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ECONOMICS IN PRACTICE: FOLLOW-UP

"Theory" and "Models": Terminology Through the Looking Glass

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ABSTRACT

A COMMENT ON: DANIEL B. KLEIN AND PEDRO P. ROMERO, "MODEL BUILDING VERSUS THEORIZING: THE PAUCITY OF THEORY IN THE *JOURNAL OF ECONOMIC THEORY*," *ECON JOURNAL WATCH* 4(2), MAY 2007: 241-271. LINK.

> 'Model' is a ubiquitous term in economics, and a term with a variety of meanings.
>
> —Kevin Hoover 1995, 33.

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> "When I use a word," Humpty Dumpty said in rather a scornful tone, "it means what I choose it to mean, neither more nor less."
>
> "The question is," said Alice, "whether you CAN make words mean so many different things."
>
> —Lewis Carroll 1871, 188

WHAT DO CONTEMPORARY ECONOMISTS MEAN BY THE TERMS "THEORY" and "model"? Of course, the meaning of those terms is known by every economist, just as Humpty Dumpty presumably knows precisely what he means. But is the term "theory" in fact a matter of broad consensus among economists? Ditto for the term "model." The Hoover epigraph above suggests the answer may be "no."

Klein and Romero's recent (2007) interesting article in this journal sparked

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these questions for us, although the questions are peripheral to their main concerns. They evaluate articles in the *Journal of Economic Theory* for what one might call "intellectual usefulness." They find that many *JET* articles earn very poor usefulness ratings. Regarding these ratings, we agree in general with both the method and the substance.

In reading their article, though, we noted that the Klein-Romero use of "theory" and "model" differed markedly from how we use those terms in a paper on a related topic.[3] In fact, their usage seems one-sided, omitting what we argue below is a widely-used meaning of the term "model." This suggests that divergent usages by economist-authors may be widespread, confronting readers with considerable ambiguity. Our sense is that clarity and understanding can be enhanced by sorting out the competing uses in economics of "theory" and "model." This note takes a step toward such a sorting-out.

Our thesis is that the terms "theory" and "model" are incapable of carrying the diverse characteristics different economists ascribe to them. As a result, descriptive clarity may require perhaps eight separate terms, but only two terms—"model" and "theory"— are currently available and used. A classificatory scheme we propose below should shed light on this possibility, at least for models.

THE KLEIN-ROMERO DELINEATION OF "MODEL" AND "THEORY"

What do Klein-Romero mean by "theory" and "model"? They define a model as follows:

> By "model" we mean a system of functions and conditions that yield formal results, such as classes of equilibria within the model. The specific type of model-building that has been central to 20th century economics is a mathematical system of "agents" who maximize explicit functions subject to constraints, yielding equilibria. As many have noted, it is a kind of story-telling…. Nowadays, the term "model" is generally used by economists to mean a formal, explicit system using mathematical representation. That is how we use the term here. (Klein and Romero 2007, 243-244)

How is the term "theory" related to "model" – and what else should these terms be related to? The authors spell out three of their concerns about the usage of these terms. First, they cite Leijonhufvud's observation that the terms "models" and "theory… have been widely used as interchangeable in the profession" (Lei-

3 We examined several economic literatures in order to evaluate whether a fruitful dialogue exists in economics between conceptual analyses ("modeling"/"theory") and empirical work (Goldfarb and Ratner 2006).

jonhufvud 1997, 193). They reject this equating of the two terms, asserting that a "model is neither necessary nor sufficient for theory"[4] (244). A second related point is that, in economics, the term "theorist" usually means "model builder." Klein-Romero also reject this conjoining, since it suggests that "Hume, Smith, Marx, Menger, Keynes, Coase, Schelling etc etc did not do theory" (244).[5] Third, they argue that a theory involves more than technical/analytical desiderata: "[S]cientific culture understands theory to entail requirements of importance and usefulness"[6] (244).

In sum, after defining "model," Klein-Romero argue that "theory" has a higher normative status than "model." Moreover, a theory does not require a "model" and a "model" is not sufficient for a "theory." However, they leave the term "theory" undefined. They do specify three requirements a model must meet in order for them to deem it a theory:

1. The model is "an at-least-partial or potential description of the conditions and mechanisms giving rise to X," where X is "some real-world phenomena." They call this the "Theory of what?" criterion.
2. "The proponent believes and tries to persuade us that X is of import" and is inadequately understood, so there is "[a] need for better explanation." This is the "Why should we care?" criterion.
3. "The proponent makes a case that his explanation merits attention and resources." This is the "What merit in your explanation?" criterion.

Notice that this set of requirements for a *model-based* theory has three characteristics: First, "models" are theory wannabes**.** Only a really good model gets promoted to theory status.[7] Second, the idea that "theory" might operate at a very general level, while "models" might be specific *applications* of a theory (a theoretical framework) is missing. Third, this usage makes no allowance for the possibility that models are sometimes (though not always) *a link between theory frameworks and the activities of empiricists***.**

4 We agree that the terms need to be distinguished.

5 Not treating the terms as equivalent seems sensible. On "who is a theorist," compare Robert Solow (1997), whose viewpoint we discuss extensively below. "Keynes more or less invented macroeconomics. He was not much of a model builder himself" (49). Solow clearly considers Keynes a theorist but not "much of" a model-builder.

6 We are not persuaded that the understanding of "theory" that Klein-Romero ascribe to "scientific culture" is anywhere near universal. As we hope to show, scientific culture (at least among economists) encompasses different groups ("subcultures") that are likely to have differing interpretations of "theory." These several conceptual interpretations coexist, however uneasily. More broadly, "theory" and "model" bear more meanings than in the Klein-Romero universe.

7 We are reminded of the Star-Kist Tuna commercial of several years ago. Charlie the Tuna wants to be a Star-Kist tuna, but he is rejected because he is not good enough for Star-Kist ("Sorry, Charlie.…"). So models in the Klein-Romero view *aspire* to be theories. But to succeed they must meet the Star-Kist test: if a model is not "good enough," it **cannot become** a theory ("Sorry, Model….").

We suggest that these three characteristics of Klein-Romero's usage are not shared by, and appear antithetical to, substantial and important uses of the terms "model" and "theory" in economics. We develop this argument using two examples: (i) Robert Solow's use of the term "model" in an article that tries to explain to noneconomists what economists do; and (ii) economists' widespread use of the general terms "price theory," "game theory" and "growth theory"; these terms have implications for delineating one concept of "theory" and for the model/theory distinction.

HOW ECONOMISTS USE "MODEL" AND "THEORY" —TAKE ONE: EXAMPLES FROM SOLOW

A use of the terms "theory" and "model" quite different from Klein-Romero's has been espoused by Robert Solow:

> Today, if you ask a mainstream economist a question about almost any aspect of economic life, the response will be suppose we model that situation and see what happens.…A model is a deliberately simplified representation of a much more complicated situation…. The idea is to focus on one or two causal or conditioning factors, exclude everything else, and hope to understand how just these aspects of reality work and interact. There are thousands of examples; the point is that modern mainstream economics consists of little else but examples of this process. (Solow 1997, 43)

Solow develops three illustrative examples, which are instructive for comparing his view of 'model' to Klein-Romero's: modeling the effect of taxation on the willingness to work; explaining inventory fluctuations as an ingredient in understanding business cycles; and explaining trade patterns among nations. Notice that each of these modeling efforts is aimed at shedding light on an arguably important *empirical phenomenon*.[8, 9]

8 In Solow's view, what makes for a "good" model is the amount of "understanding" generated: "A good model makes the right strategic simplifications. In fact, a really good model is one that generates a lot of understanding from focusing on a very small number of causal arrows…."

9 This linkage to important phenomena is a point of contact between Solow and Klein-Romero as well as a point of difference. Solow relates 'models' and modeling to a focus on an important empirical phenomenon. Klein-Romero say that a model is close to being a theory when the model meets the "theory of what?" test—when it attempts to illuminate or explain an empirical phenomenon—and when the model meets the "why should we care?" test—a measure of the importance of the phenomenon modeled. However, unlike Solow, who seems content with treating models of important empirical phenomena simply as models, Klein-Romero are interested in rewarding models that pass these two tests—plus a third—with the label 'theory.'

Recall Klein-Romero's description (quoted more fully above) of a model as "a mathematical system of 'agents' who maximize explicit functions subject to constraints, yielding equilibria.…. Nowadays, the term "model" is generally used by economists to mean a formal, explicit system using mathematical representation." In contrast, Solow argues that this particular kind of mathematical representation is *not a requirement* for a model. A model can instead, for example, be described "in diagrammatic form" (46). Note that this point about diagrams is perfectly consistent with standard classroom usage. When teaching introductory or intermediate microeconomics and putting the simple demand-supply diagram on the board, many of us describe that diagram as a *model* of price-quantity determination in a market.

The Solow discussion is antithetical to the Klein-Romero idea that models typically strive—or should strive—to become theories. The kinds of models Solow describes are instead attempts to illuminate specific economic phenomena, and in Solow's view are provoked by the growing availability of data, and the puzzles that data present:

> I have a different hypothesis to suggest—that technique and model-building came along with the expanding availability of data, and each reinforces the other. Each new piece of information about the economy, especially if it is quantitative information, practically sits there and asks for explanation. Someone will eventually be clever enough to see that it is now feasible to construct a model. Reciprocally, alternative models have to compete…They compete on the basis of their ability to give a satisfying account of some facts. Facts ask for explanations, and explanations ask for new facts. (Solow 1997, 47)

So what is the relationship between the kinds of models Solow describes and the notion of theory in the Solow discussion? He is at pains to argue that economics is in the main *not* mere mathematical formalism. It is "technical," but not "formalist." He sees "formalist theory" as mathematical formalism, largely unrelated to the modeling activities that occupy the majority of applied economists:

> The past 50 years has indeed seen formalist economics grow and prosper. But it has not grown very much. Only a small minority within the profession practices economic theory in this style. To tell the truth, not many more pay any attention at all to formalist theory. Generally speaking, formalists write for each other. The formalist school contains some extraordinarily able people, and of course attracts economists who not only are talented at economics of a certain kind but enjoy it. It is not surprising, therefore,

> that outsiders think that there is a lot of formalism in economics, just as half a cup of blood spread around a bathroom makes it look like a scene from *Psycho*. Nevertheless, it is an illusion. Modern mainstream economics is not all that formal. (43)

From this point of view, Klein-Romero's proposal to rename the *Journal of Economic Theory* as the *Journal of Economic Model-Building* is misguided. A more appropriate title-change might be the *Journal of Formalist Economics*.

In summary, two things distinguish the Solow description of models from the Klein-Romero view of "the specific type of model building that has been central to 20th century economics...a mathematical system of 'agents' ." First, *the kinds of models Solow describes are in large part attempts to shed light on specific empirical phenomena***.** While in these models the manner of exposition and much of the reasoning is often mathematical, the motivation behind the models is to make better sense of some feature of the observed world. Second, models as described by Solow do not aspire to be "promoted" to theories, since they have a much more concrete, limited, and specific purpose. Note that the kind of model Solow describes fits the first item in Klein-Romero's list of what a *theory-worthy* model would contain ("a description of….some real world phenomena"). It does not, however, fit the way those authors describe the *actual* modeling "central to 20th century economics."

We certainly are not denying the existence of the kind of (formalist) modeling from which Klein-Romero seek to remove the verbal crown of 'theory'. Instead, we are noting that a major subset of modeling activity—the kind described by Solow as *typifying* what much of what goes on in economics—is omitted from the Klein-Romero characterization of what they see as "central to 20th century economics." A commenter on an earlier draft of this paper suggested describing the two kinds of models as "applied" and "unapplied." We return later in the paper to this issue of how to categorize these disparate classes of models.

What about the relation between "model" and "theory"? Solow does not really address the *general* relationship between models and the unmodified term "theory." Instead, a particular kind of theory—"formalist theory"—is dismissed as basically unconnected to modeling. Note that classifying a subset of theory as "formalist" is consistent with the hypothesis posed at the beginning of this article that terms like "theory" and "models" are overworked. Solow modifies the term "theory" to limit it, carving out a subset of theory by adding the adjective "formalist."[10]

10 In a methodological survey of "Economics at the Millennium," Goldfarb and Leonard (2002) present a taxonomy of types of economists. They describe pure theorists as "in the business of logically deducing the implications of a set of behavioral axioms taken as fundamental (Hahn in *Economic Journal* 1991, 47), an enterprise describable as Euclidean in spirit… Pure theorists prove theorems and lemmas. Most of game theory is of the pure theory type. The connection between pure theory and the economy runs from tenuous to none. At its most archetypal (for example, the Nobel-prize-winning 1959 work of Gerard Debreu) pure economic theory does not even purport to have empirical consequences. As theorist Ariel Rubinstein puts it… 'pure theory does not pretend to predict or advise… the most [it] can

So Solow does not really address the model-theory relationship, while Klein-Romero seem to view models as theory wannabes. In the next section, we propose one *possible* relationship between models and theory that we think is both plausible and consistent with one widespread usage.

HOW ECONOMISTS USE "THEORY" AND "MODEL" — TAKE TWO: THEORY AS ENCOMPASSING VARIOUS MODELS

Our thesis in this section is simple*: A widespread use of "theory and "model" is that "theory" is a broad conceptual approach— as in "price theory"— while "models," typically in mathematical (including graphical) form, are applications of a theory to particular settings and/or represent explorations of different sets of assumptions conditionally allowable by the theory approach.* Thus, for example, the term "price theory models" has a fully understandable and standard meaning.

Support for this usage is found in the titles of classic graduate-level texts: Milton Friedman published several editions of his microeconomics lectures, and titled various iterations *Price Theory: A Provisional Text* and *Price Theory*. George Stigler published several editions of *The Theory of Price*. Gary Becker's lectures on microeconomics at Columbia, as originally transcribed by his students, were published as *Economic Theory*. More recent graduate texts include Andreu Mas-Colell, Michael Winston, and Jerry Green's *Microeconomic Theory*. Each of these books contains discussions of *various models that apply price theory* in particular contexts. Indeed, almost any price theory text will propose different models of monopoly behavior, monopsony, etc. For example, in Becker's published lectures, he sets out a *model* of an irrational consumer and compares the implications of this *model* for the existence of market demand curves to the implications of models that assume rationality.

As for "price theory models," so for "game theory models" or "growth theory models." Such models are specific applications of a general theory framework. The notion of a specific game theory model of duopoly is, we believe, self-evidently understandable within the economics profession. The same is true for the term "Cournot model." One of us took a course entitled "Growth Theory" in graduate school many decades ago. The instructor, a very distinguished economist, proceeded class after class to set forth various competing growth theory models. All the models were part of growth theory, but some models were far more "attractive" than others.[11]

This idea can be illustrated more concretely with the example of human

do is to clarify the concepts we use' " (Goldfarb and Leonard (2002, 22).

11 On one occasion, one of the co-author's fellow students was extremely frustrated by what he viewed as the ridiculous assumptions required for one of these models. In a memorable line, he left the class, muttering under his breath, "It's your model, you play with it." Some growth theory models were "better" than other growth theory models.

capital theory, a general approach to modeling individuals' decisions to "invest in themselves." That approach has led to a major reorientation in the issues on which labor economics focuses. Its development is associated with major contributions from Gary Becker and Jacob Mincer. Within human capital theory are numerous specific models. A particularly influential one was Mincer's so-called "simple schooling model." That model tried to explain variation in incomes using *only* variations in schooling. Later models, provoked in part by the possible inadequacy of considering only schooling, added other explanatory variables meant to proxy, for example, on-the-job training. There are in fact numerous models that fall under the heading "human capital theory models."

Note that these models often involve an attempt to apply the general insights of human capital theory in a way that *allows confrontation with actual data*. Sherwin Rosen wrote an appreciation of Mincer's major contributions to the human capital theory literature that appeared in the *Journal of Economic Perspectives* (1992). Rosen noted that a standard empirical procedure in labor economics is now widely known as "Mincering the data." Recall that the idea of "models as a link from theory to data" is absent from, or at least not obvious in, the Klein-Romero concept of models.

So one possible, and we believe widely-used, way of conceptualizing "theory" versus "models" is that "theory" represents a general approach, while models are ways of specifying and applying that approach to more focused situations. Notice further that our use of the terms is descriptive, not normative or evaluative. Particular theories and models may be attractive ("good") or unattractive ("bad") but the title of "theory" by itself has no strong normative connotation in this usage.

Many examples are available showing that the just-described usage of "theory" and "models" is common when economists actually do economics. The appendix contains four specific examples.

WHAT IS TO BE DONE? — A TAXONOMY OF MODELS

We suggested above that the term "model" (and, analogously, the term "theory") may be unable to carry all the weight of competing possible interpretations. We explore this idea further by suggesting a number of related-but-differentiated meanings associated with the term "model." Consider first two broad but mutually inconsistent categories of models.

1. Abstract, formal theory model/ evaluative-interpretive " toy" model/ "unapplied" model.

The Arrow-Debreu model is an example of an abstract, formalized, mathematical representation of production, exchange, and consumption. Such a model

is not intended to refer to any particular economy in historical time, nor is it intended to yield testable predictions about variables of interest. Instead, it is meant to illuminate the logical implications of certain primitive concepts, such as whether a competitive equilibrium exists as a logical matter, to be established by mathematical reasoning. This conception appears to be consistent with the sense of "model" in Klein-Romero: see the quotation early in this article, which describes what they mean by "model."

In his discussion of the term "model" as having a variety of meanings, Kevin Hoover (1995) suggests a related but not precisely identical category: "evaluative or interpretive models" (which he opposes to "observational models").[12] He goes on to describe a subclass of evaluative-interpretive models, so-called "toy models:"

> A toy model exists merely to illustrate or to check the coherence of principles or their interaction. An example of a toy model is the overlapping-generations model with money in its simplest incarnations. No one would think of drawing quantitative conclusions about the working of the economy from it. Instead one wants to show that models constructed on its principles reproduce certain qualitative features of the economy and suggest other qualitative features that may not have been known or sufficiently appreciated. (33)

This kind of model is "a testbed for general principles" (33). As noted above, a commenter suggested the general term "unapplied model" to encompass this category.

2. Models of observed phenomena/"applied" model.

John Sutton provides the following quote from John von Neumann[13]:

> By a model is meant a mathematical construct which, with the addition of some verbal interpretations, describes observed phenomena. The justification of such a mathematical construct is solely and precisely that it is expected to work (Sutton 2000, 35).

12 In a previous footnote, we cited Solow's view that Keynes, who "more or less invented macroeconomics," was not much of a model builder. Solow goes on to say "*The General Theory* was and is a very difficult book… It contains several distinct lines of thought that are never quite made consistent. It was an extraordinarily influential book…but we learned not as much from it…as from a number of explanatory articles …(that) reduced one or two of those trains of thought to an intelligible *model*, which for us became 'Keynesian economics' " (48). This rendition is consistent with the Hoover notion of some models as explanatory-interpretive.

13 Sutton does not provide a specific citation for the von Neumann quote.

This concept of "model," which stresses the tie to empirical phenomena, is clearly inconsistent with the prior "abstract formal theory model" concept. It is related to and consistent with Mary Morgan's (1998) idea of models as "mediating" between theories and data. Kevin Hoover (1995) uses the term "observational models." He further notes that "[O]ne commonly speaks of an econometric model. Here one means the concrete specification of functional forms for estimation" (1995, 33).

While the two above categories are mutually inconsistent, a third category includes a different expositional mechanism consistent with both of the above categories:

3. Diagrammatical iterations of both of the above model types.

Graphs are in fact mathematics, of course, but common usage sometimes inaccurately distinguishes "diagrammatic" from "mathematical." Both of the model categories described above can in some cases be presented diagrammatically rather than "mathematically in the nondiagrammatic sense." Simple supply-demand diagrams are a staple of economics pedagogy and frequently show up in articles. Ditto for indifference-curve or isoquant analyses.

Liebenstein's 1950 article, "Bandwagon, Snob and Veblen Effects in the Theory of Consumers' Demand," works almost entirely with diagrams. Francis Bator wrote two historically important articles developing welfare economics; one of them, entitled "The Simple Analytics of Welfare Maximization," is a marvelous, largely diagrammatic exposition. Samuelson's "A Diagrammatic Exposition of a Theory of Public Expenditure" is yet another historically important example. The fact that these articles are "old" does not change the fact that models of both of the above types can be developed graphically; their vintage testifies instead to a change in style of exposition in economics in recent decades.

This change in style—the fact that many articles are now largely "mathematical in the nondiagrammatic sense"—should not obscure the fact that graphical analysis is central to some modeling applications. For example, one's understanding of the complexity of the Earned Income Tax Credit (EITC), and how its effects must be analyzed, is immeasurably improved by a diagram showing the complex way in which the EITC rules change the individual's work-leisure constraint. The same constraint can of course be expressed algebraically, but the complexities required for the analysis are much harder to intuit and keep straight without the diagram.[14, 15]

14 One of us recently co-authored an article analyzing alternative motivations for dieting. The model uses u-shaped indifference curves (and even circular indifference curves) between weight and food. Additional pounds become a "bad" beyond the individual's desired weight, giving rise to the u-shaped indifference curves. The important (linear upward-sloping) constraint comes from a biological relationship, well-documented in the physiology literature, between weight and food intake. Different motivations for dieting are set off by different life events or by the endogenous effect on the constraint from aging. The analysis is

Having specified three broad categories of models, we propose some additional less general subcategories, each with examples.

- *"Conceptual orientation/technique" models.* These types of models are recognizable by the conceptual orientation they embody and the technique(s) they employ. In some, the conceptual orientation is a more marked feature, in others, the technique. Most if not all of the following are distinguished by both elements: Behavioral economics models, game theory models, econometric models, structural/reduced form models, calibration models, computable general-equilibrium models, input-output models/linear programming models, two- (or multi-) sector models, models of bounded rationality, Austrian school models, rational expectations models.

- *Substantive area/problem category models.* These include human capital models, growth theory models, business cycle models, overlapping generations models, market structure/oligopoly/dominant firm models, the kinked demand curve model, entry-deterrence models, labor supply models, public goods models, capital-asset pricing models, options pricing models, tax incidence models, exhaustible resource models, common pool models, inventory models, information cascade models, firm location models, fishery models, epidemic models, models of altruism.

- *"Named" models.* Examples include Harris-Todaro models, Harrod-Domar models, Tiebout models, Phillips curve models, Mincer's simple schooling model, Schumpeterian models, the Cournot model, the Stackelberg model, Hotelling models.

These three categories are in increasing order of specificity, so that, for instance, named models will each belong to one of the entries in the previous two subcategories. Thus, Hotelling models are instances of firm location models. Furthermore, models in these three subcategories do not necessarily fall neatly into one and only one of the previous three general model categories ("abstract, formal theory model," "models of observed phenomena," etc.). As a result, complex cross-classifications are possible.

Our discussion of models versus theories is "economics home-grown" in the sense that it represents our generalizations and inferences about the use of these terms in economics from our perspective as economists. However, a dif-

entirely diagrammatic and much easier to intuit because it is diagrammatic. See Goldfarb *et al.* (2006).

15 A commenter on an earlier draft of this paper suggested the following provocative hypothesis for the decline in diagrammatic analysis. When doing comparative statics, one can in fact derive results from diagrams (this is consistent with the example cited in the previous footnote). Recent economics, however, has a very large component of dynamic analysis. It is often impossible to *derive* useful results about dynamics using diagrams, though diagrams may be usable to *illustrate* some dynamic results.

ferent, far broader perspective on our subject is offered by the philosophy of science. This involves considering how philosophers of science who focus on physics, biology, etc. conceptualize the terms 'theory' and 'model.' Unfortunately, the arguments of philosophers of science are sometimes complex, expressed in terms internal to their discipline, and not easy to apply to economics. For excellent discussions of work in the philosophy of science and its implications for economics, see Mary Morgan (1998) and W. Wade Hands (2001, especially 343-352). Morgan notes that:

> Older treatments in the mainstream philosophy of science defined models in terms of their logical and semantic connections with theories, where the later are the real focus of interest. (The topic has also been beset by definitional changes which hinder attempts at simple exposition). Thus the conventional account from the logical positivist tradition defined theories as uninterpreted formal systems: sets of sentences in a formal language characterized by its syntactic structure (such as an axiomatized system). An interpretation constitutes a model of the theory if and only if all the sentences in the model are also true in the theory (the formal system). This account of models has not proved very useful in the philosophy of economics. (316)

However, the description Morgan provides of the work of the philosopher Nancy Cartwright bears a similarity to the "home-grown economics" view of models we have espoused above. As Morgan describes Cartwright's position:

> In Cartwright's account, models are idealizations or approximations in the sense that they are only partially realistic accounts of the world, but the descriptions they offer are sufficient to describe certain aspects of the phenomena. They are also required to map onto the mathematical representation of the fundamental theory, although…not necessarily in full… Although the idealization literature focuses on theorizing, in effect models appear inevitable in this procedure: models can be identified as things you get as you idealize towards theory away from reality or concretize from idealized theory to the economic reality. Models are not of the theory or of data or of phenomena… rather they form a middle element between theory and the world, incorporating different degrees of both. The process of modeling becomes the main activity of both economic theorizing and economic application. (318)

It is also worth noting Morgan's (2002) use of the following phrase: "this

mid-century…way of fitting theories to the world via models" (7). That is, in this view, and for one type of model, models *fit theories to the world.*

Conclusion

This discussion has yielded several results:

1. Klein-Romero's attempt at terminological reformation regarding "theory" faces a hard slog, given the facts of how the term is used among economists. Specifically, treating theories as models that in certain respects are desirable/preferred while regarding "non-theory" models as negative—as merely theory wannabes—does not jibe with these two terms' common usage in modern economics. Models that meet Klein-Romero's three criteria may well have greater merit than those that do not. Nonetheless, economists' usage of "theory" is more catholic than would be permitted by their three criteria. In fact, this last point underlines a difference in purpose between Klein-Romero and us: their stance is prescriptive. They want economists to reform their use of the term "theory." Our stance is mostly descriptive: we are interested in how economists use that term and the range of its usage. To the minimal extent that we are prescriptive, we suggest ways to enhance clarity of communication among economists.
2. There are models and then there are models. A number of economists distinguish between two types of models: those that involve abstract theorizing, largely devoid of empirical referents and empirical implication, and those that attempt to connect or "mediate between" theory and data. We consider this an important and fruitful distinction, in part because it enhances the quality of economists' communication. Our 2006 paper relies on that distinction; we think that incorporating it would enhance Klein-Romero's interesting analysis and argument.
3. The term "model" seems to be overburdened and hence incapable of conveying the meaning the user intends. We noted above a distinction between two types of model ("abstract" or "interpretive/toy" or "unapplied" versus "empirically oriented" or "observational" or "applied"). In fact, the term "model" has many and varied uses in economics, some antithetical to others. Consequently, to foster effective communication, categories are needed that distinguish different types of models. We suggested above one very tentative taxonomy.
4. While our view of modeling is "home-grown" in the sense that it stems from observing what economists do, it seems consistent with at least one view from the philosophy of science associated with Nancy Cartwright.

APPENDIX: SOME SPECIFIC EXAMPLES OF HOW THE TERMS "THEORY" AND "MODEL" ARE USED WHEN ECONOMISTS DO ECONOMICS

Sutton's "Taxi Supply-Demand" and Auction Model Examples. John Sutton's Gaston Eyskens Lectures at the University of Leuven were published by Leuven University Press and MIT Press in 2000 under the title *Marshall's Tendencies: What Can Economists Know?*. Sutton asks, "Is it possible to find economic models that work?" (xvi). His treatment of this question provoked considerable interest. One indication of this interest is that the April 2002 issue of *Economics and Philosophy* contains a symposium on Sutton's views, with contributions by two of the major writers on economic methodology, Kevin Hoover and Mary Morgan, and three well-known econometricians, including Franklin Fisher.

While Sutton does not explicitly address the "models versus theory" issue, several of his examples contain an implicit and useful-for-us view of that relationship. Consider first his "taxis-at-airports" example, provoked by his observation on a visit to San Diego that there were long lines of taxis waiting for passengers. His taxi driver:

> Counted on only four fares a day with a two- to three-hour wait each time. It wasn't hard to figure out what had gone wrong. The city fathers, responding to the prevailing fashion for "deregulation," had abolished restrictions on the number of licenses. Fares remained about the same as before…and new drivers entered the business[until income was driven down to that of alternative occupations.] (Sutton 2000, 2)

Later in the book, Sutton considers what model might explain why fares failed to drop. His point is that:

> the elementary competitive model of supply and demand is the wrong model here for one of its key assumptions is that consumers enjoy full information on rival firms' prices. For the taxicab market, this is rarely a good assumption. In the case of San Diego, it is badly wrong. The appropriate model for this market is one that distinguishes two groups of consumers, "informed" and "uninformed." (88)

The implication of this example for "theory versus models" is that "price theory" *applied to a particular market and circumstance generates alternative models* for explaining the behavior of that market.

A second Sutton example involves game theory applied to auctions:

> During the past ten years, the study of auctions has attracted an unusual degree of interest among applied game theorists. One reason…lies in the fact that, in an auction, the rules of the game are specified explicitly, so we are close to knowing the true model of the situation. It is not fully known, however, since we do not usually know the value each bidder places on the item, nor is this information available to rival bidders. (47)

Sutton then describes a setting in which this "not fully knowing" problem is minimized: bidding for drilling rights in offshore tracts. He then describes the results of modeling *this specific case*.

As in the taxi example, the point here is that game theory generates the analysis of auctions. But the application of game-theoretic auction theory to concrete cases involves the need for a *model* of each concrete case.

Theory versus Models of Price-Quantity Determination. Provoked by reading a previous draft of this paper, a colleague offered the following interpretation of theory versus model, a reading complementary to the Sutton taxi and auction examples. The colleague suggested that price theory implies that prices and quantities are determined by the interaction of supply-side and demand-side factors. Note that this general description is broad enough to include a variety of market structures, not just perfectly competitive markets. A *model* of price-quantity determination in a specific assumed-close-enough-to-competitive market would involve specifying actual demand and supply functions for that market.

Lind's Analysis of Rent Control Models. Hans Lind (2007) presents an analysis of a series of eight models of rent control that appeared between 1997 and 2003 in several of the major urban, regional and real estate journals. Each model presents an analysis in which rent control may lead to Pareto improvements because of special conditions in the housing market or special provisions of the control legislation. He criticizes these modeling efforts, arguing that they add nothing to our knowledge of the actual effects of rent control, in part because each analysis fails to provide telling empirical evidence that the conditions postulated by the model hold in a number of actual local markets.

Lind's analysis is relevant to our issue of models versus theories. The existence of a series of models of rent control differing in specific assumptions about "the world" illustrates yet again the idea that models are specific applications to specific (often, market) situations on which the analyst is trying to shed light. The models Lind describes are typically applications of microeconomic *theory*.[16]

16 One could in fact coin the term "rent control theory," which would be a category containing and organizing a series of alternative models of rent control. Such a category would be analogous to our use above of the term "game-theoretic auction theory." To continue the analogy, this theory category would be "price-theoretic rent control theory." An interesting question is: Under what conditions would this kind of intermediate category be helpful, in the sense of adding value beyond what the term "price-theoretic rent control models" tells us?

A Modeling Interpretation of the Leontief Paradox. Bledin and Shewmake (2004) have proposed the following modeling interpretation of the relation between the Heckscher-Ohlin framework and the Leontief Paradox:

> Once the input-output model represents the American economy in this way, Leontief can apply foreign trade statistics to measure the factor requirements for US international trade.
> In providing this measurement, the input-output model mediates between Heckscher-Ohlin Theory and the world. While the Heckscher-Ohlin Theorem suggests that a capital abundant American economy will export capital-intensive goods, international trade theory provides no mechanism to assess this conclusion. Nevertheless, Leontief's input- output model 'enables us to narrow the frustrating gap between theory and observation' (Leontief 1953, 67) by facilitating an empirical test of the Heckscher-Ohlin Theorem. (468)

Bledin and Shewmake show that a test of a theoretical proposition, the Heckscher-Ohlin Theorem, is made possible by the input-output model, a model not even from the same international-trade-theory-framework that generates the theorem. Once again, a model is used to connect a theory-result to the empirical world.

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