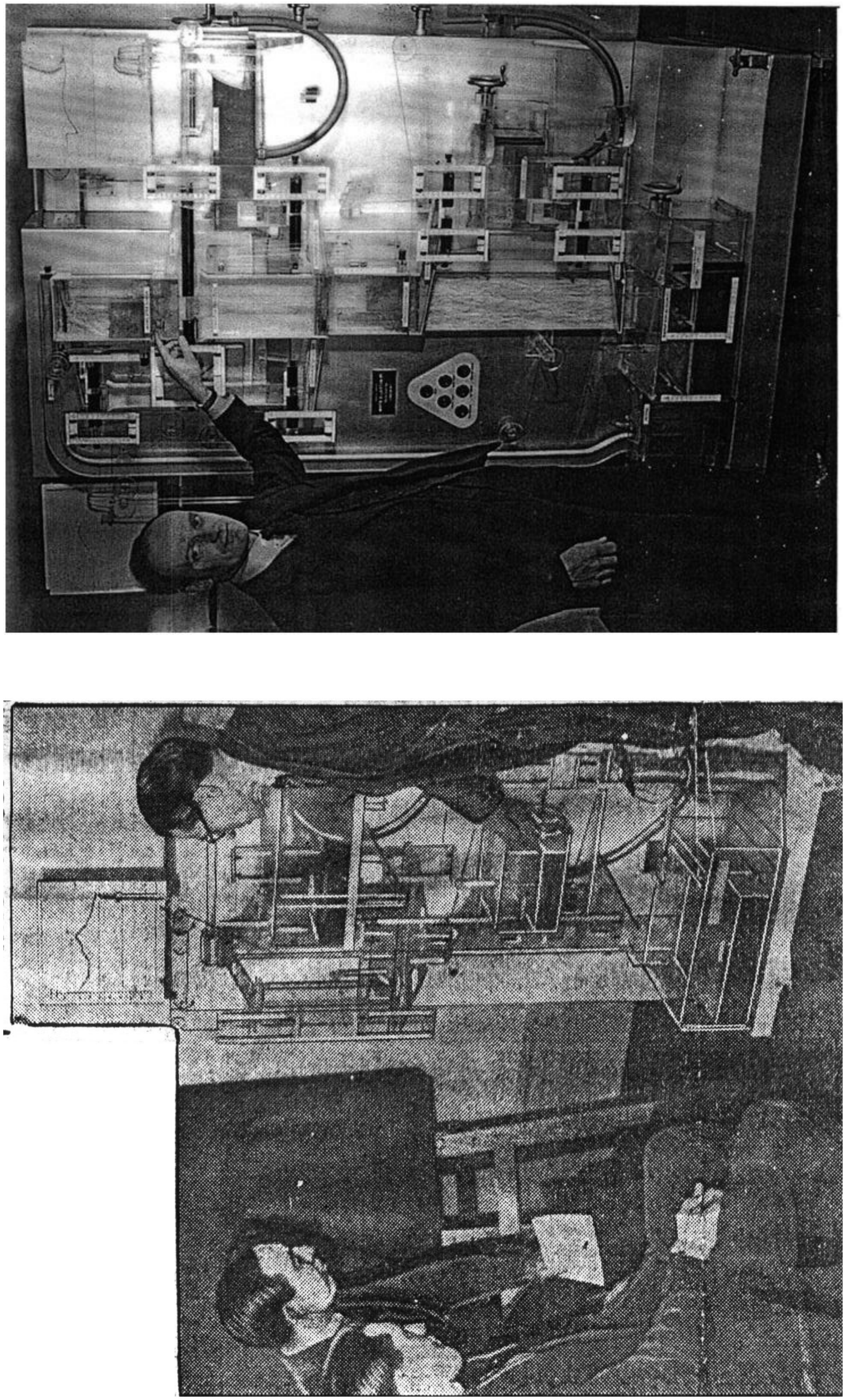


Figure 7. Walter Newlyn with the Prototype Mark I Machine (left) and Bill Phillips with the Mark II Machine (right).
Source: *Yorkshire Evening Post*, Friday, 20 January 1950; and LSE STICERD Archive.

was in some sense more important—he was the person writing the charter contracts, so he really understood how money went round the system because he was right there sending cargos round the world and writing the contracts for the monetary exchanges. Here is an extract from his theory of money, which speaks from his experience of how flows of money don't depend on large stocks:

> Take, for example, the case of a large scale merchanting firm disposing of a bulk cargo of grain purchased abroad. Having received the cheque at 2:45 pm a messenger will have deposited it at the merchant's bank at 2:50. The messenger then proceeds round the corner and at 2:55 deposits a cheque for the same amount drawn on his firm's bank account to one of the London discount houses. Moreover, he probably passes in his walk a messenger of the buying firm who has collected a cheque of similar amount from another discount house and deposited it in the buyer's bank at 2:45 pm. (Newlyn, 1971: 75)

So all the money had been moved around within 10 minutes.

Bill Phillips had a very different but equally important early experience, for in some sense he grew up inside a hydraulic machine. His childhood was spent on a New Zealand dairy farm in a very rural area, which had no electricity. His parents were both equally important in organising the stream that went through their farm not only for farm use, but to give them electricity, and a flush toilet, inside the house. This is an account from his sister about how they lived at the mercy and control of this hydraulic system:

> Of course, it was wasteful to run a generator when not required. Consequently, Dad built a neat winch into the ceiling in their bedroom and when they decided it was 'lights out' time, the sound of the winch being wound alerted us to the imminent 'blackout' . . . As the winch wound in the cable, the trap-door was raised the water was then diverted to the side of the water-wheel to rejoin the stream . . . the wheel stopped turning, generation stopped and 'LIGHTS-OUT'. (Carol Ibbotsen-Somervell: 5)[13]

In other words, he understood from his early life, how water flows can make a domestic economy work.

These two individuals who grew up with very different experiences came together, made friends at the LSE, and spent social time together, going on walks, going to the theatre and meeting over the refectory table. They invented the machine together in three important steps. First, Phillips transformed a standard economics diagram of supply and demand into a diagram of domestic plumbing. Second, Newlyn developed the blueprint for this hydraulic machine. Third, they built the prototype of the Newlyn–Phillips machine. All this happened over a short time in 1949, when they spliced their own experiences of hydraulics and of money flows in an economic system into this analogical machine.

[13] From a memoir in the STICERD archive, LSE.

In the first step, Phillips wrote an undergraduate essay where he took that standard supply and demand model and queried what was on the horizontal axis: is it quantities of something at a point in time, or is it flow per unit of time? As he pointed out, stocks and flows could not be represented on this same diagram, and the what-if experiments could not be conducted, unless stocks were constant. So he took advantage of a diagram from a recent textbook by Kenneth Boulding (1948), who had drawn a domestic plumbing example—the cistern from a lavatory—to show how one might get stocks and flows working together. Phillips developed Boulding's plumbing diagram to design his own version (both shown, Figure 8). The second half of his paper is an attempt to do the same for a macroeconomic system, but it is very clear from his text that he did not understand the macroeconomics of his time.

Phillips showed this essay to his friend Newlyn in early 1949,[14] who thought this idea of making a hydraulic account of the macroeconomy was just the way to go—in fact he had already, in one of his own undergraduate essays, played around with an idea of a hydraulic process for a monetary system. Newlyn's essay showed he really understood the professional literature on macroeconomics even as an undergraduate, and he took Phillips's ideas of plumbing and his own knowledge of macroeconomics to develop them into a blueprint (Figure 9) for a macroeconomy machine. (The copy is poor as it is on a large sheet and had been cut in half in order to be stored.) The blueprint was taken to Leeds, where Newlyn was already a lecturer, to persuade the head of the department to fund the prototype machine shown in the figure. In the summer of 1949, they constructed the machine together—Newlyn explaining the requirements of the macroeconomic system to Phillips the junior economist, and Phillips the senior engineer being helped by Newlyn the junior one.[15]

A drawing of the machine (Figure 10) made to market the Mark II machine around the world in the early 1950s, shows the machine more clearly than photographs do. The basic design is of a circular flow, the channel up the left-hand side is the income flow going up; when it starts coming down as expenditure, it divides itself. The first thing that comes off on the left is taxes, consumption goes down the centre part, investment comes off on the right-hand side and then exports and imports and the external sector are shown underneath that. There are a number of tanks, which hold different stocks of money. So the whole economy is pictured, all the bits of the Keynesian system in terms of aggregate national income, aggregate expenditure, aggregate consumption, imports and exports, savings, taxes and so forth. Each of these can be keyed back to the categories of the national income accounts, which were

[14] Newlyn kept the essay and it is now owned by his son-in-law, the economist Martin Slater.

[15] They published separate articles about the machine, expressing their own interests and knowledge (see Newlyn, 1950; Phillips, 1950).

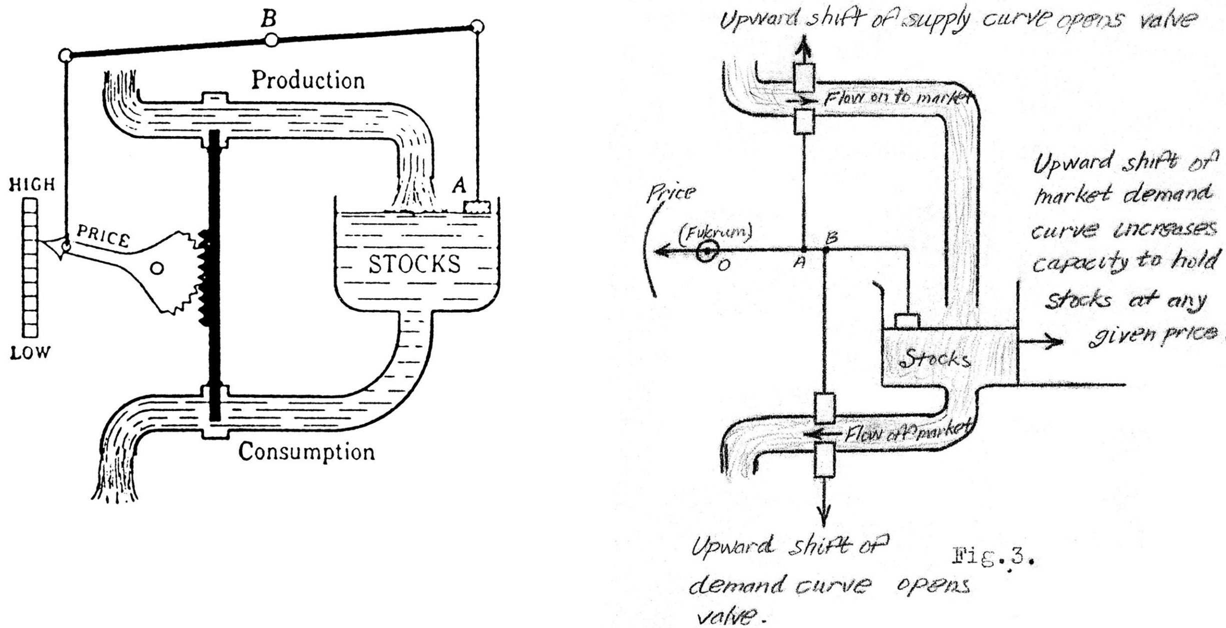


Figure 8. Kenneth Boulding's (left) and Bill Phillips's (right) Plumbing Diagrams.
Source: K. Boulding: *Economic Analysis* (1948), Figure 9, p. 117, reproduced by courtesy of the Bentley Historical Library, University of Michigan; A.W.H. Phillips's Undergraduate Essay, Figure 3, reproduced by courtesy of Martin Slater.

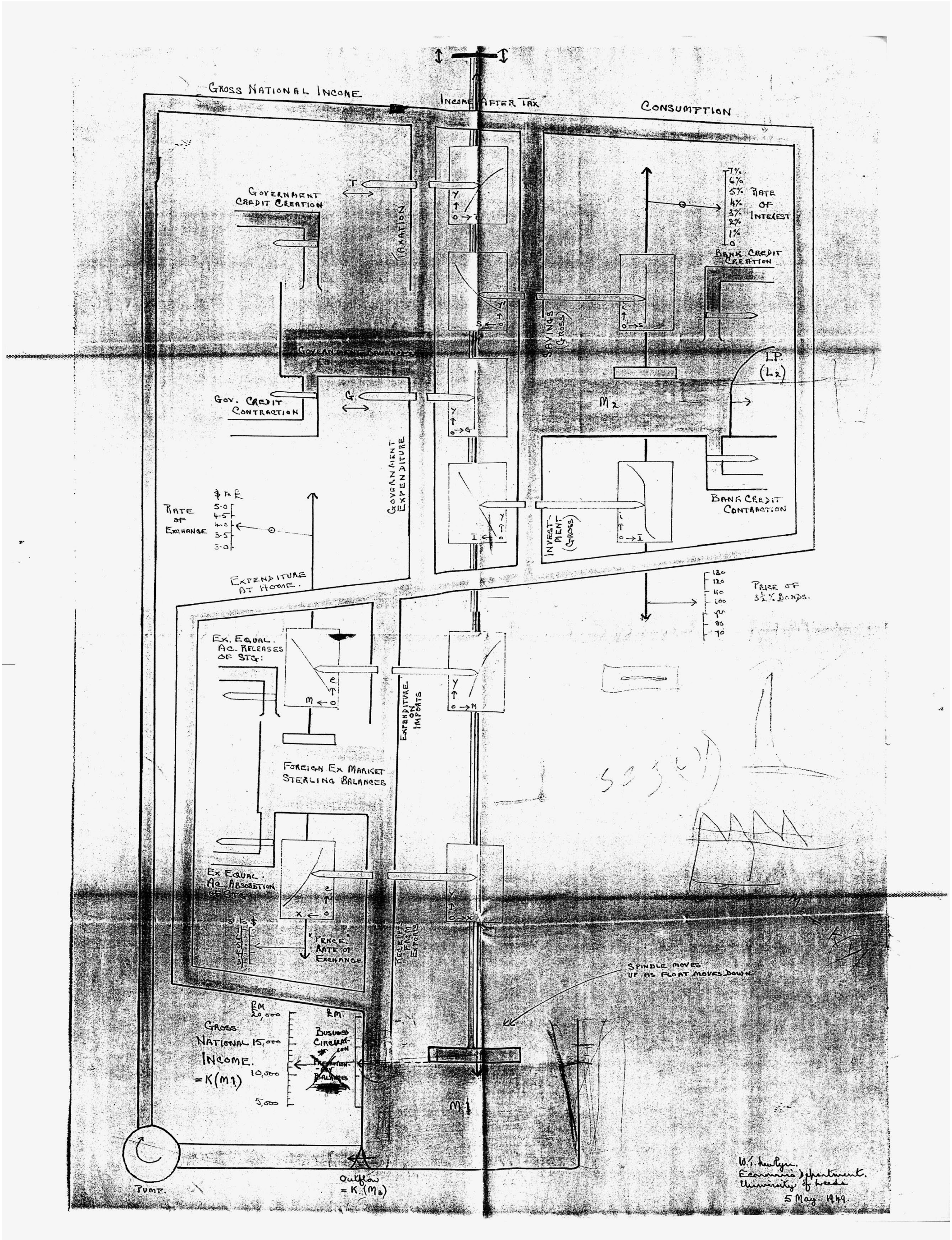


Figure 9. Walter Newlyn's Blueprint for the Machine.
Source: STICERD Phillips Machine Archive, reproduced by courtesy of Lucy Newlyn.

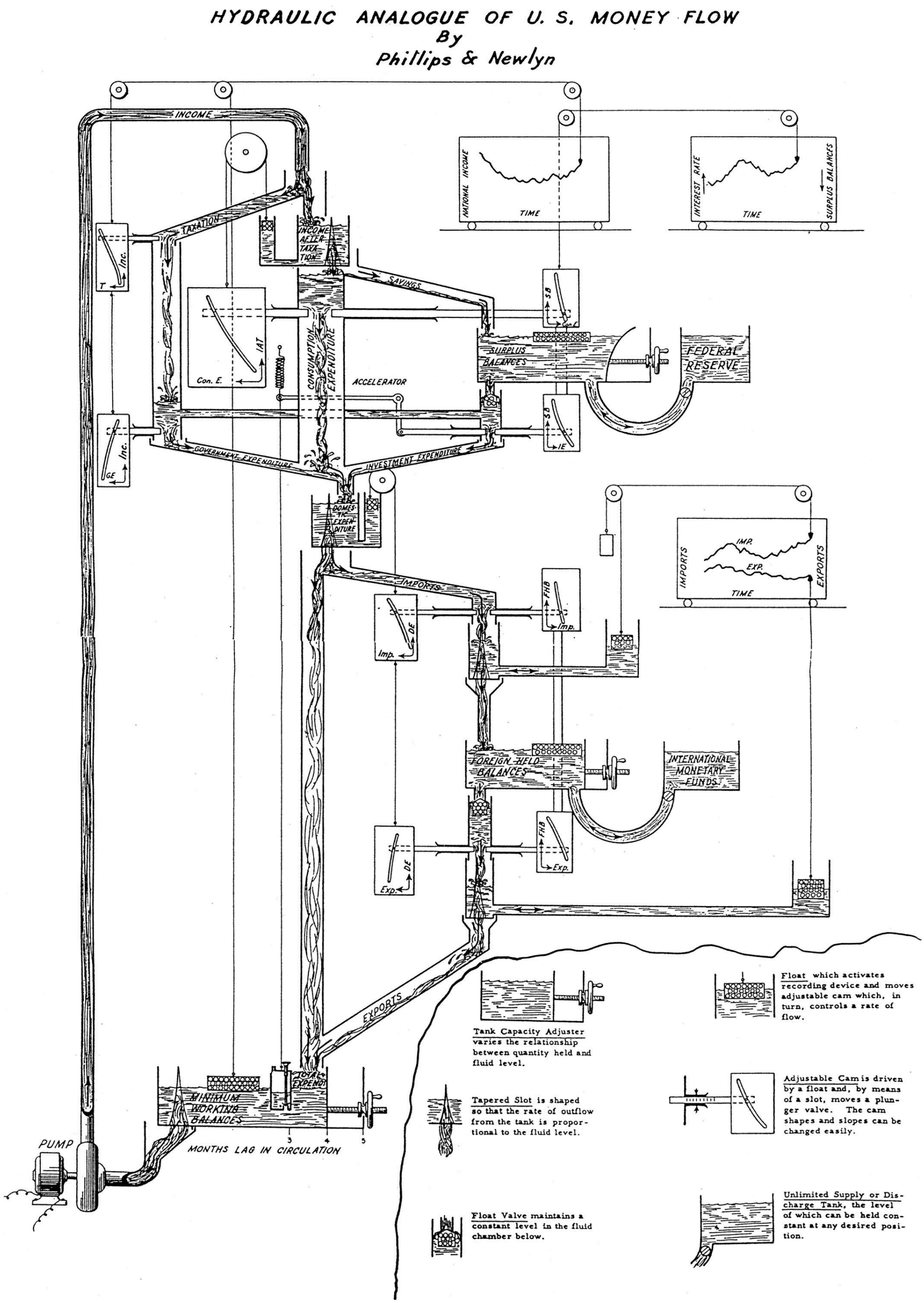


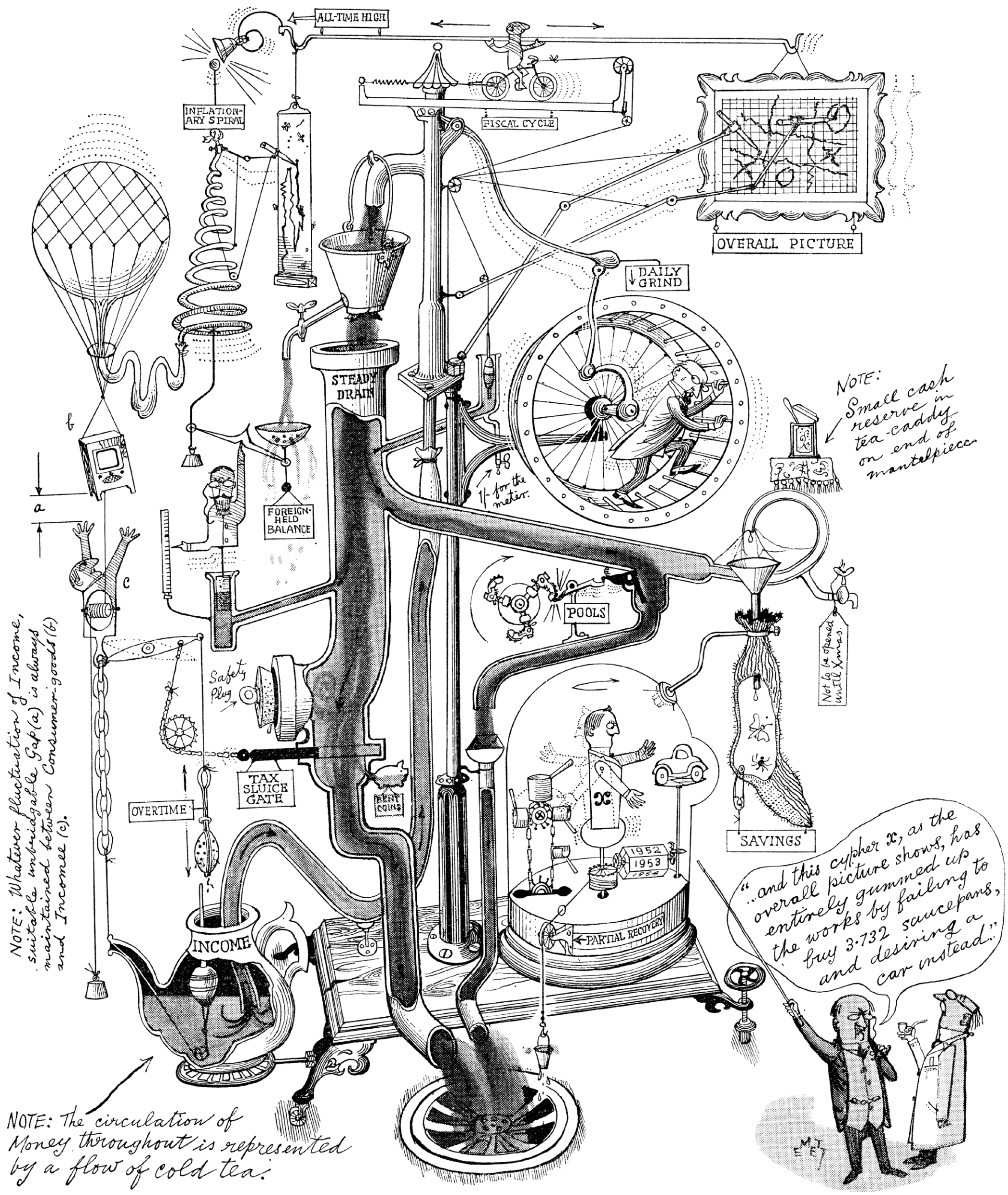
Figure 10. Marketing Drawing of the Hydraulic Machine.
Source: James Meade Archive, LSE.

established in the late 1940s largely using Keynes's aggregate economy concepts as definitions.

Despite its looks, the machine has wonderful resources for game playing which involve both the hydraulic format and the economic content of the machine. For example, the economist can set the levels of money in the tanks; change the 'function slides' that describe the aggregate behavioural relations in the system and thereby control the valves; unhook or hook-up parts of the machine; and even link two machines so that you can have an international economy (e.g. America and Britain linked up). All sorts of policy interventions can be enacted in the world of the model. It also acts as a computer (which is why it is in the Science Museum), for each time an experiment is done, the outcomes are charted in graphs on the top right corner, which show what happens to national income (and the interest rate: the price of borrowing money) over time. When economists played games with the machine, conducting what-if experiments with it, they found they could tell quite a lot of plausible stories about the world of the 1950s from the world of the machine. There were also, of course, some implausible stories from experiments when the red water (which represents the money flows) went all over the floor. The point is that the 'what-if' experiments have their own keying system, because not just the parts, but also all the relationships between parts, are embedded in the machine.

The Newlyn–Phillips (and its successor, the Phillips–Newlyn) machine is a very subtle model and its plausibility depends a lot on that subtlety—not just on the parts and their relationships, for those parts and relations are not passive when the machine is set in motion. Stocks and flows of money, income, expenditure etc., really work together, as indeed they do in real life—which is what Phillips was worried about. The time delays in the system, or the lack of them, was what Newlyn was really concerned about and understood from his chartering days: this was manifest in the time-lag in the machine because it takes time for a flow of 'money' (red water) to work its way round the system in any given experiment, and that time can be calibrated in certain respects. It also has a continuous pattern of circulation to give a real dynamics rather than the set of comparative static points taken as standard for working with models at the time (a mode of model work we saw in Marshall's diagrammatic reasoning). Finally, the interactions in the system worked in a real way as opposed to being imagined as working.

Of course, the Newlyn–Phillips model is in such a different medium that one can still think this is a pretty strange model, a wonderful world of make-believe science fiction as much as a serious piece of economic science. Its quirkiness generated a tradition of cartoons, beginning with one that Rowland Emett did in *Punch* for the government's budget day in 1953 and going on through the years up to the cover of *The Economist* in 2008 during the financial crisis. Emett depicted the machine as a



MACHINE DESIGNED TO SHOW THE WORKING OF THE ECONOMIC SYSTEM

Figure 11. Rowland Emett's Cartoon of the Hydraulic Machine.
Source: *Punch*, 15 April 1953, p. 457. © Punch Limited, reproduced by permission.

Heath-Robinson machine run on cold tea (Figure 11). (The small print and details are wonderful.) In the 1970s and 1980s economists got embarrassed by this machine and put it away in cupboards; more recently a number have been restored and it has become an artefact to be proud of, an icon of the modelling age in economics.

VI CONCLUSION

What is the importance and value of what-if questions? These examples have shown a range of different ways in which versions of that question are asked by economists in their work with models. We have also seen that economists' methods of answering such questions provide them not just with a way to understand the worlds they create in their models, but give them a variety of indirect ways of interrogating what kind of world we live in. We have also seen that these interrogations, and so the process of gaining understanding of the real world, depend on a lot of crossings over between facts and fictions, just as games of fictions do. The philosophers' of science notion of models as fictions proved a good starting point, but analysing the practical work of modelling pointed towards the multiple interactions between facts and fictions that offer scientists the chances of learning about the world from their models. These cross-overs may hold clues to why scientists often speak of 'explaining' the world with their models.

Focusing on 'keys' as the critical link that facilitates that back and forth between model world and real world was also helpful. This paper paid attention to the process of keying from the world to the model in both Fogel's case and in the Newlyn–Phillips machine case. These showed the sources for those keys to be in many respects factual: statistical data materials for Fogel and experiential knowledge for Newlyn and Phillips. This offers a contrast to the usual discussion of the building blocks of economic model building in which economists' 'as if' assumptions about behaviour have been justly criticised as unrealistic. Focusing more consistently on the usage of models in all three cases involved examining the keying processes prompted by what-if questions and the ways model stories were related back to the world. It is in the process of answering these questions (as much as in the 'as if' assumptions of model building), that the plausibility of models are questioned, their fictional/factual status assessed, and their contributions to understanding the world brought into focus. And if the plausibility of any particular model rests much more firmly on the kind of claims that come with what-if questions, then it is not whether the exact status of the model is fictional or factual that matters, rather it is the stories that are told with the model in answering those questions. Working backward from these analyses, it appears crucial for economists that models have this flexible status, sometimes as fictions, sometimes as factual, and sometimes as mixtures. This flexibility enables economists to use them for model experiments to explore their theories, and so to think about what would happen if the world was like their models, but also to think about what might be learnt about the real world when facts and statistics can be keyed into their models.

Economic models are human-made, not natural objects like the model organisms of biology (such as the fruit fly), which is the primary reason why the terminology of models as artefacts offers both a broader umbrella, and a more relevant, term. It also fits with the idea that—for the scientists—the primary function of models is to work as a technology of scientific enquiry, a function which is entirely consistent with this artefactual status and is less obviously a function of fictions. Nevertheless, fictional qualities are involved, for imagination is critical in this cognitive use of models, both in creating and using them, just as in science fiction. At this point, I return to my question about the difficulty of drawing distinctions between science fiction and economists' imaginative accounts of the world using scientific models. In the science fiction galaxy of the *Hitchhiker's Guide*, the planet Magrathea had built up an industry of making custom-made planets for rich men in the galaxy. When it had gathered all the wealth to itself, leaving the rest of the galaxy poor, miserable, and unfairly depressed, it had put itself into hibernation, awaiting a return to bursting economic activity and wealth accumulation again, sometime in the future. The ability to forget the economic disasters of the past was a feature of that world of science fiction, just as of our own world. For later explorers, Ford and his friends, the planet and its history existed only as legend, only as fiction. As the entry in the *Guide* tells them: 'In these enlightened days of course, no one believes a word of it. . . . Magrathea is a myth, a fairy story, it's what parents tell their kids about at night if they want them to grow up to become economists' (87–8). Whereas Douglas Adams's science fiction seems to assume that the 'dismal science' (as economics is often labelled) will naturally tell dismal stories, the inference that I draw from the comparisons of this paper is that what economists excel at is telling stories about the imagined worlds in their models, accounts that sometimes capture salient experiences of real life even when fictional elements are involved.

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REFERENCES

Adams, Douglas (1986/92), *The Hitchhiker's Guide to the Galaxy* (2nd edn, London, Pan Books).

Blyth, C.A. (1975), 'A.W.H. Phillips, M.B.E.: 1914–1975', *Economic Record*, 51: 303–7. http://dx.doi.org/10.1111/j.1475-4932.1975.tb00255.x

Bokulich, A. (2012), 'Distinguishing Explanatory from Nonexplanatory Fictions', *Philosophy of Science*, 79: 725–37. http://dx.doi.org/10.1086/667991

Boulding, K.J. (1948), *Economic Analysis* (New York, Harper).

Boumans, M.J. (1999), 'Built-In Justification', in Morgan & Morrison, *Models as Mediators*, (Cambridge, Cambridge University Press), 66–96. http://dx.doi.org/10.1017/CBO9780511660108.005

Chamberlin, E.H. (1948), 'An Experimental Imperfect Market', *Journal of Political Economy*, 56: 95–108. http://dx.doi.org/10.1086/256654

Clapham, J.H. (1922), 'Of Empty Economic Boxes', *Economic Journal*, 32: 305–14. http://dx.doi.org/10.2307/2222943

Defoe, D. (2003 [1719]), *Robinson Crusoe* (London: Penguin).

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

Erickson, C. (1966), 'Review of *Railroads and American Economic Growth* by Robert William Fogel', *Economica*, ns 33: 106–9.

Fishlow, A (1965), *American Railroads and the Transformation of the Ante-Bellum Economy* (Cambridge, MA, Harvard University Press).

Fogel, R.W. (1964), *Railroads and American Economic Growth: Essays in Econometric History* (Baltimore, Johns Hopkins University Press).

Friedman, M. (1953), 'The Methodology of Positive Economics', in *Essays in Positive Economics* (Chicago, University of Chicago Press), 3–46.

Frigg, R. (2010), 'Models and Fiction', *Synthese*, 172: 251–68. http://dx.doi.org/10.1007/s11229-009-9505-0

Frigg, R. & Hunter, M.C. (eds) (2010), *Beyond Mimesis and Convention: Representation in Art and Science* (Dordrecht, Springer).

Goffman, E. (1986 [1974]), *Frame Analysis* (Northeastern University Press).

Hawthorn, G. (1991), *Plausible Worlds* (Cambridge, Cambridge University Press). http://dx.doi.org/10.1017/CBO9780511621222

Hausman, D. (1994), 'Paul Samuelson as Dr. Frankenstein: When idealizations escape and run amuck,' in B. Hamminga & N. de Marchi (eds), *Idealization in Economics.* (Amsterdam, Rodopi), 229–43.

Hicks, J.R. (1937), 'Mr Keynes and the "Classics": A suggested interpretation', *Econometrica*, 5: 147–59. http://dx.doi.org/10.2307/1907242

Humphrey, T.M. (1992), 'Marshallian Cross Diagrams and Their Uses before Alfred Marshall: The origins of supply and demand geometry', *Economic Review*, 78: 3–23.

Jenks, L. (1944), 'Railroads as an Economic Force in American Development', *Journal of Economic History*, 4: 1–20.

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

Kirkland, E.C. (1967), '*Review* of *Railroads and American Economic Growth: Essays in Econometric History* by Robert William Fogel', *American Historical Review*, 72: 1493–5. http://dx.doi.org/10.2307/1847978

Klein, J.L. (2001), 'Reflections from the Age of Measurement', in J.L. Klein & M.S. Morgan (eds), *The Age of Economic Measurement, History of Political Economy, Annual Supplement to Vol 33* (Durham, Duke University Press), 111–36.

Knuuttila, T. (2011), 'Modelling and Representing: An artefactual approach to model-based representation', *Studies in History and Philosophy of Science*, 42: 262–71. http://dx.doi.org/10.1016/j.shpsa.2010.11.034

Leeson, R. (2000), 'A.W.H. Phillips: An extraordinary life', in R. Leeson (ed), *A.W.H. Phillips: Collected Works in Contemporary Perspective* (Cambridge, Cambridge University Press), 3–17. http://dx.doi.org/10.1017/CBO9780511521980.003

Lenoir, M. (1913), *Etudes sur la Formation et le Mouvement des Prix* (Paris, M. Giard et E. Brière).

McAfee, R. Preston (1983), 'American Economic Growth and the Voyage of Columbus', *American Economic Review*, 73: 735–40.

Marshall, A. (1930 [1890]), *Principles of Economics* (8th edn, London, Macmillan).

Morgan, M.S. (1990), *The History of Econometric Ideas* (Cambridge, Cambridge University Press). http://dx.doi.org/10.1017/CBO9780511522109

Morgan, M.S. (1997), 'The Technology of Analogical Models: Irving Fisher's monetary worlds', *Philosophy of Science*, 64: S304–S314. http://dx.doi.org/10.1086/392609

Morgan, M.S. (2012), *The World in the Model: How Economists Work and Think* (Cambridge, Cambridge University Press). http://dx.doi.org/10.1017/CBO9781139026185

Morgan, M.S. & Boumans, M.J. (2004), 'Secrets Hidden by Two-Dimensionality: The economy as an hydraulic machine' in S. De Chadarevian & N. Hopwood (eds), *Models: The Third Dimension of Science* (Stanford, Stanford University Press), 369–401.

Morgan, M.S. & Morrison, M. (eds) (1999), *Models as Mediators: Perspectives on Natural and Social Science* (Cambridge, Cambridge University Press). http://dx.doi.org/10.1017/CBO9780511660108

Morrison, M. & Morgan, M.S. (1999), 'Models as Mediating Instruments' in Morgan & Morrison, *Models as Mediators*, 10–37.

Newlyn, W.T. (1950), 'The Phillips/Newlyn Hydraulic Model', *Yorkshire Bulletin of Economic and Social Research*, 2: 111–27. http://dx.doi.org/10.1111/j.1467-8586.1950.tb00370.x

Newlyn, W.T. (1971), *Theory of Money* (2nd edn, Oxford, Clarendon Press).

Phillips, A.W. (Bill) (1950), 'Mechanical Models in Economic Dynamics', *Economica*, 17: 282–305. http://dx.doi.org/10.2307/2549721

Pigou, A.C. (1922), 'Empty Economic Boxes: A reply', *Economic Journal*, 32: 458–65. http://dx.doi.org/10.2307/2223427

Piketty, T. (2013), *Capital in the Twenty-First Century* (Cambridge, MA, Belknap Press).

Prest, A., Stewart, I.G. [& Lardner, G.] (1953), *The National Income of Nigeria, 1950–51*, Colonial Office: Colonial Research Study, no. 11 (London, HMSO).

Redlich, F. (1965), '"New" and Traditional Approaches to Economic History and Their Interdependence', *Journal of Economic History*, 25: 480–95.

Rubin, J. (1967), '*Review* of *Railroads and American Economic Growth: Essays in Econometric History* by Robert William Fogel', *Technology and Culture*, 8: 228–34. http://dx.doi.org/10.2307/3101979

Samuelson, P.A. (1939), 'Interactions Between the Multiplier Analysis and the Principle of Acceleration', *Review of Economics and Statistics*, 21: 75–8. http://dx.doi.org/10.2307/1927758

Smith, V.L. (1962), 'An Experimental Study of Competitive Market Behaviour' *Journal of Political Economy*, 60: 111–37. http://dx.doi.org/10.1086/258609

Suárez, M. (ed) (2009), *Fictions in Science: Philosophical Essays in Modeling and Idealization* (London, Routledge).

Suárez, M. (2010), 'Fictions, Inference, and Realism', in J. Woods (ed.), *Fictions and Models* (Munich, Philosophia Verlag), 225–45.

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

Toon, A. (2010), 'The Ontology of Theoretical Modelling: Models as make-believe', *Synthese*, 172: 301–15. http://dx.doi.org/10.1007/s11229-009-9508-x

Toon, A. (2012), *Models as Make-Believe: Imagination, Fiction and Scientific Representation* (Basingstoke, Palgrave-Macmillan).

Walton, K. (1990), *Mimesis as Make-Believe* (Cambridge, MA, Harvard University Press).

Walton, K. (1993), 'Metaphor and Prop Oriented Make-Believe', *European Journal of Philosophy*, 1: 39–56. http://dx.doi.org/10.1111/j.1468-0378.1993.tb00023.x

Williamson, G.G. (1965), 'Review of *Railroads and American Economic Growth: Essays in Economic* [*sic*] *History* by Robert William Fogel', *Economic Development and Cultural Change*, 14: 109–13. http://dx.doi.org/10.1086/450147

Woods, J. (2010), *Fictions and Models: New Essays* (Munich, Philosophia Verlag).

Wright, P.G. (1929), 'Review of *Statistical Laws of Demand and Supply* by Henry Schultz', *Journal of the American Statistical Association*, 24: 207–15. http://dx.doi.org/10.2307/2276697

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