FOUNDATIONS
OF
ECONOMIC
ANALYSIS

SAMUELSON

HARVARD
ECONOMIC
STUDIES

FOUNDATIONS OF
ECONOMIC ANALYSIS

BY

PAUL ANTHONY SAMUELSON

PROFESSOR OF ECONOMICS
MASSACHUSETTS INSTITUTE OF TECHNOLOGY

"*Mathematics is a Language*"
—J. WILLARD GIBBS

CAMBRIDGE
HARVARD UNIVERSITY PRESS
1955

FOURTH PRINTING

PRINTED IN THE UNITED STATES OF AMERICA

HARVARD ECONOMIC STUDIES

VOLUME LXXX

AWARDED THE DAVID A. WELLS PRIZE FOR THE YEAR 1941–42 AND PUBLISHED FROM THE INCOME OF THE DAVID A. WELLS FUND. THIS PRIZE IS OFFERED ANNUALLY, IN A COMPETITION OPEN TO SENIORS OF HARVARD COLLEGE AND GRADUATES OF ANY DEPARTMENT OF HARVARD UNIVERSITY OF NOT MORE THAN THREE YEARS' STANDING, FOR THE BEST ESSAY IN CERTAIN SPECIFIED FIELDS OF ECONOMICS

LONDON : GEOFFREY CUMBERLEGE
OXFORD UNIVERSITY PRESS

To

MY PARENTS

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.

CAMBRIDGE, MASSACHUSETTS
JANUARY 1945

CONTENTS

PART I

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).