MODEL BUILDING IN ECONOMICS

The failure of the models used in the run-up to the 2008 financial crash raised questions regarding the dependability and suitability of modeling, heightening concern about its role and its limits. In this book, Lawrence A. Boland provides an overview of the practices and problems faced by model builders to explain the nature of models, the modeling process, and the possibility for and nature of their testing. In a reflective manner, the author raises serious questions about the assumptions and judgments that model builders make in constructing models. In making his case, he examines the traditional microeconomics-macroeconomics separation with regard to how theoretical models are built and used and how they interact, paying particular attention to the use of equilibrium concepts in macroeconomic models and game theory and to the challenges involved in building empirical models, testing models, and using models to test theoretical explanations.

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Model Building in Economics

Its Purposes and Limitations

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To Meghan Trentin, the world's greatest granddaughter

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Preface

At one time I mistakenly thought it would be a good time to produce a second edition of my 1989 book on the methodology of economic model building. As I will explain, I changed my mind after I conducted a simple survey of my colleagues and learned that most economists today see model building very differently than I did twenty-five years ago. It became apparent to me that the planned second edition of my 1989 book would be a big mistake. So, instead, I decided to write a different book – one more appropriate for today's economists and students of economics. It might also serve as a lesson for methodologists.

The 1989 book (*The Methodology of Economic Model Building: Methodology after Samuelson*) was directed at methodologists and economic model builders who think they know something about methodology. Actually, the only thing the latter seemed to know at that time was that for one to be taken seriously, one's model must be testable. So in the 1989 book – using some examples of very simple Keynesian models – I set about demonstrating that for all practical purposes an empirical test would require far too many more observations than are possible. For example, for any non-stochastic model that includes a Cobb-Douglas production function, a non-stochastic test using a logical conjunction of exact observations of the variables involved might require a quarter-million observations! That is, the conjunction of observation reports would form a compound statement that constitutes just one possible counter-example – one that would logically refute that model. Of course, a stochastic model would take even more observations.

My original intention for conducting an informal survey of my colleagues was simply to do a little market research. So, I created a set of simple survey questions and went door to door seeking responses. I wanted to know what my colleagues saw as model building in their respective research fields. At the time I thought this would be a simple matter of asking them about the

kinds of techniques used to produce models in their fields and the purposes and limitations of those techniques. My older colleagues seemed to have no trouble understanding what I was getting at, but my younger ones could not figure out what I was talking about, making it difficult to complete my survey. That was 'Plan A' and it turned out seemingly not to be very informative.

So, I moved on to 'Plan B': I retooled my survey by first showing each of my survey subjects a quotation from page 886 of Richard Nelson and Sidney Winter's famous 1974 article on evolutionary economics. Specifically (after adding bold emphasis):

> 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.

I asked my survey subjects if they understood what this quotation said. And again, my older colleagues said they understood perfectly what was being said, but my younger colleagues said they had no clue what Nelson and Winter were trying to say. From what I could figure out (with a little bit of Internet research), the younger-older designation seemed to be centered on whether the subjects did their graduate work in economics after or before about 1980.

I am glad I did this survey because it indicated to me that the planned second edition of my 1989 book would be a complete waste of time. I needed to write a very different book – not one directed at methodologists or at methodologically minded model builders worried about testability. Instead, I decided that I needed to try to bridge this apparent generation gap. But first, I needed to know why the gap exists.

The idea of this gap turns out not to be my discovery. Had I read some recent work by my friend Axel Leijonhufvud [1997, 2006], I would have already known that the words 'model' and 'theory' mean something very different today from what they meant when I was a graduate student. At least I can claim that my informal survey results add support to Axel's empirical claim, which is that model building today is a very different enterprise from what it was thirty or forty years ago. However, I don't think Axel has provided any explanation for this generational gap.

I also found that there is an extensive literature discussing 'theories *versus* models'. Most of it is critical and is almost always complaining about the

dominance of mathematical formalism. As Axel says, the common notion among model builders is that mathematics is just a language – but as he says, so is English, and it too can be useful. Most critics of today's model building go much further and blame mathematics and formalism for what they see as the downfall of modern economics and why they think modern economics is useless. The critics may be right, of course, but their literature is not likely to have any effect on the younger generation. In addition, the obstacle is not the differing meanings of the word 'model' but rather the differing meanings of the word 'theory'.

Coincidentally, during the time I was conducting the survey interviews, a famous game theorist came to my university to give a seminar on his current research. During his talk, he did refer frequently to models and theories, but he did so interchangeably. It seemed that, to this famous model builder, different models were different merely by including *additional* behaviour elements in the form of a new mathematical object or element. In addition, model building seems usually to be done with explicit formal elements – not explicit *representations* of existing non-mathematical elements of a theory, as I discussed in my 1989 book. For a while I had difficulty understanding what he was saying since I was trying to interpret his view in terms of my 1989 book. Surely, I thought, such a famous economist would understand model building as I do. But, clearly, my expectations were wrong. At first this was puzzling until I investigated when he got his PhD and discovered – surprise! – it was after 1980. Of course, this apparent confirmation of my empirical conjecture requires further investigation, but in any case, it appears that my younger colleagues are not so strange after all. That is, today, economists commonly use the words 'theory' and 'model' interchangeably, so it is easy to understand why the younger generation has difficulty understanding the Nelson-Winter quotation and the idea of 'theories *versus* models', as there is apparently no 'versus' possible.

I think I can explain the observed generation gap, but first let me informally review the relevant history. Mathematical economics has, of course, been around a long time. However, in the 1950s and 1960s, model builders saw model building as a project of explicitly *representing* existing theories with the use of mathematics. Paul Samuelson's *Foundations of Economic Analysis* was devoted to demonstrating the usefulness of such a project. And much of his research was devoted to building mathematical models of various theories from our history in order to show how useful mathematics can be. But in the 1970s, things seemed to change. Young economists of the day would call anyone using mathematics 'a theorist'. Moreover, the distinction was no longer between models and theories but rather between

theoretical and empirical research. This is probably a result of the elevation of econometrics to be an essential part of graduate economic education. This generation of new teachers, who may have seen a difference between models and theories but no longer saw it as an important methodological idea, became the teachers of the young generation I am discussing here.

So, to explain the generation gap when it comes to discussing theories versus models, I suspect that it is the teachers and textbooks of the late 1970s that should be blamed. If so, just what is it that these teachers and textbooks did that was different from what was done by the older generation of teachers and textbooks?

Thinking back over all the students I taught in my literature-based graduate classes (which some of my colleagues considered 'philosophical'), I seem to remember that the textbooks they were using in other classes had something in common. All of their theory textbooks had mathematical problems to solve at the end of the chapters – except for those classes taught by my Chicago-oriented colleagues. What were missing in the textbooks were the usual verbal problems that we had been given when I was a student. That is, we always had as our first task to translate a verbal problem into a mathematical problem and only after that to solve it. This younger generation seems to never have had to engage in this first step, but instead to set about just solving the mathematical problems they are given.

If my conjecture is right, we should be able to examine the textbooks before and after 1980 and find different types of study problem, but a little thought suggests this will not be easy. Even if the assigned textbook had study problems of one type or the other, one cannot be sure what kind of problem any given teacher would use. Nevertheless, today, there are really only two or three key textbooks used in graduate microeconomic theory classes. And if you open any of them to the problems or exercises at the end of a chapter, you will find nothing but mathematics-oriented problems. For example, here is a typical exercise that one can find on page 38 of Mas-Colell, Winston, and Green's 1995 textbook:

> Suppose that $x(p, w)$ is homogeneous of degree zero. Show that the weak axiom holds if and only if for some $w > 0$ and all p, p' we have $p' \cdot x(p, w) > w$ whenever $p \cdot x(p', w) \leq w$ and $x(p', w) \neq x(p, w)$.

And from the 2006 textbook by Frank Cowell, page 47:

> For any homothetic production function show that the cost function must be expressible in the form
>
> $$C(\mathbf{w}, q) = a(\mathbf{w})\, b(q).$$

Interestingly, Cowell's book has an introductory chapter all about models and model building with no mention of theories, although there is a reference to 'theoretical models' (as opposed to 'empirical models', presumably).

Graduate textbooks today are probably more a symptom than the cause of the generational gap. As suggested earlier, the switch began in the 1970s when the perspective changed to theoretical-versus-empirical rather than theory-versus-model. But, I think this in turn is the symptom of the increased emphasis on the use of mathematics in undergraduate intermediate-theory textbooks. If we look at how those textbooks changed during the 1970s, we will find an increasing proportion of the problems and exercises being framed as mathematical problems. For comparison, let us consider C. E. Ferguson's 1969 intermediate textbook, which is the one I often used when teaching intermediate micro theory. In Ferguson's book there were only a handful of problems, and these would involve a table of values with which the student was to calculate averages or margins. Of course, at the ends of chapters there were lists of questions – such as this one from page 72:

> In year one, your income is \$2,000; in year two, it is \$4,000. The goods you bought in year one for \$2,000 cost exactly \$4,000 in year two. (*i*) You are better off in year two. (*ii*) You cannot be worse off in year two. Select the proper answer.

Or this one from page 216:

> In the late 1950's, the development of trilevel 'rack' cars for carrying new automobiles substantially lowered the costs of hauling such traffic. This represented (*a*) a change in demand for railroad services, (*b*) a change in supply of railroad services, (*c*) a change in supply of trucking services for new automobiles, (*d*) all of the above.

Interestingly, the only mention of models occurs in a short introductory section on methodology where theoretical analysis is said to precede model analysis.

This observation is not to deny authors promoting the use of mathematics. My colleague, the late Cliff Lloyd, was one of many activists promoting the use of mathematics in economics textbooks, as evident in his intermediate textbook published in 1967. It had no study problems at the end of chapters; instead, he put mathematical analysis in appendices to almost every chapter. The word 'models' appears only in the first paragraph of the introduction and with no explanation of any kind. His book seems to be intended to show how to discuss intermediate theory using mathematics, not to characterize model building. To him, models are just mathematical representations of basic theoretical notions. One can see textbooks such as

this as seeds planted that bore fruit in the perspective seen in the post-1980 view that no longer sees a distinction between theories and models. Yet, in textbooks by other similar activists, such as in Robert Clower and John Due's 1972 textbook, even though microeconomic theory is presented only in mathematical terms, the problems and questions at the end of the chapters still were always verbal problems or questions, for which the task for the student was to transform the verbal problems into mathematics so as to use the mathematical analysis they learned in the chapter. Interestingly, in the case of Clower and Due's book, there is a discussion of models in chapter 1. However, consistent with the pre-1980 perspective, models were the result of a two-step procedure [p. 3]. Specifically, one begins with

> the preliminary statement of a problem to be investigated together with a provisional description of a set of phenomena that are thought to be relevant for analyzing the problem . . .
>
> Having settled on a problem, the next step in the formulation of a theoretical model is to designate as *unknowns* certain variables whose values are provisionally assumed to describe salient features of the (actual or hypothetical) economics system to which the problem relates. We then seek to impose restrictions on these unknowns, usually in the form of explicit or implicit functional relations.

Now, let us by comparison look at a problem that can be found in a post-1980 intermediate textbook such as Hal Varian's [2006], page 70:

> Which of the following are monotonic transformations? (1) $u = 2v - 13$; (2) $u = -1/v^2$; (3) $u = 1/v^2$; (4) $u = \ln v$; (5) $u = -e^{-v}$; (6) $u = v^2$; (7) $u = v^2$ for $v > 0$; (8) $u = v^2$ for $v < 0$.

Exercise questions such as this are often followed by an appendix of more detailed mathematics. Perhaps less surprising, let us consider an 'exercise' from Varian's more advanced 1992 textbook, page 39:

> Let $f(x_1, x_2)$ be a production function with two factors and let w_1 and w_2 be their respective prices. Show that the elasticity of the factor share (w_2x_2/w_1x_1) with respect to (x_1/x_2) is given by $1/\sigma - 1$.

And like Cowell's book, models and model building are discussed but with no mention of any relationship to theories. However, Varian does specify on page 1: 'By a model we mean a simplified representation of reality.'

As I am not sure examining textbooks tells me as much as I would like, I went back to my younger colleagues and discussed my conjectured explanation for the generation gap. They all agreed that in their graduate education they never had to do the old first step of translating a verbal problem or question into mathematics to solve or answer it. They were usually given

problems or questions in mathematical form, such as the ones from Varian's books I mentioned earlier.

Somewhat arbitrarily, I have put 1980 as the watershed, but the current notions of theories and models began much earlier. I remember attending a 'theory conference' at Queens University in 1977. I was there replacing my late friend, the aforementioned Cliff Lloyd. It became clear that, among the participants at the conference, the use of the term 'theory' merely meant any mathematical model. I thought that was strange at the time but dismissed it as merely a sign of immaturity that would soon go away.

As I said at the beginning, I am abandoning my original intention to produce a second edition of my 1989 book. Ironically, I always tell my students that they must always be clear about their intended audience when writing their papers or giving a seminar presentation. Looking back after my recent survey results, I am now wondering who I thought the audience was for the 1989 book. If I thought I was going to enlighten the younger generation, I am sure I failed. Clearly I was not practicing what I preached.

Still, I am convinced that the old way of looking at theories versus models is correct, makes sense, and is certainly more intellectually informative – that is, more about economics *ideas* than about the latest modeling techniques. Nevertheless, one proceeds with the cards one is dealt. So, I have written this book such that it does not matter whether or not a reader would understand the quotation from Nelson and Winter. Throughout, however, I will not pass over any opportunities to discuss the theoretical ideas that lie hidden behind the various models discussed. After all, it is how our theoretical notions change as a result of successful model building that should be the most important thing we should be learning.

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