

# THE PRESENT STATE OF CONSUMER THEORY

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Second Edition

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Timothy P. Roth

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**Timothy P. Roth**

**A.B. Templeton Professor and Chairman,  
Department of Economics and Finance,  
The University of Texas at El Paso**

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To my parents, Hildegard and Conrad

## PREFACE TO THE SECOND EDITION

The consumer theory literature continues to burgeon. Generally speaking, the methodological issues addressed in the first edition have been unattended. Classical rationality continues to be the dominant behavioral assumption, with stable preferences either assumed or taken to be a technical datum. This conception of the consumer's decision environment underlies the bulk of both the theoretical and the empirical work, whether econometric or experimental. This literature has been incorporated into the second edition. More important, the second edition is distinguished by the addition of a chapter on the New Institutional Economics. While demand theorists have generally not embraced the notion of cognitive limits, the new institutional economists regard economic agents as "intendedly rational, but only limitedly so." Proponents of the new approach seek to deepen our understanding of the processes of production and exchange and, pari passu, to expand the reach of neoclassical theory. Central to all of this is the notion that, in a world characterized by "effective commodities" and "opportunism" bounded rationality limits "are quickly reached". Economic agents are neither nonrational nor irrational. Rather, they are confronted with an enormously complex decision environment in which economic agents' cognitive abilities affect both the arguments of their objective functions and their perceived opportunity sets.

The central message of the first edition is echoed in the second: The evolution of economics as an empirical science hinges importantly on the employment of realistic generative and auxiliary assumptions. Practitioners of the new institutional economics understand this. Those who are concerned with the empirical confirmability (disconfirmability) of demand theory are well advised to adopt the same perspective.

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I

Some Thoughts on Models  
and Model Building



Methodological discussion, like calisthenics and spinach, is good for us.....

Paul A. Samuelson

## INTRODUCTION

That much has been written about the theories of utility and demand is a brute fact familiar to all economists. What is also clear even--perhaps particularly--to the dispassionate observer is the fairly homogeneous character of much of the work:

"... it would seem that any observer must be struck by one central characteristic of the received work on the theory of the consumer - the tendency of so many studies to accept the objectives and basic preconceptions of earlier models without much change . . . the main thrust of postwar writing has been toward the extension and refinement of the classical theory of demand that has come down to us through Marshall, Hicks, et al" [Ekelund, Furubotn, Gramm 1972, p. 57].

This is not to say that theoretical and empirical work has been confined solely to stretching the analytical limits of the traditional or ordinal utility analysis. Neither should a suggestion be broached that the received doctrine has been devoid of intellectual or "scientific" achievement. Theoretical lacunae have been at least tentatively filled, and new frontiers have been penetrated via systematic extensions and revisions of the traditional model. I have in mind, for example, the revealed preference approach due to Samuelson (1938); a formulation which, while logically equivalent to the ordinal utility theory [Houthakker 1950], nevertheless shed light on the logical foundations of demand theory.<sup>1</sup> Other examples abound: The work on the indirect utility function [Hicks 1956, 1958] which has led, inter alia, to important new developments in the theory of index numbers [Theil 1975]; the utility tree formulation and the literature derivative of it [Strotz 1957; Sono 1961; Green 1964; Muth 1966; Furubotn 1967, 1974]; the Lancasterian "New

Approach" [Lancaster 1966], and the work on choice under uncertainty pioneered by von Neumann and Morgenstern (1947), Friedman and Savage (1948) and others. More recently, Stigler and Becker have proffered an intriguing hypothesis relating to the intertemporal stability of preference structures; an hypothesis which, while empirically untestable, has important implications for empirical work [Stigler and Becker 1977].

While the list is by no means exhaustive, it is heuristic. Much interesting and important work has been done. It remains true, however, that

"(an) examination of demand theory indicates, if anything, that great effort remains to be applied in this area which is so fundamental to the entire corpus of economic theory and policy" [Ekelund, Furubotn, Gramm 1972, p. 93].

Granting this, my purpose here is to undertake a critical evaluation of the received doctrine and to proffer, based upon this evaluation, a menu for future work.<sup>2</sup> I shall argue that there is a clear need for a reorientation of effort in this important area; a reorientation that is, moreover, technically feasible.<sup>3</sup>

While the catalysts to this effort have been many, Professor Knight's elegant statement has contributed most to the warp and woof of the final product:

"The treatment of demand is the branch of economic theory in which methodological problems are most important and most difficult. This is because it is here that behavior facts are most inseparably bound up with motivation and that objective data call most imperatively for interpretation by subjective facts and meanings" [Knight 1944, p. 289].

Having come this far, it is important to indicate the nature of the evaluative criteria to be employed. As a general rule, I shall not be concerned with either the mathematical manipulations or the deductive processes inherent in the extant models. The efficacy of this approach follows from the compelling logic of Professor Leontief:

"In the presentation of a new model, attention nowadays is usually centered on a step-by-step derivation of its formal properties. But if the author - or at least the referee who recommended the manuscript for publication - is technically competent, such mathematical manipulations . . . can . . . be accepted as correct" [Leontief 1971, p. 2].

Of course, it does not follow that the mathematical structure of the various models will not be of interest. Indeed, as we shall see, an explicit consideration of the mathematical structure of a model can be quite revealing.<sup>4</sup> It can shed light on such substantive questions as these: To what sort of decision environment does the model have basic relevance, and under what circumstances does the predictive power of a model collapse? The point to be emphasized is that:

". . . reconsideration of the mathematical structure of a model frequently raises questions of the greatest significance for economic interpretation. In effect, we are forced to reexamine our assumptions about the real phenomena under investigation and make sure the economic problem is well posed" [Ekelund, Furubotn and Gramm 1972, p. 63].

This last point serves as my point of departure.<sup>5</sup> What is at issue is the efficacy of the assumptions upon which a particular theoretical edifice is built. As we shall see, the efficacy of a particular set of assumptions turns on the answers to the following questions: a) What is(are) the purpose(s) of the model?, and b) What are the conditions requisite to the achievement of the model's objective(s)? Unfortunately, as Professor Leontief points out,

"By the time it comes to interpretation of (the model's) substantive conclusions, the assumptions on which the model has been based are easily forgotten. But it is precisely the empirical validity of these assumptions on which the usefulness of the entire exercise depends" [Leontief 1971, p. 2].

If one adopts the view that a theory should be both explanatory and informative, that it should provide at least a tentative answer to the question "Why?", then rigorous conditions must be met. Included among these is the "empirical validity" of the model's assumptions [Nagel 1963; Melitz 1965; Wong 1973]. On the other hand, if the avowed purpose of a model is simply "prediction", realism of assumptions is not mandatory; it is simply judicious [Melitz 1965; Wong 1973]. It follows that before it can be established that a sine qua non for its usefulness is the empirical validity of its assumptions, a model's purpose must be unambiguously established.<sup>6</sup> Granting this, before an explicit statement of the evaluative criteria to be employed can emerge, we must "plant our methodological feet." We must ask ourselves, "What is it that we want our models of consumer behavior to do?"

#### THE PURPOSES OF MODELS

The question, "What is it that we want our models of consumer behavior to do?" is answered according as we adopt a particular methodological posture. Because each has gained wide acceptance among economists, our interest centers on the following methodological positions: a) descriptivism, b) instrumentalism, and c) the view that a theory should be both explanatory and informative.<sup>7</sup>

That an understanding of the nature and the implications of the three positions is of interest to a philosopher of science is obvious. That the same understanding is of practical significance to an economist is perhaps less clear. What is fundamentally at issue, however, is the role of the theories which the economist brings to bear in analyzing systematically the complex phenomena which come under his purview. And it is precisely the economist's perception of the role of a theory that must determine the character and content of the assumptions upon which that theory is built. To see this, we must first undertake a systematic review of the alternative methodological positions.

## THE DESCRIPTIVIST VIEW

The descriptivist view holds that theories are only descriptive of observable experience; that they can never be explanatory. An influential proponent of this view is Paul Samuelson. On his view, a) a theory is just a description of observable experience; a convenient, mnemonic representation of empirical reality [Samuelson 1963, p. 236; 1956, p. 1171]. It follows that b) "knowledge" consists essentially of observable reports; indeed, all well-known theories in science are expressible in terms of basic statements [Samuelson 1965, p. 1167]. The latter are understood to have a specific spatio-temporal reference.

Samuelson's rejection of the view that theories are explanatory is itself explicable [Wong 1973, p. 319]: a) He regards explanations as ultimate, and b) It is his view that apriorism must be avoided and that, therefore, theories must be expressed in terms of basic statements.

While it would be redundant simply to reproduce Professor Wong's elegant critique of Samuelson's methodological position, it is essential that the rudiments of the argument be understood. In his view [Wong 1973, p.320]:

"Explanations are not ultimate. We can give explanations of explanations. Samuelson mistakenly identifies the explanatory view with an essentialist view. Essentialism aims at ultimate explanation which is neither capable nor in need of further explanation."<sup>8</sup>

So much for the view that explanations are ultimate. On Samuelson's rejection of apriorism, Wong is equally persuasive [Wong 1973, p. 320]:

"Samuelson has rightfully condemned apriorism, the view that all phenomena can be explained as consequences of self-evident first principles, the truth of which is independent of all possible experience. But the alternative . . . is not to ground theories in observational statements. For such a program is impossible to achieve because of insurmountable difficulties, both logical and epistemological."

The logical difficulty arises from the logical form of a theory: A theory includes at least one unrestricted universal statement; a statement with no spatio-temporal reference. And, as Wong has emphasized, ". . . an unrestricted universal statement is not equivalent to a finite conjunction of observational statements . . . " [Wong 1973, p. 320].

All of this suggests that the position that well-known theories in science are expressible in terms of basic statements [Samuelson 1965, p. 1167] is, at best, fallacious. Indeed, all the theories cited by Samuelson in support of his position claim to be universal.<sup>9</sup> To put the matter differently, each of the theories cited includes at least one unrestricted universal statement. It is clear that these theories cannot, therefore, be expressible as basic statements.

There are, moreover, epistemological grounds for rejecting the view that theories must be grounded in observational statements: We simply do not have an independent language in which to ground theories. Indeed, "All observational terms are theory laden" [Wong 1973, p. 321]. That is to say, all observational statements and terms implicitly assume theories; constructs which, via their logical structure, go beyond observational statements.<sup>10</sup>

From what has been said, it is clear that the logical foundations of the descriptivist position are, at best, tenuous. Indeed, one could plausibly argue that implementation of descriptivist strictures would consign economists to the role of observers of empirical reality. Consistent employment of basic, to the exclusion of universal statements would not only impede the development of theories in economics; it would prohibit their emergence. Granting this, the methodological position to be adopted here must be found elsewhere. It must, in fact, be either the instrumentalist view, or the view that a theory must be both explanatory and informative. We turn next to the instrumentalist view.



## THE INSTRUMENTALIST VIEW

Stripped to its essentials, the instrumentalist view holds that a theory is an instrument for prediction. On this view, ". . . the only relevant test of the validity of a hypothesis is comparison of its predictions with experience" [Friedman 1953, pp.8-9]. Because the theory constitutes the machinery through which predictions about observable reality are to emerge, the descriptive realism of the theory's assumptions is a moot question. Indeed, the only relevant question is [Friedman 1953, p. 15]:

" . . . whether [the assumptions] are sufficiently good approximations for the purpose at hand. And this question can be answered only by seeing whether a theory works, which means whether it yields sufficiently accurate predictions. The two supposedly independent tests [test of the assumptions and test of the theory by a test of its predictions] thus reduce to one test."

While much can be said about this, it is clear that there exist no logical or epistemological arguments with which successfully to challenge this position.<sup>11</sup> As the noted philosopher of science Ernest Nagel puts it [Nagel 1963, p. 218]:

"Is [Friedman] defending the legitimacy of unrealistic assumptions because he thinks theories are at best only useful instruments valuable for predicting observable events but not to be viewed as genuine statements whose truth or falsity may be significantly investigated? But if this is the way he conceives theories (much in his argument suggests that it is), the distinction between realistic and unrealistic theoretical assumptions is at best irrelevant, and no defense of theories lacking in realism is needed."