

Model building in economics post-1980 vs. pre-1980

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Abstract

There are many methodology articles about model building in economics that distinguishes models from theories. Most were criticisms against the overuse of formal mathematics in economics. Proponents of using and developing formal mathematical models of economic theories defended this by claiming that mathematics is just a language, nothing more. But as Axel Leijonhufvud noted, 'English is a language too'.

The latest generation of economic model builders do not understand such criticisms as they do not see a meaningful distinction between models and theories whenever that distinction is to be based on mathematics because theory today is seen as being any model involving mathematics. If any distinction is to be made, it will be between theoretical versus empirical models.

This article will explain the various ways model building has changed in the last 30 years and how some model builders are returning to the old idea of a model of economics where mathematics is not the central aspect of model building.

Key-words: models, mathematics, economics.

Resumen

Existe una multiplicidad de artículos metodológicos referidos a la construcción de modelos en economía que diferencian los modelos de las teorías. La mayoría de ellos critican el abuso de las formalizaciones matemáticas en economía. Quienes adscriben a la utilización y el desarrollo de modelos matemáticos formales de teorías económicas, defienden esta práctica argumentando que la matemática es sólo un lenguaje. Pero como ha señalado Axel Leijonhufvud, "el castellano también es un lenguaje".

La joven generación de constructores de modelos económicos no comprende tal crítica porque no ven ninguna distinción significativa entre modelos y teorías, en la medida en que tal distinción esté basada en las matemáticas, dado que "teoría" hoy es entendida como todo modelo que involucre matemáticas. Si es necesario hacer alguna distinción, se hace entre modelos empíricos y modelos teóricos.

Este artículo explicará las distintas maneras en las cuales la construcción de modelos ha cambiado en los últimos 30 años, y cómo algunos constructores de modelos están retornando a la vieja idea de un modelo de economía donde la matemática no constituya el aspecto central en la construcción de modelos.

Palabras claves: modelos, matemáticas, economía.

Introduction.

There are many methodology articles about model building in economics that distinguishes models from theories. However, most, maybe all of them until recently were mostly criticisms against the overuse of formal mathematics in economics. Proponents of using and developing formal mathematical models of economic theories defended this by claiming that mathematics is just a language, nothing more. But as Axel Leijonhufvud noted, ‘English is a language too’ (Leijonhufvud 1997, p. 198).

Undoubtedly, the latest generation of economic model builders do not understand such criticisms as they do not see a meaningful distinction between models and theories whenever that distinction is to be based on mathematics. And this is for a good reason. Theory today is seen as being any model involving mathematics. So, no longer is mathematics used to distinguish between models and theories. If any distinction is to be made, it will be between theoretical versus empirical models.

For me, realization of this situation turned out to be problematic when I set about producing a second edition of my 1989 book on the methodology of economic model building. The basic presumption of that book was, of course, that theories and models were very separate things. Specifically, model building in my 1989 book was seen to be a three-step process. The first step involved a non-mathematical behavioural economics hypothesis that one would use to explain some economic phenomena. At minimum, this step identified a list of endogenous variables of interest to be explained and a list of ‘causes’, that is, a list of exogenous variables. This separation between exogenous and endogenous variables is central to economic explanations. Changes in exogenous variables are deemed to cause changes in the endogenous variables. But, any conceivable change in an endogenous variable (perhaps as a movement towards an equilibrium) will not cause any exogenous variable to change. The posited behavioural hypothesis specifies how the values exogenous variables cause the values of the endogenous variables. An example might be how a exogenous change in technology might lead to an increase in the output of a good without changes in other inputs such as labour and capital.

The second step in model building would be to represent the behavioural hypothesis with one or more equations. In macroeconomics, an example might be the notion that the level of aggregate consumption (**C**) is a *linear* function involving the level of aggregate income (**Y**) – that is, we would assume $C = \alpha + \beta Y$.

If the product of the first two steps are to be applied to observable data, the third step would be to specify criteria to be used to decide whether the resulting model does or does not ‘fit’ in some sense. If the model is not applied in this manner, one might instead specify an initial value for the α and perhaps one for the β which would be used to generate potentially observable data. If one is not just assuming initial values, one could ‘calibrate’ the model by using a separate source of information about the values of α and β .

The youngest generation of economic models builders would not have seen the first two steps a separate events. And so, I doubt any of them would have understood much of my 1989 book. And as such, I decided it would be pointless to produce a second edition as the audience, namely those who obtained their PhDs before 1980, would be small and shrinking as the older generation of model builders retire from the scene. And as a result, I decided to write a different book, one that would try to bridge this gap as well as, perhaps, show that what I will be calling the pre-1980 view of models has not gone away but is now reappearing in a different form. In this article I will briefly explain some of what I learned about model building in today’s economics and what will be more fully discussed in my forthcoming book, *Model Building in Economics: Its Purposes and Limitations*.

The reason for recognizing the 1980 turning point.

The specific year of 1980 is somewhat arbitrary, but the turning point is real. In starting to work on my forthcoming book, I first tried to survey my colleagues about how models are used in their various sub-disciplines of economics. But I was having a difficult time. My younger colleagues could not understand what I was talking about when asking them how they would build models of their proffered theoretical ideas. My older colleagues had no difficulty helping me. So, I decided to try a different, more fundamental survey by asking them if they understood the following quotation from page 886 of a 1974 article by Richard Nelson and Sidney Winter (bold emphasis added):

In economics (as in physics) what we refer to as a **theory** is more a set of basic premises – a point of view that delineates the phenomena to be explained and modes of acceptable explanation – than a set of testable propositions. The **theory** points to certain phenomena and key explanatory variables and mechanisms, but generally is quite flexible about the expected conclusions of

empirical research, and a wide class of models is consistent with it. (Richard Nelson and Sidney Winter 1974, p. 886).

First I asked if they understood this quotation. And again, my older colleagues said they did, but my younger colleagues said they had no clue what Nelson and Winter were talking about. From a little research on the internet, I decided the key determinant of whether they did or did not understand the quotation seemed to be whether my survey subjects did their graduate work in economics after or before the aforementioned 1980. Of course, the question is: why 1980?.

While discussing with some of my graduate students a few years ago, I was told that in most of their classes where they were given problems to solve – the kind you find at the end of textbook chapters – the problems were always mathematical problems. Many years ago, when I was a student, this was not the case. Instead, we were always given verbal problems or questions and our first task was to turn these into mathematical problems or questions to solve and answer. It should be noted that those of my younger colleagues who grew up in the old Chicago tradition still do it this old way, but that is about it. From what I could determine, the transformation to what we now see almost everywhere began in the 1970s as more and more graduate students discovered the virtues of mathematical rigor. And by 1980, the transformation was complete. With this in mind, I now turn to looking at some of what I have learned about how models are used in economics today.

Microeconomic vs. macroeconomic theoretical model building.

While many still think of John Maynard Keynes when talking about macroeconomics, Keynesian macro models are only tangentially related to what Keynes argued for in his famous 1936 *General Theory*. The Keynesian macro models are called Keynesian only because the government is given a recognized role in those models. Keynesian models, which were originally promoted in the 1950s by Donald Patinkin, were based on general equilibrium models. After all, it can be said, what's more macro than the whole economy represented by a Walrasian general equilibrium model. In effect, it would be argued, macroeconomics is just aggregative economics (see Boland forthcoming, ch. 1 and 2).

Macroeconomics did not start being recognized in economics curricula as a distinctly different economics from microeconomics until the late 1940s. And many of the courses that were to be called macroeconomics or

macroeconomic theory courses were just renamed business cycle courses. And in many cases, microeconomic theory courses were just renamed price theory courses. It was not until the 1960s that macroeconomics courses were clearly on an equal footing with microeconomics courses.

For the most part, what was taught in microeconomics courses did not change much from what was taught in price theory type courses in the mid-1930s so it was easy to write a microeconomics textbook. Writing or selecting a macroeconomic textbooks, however, was often problematic, due mostly to the ideological tensions created by the explicit recognition of a role for government. Given such recognition, those who thought the market can solve all problems were usually reluctant to embrace Keynesian models. And there was always the struggle between those who tried to follow Keynes and promote aggregate economics where an equilibrium perspective would not be an essential perspective and those who adopted John Hicks' ISLM view of Keynes which was equilibrium oriented. As a result, choosing a textbook to use to teach a macroeconomics course was always a challenge. Choosing a microeconomics textbook was rarely problematic. All microeconomic textbooks would just present a version of *Book V* of Alfred Marshall's 1890 *Principles of Economics*.

Today, things seem very different. Macroeconomics, at least when it comes to most graduate research, has now been unified around what is called the Dynamic Stochastic General Equilibrium (DSGE) model – a model that is often attributed to the work Kenneth Arrow and Gerard Debreu (1954). What their work did was to expand the Walrasian static equilibrium model to cover both equilibria over multiple years and do so with a recognition that the future is uncertain. What is interesting about this is that both 1960s Keynesian model builders and those who in the 1970s rejected Keynesian models as useless for government policy assessment can now use the same basic model, the DSGE model. It is also claimed that this model makes it possible to incorporate imperfect competition, sticky prices and so on without giving up the equilibrium nature of the model.

Microeconomics is now becoming less united. This is primarily the result of the development of behavioural and evolutionary economics and as well as the ongoing critical examination in the form of experimental economics that continues to put some of the basic notions of Marshallian economics in doubt. And of course, the mathematics of game theory also presents an alternative to the math that Marshall relied on.

Keynesian macroeconomics detractors in the 1960s thought that any complete macro model must have microfoundations. And in the 1970s, this re-

appeared as part of the Lucas Critique which noted that Keynesian models presumed their parameters were like physical constants such as the gravity constant – as if people were mechanical decision makers rather than intelligent decision makers who, perhaps, can anticipate effects of government policy changes. Such anticipations can easily result in changes to the parameters. So, again, one must rely on microeconomics to completely understand the macro-economy.

For those macro model builders who do think microfoundations are necessary, two primary approaches have been taken. One is the aforementioned DSGE models which abandon aggregative economics for general equilibrium economics. The other is the infamous Representative Agent device first employed by Marshall in his later editions where he attempted to represent a whole industry with a single firm.

This is not the place to go into details about these approaches – interested readers will find more about these in my forthcoming book. Here it is enough to note that, as explained by Alan Kirman (1992), there are serious logical problems with the idea of a representative agent given the diversity of any real economy. Interestingly, proponents of the representative agent never seem to recognize Kirman's article. Of course, diversity is not ruled out in DSGE models and as such many model builders prefer not opting for the mathematical convenience of the representative agent.

Some identify two separate ways microeconomic models are distinguished from macroeconomic models. One is to claim that macroeconomic models – particularly in the form of DSGE models – can be dynamic where as Marshallian microeconomic models are limited to a static analysis of the neighbourhood properties of deviations about an equilibrium (viz, Marshall's partial equilibrium analysis of his Book V). There is considerable doubt about whether the dynamics of the DSGE are real dynamics in the sense that real time is not reversible. The other way is to claim that microeconomic models can provide causal explanations but macroeconomics cannot – at least, not those without microfoundations. And there is doubt about this since a cause can be identified only in the case of Marshall's *ce-teris paribus* methodology of explanation. Again, there is more discussion of these claims and counter-claims in Chapters 2 and 5 of my forthcoming book.

Microeconomic vs. macroeconomic empirical model building.

In the 1960s it was common for economists to distinguish between theoretical and applied economics – so, the distinction between theoretical

versus empirical models is not particularly new. What is relatively new over the last few years is the explicitly identified field of *empirical* microeconomics that is increasingly being identified as a distinctive research field by new PhDs. I suspect that this field has been created just to make this all conform to the theoretical versus empirical distinction used for models in general. What is included in the field of empirical microeconomics seems merely to be various pre-existing sub-fields of applied microeconomics such as labour theory, family or household economics, educational economics, health economics, etc. In other words, the distinction between micro- and macro-economic models is, as it has usually been, between the kind of data being explained. Probably, the only relatively new aspect of empirical microeconomics is the widespread use of econometrics (for more, see Boland forthcoming, ch. 5 and 6).

It has been recently argued that unlike large macro models, empirical microeconomic models are intended to address questions of causality in a manner that is claimed to be reliable and convincing (e.g., Angrist and Pischke 2010). Some critics will claim that the use of econometrics by itself would make this claim questionable. Primarily, this is because to use econometrics in building an empirical model one has to make many assumptions for the convenience of using econometrics that some claim are themselves fragile at best and unrealistic at worst. If the only reason for building empirical microeconomic models is to be able to use econometrics to explain (i.e., ‘fit’) observed microeconomic data, then perhaps this can be acceptable. But, surely, identifying causes is a more demanding purpose than just trying to get a good ‘fit’. Identifying causes requires an appropriate research design. Of course, one’s research design itself depends on why one is building the model in the first place. And, more important, any claimed credibility or reliability will obviously depend primarily on the quality of one’s research design.

N. Gregory Mankiw (2006) identifies two different purposes for building models. On the one hand, it can be that of a social *engineer* – that is, for the purpose of solving practical problems. On the other hand, it can be that of a *scientist* – that is, for the purpose of understanding how the world works. Mankiw was talking about the history of macroeconomics but it could just as easily apply to empirical microeconomics today.

Given Mankiw’s perspective, it could be said that if one’s purpose for building an empirical microeconomic model is scientific, we would usually find that one’s research design would involve experimentation. In this regard, almost everyone agrees that it would be impossible for one

to conduct an experiment on an entire macro-economy. The data to be used, even if deliberately obtained, are just passive macro observations. As such, if this is all that one has to use, it would be difficult to distinguish such activity from that conducted to study astronomy. Nevertheless, it is conceivable that one could use such a model if it is only to be used to understand the workings of a very small sector of an economy or just one particular market. In this limited sense, perhaps conducting an empirical microeconomics experiment is not out of the question. A typical empirical microeconomic experiment might concern the effectiveness of capital punishment or the effects of class size (see Angrist and Pischke 2010). It can be argued that if the research is properly designed, then it can be reliable and convincing. Skeptics will probably not be easily convinced.

Non-experimental empirical macroeconomic model building has been around for a long time. The first models are usually attributed to Jan Tinbergen and his work in the late 1930s. Subsequently, work begun in the 1940 led in the 1950s to the so-called Klein-Goldberger models with 25 equations and followed in the 1960s with a larger Brookings model that had 400 equations. Today, there are many commercial econometric macroeconomic models such as the DRI/McGraw-Hill model. Some of these have more than a 1000 equations.

Fans of building macroeconomic models (e.g., Chari and Kehoe 2006) see much progress over the last thirty or more years leading to many governments undertaking policy changes suggested by what these macro-models propose. Critics (e.g., Mankiw 2006, Solow 2008) see this as the view of a very limited group of macroeconomic model builders, specifically a group including those who developed macro models using the representative agent approach that many find very suspect. It can be claimed, that is, that such models are not really macro models as they are usually based on a single all-knowing maximizing individual. Also, some critics see these models to be relying too heavily on quickly clearing perfect markets. In other words, the critics do not see these models being sufficiently realistic. But, above all, the critics can also question the credibility of just these types of macroeconomic model built over the last thirty years since they are the same types of model that was guiding the governments leading to the 'Great Recession' of 2007–08!

Macro-econometric models: two approaches.

As already noted, DSGE models dominate macroeconomic model building. This is clearly the case in North America, though, perhaps, not so

much in parts of Europe and South America where DSGE methodology is often rejected. The main issue that separates the two approaches is whether one builds their model (usually, a DSGE model) first and then apply it to available data by econometrically estimating the parameters of the model or instead, analyze the data by first creating a model of the data, making assumptions about the nature of the data to decide the statistical method needed for any estimation of any model. This latter approach is advocated because the usual textbook econometric estimation methods are often applied to models whose assumptions are inconsistent with the statistical or probabilistic structure of the data (see Spanos 2011).

Needless to say, this recognition has led to charges and counter charges as well as various methodological disputes. But it can also be argued that there are reasons for the dominance of approach based on using DSGE models first and then applying them to data. The primary reason involves matters of academic sociology – specifically, the institutional activities involved with promotion and tenure. Beginning in the mid-1960s, the number of PhD-granting universities began to increase at a fast rate. One of the outcomes of the increase was that departments grew larger and more diverse such that the task of assessing performance of promotion or tenure candidates became increasingly difficult. Few members of any tenure committee would likely know much about all the various sub-disciplines represented in any department. Eventually, to get around this difficulty, most departments started counting publications as a primary measure of performance rather than, say, teaching abilities. In this regard, any smart candidate for promotion or tenure would chose to engage in research that would maximize the number of publications. Eventually, the DSGE models became recognized as a useful tool for this maximization. After all, the other approach that requires tedious assessing and modeling the structure of the data would require a lot more time – and to a certain extent would often show some empirical macro models to be unacceptable if realism or even logical consistency matters. And of course, with this other approach, far fewer publishable papers would be the ultimate result. Hence the dominance of the DSGE modeling approach.

Building theoretical models using game theory.

While most of the current game theoretical model building is post-1980, one can make a case that it may still indicate a slow movement back to the pre-1980s view of the relationship between theories and models. This is partly due to the fact that game theory is nothing more than a form of

mathematical modeling. This is how it was understood in the 1950s and '60s. It is also because at one time it was seen to be a way of modeling an Edgeworth type of general equilibrium, usually of 2-goods and 2-agents in mutual equilibrium as illustrated in the well-known Edgeworth-Bowley box. Recall: that box is constructed by putting in opposition the indifference maps for the two agents such that their respective zero-utility points are in opposing corners of the box. In this way, the box was usually used to distinguish a Pareto optimal exchange from that of a non-Pareto optimal exchange. The former being a point on a line – usually called the contract curve – that connects all the tangency points between the two agents' indifference maps whereas the latter would be any point not on that contract curve and any such point is not Pareto optimal since it is possible for one agent to gain without the other losing in an adjusted exchange. As a matter of formal mathematical analysis, I think interest in game theoretical models seemed to fall off once it was recognized that the solution for a zero-sum, non-cooperative game turned out to be formally equivalent to the solution for any linear programming problem (see further, Boland forthcoming, ch. 3 and 4).

With the notion of the non-Pareto optimal points off the contract curve in mind, movement towards the contract curve and thus towards a Pareto optimum has many possibilities. The possible-to-reach Pareto optimum points extend between two extremes along the contract curve. At one extreme, one agent merely moves along an indifference curve and thus gains nothing while the other agent gains the maximum possible. Of course, if there were some way to bargain, the other points between the extremes could be reached and these of course would also be Pareto optima. Obviously, it would seem, game theory would be an easy way to characterize such a bargain. What is not always mentioned is that the equilibrium illustrated in an Edgeworth-Bowley box is not a general equilibrium, *per se*. It is an illustration of one necessary condition for a general equilibrium to exist. Specifically, it says if you have a general equilibrium, then it must be possible to choose any two goods and any two agents and they will be found on their respective contract curve and their indifference curves will both be tangent to a line with a slope equal to the relative prices for the two goods.

My purpose here was not to discuss the history of game theory. Instead, I have presented game theory this way so as to illustrate how one can discuss an important theoretical notion – the Edgeworth-Bowley box idea of a Pareto optimum as a necessary condition for a general equilibrium – without building a model but it is a notion that can be represented with

a game theoretical model if one so chooses. Seen this way, one can see how a game theoretical model is just a case of the pre-1980 perspective discussed earlier.

There are, of course, several reasons for why game theory may seem to be a worthwhile form of mathematics to use to model exchange equilibria or, for that matter, other types of equilibria. One not often discussed is that it avoids having to assume continuous differentiable objective functions needed for applications of calculus as is common in Marshallian models. Another reason is that it allows for involving the diversity that is denied by devices such as the representative agent. One could even go so far as to argue that evolutionary game theory might be a suitable alternative to even macroeconomics (e.g., Boland 2003, ch. 9).

It should be noted, however, that the requirements for a useful game theoretical model are somewhat problematical. The usual game theoretical model has to identify not only a finite set of options for each player, but the players must know the rules (Kreps 1990, ch. 5). How do they know the rules? A key assumption used to assure the existence of an equilibrium is the additional notion that both players know that the other player is 'rational' (meaning: a maximizer) and they know the other player knows this about themselves. But, as is well recognized, not all games have a solution, that is, have a pair of choice options (one for each player) whereby neither player would want to change. And some games, as defined by the rules and the payoffs, may have multiple solutions. Many tricks ('refinements') are proposed to overcome the absence of a unique solution, but even when there is only one, as with all equilibrium-based explanations, unless there are good reasons for why the equilibrium will be attained, such explanations will always be questionable (I explain this in much more detail in Chapter 2 of my forthcoming book).

The role and limitations of experimental and behavioural economics.

While today it is not often mentioned by behavioural or experimental economists, the history of experimental economics has been closely connected with the history of game theory. It is easy to conceive of how one could see using a particular game theoretical model as a promising basis for an experiment simply by repeatedly using the game as the experiment. But, as the experimental economics pioneer Vernon Smith (1992, p. 275) explained, doing so 'provided an elegant means of demonstrating equilibrium concepts and elements of conflict and cooperation in markets' (Ver-

non Smith 1992, p. 275) . But, as went he on to note, he ‘never viewed these austere environments as constituting the corpus of experimental economics’ (Vernon Smith 1992, p. 275). The problem was that the usual form of a game with its rules and payoffs seems to trivialize the institutional arrangements of a real market thereby making any experimental results mostly uninformative particularly with respect to market equilibria. Today, mere applications of game theory do not seem to be playing much of a role in experimental or behavioural economics. The emphasis in experimental economics methodology now focuses more on the task of designing an experiment for a laboratory setting. One can, however, think of the experimental design playing the role of a model of whatever the experiment is attempting to test. And ironically, it can easily be argued that, as such, experimental and behavioural economics involves just a new version the old pre-1980s view of theories and models. Isolating a chosen central behavioural hypothesis of interest is usually the main task for producing an experimental design. But, what I am suggesting here is that an experimental design is a type of model; it is a model designed to be an application of a specific behavioural theory or theoretical proposition (see Boland, forthcoming, ch. 8).

The early days of experimental economics were devoted to testing mainstream institutions used in microeconomic theories. Vernon Smith is a well-known pioneer who in the 1950s and 1960s set about testing basic theoretical propositions in the laboratory rather than in the field. One of his colleagues in the 1960s, Cliff Lloyd, had a plan (unfulfilled due to his death in 1977) to conduct a field test of traditional demand theory based on a particular idea about what it would take to empirically refute demand theory (see Lloyd 1965). His plan was to install an agent in a Hudson Bay Company general store in Labrador, Canada. This agent would simply adjust prices and observe how the customers would respond (Lloyd 1980). For Smith (1962) the laboratory setting allows one to control intervening factors such that any falsification or confirmation can be logically defended.

Today, we find most experimental economics being dominated by those who have followed Smith’s lead. The laboratory is the main venue for generating what are considered to be observable and repeatable evidence or patterns of behaviour. Robert Sugden (2008) considers these to be laboratory produced ‘exhibits’. In this regard, it is easy to see that the experimental designs are actually what would have previously been called models – models being designed for the purpose of adding support for

and giving credibility to a chosen proposition of interest rather than the more common notion of their being instead designed for producing falsifying evidence against a theoretical proposition. So, as it stands, one can design experiments in order to conduct repeatable laboratory experiments either to general credible ‘exhibits’ or one can design them to generate evidence that can be used to refute or at least question traditional economic theories and behavioural propositions. But many experimental model builders are beginning to recognize some unavoidable problems are inherent in the logic of model building. Specifically, there are two non-mutually-exclusive main problems: (1) the questionable ‘external validity’ of any test or of any exhibits produced by an experimental design – just because it works in the laboratory, does not guarantee it works in the external real world – and (2) the logical problem that Smith (2002) identifies – namely, the inherent ambiguity of any experimental test whenever the results depend on the additional assumptions or conditions introduced to conduct the experiments. The latter problem is well known in philosophy of science literature. It is what Smith calls the Duhem-Quine problem. This problem is that, as Smith puts it, ‘experimental results always present a joint test of the theory (however well articulated, formally) that motivated the test, and all the things you had to do to implement the test’ (Smith 2002, p. 98).

The Duhem-Quine problem is an unavoidable because it is a matter of logic. For the purpose of using the force of logic, any model consisting of more than one assumption – all of which are claimed to be true – is really just a compound statement – namely, the conjunction of all of the assumptions. As such, the compound statement is true only if all of the constituent assumptions are true. And, by assuming all of the constituent assumptions are true, one is using the logically valid of the model to be able to claim that any and all statements logically deduced from the conjunction (e.g., a prediction) is true because the compound statement is true as required for the use of logic. Of course, economists use models (and theories) this way to form explanations of observable events. And most important, should a logically (i.e., validly) deduced statement prove to be false, then we know only that at least one of the constituent statements of the model is false. And this is where the Duhem-Quine problem arises. It is because we simply cannot know which of the constituent statements ‘caused’ the false prediction. Is the deduced statement false due to false theoretical behavioural assumptions being tested with the model? Or is it due to the false status of one of the assumptions added in the construction of the model. And even for an application of a particular ex-

periment designed to produce credible exhibits, we cannot know whether any observed behavioural regularity was not a result of those additional assumptions that we would need to make sure the experiment is practical. In addition to the Duhem-Quine problem and the problem of external validity, Smith identifies a third and unavoidable problem that faces any experimental economist. Any experimental model builder who thinks an experiment can be designed to produce exhibits that can be used to 'induce' the truth status of some behavioral propositions or hypotheses are mistaken, again as a matter of logic (see Boland 1982; 2003, ch. 1). As Smith put it, 'Particular hypotheses derived from any testable theory imply certain observational outcomes; the converse is false' (Smith 2002, p. 94). The converse is false because, as David Hume recognized over 200 years ago, it is impossible to prove (by pure induction) that any empirical general (i.e., an 'all') statement (such as 'all decision makers are maximizers') is true by listing a finite set of particular observations that would be true whenever that empirical statement is actually true. Instead, to prove the truth status of a general empirical statement, one would have to prove the non-existence of any conceivable observations that would be logically denied by truth status of that general empirical statement (or in other words, it requires 'proving the negative' which is not possible for an unlimited general statement).

The Duhem-Quine problem is the one methodological problem that almost all experimental economists seem to recognize and recognize that it cannot be avoided. Many experimental economists have even abandoned attempting to straightforwardly refute core theory and now are instead trying to use experiments to provide 'positive evidence of a particular regularity in behaviour' (Sugden 2008, p. 625). Perhaps they are doing this simply to avoid making claims about the truth status of falsifying test or positive exhibits. For this reason Smith thinks that much of recent laboratory work has been about improving experimental laboratory technique. He thinks this 'process is driven by the [Duhem-Quine] problem, but practitioners need have no knowledge of the philosophy of science literature to take the right next local steps in the laboratory. Myopia here is not a handicap' (Sugden 2002, p. 103).

What is most important today is that laboratory experiments are being used to discover puzzles that theorists need to address. The basic methodological idea involved is simple. Even within the limits of the nature of laboratory experiments, microeconomic theory should apply to any individual participants in the experiment whenever that experiment in-

volves making choices. More importantly, whenever an experimental design produces more than a puzzle by producing what appears to be an anomaly, it surely at least indicates that there is work to be done. Maybe some aspects of traditional theory do need to be fixed. I think this is what Smith means by saying that myopia may not be a handicap, and moreover, he is allowing that it still does not avoid the Duhem-Quine problem involved in establishing external validity.

Experiments are designed to control the quality of the evidence that will be produced but of course no single experiment can produce evidence that will be 100 percent accurate. This fact is an obstacle for any attempt to establish external validity. This of course applies to any evidence whether it is concerns a claimed anomaly or just some claimed support for a behavioural hypothesis being examined in the laboratory. Either type of evidence or claim must be qualified since it can only be stochastic – that is, it can only be about the probability distribution of observations. Needless to say, recognizing this at least also puts a limit to any claims of external validity.

As I have been arguing here, an experimental design is a model of some chosen economic behaviour that is supposedly explained by microeconomic theory. As such, can anyone using an experiment to claim that some observed anomaly constitutes a refutation of some traditional theoretical explanations of economic choice behaviour also claim that it carries some degree of external validity? Again, claiming something that has occurred in a laboratory setting has external validity is problematic. And there is what Smith (2002) was warning about in terms of the unavoidable Duhem-Quine problem.

Perhaps the real problem here with on going research in any economic laboratory-based claims about refutations, puzzles or ‘exhibits’ is that they are all too premature. As Smith (2002) noted, laboratory-based research is in effect an ongoing evolutionary process. It involves learning and refining the experimental techniques, particularly those used to test microeconomic theories and models. Although many of the claims, particularly claims of external validity, can be dismissed on the grounds of the Duhem-Quine problem, one can see it leading to improvements in experimental designs. Nevertheless, unlike the experiments conducted in the laboratories of the physical sciences involving inanimate objects like atoms rather than the sentient beings involved in experiments conducted in economics laboratories, whether the resulting improved experimental techniques that Smith sees being fostering might overcome the problem

of external validity is, I think, still at least an open question.

Economic model building from a philosophy of science perspective.

Unlike many decades ago, few if any practicing academic economists today are well versed in the philosophy of science. And worst of all, some might complain, few if any economic model builders today would ever see themselves as a part of an explicit philosophical program – one that methodologists or philosophers might spend any of their time explaining. But economic methodologists have been trying to explain how models are used in economics for many years (see Boland forthcoming, ch.11).

As I noted already and methodologists will note as well, models of the economy have been around for a long time and often the first is credited to Tinbergen and his macro-econometric models about data available to him in the late 1930s (see Morgan 2008). Interestingly, prior to the interest in building econometric models, economic data or economic ideas were represented with abstract objects. For example, there are diagrams such as those in Marshall's 1890 book, *Principles of Economics*, that illustrate market demand and supply curves – the same ones that we still use in beginning economics classes. And when it comes to the macroeconomics still found in textbooks, there is the IS-LM diagrams first created by Hicks. And going much further back, there is a much less well-known physical macro-model created by Irving Fisher (1892). It was actually a model consisting of hydraulics and tubes such that with the functioning of which we could actually see it representing an economy involving a three-good, general equilibrium system in process.

As I have already explained, after I conducted the surveys on my colleagues, it seems that among economic model builders there is a clear difference between the pre- and post-1980 view about what economists think constitutes a model in economics. However, if one were to consider the writings of most philosophers and methodologists of economics (see Hands 2001), one would get the impression that their view of the relationship between economic models and economic theories is stuck in the pre-1980s perspective – just as I was in 1989! The most common view about models is about their role in economics. That view is that models are representations of theories constructed for the purpose of serving as tools or instruments. Such models, particularly empirical models, have been created purely for the purpose of measurement or empirical investigation (see Boumans 2001). This includes those models thought to be

built for quantifying a theoretical hypothesis so as to be able to express the hypothesis in terms of parameters and coefficients that can be ‘measured’ using observable data. This aspect of model building, particularly when it is done for the purpose of testing theories and models – particularly macroeconomic models – is explored in more detail in Part 3 of my forthcoming book.

It should be noted that the idea of a model being used as an instrument is a very old idea, one that existed long before economists began building models. It is the common methodological idea that philosophers of science call ‘instrumentalism’ (see Boland 2003, ch. 1 and 5). While today many now associate the idea of instrumentalism with Milton Friedman’s 1953 view of methodology (as I explained in Boland 1979), it has a much longer history going back at least to the beginning of the 18th Century maybe even further back to the 17th Century’s Cardinal Bellarmino. While the Cardinal was merely suggesting that Galileo should consider his heliocentric view of the planets to be just a convenient instrument (*viz.*, nothing more than a useful mathematical hypothesis), among historians of science the common view of instrumentalism is more due to a later attempt by a Bishop Berkeley who wished to preempt the growing interest in Newton’s mechanics. The problem that the Bishop addressed was that Newton’s theory (and his widely recognized scientific authority) was being used by some 18th Century philosophers to show that the Church was unneeded for understanding the Heavens and Earth (see Becker 1932). What Berkeley wanted was that we should consider Newton’s theories to be limited to mere measuring tools for predicting the movements of planets. If we were to do this, he thought Newton’s theories would not need be considered true. And as such, Newton’s theories should also not be considered competitors for the universal truths that Berkeley’s Church had the responsibility for determining, of course.

For most academics, the role of the Church is rarely if ever at issue. Nevertheless, the idea of economic models being built to serve as instruments clearly does seem to mean that they should be judged only in terms as to whether they successfully do the intended job rather than whether they are to be considered empirically true or false representations of the economy. Instrumentalism in economics and economic model building today is, as I said, simply an application of Friedman’s 1953 methodology. One suspects that all too often in the 1950s and ‘60s many critics of his methodology were to merely eager (dare I say ideologically) to dismiss his methodology given that instrumentalism then as now is alive and well in the business of building economic models, particularly econometrics-

based models. But, the only effective criticism of Friedman's methodology, as I explained at the end of my 1979 article, is to point out that the only consistent defense of his methodology is at best circular. And as such, I think the best that one can say about Friedman's methodology is that it seems to be stuck in the 18th Century.

One methodological complaint about the usual microeconomics textbook theory or model of a decision maker – whether that be an individual consumer or any single agency such as a firm – is that we rarely find a model builder considering a model's behavioural and situational assumptions as literally true. One could even say that there is a tendency to treat the textbook firm in particular as a 'black box'. Specifically, the firm is considered only in terms of measured inputs and outputs. The behavioural assumptions we make to build models are in effect merely about the unseen contents of the black box. But more important, the black box characterization of, say, the model of a firm invites an instrumentalist viewpoint. If the observed outputs of an observed real firm matches (by some acceptable criterion) the model's predicted output, some model builders will ask you whether the truth status of the model's behavioural assumptions even matters. For anyone who would say it does, then they are accepting, at least, the limits to the usefulness of instrumentalism. For those who do not think it matters, what is the alternative if it is not just instrumentalism?

If one does not accept the black box perspective, then perhaps at minimum one is asking instead for some sort of transparent box. We could go as far to say that this is a goal of model builders engaged producing 'exhibits' in experimental and behavioural economics. Their models are simply rejections of the black box perspective for one where observable data are to be our real concern. Of course, it may be all a matter of attitude, particularly whenever instrumentalism can be seen to be an acceptable methodological or philosophical position regarding the contents of the textbook's black box.

Concluding remarks.

Well, I think that the old pre-1980s view of models is alive and well, both in the practice of game theory as well as behavioural and experimental economics even though most young participants will not think so. This is, I think, merely a terminological problem, not one of actual practice of model building in economics.

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