SAMUELSON

PAUL ANTHONY SAMUELSON

FOUNDATIONS OF ECONOMIC ANALYSIS

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PREFACE

THE ORIGINAL version of this book submitted to the David A. Wells Prize Committee of Harvard University in 1941 carried the subtitle, "The Operational Significance of Economic Theory." At that time most of the material presented was already several years old, having been conceived and written primarily in 1937. Further delay in publication has been necessary because of the war, and because of the addition of supplementary treatise-like material going beyond the original conception of the work as indicated by its subtitle.

Because of the pressure of war work I have not been able to do full justice to the literature of the last few years, nor even to include all of the developments of my own thinking. Fortunately, the passage of time has dealt kindly with the analysis contained here, and where it abuts upon the topics treated in Professor Hicks's masterly *Value and Capital*, the similarity in point of view has been reassuring.

My greatest debt is to Marion Crawford Samuelson whose contributions have been all too many. The result has been a vast mathematical, economic, and stylistic improvement. Without her collaboration the book would literally not have been written, and no perfunctory uxorial acknowledgment can do justice to her aid. Nor can the quaint modern custom of excluding the value of a wife's services from the national income condone her exclusion from the title page.

My thanks for prolonged stimulation over many years must go out to Professors Schumpeter, Leontief, and E. B. Wilson, while each of a legion of Harvard graduate students has left his mark upon what follows. The reader will note my dependence upon the sterling contribution to Welfare Economics of Professor Abram Bergson. Grateful acknowledgment is made to the Social Science Research Council and to the Society of Fellows of Harvard University for the opportunities they provided for pursuit of independent research, and to the Department of Economics of Harvard University for their courteous acceptance of the wartime delays in publication.

Acknowledgment is made to the editors of *Econometrica* and the *Review of Economic Statistics* for permission to reproduce parts of my previously published articles. Chapters IX and X are taken almost entirely from two articles that appeared in *Econometrica*, while part of Chapter XI appeared in the *Review of Economic Statistics*.

P. A. S.

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FOUNDATIONS OF ECONOMIC ANALYSIS

PART I

CHAPTER I

INTRODUCTION

The existence of analogies between central features of various theories implies the existence of a general theory which underlies the particular theories and unifies them with respect to those central features. This fundamental principle of generalization by abstraction was enunciated by the eminent American mathematician E. H. Moore more than thirty years ago. It is the purpose of the pages that follow to work out its implications for theoretical and applied economics.

An economist of very keen intuition would perhaps have suspected from the beginning that seemingly diverse fields—production economics, consumer's behavior, international trade, public finance, business cycles, income analysis—possess striking formal similarities, and that economy of effort would result from analyzing these common elements.

I can make no claim to such initial insight. Only after laborious work in each of these fields did the realization dawn upon me that essentially the same inequalities and theorems appeared again and again, and that I was simply proving the same theorems a wasteful number of times.

I was aware, of course, that each field involved interdependent unknowns determined by presumably efficacious, independent equilibrium conditions—a fact which has always been generally realized. But, and this leads me to the second fundamental purpose of this work, it had not been pointed out to my knowledge that there exist formally identical *meaningful* theorems in these fields, each derived by an essentially analogous method.

This is not surprising since only the smallest fraction of economic writings, theoretical and applied, has been concerned with the derivation of *operationally meaningful* theorems. In part at least this has been the result of the bad methodological preconceptions that economic laws deduced from *a priori* assumptions possessed rigor and validity independently of any empirical human behavior. But only a very few economists have gone so far as this.

The majority would have been glad to enunciate meaningful theorems if any had occurred to them. In fact, the literature abounds with false generalization.

We do not have to dig deep to find examples. Literally hundreds of learned papers have been written on the subject of utility. Take a little bad psychology, add a dash of bad philosophy and ethics, and liberal quantities of bad logic, and any economist can prove that the demand curve for a commodity is negatively inclined. His instinct is good; the attempt to derive a meaningful useful theorem is commendable—much more so than the innocuous position that utility is always maximized because people do what they do. How refreshing then is a paper like Slutsky's [1] which attempted, with partial success, to deduce once and for all the hypotheses upon price-quantity budget behavior implied in the utility approach.

The economist has consoled himself for his barren results with the thought that he was forging tools which would eventually yield fruit. The promise is always in the future; we are like highly trained athletes who never run a race, and in consequence grow stale. It is still too early to determine whether the innovations in thought of the last decade will have stemmed the unmistakable signs of decadence which were clearly present in economic thought prior to 1930.

By a *meaningful theorem* I mean simply a hypothesis about empirical data which could conceivably be refuted, if only under ideal conditions. A meaningful theorem may be false. It may be valid but of trivial importance. Its validity may be indeterminate, and practically difficult or impossible to determine. Thus, with existing data, it may be impossible to check upon the hypothesis that the demand for salt is of elasticity -1.0. But it is meaningful because under ideal circumstances an experiment could be devised whereby one could hope to refute the hypothesis.

The statement that if demand were inelastic, an increase in price would raise total revenue is not a meaningful theorem in this sense. It implies no hypothesis—certainly not even that a demand exists which is inelastic—and is true simply by definition. It may possibly have had a certain "psychological" usefulness in helping

[1] E. Slutsky, "Sulla teoria del bilancio del consumatore," *Giornale degli Economisti*, LI (1915), 1–26.

economists ask the right questions of the facts, but even here I have some doubts.

In this study I attempt to show that there do exist meaningful theorems in diverse fields of economic affairs. They are not deduced from thin air or from *a priori* propositions of universal truth and vacuous applicability. They proceed almost wholly from two types of very general hypotheses. The first is that the conditions of equilibrium are equivalent to the maximization (minimization) of some magnitude. Part I deals with this phase of the subject in a reasonably exhaustive fashion.

However, when we leave single economic units, the determination of unknowns is found to be unrelated to an extremum position. In even the simplest business cycle theories there is lacking symmetry in the conditions of equilibrium so that there is no possibility of directly reducing the problem to that of a maximum or minimum. Instead the dynamical properties of the system are specified, and the hypothesis is made that the system is in "stable" equilibrium or motion. By means of what I have called the *Correspondence Principle* between comparative statics and dynamics, definite *operationally meaningful* theorems can be derived from so simple a hypothesis. One interested only in fruitful statics must study dynamics.

The empirical validity or fruitfulness of the theorems, of course, cannot surpass that of the original hypothesis. Moreover, the stability hypothesis has no teleological [2] or normative significance; thus, the stable equilibrium might be at fifty per cent unemployment. The plausibility of such a stability hypothesis is suggested by the consideration that positions of unstable equilibrium, even if they exist, are transient, nonpersistent states, and hence on the crudest probability calculation would be observed less frequently than stable states. How many times has the reader seen an egg standing upon its end? From a formal point of view it is often convenient to consider the stability of nonstationary motions.

In a good deal of Part II the dynamical behavior of systems is analyzed for its own sake, regardless of implications for comparative statics. And in the last chapters of Part I, I have gone beyond the original conception of the book to include such subjects as

[2] L. J. Henderson, *The Order of Nature* (Cambridge, Massachusetts: Harvard University Press, 1917).

welfare economics. Although the logical content of the theorems enunciated here is different, there is an underlying unity of method.

In the beginning it was hoped that the discussion could be made nontechnical. Very quickly it became apparent that such a procedure, while possible, would involve a manuscript many times the present size. Moreover, I have come to feel that Marshall's dictum that "it seems doubtful whether any one spends his time well in reading lengthy translations of economic doctrines into mathematics, that have not been made by himself" should be exactly reversed. The laborious literary working over of essentially simple mathematical concepts such as is characteristic of much of modern economic theory is not only unrewarding from the standpoint of advancing the science, but involves as well mental gymnastics of a peculiarly depraved type.

On the other hand, I have attempted to avoid all mathematical flourish, and the pure mathematician will recognize all too readily the essentially elementary character of the tools used. My own interest in mathematics has been secondary and subsequent to my interest in economics. Nevertheless, the reader may find some parts hard going. To lighten the way I have placed the purely mathematical theorems in two separate appendices, of which the second gives a reasonably self-contained introduction to the theory of difference equations.

CHAPTER II

EQUILIBRIUM SYSTEMS AND COMPARATIVE STATICS

MOST ECONOMIC TREATISES are concerned with either the description of some part of the world of reality or with the elaboration of particular elements abstracted from reality. Implicit in such analyses there are certain recognizable formal uniformities, which are indeed characteristic of all scientific method. It is proposed here to investigate these common features in the hope of demonstrating how it is possible to deduce general principles which can serve to unify large sectors of present day economic theory.

In every problem of economic theory certain variables (quantities, prices, etc.) are designated as unknowns, in whose determination we are interested. Their values emerge as a solution of a specified set of relationships imposed upon the unknowns by assumption or hypothesis. These functional relationships hold as of a given environment and milieu. Of course, to designate this environment completely would require specification of the whole universe; therefore, we assume implicitly a matrix of conditions within which our analysis is to take place.

It is hardly enough, however, to show that under certain conditions we can name enough relations (equations) to determine the values of our unknowns. It is important that our analysis be developed in such terms that we are aided in determining how our variables change qualitatively or quantitatively with changes in explicit data. Thus, we introduce explicitly into our system certain data in the form of parameters, which in changing cause shifts in our functional relations. The usefulness of our theory emerges from the fact that by our analysis we are often able to determine the nature of the changes in our unknown variables resulting from a designated change in one or more parameters. In fact, our theory is meaningless in the operational sense unless it does imply some restrictions upon empirically observable quantities, by which it could conceivably be refuted.

This in brief is the method of *comparative statics*, meaning by this the investigation of changes in a system from one position of equilibrium to another without regard to the transitional process involved in the adjustment. By equilibrium is meant here only the values of variables determined by a set of conditions, and no normative connotation attaches to the term. As will be shown later, it is always possible to set up completely trivial equilibrium systems.

This method of comparative statics is but one special application of the more general practice of scientific deduction in which the behavior of a system (possibly through time) is defined in terms of a given set of functional equations and initial conditions. Thus, a good deal of theoretical physics consists of the assumption of second order differential equations sufficient in number to determine the evolution through time of all variables subject to given initial conditions of position and velocity. Similarly, in the field of economics dynamic systems involving the relationship between variables at different points of time (e.g., time derivatives, weighted integrals, lag variables, functionals, etc.) have been suggested for the purpose of determining the evolution of a set of economic variables through time.[1] At a later stage I deal with these dynamic problems.

The concept of an equilibrium system outlined above is applicable as well to the case of a single variable as to so-called *general equilibrium* involving thousands of variables. Logically the determination of output of a given firm under pure competition is precisely the same as the simultaneous determination of thousands of prices and quantities. In every case *ceteris paribus* assumptions must be made. The only difference lies in the fact that in the general equilibrium analysis of, let us say, Walras, the content of the historical discipline of theoretical economics is practically exhausted. The things which are taken as data for that system happen to be matters which economists have traditionally chosen not to consider as within their province. Among these data may be mentioned tastes, technology, the governmental and institutional framework, and many others.

[1] R. Frisch, "On the Notion of Equilibrium and Disequilibrium," *Review of Economic Studies*, III (1936), 100–105. J. Tinbergen, Annual Survey: Suggestions on Quantitative Business Cycle Theory, *Econometrica*, III (1935), 241–308.

It is clear, however, that logically there is nothing fundamental about the traditional boundaries of economic science. In fact, a system may be as broad or as narrow as we please depending upon the purpose at hand; and the data of one system may be the variables of a wider system depending upon expediency. The fruitfulness of any theory will hinge upon the degree to which factors relevant to the particular investigation at hand are brought into sharp focus. And if, for the understanding of the business cycle a theory of governmental policy is demanded, the economist can ill afford to neglect this need on the ground that such matters lie outside his province. As for those who argue that special degrees of certainty and empirical validity attach to the relations encompassed within the traditional limits of economic theory, we may leave to them the task of proving their case.

It is not to be thought that the content of systems as described above must be restricted to the variables usually considered in price and value theory. On the contrary, one employs such constructions throughout the whole field of theoretical economics including monetary and business cycle theory, international trade, etc. It goes without saying that the existence of such systems in no way hinges upon the employment of symbolic or mathematical methods. In fact, any sector of economic theory which cannot be cast into the mold of such a system must be regarded with suspicion as suffering from haziness.

Within the framework of any system the relationships between our variables are strictly those of mutual interdependence. It is sterile and misleading to speak of one variable as causing or determining another. Once the conditions of equilibrium are imposed, all variables are simultaneously determined. Indeed, from the standpoint of comparative statics equilibrium is not something which is attained; it is something which, if attained, displays certain properties.

The only sense in which the use of the term causation is admissible is in respect to changes in external data or parameters. As a figure of speech, it may be said that changes in these *cause* changes in the variables of our system. An increase in demand, i.e., a shift in the demand function due to a change in the data, tastes, may be said to cause an increased output to be sold. Even here, when several parameters change simultaneously, it is im-