Models and Modelling in Economics

Mary S. Morgan and Tarja Knuuttila

<u>**Contents**</u>

1. Introduction

Interest in modelling as a specific philosophical theme is both old and new. In the nineteenth century the word model usually referred to concrete objects, oftentimes to the so-called mechanical models, that were built in an effort to grasp the functioning of unobserved theoretical entities (e.g. Bolzmann, 1911). Since then, the kinds of things called models in science have multiplied: they can be physical three-dimensional things, diagrams, mathematical equations, computer programs, organisms and even laboratory populations. This heterogeneity of models in science is matched in the widely different philosophical accounts of them. Indeed, discussion of models in the philosophy of science testifies to a variety of theoretical, formal, and practical aspirations that appear to have different and even conflicting aims (e.g. Bailer-Jones 1999). In addition to approaches concerned with the pragmatic and cognitive role of models in the scientific enterprise, attempts have been made to establish, within a formal framework, what scientific models are. The syntactic view of models, once the "received view", and the semantic approach to models, the prevailing model-theoretic approach until recently, were both attempts of this kind. Yet the discussion of models was originally motivated by practice-oriented considerations, guided by an interest in scientific reasoning. This is perhaps one reason why the general philosophy of science has tended to downplay models relative to theories, conceiving them merely as - for example - heuristic tools, interpretations of theories, or means of prediction. Recently, however, this situation has changed as models have come to occupy an ever more central epistemological role in the present practice of many different sciences.

Models and modelling became the predominant epistemic genre in economic science only in the latter part of the twentieth century. The term "model" appeared in economics during the 1930s, introduced by the early econometricians, even though objects we would now call models were developed and used before then, for example, Marshall's (1890) supply-demand scissor diagrams (see Morgan, forthcoming). Yet, it was only after the 1950s that modelling became a widely recognised way of doing economic science, both for statistical and empirical work in econometrics, for theory building using mathematics, and in policy advice. Indeed, it became conventional then to think of models in modern economics as either mathematical objects or statistical objects thus dividing the economics community for the last half-century into those who were mainly practising econometric (statistically based) modelling and those who engaged in mathematical modelling. This community division is reflected in parallel bodies of commentary by philosophers of economics, analysing mathematical models in relation to economic theories and econometric models in relation to statistical theories and statistical data. Consequently, these have usually been viewed as different sorts of models, with different characteristics, different roles, and requiring different philosophical analysis.

This account deals with both so-called theoretical and empirical models of economics without assuming any principled difference between the two and in contrast to the general philosophy of science which has typically concentrated on mathematical modelling. We cover various perspectives on the philosophical status and different roles of models in economics and discuss how these approaches fit into the modern science of economics. Section 2 spells out some main accounts on the kind of entities economic models are thought to be, although, in order to categorise them in a general way, it is inevitable that the original accounts given by the different philosophers and economists presented below are certainly more subtle and versatile than our classification suggests. Section 3 in turn focuses on how models are used in economics. Since the status and function of models are not separable issues, there is some overlap between the two sections: the various accounts of the nature of models imply more often than not specific views on how models are supposed to be constructed, used and justified in scientific practice.

2 Nature of Economic Models

Modern economics does not differ from the other sciences, such as physics and biology, in its dependency on modelling, yet it lies in an interesting way between the natural and the social sciences in terms of its methods and the variety of models it utilizes. Core micro-economic theory has been axiomatized and economists use sophisticated mathematical methods in modelling economic phenomena. Macro-economics relies in turn more on purpose-built models, often devised for policy advice. And a range of empirical and statistical models operate across the board in econometrics. Although the various model-based strategies of economics seem much like that of those of the natural sciences, at the same time economics shares an hermeneutic character with other social sciences. Economics is in part based on everyday concepts, and as economic agents ourselves we have a more or less good pre-understanding of various economic phenomena. Moreover, individuals' knowledge of economics feeds back into their economic behaviour, and that of economic scientists feeds in turn into economic policy advice, giving economics a reflexive character quite unlike the natural sciences. Recent literature has focussed on the various different kinds of "performativity" this creates for economics, particularly in the context of financial models (see MacKenzie, 2006), but the interactions between economic science and the economy have long been discussed amongst historians of economics and, indeed, economists themselves.

This very complexity of economic science has, without doubt, contributed to the fact that the status and role of economic models – being always apparently simpler than the economic behaviour that economists seek to understand - have been a constant concern for both philosophers and economists alike. In this situation, two major positions have been taken regarding the epistemic status of economic models. Firstly, economic models have been conceived of as idealized entities. From this perspective economists are seen to make use of stylized, simplifying, and even distorting assumptions as regards the real economies in their modelling activities. Secondly, it has been suggested that models in economics are various kinds of purpose built constructions: some are considered to have representational status, others are considered as purely fictional or artificial entities. Seeing models as constructions has been also been related to a functional account of models as autonomous objects that mediate between the theory and data, a perspective which conveniently brings together mathematical and econometric models.

2.1. Models as idealizations

In the general philosophy of science, models and idealization are topics that tend to go together. The term 'idealization' is generically used, but it is very difficult to find a single or shared definition. A variety of usages of the term in economics appear in the rich collection of essays in Bert Hamminga and Neil De Marchi (1994), including their introduction, in which models are said, variously, to be the result of processes of generalizing, simplifying, abstracting, and isolating, following technical, substantive and conceptual aims or requirements (see also Morgan 1996, 2006, Mäki 1992, 1994). These processes can also be portrayed as working from general to more specific target systems (e.g. moving from a full equilibrium account down to the events in a single particular market); or as ones that start with the complicated world with the aim of simplifying it and isolating a small part of it for model representation; or, as in the general analysis of the Poznań approach, where "idealization" is complemented with a reverse process of "concretization" (Nowak, 1994). (This latter approach began to analyse idealization and modelling in the 1970s, but for some time was unrecognised by the mainstream of philosophy of science.) Three commentators particularly associated with questions of idealization in economic modelling, Nancy Cartwright, Daniel Hausman and Uskali Mäki, all draw on an old and venerable discussion going back in economics to John Stuart Mill (1843) whose account of how scientific theorizing could go ahead in economics relied on developing simple models in order to develop a deductive analysis (although of course he did not use the term model).

However, because of the disturbing factors that always attended economic analysis in application to the world, he believed that economic laws could only be formulated and understood as tendency laws.

2.1.1 Idealization

The basic idea that philosophers of economics have derived from Mill is to conceive of models as abstracting causally relevant capacities or factors of the real world for the purpose of working out deductively what effects those few isolated capacities or factors have in particular model (i.e. controlled) environments. However, the ways they have adapted the Millian ideas has varied.

Cartwright focusses on causal capacities that actually work in the world, associating the aim of discovering them as being evident in and applicable to both mathematical and econometric modelling (1989, 1994). According to her, despite the messiness of the economic world, there are sometimes found invariant associations between events. In these associations, causal capacities work together in particular configurations she calls "nomological machines" (e.g. Cartwright, 1999, ch 3 and 6). Mathematical models in economics are constructed as blueprints for those nomological machines, and may serve - in particular circumstances where those machines can be thought to operate without interference from the many other factors in the economy - to enable the scientist to study the way those capacities operate in the real world. Nevertheless, the conditions under which models can be used in econometrics to study such capacities are, she claims, particularly demanding and difficult. In contrast, Hoover (2002) and Boumans (2003) in reply, are more optimistic, arguing that econometric models can be used to help discover regularities, and invariant relations, of the economy even though economists do not know, *a priori*, the machines or the blueprints. So, models are rather to be thought of as working diagrams for the analysis of causal relations, rather than blueprints of already known machines. Indeed, Hoover discusses the difficult task of finding causal relationships in economics precisely in terms of "the mapping between theoretical and econometric models" (this volume, p 6 in type-script).

Hausman (1990) discusses the process of figuring out the causal factors at work by the use of *ceteris paribus* clauses in theoretical models in a way that appears close to the Marshallian comparative static approach of a century earlier. For example, by an analysis of causal factors in the supply and demand diagram, he shows how economists argue using theoretical models by selecting additional factors from the *ceteris paribus* pound in order to explain, in casual rather than econometric terms, the simple observations of everyday economic life (such as "Why is the price of coffee high just now?"). Although Hausman's analysis does not go beyond casual application (see below), we can understand Boumans's (2005) dissection of the various kinds of ceteris paribus clauses that have to be fully labelled and accounted for in models as being relevant here. For Boumans, working with econometric models requires not just a commitment to decide which factors can be considered absent (*ceteris absentibus*), but to those which can be legitimately ignored because of their small effect (*ceteris neglictis*) as well as to those that are present but remain largely unchanged (*ceteris paribus*). This extends and partly replaces an earlier typology of Musgrave (1981) for economic theory models, and draws on a comparison of such clauses in the use of simulations and laboratory experiments with economic models in economics (see Boumans and Morgan, 2001, see also Mäki, 2000, and Hindriks, 2006, for further developments of Musgrave, 1981).

Mäki's account, which builds on Nowak as well as on Mill, is, like Hausman's *ceteris paribus* discussion, dependent on "sealing off" the relations of interest from other influences. For Mäki a theoretical model is an outcome of the method of isolation, which he analyses as an operation in which a set of elements is theoretically removed from the influence of other elements in a given situation through the use of various kinds of often unrealistic assumptions (Mäki, 1992 and 1994). Thus in positing *unrealistic* assumptions economists need not adopt an *anti-realist* attitude towards the economic theory. Quite the contrary, unrealistic assumptions can even be the very means of striving for the truth, which Mäki puts as boldly as stating that "an isolating theory or statement is true if it correctly represents the isolated essence of the object" (1992, 344, see also Mäki, forthcoming a).

The authors mentioned above - Cartwright, Mäki, and to a more limited extent, Hausman – can be interpreted as proponents of a distinct strategy of idealization, one that we might refer to as one of isolation in the sense that the point is to capture only those core causal factors, capacities or the essentials of a causal mechanisms that bring about a certain target phenomenon. Weisberg (2007) suggests we characterise such models as products of “minimalist idealization” since they contain “only those factors that *make a difference* to the occurrence and essential character of the phenomenon in question” (p. 642, italics of the original). This very Millian characterisation immediately raises a number of problems that arise in trying to separate out what those causal factors are. A convenient way - even an idealized case - to demonstrate these difficulties is to invoke the Galilean experiment (McMullin 1985) as discussed by Cartwright (1999, 2006). The aim of the Galilean experiment is to eliminate *all* other possible causes in order to establish the effect of one cause operating on its own (1999, p.11). From this analysis, Cartwright (in her more recent writings) has come to doubt whether the idea of looking at how one factor behaves in isolation works for economics remembering that her interest is in locating causal capacities in the world, while others, such as Boumans (2003 and 2005), invoke the same ideal case to pose the question in terms of how to design econometric models which have sufficient statistical control features to locate invariant and autonomous relations in the data, while still others, like Mäki (2005), understand the issue in terms of how modellers use theoretical assumptions to seal off the effect of other factors. All these authors, explicitly or implicitly, appeal to the physical controls of laboratory experiments as a way to motivate their account of how models may be built to isolate elements of economic behaviour.

Terminology is important. The notion of ‘idealization’ does include more than a process to isolate causal factors, and no two commentators use the term in the same way. Mäki uses the term “isolation” as his central concept, under which he subsumes other related notions frequently dealt with in the discussions on modeling. Thus he treats for example, “abstraction” as a subspecies that isolates the universal from particular exemplifications; idealizations and omissions, in turn, are techniques for generating isolations: idealizations being deliberate falsehoods, which either understate or exaggerate to the absolute extremes. For Cartwright, in contrast, “idealization” and “abstraction” are the basic terms and categories involving two different operations. For her, too, idealization involves distortion, by which she means *changing* some particular features of the concrete object so that it becomes easier to think about and thus more tractable to model (Cartwright 1989). Abstraction in turn is a kind of omission, that of *subtracting* relevant features of the object and thus when it comes to abstraction it makes no sense to talk about the departure of the assumption from truth, a question that typically arises in the context of idealization (see Cartwright, 1989 ch. 5, Jones and Cartwright, 2005). But these views by no means exhaust the ways in which idealization is understood with respect to economic models. One interesting set of notions (found amongst the many others in Hamminga and De Marchi's 1994 collection), is Walliser's analysis of idealization as three different kinds of processes of generalisation: *extending* the domain of application (so as to transfer the model to other domains); *weakening* some of the assumptions to extend the set of applications; and *rooting*, providing stronger reasons for the model assumptions. (These generalising projects might also be interpreted as de-idealizing processes - see immediately below.) For Hausman, the label is less important than the variety of things that it covers, though in his 1992 account of economic theorizing using models, we find an emphasis on the conceptual work that modelling plays and see this too in his account of the overlapping generations model, where idealization works through falsehoods and generalisations as much as through omissions and isolations. It is not difficult to find examples of such concept-related idealizations in economics, where assumptions such as perfect knowledge, zero transaction costs, full employment, perfectly divisible goods, and infinitely elastic demand curves are commonly made and taken by economists not as distortions, but as providing conceptual content in theoretical models, a point to which we return in section 3.3.2 below.

2.1.2 De-Idealization

We have seen above that the term 'idealization' covers different strategies and, consequently, of ways of justifying them. One influential defence of idealization is the idea of de-idealization, according to which the advancement of science will correct the distortions effected by idealizations and add back the discarded elements, thus making the theoretical representations become more usefully concrete or particular. A classic formulation of this position was provided by Tjalling Koopmans who thought of models only as intermediary versions of theories which enabled the economist to reason his way through the relations between complicated sets of postulates. In the process of this discussion, in a much quoted comment, he portrayed "economic theory as a sequence of models":

> "Considerations of this order suggest that we look upon economic theory as a sequence of conceptional *models* that seek to express in simplified form different aspects of an always more complicated reality. At first these aspects are formalized as much as feasible in isolation, then in combinations of increasing realism." (Koopmans, 1957, p 142)

Nowak also thought that science should eventually remove the "counter-actual" idealizations in a process of "concretization" (Nowak 1992). But although economics may experience a process like this in locally temporal sequences of econometric and mathematical modelling (see, for example, the case discussed by Hindriks, 2005), it is difficult to characterise the more radical and noticeable changes in models as moves towards greater "realism" (to use Koopmans' term).

It is also possible to see the move to greater realism as a process of reversing idealizations. Considering such a project in economics gives us considerable insight into idealization and, indirectly, points to difficulties not just in Koopman's justification for idealization, but also in the other arguments made (above) about its usefulness. The potential processes of de-idealization, then, reveal a number of interesting and important points about the strategies of idealization.

First, idealization frequently involves particular kinds of *kinds of distortions* that often are motivated by tractability considerations, such as setting parameters or other factors in the model to a particular value, including extreme ones (such as zero or infinity). When such a model is de-idealized the importance of these assumptions to the model will become evident, though the particular problems they cause in the model are not likely to follow any standard pattern or share any obvious solution. So for example, Hausman's account of Samuelson's "overlapping generations model" refers to a paper which has been "carried away by fictions" (1992, p. 102). By carefully unpacking Samuelson's various model assumptions - that is by informally attempting to de-idealize the model and by analysing the immediate critiques that offered similar analyses - Hausman shows how critical some of these idealizations are to the results of the model. He points out, for example, that: "The appeal of the overlapping-generations framework is that it provides a relatively tractable way to address the effects of the future on the present. It enables one to study an economy that is in competitive equilibrium with heterogeneous individuals who are changing over time. Yet the heterogeneity results from the effects of aging on an underlying homogeneity of tastes and ability." Hausman's deconstruction of the assumptions explores why some questions get left aside during the paper, and why such a well-used model nevertheless rests on some quite strange idealizing foundations.

Second, the justification for an idealization can be directly related also to the needs of computability. The economist achieves computationally tractable models in two ways. One kind is by the use of a particular twist or piece of *mathematical moulding* that will fit the pieces of the model together in such a way as to allow deductions with the model to go through (see Boumans, 1999). Once again, it is difficult to foresee in any general way what will happen when that twist is unravelled. While advances in mathematical techniques and computational power may change aspects of this problem, it seems unlikely to remove it altogether. Moreover, moving from a model which is analytical in mathematical terms to one that is tractable as a simulation does not in itself solve the problem, since each mode of using models requires a different idealization to make the model tractable. A related pragmatic move is found in idealizations that allow derivations to be made: it is often difficult to make sense of the very idea of relaxing those assumptions that are mainly aimed at facilitating the derivation of the results from the model. As Alexandrova (2006) asks of such assumptions:

> "In what sense is it more realistic for agents to have discretely as opposed to continuously distributed valuations? It is controversial enough to say that people form their beliefs about the value of the painting or the profit potential of an oil well by drawing a variable from a probability distribution. So the further question about whether this distribution is continuous or not is not a question that seems to make sense when asked about human bidders and their beliefs" (2006, 183).

As she argues, one simply does not know how statements concerning such "*derivation facilitators*" should be translated back into statements about the real entities and properties.

Third, taking Boumans' 2005 analysis of the *various ceteris paribus assumptions* seriously suggests that the difference between factors that can legitimately be assumed absent, those that are present but negligible, and those that are present, but within a range constant, may be critical in any de-idealization even before moving to an econometric model, yet economic modellers tend to lump these all into one bundle in the process of idealization.

Fourth, is the vexed question of de-idealizing with respect to the *causal structure*. If it really is the case that there are only a very few or one strong causal factor and the rest are negligible then the minimalistic strategy suggests that adding more detail to the models may in fact render the model worse from the epistemic point of view. It makes the explanatory models more complicated and diverts attention from the more relevant causal factors to the less relevant (see Strevens forthcoming). More likely however, there are many causal factors operating, some of which have been idealized away for theoretical purposes, while simpler relations may have been assumed for the causal interactions. Yet, in econometric work, it is often found that the causes are not separable and so they should not have been be treated as independent of other previously included and omitted factors. De-idealization thus recreates a great deal of causal complexity in the model that may have been mistakenly assumed away in making the theoretical model . So, as soon as de-idealization begins – this notion of being able to study individual causal factors in isolation begins to crumble. All these problems may not appear so acute during a process of theorizing, but become immediately apparent for those concerned with models applied to the world, where far ranging idealizations about causal structures are likely to be invalid starting points in the attempts to map from economic to econometric models. The problem of unravelling causal claims in economic models has been the subject of much debate within economics in a literature that is well integrated into the general philosophical debates on causality (see Heckman, 2000, on micro-economics models; Hoover, 2001 on macro-economic models and more geneally,Hoover, 2008 and this volume, and Cartwright 2006).

Fifth, the *different levels of idealization* within a model may not be compatible with each other and this may become particularly evident if and when de-idealizations are made. Hoover (2008a) unpicks the idealizations of recent macroeconomic models to show how the reductionist idealizations embedded in their micro-foundations are not only individually problematic as separate idealizations (see Kirman, 1992), but problematic in that the various idealizations are either incompatible, or make seemingly contradictory assumptions in the model about the nature of the individuals with the aggregates.

Sixth, some idealisations in models are associated with concept formation. It is not at all clear what it means to de-idealize a concept within a mathematical model, though econometricians face this problem on a daily basis in their modelling (see below, section 2.1.3).

Lastly, of course, these *different kinds of idealizations are not independent* in the model, so that the effects of de-idealization are manifestly very difficult to predict. The assumptions needed to make the model mathematically tractable often threaten the very idea that causes can be isolated, since they often make the results derived from a model dependent on the model as a whole. And, if it is unclear which model assumptions "do the work", it is difficult to see how the model can isolate the behaviour of any specific causal factor or tendency and how the various other assumptions can be reversed satisfactorily. Consequently, de-idealization does not succeed in separating out what is negligible and thus irrelevant and what is not. All these problems must be acute in minimalist models because they are typically relatively thin and simple in order to isolate only a few causes, and must be constructed with the help of clearly purpose-built assumptions in order to provide a way to secure deductively certain results. As Cartwright (1999) has argued, the model economy has to be attributed very special characteristics so as to allow such mathematical representation that, given some minimal economic principles such as utility maximization, one can derive deductive consequences from it. Yet at the same time the model results are tied to the specific circumstances given in the model that has been created, making all the assumptions seem relevant for the results derived.

These difficulties all tend to water down the idea that as economic investigations proceed, one could achieve more realistic models through de-idealization. It also suggests that the notion of models as providing a forum for Galilean experiments sets too strict an ideal for economic modelling. Perhaps it provides a more useful philosophical basis in such a science as physics, where in many cases comprehensive and well-confirmed background theories exist giving the resources with which to estimate the effect of distortions introduced by specific idealizations, and provide guidance on how to attain particular levels of accuracy and precision. The method of modelling in economics should perhaps rather be compared with the use of models in sciences such as meteorology, ecology and population biology, sciences which do not so much lack comprehensive foundations as the relatively well behaved systems and well confirmed background theories that can be connected to specific knowledge of particular cases which allow idealizations and de-idealizations to be informative.

An alternative defence and interpretation of this modelling activity has been claimed in what several analysts, following Richard Levins (1966), have called "robustness analysis" (Wimsatt, 1987). Robustness can be characterized as stability in a result that has been determined by various independent scientific methodologies, for instance through observation, experiment, and mathematical derivation. Applied just to modelling, where it has been taken to mean the search for predictions common to several independent models (Weisberg, 2006), the notion must however have a weaker epistemological power. Worse, in economics, such robustness claims are based on analysis carried out on models that are far from independent, usually being variations of a common "ancestor" and differing from each other only with respect to a couple of assumptions. While it is possible to claim that by constructing many slightly different models economists are in fact testing whether it is the common core mechanism of the group of models in question that is responsible for the result derived and not some auxiliary assumptions used (Kuorikoski, Lehtinen and Marchionni, 2007), this may not help in validating the model as stable and robust beyond the mathematical laboratory. In contrast, in the statistical laboratory of econometrics, robustness in model performance has been understood not in terms of core mechanisms, but as a relative quality of models in relation to data sets judged according to a set of statistical criteria applied within a modelling process (see Spanos, this volume), though there are cases where such tests have been carried out on related families of econometric models (see eg Wallis, 1984).

2.1.3 The Idealization vs. De-idealization Debate

While the language of idealization and de-idealization is not so familiar in the philosophy of econometric models (with notable exceptions, for example, Hoover, 1994), these processes are endemic in the practises of econometrics at both grand and everyday levels. At a meta-level, though it has not been couched in these terms, the argument about the process of modelling in econometrics is exactly one as to whether it should proceed by processes of idealization or by ones of de-idealization. At a more everyday level however, we find that practical modelling in econometrics involves many processes of idealization and de-idealization at the same time.

At the practical level then, making and testing the validity of idealization decisions in econometrics covers a similar range of economic questions as those for mathematical models: Which variables should be included and omitted? What are the key causal relations between them? What simplifying assumptions can be made? What *ceteris paribus* clauses are involved? What tractability assumptions need to be made? What is the nature of their statistical and mathematical form? And so forth. But econometric modelling also includes making, and testing, idealizing assumptions about the nature of the economic data: about the probability distributions assumed, the nature of errors, the stochastic behaviours found in particular kinds of data, and so on.

However, in a significant difference with mathematical modelling, econometric modelling additionally involves a whole lot of de-idealizing decisions that are required to bring the requirements of the theory into some kind of coherence with the available data. Thus, for example, economic theory models rarely specify very clearly the details of time relations or the particular form of entities or relationships involved, and all these details have to be filled in the model. And from the data side, decisions must be made about which data set most closely resembles the economic entity being modelled, and so forth. This last activity reveals indeed how very deeply abstract and concept-ridden economists' economic terms are, even when they share the same name with every-day economic terms. Every modelling decision in econometrics involves a dilemma of how to measure the terms that economists use in their theories. Sometimes these measures are termed "proxies" because the theoretical term wanted is not one that is measured; other times it is a choice of what data best matches the conceptualised, abstract, terms of economists' models. Sometimes the model itself is used to derive the measurements needed within the model (see Boumans 2005, and this volume, on the role of models in obtaining economic measurements). Modelling is carried out for many purposes in econometrics: to test theories, to measure relations, to explain events, to predict outcomes, to analyse policy choices, etc, each needing different statistical and economic resources and invoking different criteria in the modelling processes. All this activity means that econometric modelling - involving processes of both idealization and de-idealization - is very much an applied science: each model has to be crafted from particular materials for particular purposes, and such skills are learned through apprenticeship and experience as much as by book learning (see Colander, 2008 and Magnus and Morgan, 1997).

At the meta-level, the argument over modelling is concerned with the relative role of theory and data in model making and goes on at both an abstract and specific level. Econometricians are more deeply engaged in thinking through the philosophical aspects of their modelling strategy compared to their mathematical modelling colleagues. These discussions indeed go back to the foundations of modelling in econometrics during the 1930s and 1940s. Thus, the infamous "measurement without theory debate" over the role of theory - both economic and statistical - in the making and using of econometric models, lead, in the post 1950s period, to an economics in which it was thought economists should provide mathematically expressed theoretical models while the econometrician should use statistics for model estimation and theory testing. Yet, in spite of this rhetoric, it is not possible simply to "confront theory with data", or "apply theory to data", for all the prosaic reasons mentioned above: economic theory does not provide all the resources needed to make econometric models that can be used for measurement or testing, or as Hoover so aptly puts it: "theories are rarely rich enough to do justice to the complexities of the data" (2000, p 221). This is why those who developed econometrics introduced and developed the notion of model in the first place - namely as a necessary object in which the matching between theory and data could be accomplished. Whether, in this "new practice" of models, as Boumans (2005) terms it, the notion of model was rather straightforward (as in Frisch and Tinbergen's work) or philosophically sophisticated (as in Haavelmo's work, below), models were conceived as a critical element in the scientific claims of economics (see Morgan, 1990).

Yet, despite these debates, there are no general agreed scientific rules for modelling, and there continue to be fierce arguments within the econometrics community over the principles for modelling and the associated criteria for satisfactory modelling (particularly given the variety of purposes to which such modelling is addressed). For the past two decades or so, the major question is no longer understood simply as to whether models should be theory driven or data driven; but as to whether the modelling process should be "*general to specific*" or "*simple to general*", and given this, the relative roles of theory and data in these two different paths. (There are other positions and approaches, but we concentrate on just these two here.) That is, should econometric modelling proceed by starting with a most general model which incorporates all the possible influencing factors over the time frame that is then refined into one relevant for the specific case in hand; this is a kind of isolating process where the reducing or simplifying moves are validated by the given data resulting in a model with fewer factors (see Cook and Hendry, 1994). The alternative process starts with an already idealized model from economic theory that is then made more complex – or more general in the above sense – as factors are added back in to fit the data for the case at hand, ie a process of de-idealization. (That is, in this literature, "general" can not be equated to "simple".) However, it is not quite so simple as this because, associated with this main question, go issues of how statistical data are analysed and how statistical testing goes ahead. This current debate therefore can be well understood in terms of idealization and de-idealization, provided we include notions about the statistical aspects of models as well as the economic and mathematical in the resource base for modelling.

The "general-to-specific" school of modelling follows a practise (which is also embedded in computer software, and may even involve automatic model selection mechanisms) of beginning with the most general economic model relevant to the problem to decide which subset of its models are congruent with the data. At the same time, the econometrician conducts an extensive process of data analysis to ascertain the statistical and probability characteristics of the data. The choice of models within the subset is then made based on principles which include "encompassing" criteria: searching for the models which explain at least as much as other models explain and which do so most efficiently with respect to the data. In this process, the model gets leaner, as terms which play no statistical role *and* which have no economic rationale for inclusion are discarded. Thus, both economic elements and statistical criteria go into the modelling process and final choice of specific model. We might describe these joint statistical and economic modelling choices as a combination of different kinds of idealizations in the sense that the modelling seeks to extract - or isolate or discover - by using these processes the model that best characterises the economic behaviour represented in the specific data set.

Both data and theoretical aspects also go into the alternative “simple-to-general” approach, but here, in contrast, the process begins with a commitment to the already idealized mathematical model from theory, and aims to apply that to the data directly. A limited amount of adding back in relevant associated causal variables is carried out to obtain statistical fit. At the same time, the econometrician here makes assumptions about distributions, or fixes the particular statistical difficulties one by one, in processes that might be thought equivalent to the ways in which economic models are made tractable. So, on the economic side, such modelling is a process of de-idealizing, of adding back in previously omitted economic content. But on the statistical side, it looks more like a process of idealization, fixing the model up to the ideal statistical conditions that will validate inferences.

In this interpretation, we can see that when the general-to-specific modellers complain of the likely *in*validity of the inferences based on the statistical idealizations used by the theory-first modellers, they are in effect pointing to the implicit set of difficulties accompanying any de-idealization on the statistical side, which their own approach, because of its prior attention to those statistical issues, claims to minimize. On the other side, the theory-first modellers can be seen as complaining about data driven models and the lack of theoretical economic foundations in their rivals’ approach, referring back (sometimes explicitly) to older philosophy of science arguments about the impossibility of theory-free observations and the dangers of empiricism. The arguments are complex and technical, but, as with those on causal modelling, well tuned into more general arguments in the philosophies of science and statistics (for recent discussions of the debate, see Chao, 2007 and Spanos, this volume; and for a less technical discussion, see Colander, 2008 and Spanos, 2008).

2.2 Models as constructions

As an alternative to the idea that models idealize, isolate or abstract some causal factors, mechanisms or tendencies of actual economies it has been suggested that economic models are rather like pure constructions or fictional entities that nevertheless license different kinds of inferences. There are several variants of this option, which differ from each other in the extent to which they nevertheless are committed to the representational status of models and how much they pay attention to their actual construction processes. Moreover, the constructedness of models has been associated with a functional account of models as autonomous objects,.rather than by characterizing them in relation to a target systems as either theoretical models or models of data.

2.2.1 Ideal Types and Caricatures

As we have seen idealization involves not just simplifications or omissions, but also distortion and the addition of false elements. When it comes to distortion in the social scientific context, Max Weber (1904) launched the famous idea of ideal types which present certain features in an exaggerated form, not just by accentuating those features left by the omission of others, but as a strategy to present the most ideal form of the type. Weber considers both individual economic behaviour and the market as viable subjects to consider as ideal types, in which a certain kind of pure economic behaviour might be defined. This kind of exaggeration, appears again in Gibbard and Varian's (1978) idea of economic theory modelling being one of creating caricatures, the purpose of which is to allow the economist to investigate a particular caricatured aspect of the model and thus to judge the robustness of the particular assumption that created such exaggeration. This has similarities to the idea of a robustness analysis of core causal features (as above).

Morgan (2006) interprets the caricaturing process as something more than the exaggeration of a particular feature, rather it involves the addition of features, pointing us to the constructed nature of the exaggeration rather than to it as an idealization, abstraction or isolation of causal factors. Take as an illustration, Frank Knight's 1921 assumption that economic man has perfect information: this can not be specified just as a lack of ignorance, for the model has to be fitted out with descriptions of what that means and this may be done in a variety of different positive ways. For example, one way to interpret the assumption of perfect knowledge is that such an economic man has no need of intelligence or power to reason, thus he could be re-interpreted as a mechanical device responding to stimuli, or, as Knight (later) suggested, as a slot-machine. At this point, the caricature is less clearly a representation of economic man as an idealization, isolation or abstraction, but rather his character was constructed as a positive figure of science fiction (see Morgan, 2006).

So, while idealizations can still be understood as representations of the system or man's behaviour (however unrealistic or positively false these might be), the more stylized models get, the less they can be considered as *models of* some specific systems or characters in the economy. As properties are *added* and attributed to the modelled entities and their behaviour, the model starts to look like an intricate, perhaps fictional, construction rather than an idealized representation of some real target system. Taking heed of these problems some economists and philosophers have preferred to approach models as pure constructions rather than as idealizations from real world systems.

2.2.2 Fictions and Artificial Systems

A strong tradition in economics has understood economic models as fictions, able to give us some understanding of real economic mechanisms, even though they are not interpreted as representations of real target systems. This approach has also found adherents amongst philosophers of economics (see Suárez (ed.), 2008).

An early treatment of the role of fictions in economics is given by economist and philosopher Fritz Machlup, who has in his methodological writings considered the nature and role of economic agents in economic theory. He suggests that *homo oeconomicus* should be regarded along Weberian lines as an ideal type (above), by which he means that it is a mental construct, an "artificial device for use in economic theorizing", the name of which should rather be *homunculus oeconomicus,* thus indicating its man-made origins (Machlup, 1978, p. 298). As an ideal type homo oeconomicus is to be distinguished from real types. Thus economic theory should be understood as a heuristic device for tracing the predicted actions of imagined agents to the imagined changes they face in their environment. Machlup treats neoclassical firms likewise: they should not be taken to refer to real enterprises either. According to traditional price theory, a firm - as conceptualized by economists - is only “a theoretical link” that is “designed to explain and predict changes in observed prices […] as effects of particular changes in conditions (wage rates, interest rates, import duties, excise taxes, technology, etc).” (Machlup, 1967, p. 9). To confuse such an heuristic fiction with any real organization (real firms) would be to commit “the fallacy of misplaced concreteness”. The justification for modelling firms in the way neoclassical micro-theory does lies in the purpose for which the theory was constructed. In explaining and predicting price behaviour only minimal assumptions concerning the behaviour of the firm are needed if it is assumed to operate in an industry consisting of a large number of similar such enterprises. In such a situation there is no need to talk about any internal decision-making because a neoclassical firm, like a neoclassical consumer, just reacts to the constraints of the environment according to a pre-established behavioural - in other words, maximizing - principle.

The fictional account of economic modelling contrasts with the realist interpretation of economic modelling, which has been defended especially by Cartwright and Mäki (above). The fictionalists question the realist assumption that economists strive—in their actual practice and not in their *a posteriori* methodological statements—to make models represent the causally relevant factors of the real world and then use deductive reasoning to work out what effects these factors have. Robert Sugden, who is a theoretical economist himself, has claimed that this does not match the theorizing practice of economists. He uses Thomas Schelling’s “checker board model” of racial sorting to launch his critique (2002) against the realist perspective which assumes that although the assumptions in economics are usually very unrealistic, the operations of the isolated factors may (and should) be described correctly. From this, Sugden claims that economic models should rather be regarded as constructions, which, instead of being abstractions from reality, are *parallel realities*.

Schelling (1978) suggests that it is unlikely that most Americans would like to live in strongly racially segregated areas, and that this pattern could be established only because they do not want to live in a district in which the overwhelming majority is of the other skin colour. He develops and uses a “checker board model” to explain this residential segregation. The model consists of an 8 x 8 grid of squares populated by dimes and pennies, with some squares left empty. In the next step, a condition is postulated that determines whether a coin is content with its neighbourhood. Whenever we find a coin that is not content we move it to the nearest empty square, despite the fact that the move might make other coins discontented. This continues until all the coins are content. As a result, strongly segregated distributions of dimes and pennies tend to appear—even if the conditions for contentedness are quite weak.

According to Sugden (2002), it seems rather dubious to assume that a model like the checkerboard model is built by presenting some key features of the real world and sealing them off from the potential influence of other factors at work: "Just what do we have to seal off to make a real city - say Norwich - become a checkerboard?" he asks (p.127). Thus, "the model world is not constructed by starting from the real world and stripping out complicating factors: although the model world is *simpler* than the real world, the one is not a simplification of the other." (p. 131). Rather than considering models as representations he prefers to treat them as constructions, the checkerboard plan being something that "Schelling has *constructed* for himself" (p. 128).

Considering models as constructions is inherent in the fictional account of them. This is hardly surprising since constructedness gives the minimal criterion for what may be regarded as fictional: fictional worlds are constructed, and do not exist apart from having once been represented. Thus fiction contrasts at the outset with reality, which we take to exist quite apart from our representational endeavours. This also shows why it is misleading to associate fiction with falsehood. Fiction deals rather with the possible and the imaginary, with non-actual states in general, which is the reason why the fictional mode is not limited to the literary realm but can be extended to cover scientific accounts, too (see Frigg, forthcoming). Thus while fictionalists can be considered as constructivists at the outset, they usually tend to stress the imaginary characteristics of models whereas other constructivists stress instead the artificiality of model systems that strive to *mimic*, at some level, some stylized features of the real systems. This is evident particularly in the macro-econometric field and often associated with Robert Lucas, who has famously written:

"One of the functions of theoretical economics is to provide fully articulated, artificial economic systems that can serve as laboratories in which policies that would be prohibitively expensive to experiment with in actual economies can be tested out at much lower cost. To serve this function well, it is essential that the artificial "model" economy be distinguished as sharply as possible in discussion from actual economies [...]. A 'theory' is not a collection of assertions about the behaviour of the actual economy but rather an explicit set of instructions for building a parallel or analogue system - a mechanical, imitation economy. A 'good' model, from this point of view, will not be exactly more 'real' than a poor one, but will provide better imitations." (Lucas, 1980, p. 697)

So, whereas Cartwright has models as blueprints for nomological machines that might exist in the world, Lucas has theories as blueprints for building models that might mimic the world. This constructivist move transforms the relation between models and theory, for now the task of the theory is to produce models as analogues of the world, rather than to use them to understand how the world works (see Boumans, 1997, 2006). This move also transforms the sense of how theories or models are supposed to "fit" to the world, namely the notion that such analogue world models can be fitted by calibration to particular data set characteristics rather than by parameter estimation and statistical inferences (see Hoover, 1995). Moreover, it parallels the post 1960s development of different kinds of purpose-built simulation models, which share the same mimicking aims though with a different mode of use, and which, contra Lucas, often claimed to be constructed as representational models - at some specified level - of a target system such as the operating structures of firms, the way people use economic information, or the basic behavioural functions of the macro economy. (see Morgan 2004, and section 3.3.1).

2.2.3 Constructed representations

Many economists think of constructing their models expressly to represent certain, possibly stylized, aspects of economies (rather than getting to them via processes of idealization). Such constructivist accounts of models pay specific attention to the various elements of models as well as to the means of representation and the role of tractability. The problems of tractability suggests that increasing realisticness in some aspects of the representation will have to be traded off against simplification and distortion in other aspects, as Tinbergen recognised right from the start of modelling in economics:

> "In order to be realistic, it [the model] has to assume a great number of elementary equations and variables; in order to be workable it should assume a smaller number of them. It is the task of business cycle theory to pass between this Scilla and Charybdis. If possible at all the solution must be found in such simplifications of the detailed picture as do not invalidate its essential features." (Tinbergen, 1940, p 78.)

From this perspective models feature as intricate constructions designed and assembled to answer specific questions, as in the early use of of business cycle models, where Boumans (1999) has shown how various ingredients can go into a single model: analogies, metaphors, theoretical notions, mathematical concepts, mathematical techniques, stylized facts, empirical data and finally relevant policy views. Striving to combine such diverse elements to one another tells us something interesting about modelling: it hints at the skill, experience, and hard work that are required to make a new model. Here, the image of a scientist as a modeller is very different from that of a theoretical thinker. Boumans, in fact, likens model construction to baking a cake without a recipe (1999, p. 67) That econometric models are constructed from various ingredients including theoretical relations and statistical elements, is, as we have seen already, a reasonable description. But that mathematical economic models are also constructed in a similar manner may be a more surprising claim. Yet these mixtures are equally characteristic in mathematical models as Boumans' study shows, where mathematics plays the critical role of "moulding" these various different ingredients into one model. He argues that "new recipes" are created, then adapted and adopted to new circumstances and questions to form not a sequence of de-idealized or more realistic models as Koopmans suggests, but a sequences of related models rather more like a kinship table (see Hoover, 1991, for an example of such a kinship analysis of models). This account nicely captures the ways in which some models, such as the venerable IS-LM model in macroeconomics, experience an incredibly long life in which they are adapted to represent new theories, used to analyse new problems, and generally re-interpreted (see De Vroey and Hoover, 2006). The history of modelling strongly suggests that such constructed model sequences are as much driven by changes in the purposes of models as by the changes in theories.

This constructivist perspective on models goes against traditional views and philosophizing, even by economists themselves, probably because models have conventionally been approached as theoretical and abstract entities, whose seemingly simple and unified mathematical form disguises their very heterogeneity. Yet, in economists' own writings, we see discussions of how model construction takes place suggesting that it is more an intuitive and speculative activity than one of rule-following in which models are derived from theory via processes of idealization, though this does not mean that some idealizations are not involved (see for example Krugman, 1993, and Sugden, 2002).

From the constructivist perspective, then, models are conceived as especially constructed concrete objects, in other words, as epistemic artefacts that economists make for a variety of different purposes. Knuuttila (2005) argues that, contrary to the philosophical tradition, one should take into account the media and representational means through which scientific models are materialized as concrete, inter-subjectively, available objects. The use of representational media and different modelling methods provide an external scaffolding for the scientist's thinking, which also partly explains the heuristic value of modelling. It is already a cognitive achievement to be able to express any tentative mechanism, structure or phenomenon of interest in terms of some representational media, including assumptions concerning them that are often translated into a conventional mathematical form. While such articulation enables further development, it also imposes its own demands on how a model can be achieved and in doing so requires new kinds of expertise from the scientists. A nice example of this is provided by development of the Edgeworth-Bowley Box models. In discussing its cognitive aspects, Morgan (2004a, and forthcoming) notes how its various representational features were initially a considerable cognitive step whereas today the Egdeworth-Bowley diagram is presented in the introductory courses of economics, but also how some of its early cognitive advantages were lost to later users as the model developed into its current stable form.

This artefactual character of models drawn in any media (including the abstract languages of mathematics) is enhanced by the way models typically also constrain the problem at hand, rendering the initial problem situation more intelligible and workable. So, in this sense, any representational media is double-faced in both enabling and limiting. This is easily seen in a case like the Phillips-Newlyn model, a real machine built to represent the aggregate economy in which red water circulated around the machine to show how the Keynesian economic system worked in hydraulic terms (see Boumans and Morgan, 2004). This material model enabled economists of the time to understand the arguments about stocks and flows in the macroeconomy, and enabled them to think about a wider set of possible functions at work in the economy, while at the same time, the media or representation created very specific limitations on the arrangements of the elements and their relation to each other. Another good example of how a model can both enable and constrain is provided by the IS-LM model, the most famous of several contemporary attempts to model the key assumptions of Keynes's *The General Theory of Employment, Interest and Money* (1936) (see Darity and Young, 1995). This model could be used to demonstrate some of Keynes's most important conclusions, yet at the same time it omitted many important features of his theory leading some economists to distinguish between the economics of Keynes and Keynesian economics (see Backhouse, 2006, Leijonhufvud, 1968).

Consequently, modellers typically proceed by turning these kinds of constraints built into models (due to its specific model assumptions and its medium of expression) into affordances. This is particularly evident in analogical modelling, where the artefactual constraints of both content and model language may hold inflexibly. Whether the model is an analogical one or not, scientists use their models in such a way that they can gain understanding and draw inferences from "manipulating" their models by using its constraints, not just its resources, to their advantage. It is this experimentable dimension of models that accounts for how models have the power, in use, to fulfill so many different epistemic functions as we discuss next and below (see Morgan, 1999, 2002 and forthcoming; Knuuttila and Voutilainen, 2003, and Knuuttila, forthcoming)

2.2.4 Models as Autonomous Objects

From a naturalist philosophy of science viewpoint, the way that economists work with models suggests that they are regarded, and so may be understood, as autonomous working objects. Whereas the approaches mentioned above located the constructedness of models in relation to the assumed real or imaginary target systems, the independent nature of models can fruitfully be considered also from the perspectives of theory and data. Without doubt many models are rather renderings of theories than any target systems and some are considered as proto-theories not having yet the status of theory. On the other hand econometric models have at times been considered as versions of data.

In a more recent account, economic models are understood to be constructed out of elements of both theory and the world (or its data) and thus able to function with a certain degree of independence from both, The divide between theoretical models and econometric models seems misleading here since, from this perspective on model construction, both kinds of models are heterogeneous ensembles of diverse elements (see Boumans above). This account understands models as autonomous objects in a science within the "models as mediators" view of the role of models, which analyses them as means to carry out investigations on both the theoretical and the empirical sides of scientific work, particularly it treats them as instruments of investigation (see Morrison and Morgan, 1999). This power to act as instruments that enables the scientist to learn about the world or about their theories depends not only on their functional independence built in at the construction stage, but on another construction feature, namely models are devices made to represent something in the world, or some part of our theory, or perhaps both at once.   These two features, function independence and representing quality – loosely defined,make it possible to use models as epistemic mediators (see section 3.3 below). Even the artificial world models of Lucas which are constructed as analogues to represent the outputs of the system, not the behaviour of the system, can be understood under this account, though their functions in investigations may be more limited. In this sense the models as mediators view takes also a mediating view in respect to the models as idealizations vs. the models as constructions divide – itself of course partly an idealization made up for expository reasons – since it takes a liberal attitude both as to what models are supposed to represent and also to the mode of their making via idealization and de-idealization or via a process of construction.

3) Working with Models

Looking at models as separately constructed objects pays specific attention to their workable aspects. Indeed, from the perspective of doing economics it is more useful to see that contemporary economics, like biology, uses a variety of different kinds of models for a variety of purposes, and that whether models are understood as idealizations or as constructions does not necessarily dictate *function*. Thus instead of trying to define models in terms of what they are, a focus could be directed on what they are used to do. This shifts also the unit of analysis from that of a model and its supposed target system to the very practice of modelling. Traditionally models are taken as representations and thus they are assumed to be useful to the extent that they succeed in representing their target systems correctly. In view of recent discussions on scientific representation this account of modelling is deemed problematic if only because representation seems such a tricky notion. One way to circumvent this problem is to proceed directly to the study the different roles models can take as instruments of investigation, but before this, we briefly consider the issue of representation.

3.1 Representation

The theme of representation has featured already at several points of this account. This is certainly no accident, since if we are to believe the philosophers of science, the primary epistemic task of models is to represent some target systems more or less accurately or truthfully (the idea of models as representations can be traced back to Heinrich Herz, see Nordmann 1998) . From this perspective, working with models amounts to using models to represent the world. According to the general philosophy of science the link between models and representation is as intimate as coming close to a conceptual one: philosophers have usually agreed that models are essentially representations and as such “models of” some real target systems. Moreover, the knowledge-bearing nature of models has been ascribed to representation. Whereas the representational nature of mathematical models in economics has been contested, this is certainly one way to read the debates about the status and functions of different kinds of models in econometrics where the notion that models represent is somehow taken for granted. The arguments are over how, and where, and with what success, econometric models, by representing the economy at different levels and with different aims, can be used to learn about measurements, patterns, regularities, causes, structures and so forth (see for example, Backhouse, 2007; and, for philosophical treatments, Chao, 2007, and forthcoming).

Although there has been this consensus among the philosophers regarding the representational nature of models, the accounts given to the notion of representation have differed widely ranging from the structuralist conceptions to the more recent pragmatist ones (e.g. Bailer-Jones, 2003, Suárez, 2004; Giere, 2004). The pragmatist approaches to representation can be seen as a critique of the structuralist notions that are part and parcel of the semantic conception, which until recently has been the most widely held view on models in the philosophy of science (see Hands, 2001, Chapter 7.4.1, and Chao, forthcoming, for a discussion of structuralist notions applied to economics). The semantic conception provides a straightforward answer to the question of how models give us knowledge of the world: they specify structures that are posited as possible representations of either the observable phenomena or, even more ambitiously, the underlying structures of the real target systems. Thus, according to the semantic view, the structure specified by a model represents its target system if it is either structurally isomorphic or somehow similar to it (see Brodbeck, 1968 on social science models, and more recently van Fraassen, 1980; French and Ladyman, 1999; Giere, 1988). The pragmatist critics of the semantic conception have argued, rather conclusively, that the structuralist notion of representation does not satisfy the formal and other criteria we might want in order to affirm representation (see e.g., Suárez, 2003; Frigg, 2003). The problem can be located in the attempt to find such properties both in the representational vehicle (the model) and the real object (the target system) by virtue of which a representational relationship can be established between a model and its target object.

So far, despite the numerous philosophical trials, no such solution to the general puzzle concerning representation has been presented. Hence the continued referral to representation does not seem to provide a reliable foundation to discuss the epistemic value of models. The alternative pragmatist accounts of representation seek to circumvent this traditional problem by making the representational relationship an accomplishment of representation-users. Consequently, it is common among pragmatist approaches to focus on the intentional activity of representation users and to deny that representation may be based only on the respective properties of the representing vehicle and its target object. However, if representation is primarily grounded in the specific goals and the representing activity of humans as opposed to the properties of the representing vehicle and its target, it is difficult to say anything very substantial and general about it from a philosophical point of view (cf. Giere, 2004, Suárez, 2004). Recently, Uskali Mäki has proposed a two-tiered notion of representation that attempts to overcome this problem by analysing the representational character of models into two parts. Thus, according to him, a model represents in two ways: Firstly, by being a *representative* of some target system for which it stands for as a surrogate system. Secondly, Mäki claims that our only hope to learn about the target by examining its surrogate is if they *resemble* one another in *suitable respects and sufficient degrees* (Mäki, forthcoming b). In conceiving of representation as jointly constrained by both the purposes of model users and the ontology of the real target Mäki's account mediates between the semantic and pragmatic notions of representation, remaining however open to the pragmatist criticisms concerning similarity as a basis of representation and simultaneously – perhaps – to the same difficulties of making general philosophical claims based on users' purposes as pragmatists.

. One obvious way out of this problem is not to focus the discussion on the nature or properties of models and the initial representation, nor the representing aims of users, but to focus attention instead on the kinds of work that models do in economics. As we have already seen, economists (including of course econometricians) have used models for many different purposes: to explore the world, explain events, isolate causal capacities, test theories, predict outcomes, analyse policy choices, describe processes, and so forth; and philosophers of economics have tended to offer commentaries and analyses of how models may or may not fulfil these different particular purposes. Fulfilling such different functions can be gathered together under the broad notion that models operate as instruments of various kinds in science.

3.2 Instruments and Narratives

Milton Friedman's "The Methodology of Positive Economics" (1953) has probably become the single most read work on the methodology of economics, its very fame testifying to its success in capturing some basic convictions held by economists. Most importantly, Friedman has been taken to claim that the "unrealism" of the *assumptions of economic theory* do not matter, the goal of science being the development of hypotheses that give "valid and meaningful" predictions about phenomena. Whereas this can be understood as a kind of naive instrumentalism, Friedman's famous essay can be read in many other ways (see the papers in Mäki (Ed.), forthcoming).

Yet, Friedman's remarks on the *nature of models* (as opposed to theories) are both less naive, and more conventional in terms of the discussion of the idealized nature of models and their representational qualities. Indeed, in one interpretation of his words below, they seem close both to Mills' earlier arguments about isolating causes in economics (e.g. Mäki 1992), as well as to later arguments about econometric models (see above). This latter congruence may reflect the fact that Friedman was also an empirical economist, as we see his in concern with the issue of the correspondence rules for working with models:

> "... a hypothesis or theory consists of an assertion that certain forces are, and by implication others are not, important for a particular class of phenomena and a specification of the manner of action of the forces it asserts to be important. We can regard the hypothesis as consisting of two parts: first, a conceptual world or abstract model simpler than the "real world" and containing only the forces that the hypothesis asserts to be important; second, a set of rules defining the class of phenomena for which the "model" can be taken to be an adequate representation of the "real world" and specifying the correspondence between the variables or entities in the model and observable phenomena" (Friedman, 1953, p 24)

Friedman here suggests we think of a model as both a theory or hypothesis, and at the same time a representation of the real world. So, interpretations of his position could take us to the models as mediators route, or to the earlier and related simulacrum account of models in physics found in Cartwright (1983). Of course, Friedman's terminology invokes the shadow of the notorious correspondence rules of logical positivism, yet, on the other hand one could argue that empirical modelling must depend upon the establishment of such rules in a practical sense (see Hirsch and De Marchi, 1990, particularly Chapter 8, for an analysis of Friedman's empirical work with models).

Certainly developing correspondence rules has formed one of the major difficulties for economists seeking to defend the method of modelling, and for philosophers and methodologists seeking to account for the work done by economic models. This difficulty is immediately evident in the way that mathematical and other theoretical models are linked to the world in a way which seems quite ad hoc. Indeed, "casual application" is the term that Alan Gibbard and Hal Varian (1978) use to describe the way that economists use mathematical models to *approximately* describe economic phenomena of the world without undertaking any form of measurement or testing. In their account, mathematical models are connected to the world by "stories" which interpret the terms in the model in a way which is reflected in Hausman's (1990) argument that economists use such stories to explain particular real world events using ceteris paribus arguments (above). Morgan (2001 and 2007) argues for taking a stronger position, suggesting that such narratives form an integral part not just in applying models to the world in both imagined and real cases, but constitute an integral element in the model's identity. For example, the prisoner's dilemma game is defined not just by its matrix, but by the rules of the game that are defined in the accompanying narrative text, and then it is the text that mediates the application of the game to various economic situations. Grüne-Yanoff and Schweinzer (2008) extend this argument to show how narratives also figure in the choice of solution concepts that are given by the theory of games. These accounts suggest that economists rely on experiential, intuitive and informal kinds of rules to establish correspondence for mathematical models and thus to make inferences from them.

In sharp contrast to this casual correspondence found in the use of mathematical models, the different econometric approaches to models (above) focussed seriously on what might be considered correspondence problems. That is, econometricians's arguments about model derivation and selection, along with their reliance on a battery of statistical tests, are really all about how to get a correspondence via models in fitting theory to the world: one might even say that econometrics could be broadly described as a project of developing the theory and practices of correspondence rules for economics. Certainly some of the most interesting conundrums of theoretical econometrics fall under this general label - such as the identification problem: an analysis of the statistical and data circumstances under which a model with relevant identifiable mathematical characteristics may be statistically identified, and so measured, using a particular data set. (There is a rich philosophically interesting literature on this fundamental problem from which we mention four examples - see Fennell, 2007 for a recent contribution that relates to questions of mathematical form; Aldrich, 1994 and Boumans, 2005 for a discussion in terms of "autonomy"; and Morgan, 1990 for an account of their early conceptualization.) From this perspective then, Friedman's position on models, in the quote above, is far from naïve – or philosophically dated.

More recently, a kind of sophisticated instrumentalism has been advanced by two philosophers of economics who specialise in econometrics - Kevin Hoover and Marcel Boumans. For them, models can function as instruments of observation and measurement in the process of identifying invariant economic phenomena. For Hoover (1994 and 2000), this follows from recognising that economics is largely (and particularly at the macroeconomic or market level, rather than the individual level) a science of observation rather than one of experiment, so that he regards the many models generated even within one task of applied econometrics as instruments of observation that bring economic data into the economist's focus using both statistical and economic theories at various points in the modelling process.

Boumans (2005) follows Trygve Haavelmo, a contemporary of Friedman, whose less famous but more subtle philosophical tract of 1944, argued that the problem of econometrics be attacked not by thinking of models as matching devices, but by treating them as experimental designs. The link from models to experiment comes from statistical theory: the observed data set for any particular time and place being one single outcome from the passive experiments of Nature, so that the aim of econometric modelling is to design a model that will replicate Nature's (the Economy's) experiment. Then probability theory can be used to assess the model design given those experimental produced outcomes (the economic observations) (see Morgan, 1990, Qin, 1993). It should be noted that the notion of passive experiment here is that of any uncontrolled experiment carried out by the natural workings of the economy, whereas economists also work with the notion of "natural experiment": experiments that have occurred naturally but under conditions of such stability (nothing else changing) that they can count as controlled experiments.

Boumans develops this argument to show how models function as the primary instrument in this process, which enable measurement of the economic world. For Boumans, unlike Haavelmo, the task is not so much to advance theory testing, but to develop the relevant measuring instruments on which economics depends. This entails discussion of exactly how models are used to provide measurements, how to assess the reliability of such model instruments (via calibration, filtering etc), and how to understand precision and rigour in the econometric model context. For him, models are not just for observing the economy, but are complex scientific instruments that enable economists to produce measurements to match their concepts (see also Boumans, 2007).

3.3 Models and Their Epistemic Functions

As Scott Gordon remarked of economic models "the purpose of any model is to serve as a tool or instrument of scientific investigation" (1991, p. 108). That statement leaves a wide open field of possibilities. Models have been used in a variety of functions within economics to:

* suggest explanations for certain specific or general phenomena observed or measured by using a model;
* carry out experiments to design, specify and even help execute policy based on a model;
* make predictions, ascertain counterfactual results, and conduct thought experiments using a model;
* derive solutions to theoretical problems that might be treated within a model;
* explore the limits and range of possible outcomes consistent with questions that can be answered using a model; and
* develop theory, concepts and classificatory systems with the model.

The very varied nature of these functions emphasizes how much models are the means for active work by economists rather than passive objects. A characteristic point is that such use generally involves some kind of manipulation and accompanying investigation into the model as an object. Whereas both Boumans and Hoover depict models as instruments to achieve something via an intervention elsewhere, in many of these uses of economic models, economists investigate the models as a way to investigate either the world for which it stands, or the theory that those models embed (see Morgan forthcoming). When models are understood as a particular kind of tool or instrument, namely as investigative devices, their epistemic versatility is more fully revealed.

3.3.1 Experimental exploration

Because experiments are seen has having a valid epistemic function within the philosophy of science, we begin with this notion as analogous for working with models. This also has the virtue of continuing the thread from Haavelmo's notions about the role of models in econometrics. Haavelmo (1944), recall, thought of models as designs for experiments that might replicate the activities within the economy. Probability reasoning was needed both because it provided a way to think about how observations were produced by those experiments of the economy, but also because it provided the basis for making valid inferences about how well the relations specified in the model matched those thrown up in the observations. Haavelmo was drawing on two traditions in statistical work: one that interpreted the measuring methods of statistics as a substitute for control in passive experiments; and another in which the design of real experiments relied on probability elements to obtain valid control. Such a combination confers a considerable advantage on those scientific fields that rely on statistically controlled experiments for it provides relevant rules for making valid inferences which are both more formal and more general when compared to the informal and purpose-specific practices that may be used to draw inferences from experiments in the laboratory and to extend such inferences beyond the laboratory in other sciences. The point here however is not the comparison with other scientific modes of experiment, but between the use of econometric models with other modes of modelling in economics.

Morgan (2002 and 2003) has argued that we can also understand the habitual way that economists use mathematical models in economics as a form of experimental activity, while Mäki 1992 and 2005 makes a somewhat different claim that models are experiments based upon his analogy between theoretical isolation and laboratory controls in making mathematical models. Such model experiments found in economics consist of posing questions and manipulating the model to answer them. Questions such as: "What will happen if a particular element in the model changes?" "Let us assume that a particular element has a particular value: what difference will this make?" and so forth. The final step in the model experiment is some kind of inference statement, inference about the world being modelled, or even inference about some theoretical puzzle. Of course, the inferences involved are clearly more informal than those in econometric models - recall the role of narratives (above) as one format in which economists may relate the work of model experiments to the world or to theory. And, in comparison with the possibilities of real experiments, Morgan (2005) suggests that model experiments have less epistemic power: model experiments have the power to surprise economists though these surprises can in principle be explained, but real experiments may confound them with findings that remain unexplainable.

How does work with models create surprising outcomes? Remember that models are not passive objects, but their usefulness as investigative instruments depends on them having sufficient internal resources for manipulation. Very simple models have few resources that can be made the subject of experiment. More complex models do have such resources, though very complex ones may become too complicated to experiment with. Models are of course built or made by the scientist, but, as we can learn from Koopmans's argument, it is not always obvious what kinds of economic behaviour a model implies. So, as Koopmans suggests, one of the uses of models is to enable the economist to understand the implications of taking several postulates together - and this may give surprising outcomes, as Hoover's discussion of the way micro and macro assumptions fit together (see earlier) examplifies. On the other hand, economic models are not made in the materials of the economy: hydraulic machines, diagrams, equations, are not economic actors and these artefacts of economic science are rarely directly performative as models. (There are, of course, some interesting exceptions: see MacKenzie, 2006, on how models in finance made economic behaviour and outcomes more like the models of that behaviour.) This material difference limits the inferences that may be made from such models, just as it limits the possibilities of producing unexplainable outcomes (see Morgan, 2005). Despite these comparisons on inference and epistemic power which operate to the disadvantage of the mathematical models compared to the econometric ones, the experimental limitations of such models may be weighed against the variety of other epistemic functions that may be fulfilled when economists use mathematical models in investigative modes.

As an example of what is meant by a model experiment, consider the possibilities of simulation with models. While simulations in other sciences have often been used to produce numerical solutions where analytical solutions are problematic, in economics, simulation has more often been used to explore in a systematic fashion the range of outcomes consistent with a particular model structure. This experimental activity with a model - each simulation run constitutes an individual model experiment - provides information about the model. It may enable the economist to rule out certain values for the model because of the implausibility of the simulated behaviour, or it may offer support for particular versions of the model or for particular idealizations as a result of simulation experiments on closely related models (see Morgan, 2004). Simulation offers a form of experiment which is compatible with mimicking models, but also equally useful with representational constructions or idealized models. Similarly, the ordinary usage of mathematical models endemic in policy circles is as an aid to the framing of particular tax regimes, trade relations, and so forth in which simulation experiments with the mathematical structures by varying assumptions suggest answers to particular policy questions. Even econometric models may be subject to simulation: for example, the analysis of policy options on models that have already been validated for a specific country at a specific time.

3.3.2 Conceptual Exploration

Perhaps because of the dominance of modelling in later twentieth century economics, models have come to be generally associated with functions that are more usually seen as the preserve of theory making. For example, the Edgeworth Box had a very long history in which economists from 1881 to the 1950s used it to derive solutions to various theoretical problems in several different domains of economics (see Humphrey, 1996). But not only was it used in the development of theory results, it was also critical in the development of new theoretical concepts - Edgeworth's contract curve and Pareto's optimal position were both framed within the Edgeworth Box (see also Morgan, 2004a). More broadly, Daniel Hausman, suggests that theoretical modelling is the main site in current economics in which concepts are formed and explored:

> "A theory must identify regularities in the world. But science does not proceed primarily by spotting correlations among various known properties of things. An absolutely crucial step is constructing new concepts - new ways of classifying and describing phenomena. Much of scientific theorizing consists of developing and thinking about such new concepts, relating them to other concepts and exploring their implications.
>
> This kind of endeavor is particularly prominent in economics, where theorists devote a great deal of effort to exploring the implications of perfect rationality, perfect information, and perfect competition. These explorations, which are separate from questions of application and assessment, are, I believe, what economists (but *not* econometricians) call "models"." (Hausman, 1984, p 13)

We can see how this happens more generally by looking at the way in which the basic assumptions of micro-economics circa 1950 have been unpicked, reformed, and refined over the period since around 1970 as economists have used models as their preferred site to give content to, and explore notions of, concepts such as bounded rationality and imperfect information. This re-generation of theories has depended on working with models.

The classificatory functions of model using are almost a by-product of the modelling manipulations and experiments that go on in these processes of concept formation. Each run with a model, each slight change in the assumptions, each minor change in the set up, each substitution of a particular element, may give rise to a different result from the previous one with same or almost the same model. It is this variation in outcomes that leads to new classifications. An obvious example in modern economics is game theory, where minor changes in rule, and in matrix numbers, may lead to different outcomes Each of these games can be thought of as a model in the sense that these games are treated by economists as models for economic situations (see Morgan, 2007). But as economists work on these models, they begin to classify their games along different dimensions: such as levels of co-operation, the number of times a game is played, and so forth, and thus develop conceptual labels within game theory: co-operative games, n-person games, etc. Similarly, the different forms and concepts of industrial competition were developed in industrial economics during the 1930s as models were used to develop the different cases and to classify them according to the number of firms and nature of competition. The proliferation of cases and the labelling activity suggests that we think of both these fields not as consisting of one general theory (of industry or of games) accompanied by an additional set of special cases, but as theoretical fields in which the main material consists of a carefully classified set of well defined models (see Morgan, 2002).

From an applied economics viewpoint, this makes the class of models the relevant space within which to "observe" stable regularities. The set of classes of models together make up the general theoretical field, such as game theory or industrial economics. That is, in such fields, the answer is not to seek complete homogeneity in economic life nor complete heterogeneity, but to use models to define the economic objects in the world within which a particular kind or class of behaviour can be isolated. This kind of vision underlies John Sutton's work and his "class of models" approach (2000) to applied economics where once again, models form investigative devices for finding out about the world, but the project depends on the classificatory and conceptual work of modelling that has gone beforehand.

3.3.3 Inferences from Models

Thinking about the wide use of models in the experimental mode, picks up the practitioners' sense that working with models involves making inferences. These inferential relations are described under different terms ranging from deductive to inductive inference, and with forms of making inference that range from the stories of the casual application of mathematical model experiments to the formally rule-bound statistically-based inferences of econometric models. Both stories and econometric inference forms have been discussed at various points earlier in the essay (see particularly sections 3.2 and 2.1.3 with 3.3.1). Here we take the more traditional philosophers' puzzle, namely: how it is that by working with models, particularly the mathematical ones, and by using them in various modes, economists gain knowledge about the real world?

Traditionally, the form of inference invoked by economists for mathematical models has been *deductive inference*. The idea of models as a basis for deductive inference fits squarely with the conception of models as idealizations or isolations. From this perspective, such models are stand ins or substitute systems that are used indirectly to study the causal workings of the real economies (Maki uses the term ‘surrogate system’, see his forthcoming b). Using models as stand ins or surrogates for real world systems, economists study the consequences of abstract, isolated facts, that is, what these factors or mechanisms would produce if unimpeded (e.g. Cartwright, 1998). This happens by way of studying the deductive consequences of the model assumptions, an idea formulated by Hausman as “the economists’ inexact deductive method” (1992). According to this, economists formulate, with the help of *ceteris paribus* clauses (other things being equal), plausible and useful generalizations concerning the functioning of relevant causal factors. Predictions are then deduced from these generalizations, certain initial conditions, and further simplifications. Although these predictions are supposed to be testable, they often are not that in practice, since, claims Hausman, the economic reality is so complex that the economists are not usually able to explicate the content of their ceteris paribus clauses, which takes us back to Mill’s problem (and see Cartwright, 2006).

In an alternative argument to this view, Sugden (2002, forthcoming) has claimed that economists in fact infer inductively from their models. Studying examples from both economics and biology, Sugden notes that even though modellers are willing to make empirical claims about the real world based on their models, it is difficult to find from their texts any *explicit connections* made between the models and the real world. Their claims about the real world are typically not the ones they derive from their models, but something more general. Consequently, Sugden suggests that modellers are making *inductive inferences* on the basis of their models. One commonly infers inductively from one part of the world to another, for instance expecting that the housing markets of Cleveland resemble those of other large industrial cities in the northeastern USA, for instance. However, just as we can infer from real systems to other real systems, we can also infer from theoretical models to real systems. A modeler constructs “imaginary cities, whose workings he can easily understand” (Sugden, 2002, p.130) in order to invite inferences concerning the causal processes that might apply to real cities. This possibility is based on our being able to see the relevant models as instances of some category, other instances of which might actually exist in the real world. Moreover, for inductive inference to work we have to accept that the model describes a state of affairs that is *credible* given our knowledge of the real world (see also Mäki forthcoming c).

What is common to both views is the insight that models are typically valued for their results or output. However, the two perspectives diverge on the question of the extent to which some selected features of a given target system can be understood to be represented reasonably correctly in the model. Philosophically, it seems a more safe option to assume that this is the case, because then as a result of deductive inference one can assume that the results achieved depict at least one aspect of the total behaviour of the system under study. However, such an approach needs to assume that economic phenomena are separable, and that models provide us with some of the components, and that their arrangements are exhibited in the real world (see Cartwright, 1989, p. 47, where she also discusses what else has to be assumed for a model to establish what she calls causal capacities). These are rather stringent conditions not met by many economic models as we discussed in the early sections of this essay.. Thus while this option seems philosophically more straightforward, it is more difficult to see it working effectively in applying mathematical models casually to the world. On the other hand, it may indeed appear difficult to see how models as credible constructions can license inferences concerning the real world.

In this respect of model inferences the idea of models as investigative instruments does a lot of philosophical work. In this perspective economists are thought to gain knowledge from models by way of building them, manipulating them, and trying out their different alternative uses. Thus one can consider models as experimentable things, which through their different uses and interpretations facilitate many kinds of inferences: in helping researchers to systematically chart different theoretical options and their consequences thus enabling them to proceed in a more systematic manner in answering the questions of interest. The starting point of modelling may not be representing some real causal factors accurately but rather trying to make the model to produce certain kinds of results. In fact, modellers often proceed in a roundabout way, seeking to build hypothetical model systems in the light of the anticipated results or of certain features of the phenomena they are supposed to exhibit. If a model succeeds in producing the expected results or in replicating some features of the phenomenon, it provides an interesting starting point for further conjectures and inferences. These further investigations might be theory related or world related.

Reiss (2008), in a detailed study of how models are used by economists in drawing inferences, holds a middle position between that of Cartwright and Sugden. He argues that while both "models and thought experiments are representational devices, which sketch mechanisms that may be operative in an economy" (p. 124), investigations using them (such as Schelling's checkerboard model), offer only "prima facie, not valid (or sound) evidence" to support valid inference (about the reasons for segregated neighbourhoods), and, so that further empirical work would be needed to justify claims that such a model explains the observed phenomenon: that is, plausibility or credibility is not sufficient.

There is another sense in which Reiss's position can be taken as middle ground, for we might also consider the work done by economists with mathematical models as either thought experiments or the tracing out of counterfactuals. For thought experiments, we might look to the position taken by Steven Rappaport, who regards mathematical modellers as resolving "conceptual problems" by providing answers to such questions as Tiebout's (1956) problem "Is it *possible* for there to be a set of social institutions in which people will reveal their true preferences for public goods, and for the approximate quantities of these goods people want to be provided? The short version of the Tiebout's own answer to this problem is 'Yes', and his model explains and justifies this answer." (Rappaport, 1998, p. 133). For Rappaport, mathematical models are used for learning about the structure and behaviour of possible economies which fulfil certain requirements or have certain characteristics, and they are answered by constructing models of the world in which those characteristics hold true, that is, in thought experiments. At the opposite side, we could point to the classic cliometric work of Robert Fogel, whose counterfactual investigation of the historical claim that railways had been indispensable for the growth of the economy, depended upon investigations using mathematical models to construct a counterfactual world without railways for the American economy in 1890, that is to answer counterfactually a question about the real economy. This work was highly controversial partly because of the way that idealized mathematical models were used in answering the historical question. Both thought experiments and counterfactuals are traditional topics in philosophy, let alone philosophy of science and while there is some overlap with the modelling literature, models are not usually central to those discussions (see McCloskey, 1990, Schabas, 2008)

This orientation of modelling towards their results also accounts for why modellers frequently use the same cross-disciplinary *computational templates* (Humphreys, 2004), such as well-known general equation types, statistical distributions and computational methods. A good example of such a template is the logistic function, which applies to diverse dynamic phenomena across the disciplines, and has been used in economics amongst many other sciences. Sugden, following Schelling (2006), combines the idea of templates to that of social mechanisms. For Schelling the discovery of social mechanisms begins with previously observed empirical regularities, for which suitable often cross-disciplinary mathematical structures can be applied "inviting the explanation" in terms of underlying social mechanisms. This kind of reasoning that starts from conclusions, i.e. from previously observed empirical regularities to the tentative mechanisms that could have produced them, is abductive. Abduction starts from a set of accepted facts inferring their most likely explanations. Applying a well-defined tractable template to a new domain hardly qualifies as a most likely explanation but rather points at the element of opportunism present in modelling: the templates that have proven successful in some domain will be applied to other domains perhaps based on some similarity of behaviour or regularity. Certainly, the transporting of models between different domains of economics is relatively common, particularly in micro-economics, where, for example, models of consumer choice between goods were moved sideways to apply to other choices (which seem similar in the economists' way of thinking) such as that between leisure versus work, or to the number of children a family decides to have.

It is characteristic of modelling that a domain of interest is sometimes described (i.e. modelled) with the help of the terms, mechanisms and structures that are borrowed from another domain, which maybe familiar, or differently, or better, organised in certain respects (Black, 1962, Hesse, 1966). Mary Hesse's work claims that many useful models represent the world in terms of analogies, and these enable scientists to infer various things about the domain of interest by making use of the analogy between it and another domain. In her account, the positive analogical features in common between the two domains support potential inferences based on the neutral analogous features of the comparison leading to potential theory development. In the context of economics, Morgan (1997) goes further in suggesting that even the negative features that come from such analogical comparison can be used as inference opportunities prompting theory development from using the model. She illustrates this with some work by Irving Fisher who used the mechanical balance as an analogical model to make new claims based on the aggregate level equation of exchange. The use of analogies from other fields has been quite common in the history of economics, with many and varied intersections between economics and physics and between economics and biology, and where metaphors, mechanisms, models and terms have been borrowed in both directions (see, for example, Mirowski, 1994). Indeed, modelling can be seen as a productive practice that uses as-if reasoning, analogies, familiar computational templates and other constructive techniques to probe the possible mechanisms underlying the phenomena of interest.

5. Conclusions: From Models to Modelling

The recent re-orientation of philosophy towards the practices of science offers an account very different from those earlier philosophy of science writings on the syntactic versus semantic accounts of models that dominated the field until recently. Whereas earlier philosophers worried about what models were and how to define them, particularly in relation to theory, over the last twenty years, as this essay has shown, philosophers of economics have aimed to analyse how economic scientists build models and what they do with models: how they use them, how they argue with them, and what they learn from using them. At the same time, commentaries by economists on their own practices are by no means a-philosophical as we have seen, and while they have not been particularly worried about defining models, they have taken up discussions, in their own terms, of classic philosophical questions such as idealization, correspondence rules, and so forth. At the intersection of these two positions, we have found philosophically-inclined economists and naturalistically-inclined philosophers engaged with economics, who together have opened up a rather new set of questions and agendas for philosophical commentary and analysis. This essay has examined a set of the issues that have emerged from this work and that in many respects beg for further analysis: the problems of de-idealization and what these say about idealization; the implications of models conceived of as fictions, artefacts and mediators; the different ways in which models are taken to represent and mimic; the importance of how models are used and thus their experimentable potential; the roles of content and materials in providing resources and constraints to modellers; the functions of stories, analogies, templates, credible world comparisons, and statistical rules in making and supporting different kinds and modes of inferences; and so forth. These various new foci are both distinctive in terms of topics, and thought provoking, if not challenging, to the older conventional philosophical positions. They follow, however, not just from a naturalistic turn towards the study of science, but also from a reframing of the basic object of study: from models to modelling, that is, to how economists construct models and work with them.

The analysis offered here reflects not only on the resources that a model-based discipline like economics offers to the philosopher of science interested in how scientific knowledge is established using models; but to a change going on in the current status of studies of modelling in philosophy. Whereas physics with its mathematical models was for long the base case for thinking about models and the benchmark for analysing modelling in all other fields, this is no longer the case. Biology, with its model organisms, and its reliance on them as experimental systems, now offers an alternative paradigmatic case, with very different characteristics, for the philosophical analysis of models (Creager et al, 2007). Economic modelling, as the evidence of this essay suggests, offers the kinds of rich and varied materials, ranging from the statistical models used by econometricians to the mathematical objects used by theorizers, to provide an important third site for the serious philosophical study of models. Without benefit of the manipulable real objects, model organisms and experimental systems of biology, nor the well behaved and attested mathematical descriptions of physics, economics offers a scientific field in which models may look like the models of physics, but are used more like the experimental model systems of biology, and yet, whose inference regimes depend on modes of comparison that range from the heuristic to the statistical as befitting its social science domain.

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