

# **FOUNDATIONS OF ECONOMIC ANALYSIS**

**PAUL A.  
SAMUELSON**

**ENLARGED EDITION**

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Enlarged Edition

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HARVARD UNIVERSITY PRESS  
CAMBRIDGE, MASSACHUSETTS, AND LONDON, ENGLAND

1983

# HARVARD ECONOMIC STUDIES

VOLUME 80

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## Library of Congress Cataloging in Publication Data

Samuelson, Paul Anthony, 1915 -  
Foundations of economic analysis.

(Harvard economic studies; v. 80)

Bibliography: p.

Includes index.

1. Economics, Mathematical. I. Title. II. Series.

HB135.S24 1982 330'.01'51 82-21304

ISBN 0-674-31301-1

ISBN 0-674-31303-8 (pbk.)

## PREFACE TO THE ENLARGED EDITION

IN THIS ENLARGED EDITION the original text (pp. 1 – 439) has been reproduced exactly as it appeared in the first edition of 1947, except that some typographical errors have been corrected. For this edition I have written three new pieces: an Introduction, Mathematical Appendix C, and the Bibliography.

The new Introduction comments on the book's genesis and subsequent developments in analytical economics. Mathematical Appendix C gives in nine sections a sampling of important trends in mathematical economics that are directly related to the concerns of this book. It includes selective summaries of linear and nonlinear programming; the various aspects of duality theory, including stochastic dynamic programming; the testable content of monetary models in which money is put directly into utility functions for the convenience it renders at different prices; reflections on the nature and logic of probabilistic decision making, within the Expected Utility dogma and in more general formulations; the merits and demerits of mean-variance analysis in comparison with general analytical methods of portfolio optimization. In the concluding section of the new Mathematical Appendix, time-phased models of production are analyzed, both in their Sraffa and Leontief mainstream economics versions and in their Karl Marx versions. Finally, the Bibliography contains references to the new Introduction and to Mathematical Appendix C.

By concentrating on what is fundamental I have been able to cover much that has concerned economic theorists in recent decades. But to do full justice to these subjects my book would have had to double in size at least. Therefore, I beg readers' indulgence in viewing these broad pen strokes and commend to them the wise caveat that the mathematician George Mackey once invoked in the preface to a book on the mathematics of quantum theory: "If the reader feels a sign should be changed, he is probably right."

P.A.S.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY  
JANUARY 1983

## PREFACE TO THE FIRST EDITION

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

## INTRODUCTION TO THE ENLARGED EDITION

TIME LENDS DISTANCE. When I read words written four decades ago, I see them with an objectivity as if they were composed by another scientist. And yet, blessed with a good memory, I have access now in 1983 to the process of creative genesis for this book's theories that no one else can ever enjoy.

John Livingston Lowes won scholarly esteem for himself, and added to Coleridge's immortal fame, when he explored in *The Road to Xanadu* the books and poems Coleridge was known to have read before *The Rime of the Ancient Mariner* was written, tracing in one or another of these works the inputs of images, metaphors, and conceits out of which the great poem's contents were forged. Posterity is grateful to Lowes. But how much more valuable it would be if we had from Coleridge himself an accurate sampling of the creative travail that went into his masterpiece.

Science is not art. Yet, despite the lack of complete identity between art and science, there is much in common among different creative processes. How Mozart produced his music, Shakespeare his plays, Frost his short poems and Milton his great epics, Tolstoy and even Trollope their novels—what would we not give to learn? No less interesting to those of us who consider science the most exciting game in the universe would be autobiographical accounts of the young and old Newton, of Euler at work and Clerk Maxwell at play.

We are eternally grateful to Henri Poincaré for his detailed exposition of the role that the subconscious plays in the discovery of mathematical theories: how one wrestles consciously and unsuccessfully with a theorem, then puts it aside, as if out of mind, but apparently not really out of mind; for suddenly, in a Coleridgean dream—or, in the case of Poincaré when he was stepping on a bus while doing his military service—the successful solution arrives as if the unconscious had all along been grinding away on the puzzle. I once advised the head of the Harvard University Press to publish James Watson's double-helix manuscript—exactly in its first naked form, with its original title, *Honest*

*Jim*—for the insight it gave into one aspect of science: the struggle for priority and fame. If a man would write such an account, let him be read, even if it is an exaggeration of what is a normal element in the scientific way of life.

We have all been bored by pedants' postmortems on the essence and nature of humor. And working scientists, to tell the simple truth, have neither the time nor the patience to bother with the history of their subject: they want to get on with making that history. Philosophers of science, historians of science, sociologists of science, may not be without honor in their own houses; but the customers who take in their washings, and swap garments with them, are unlikely to be working scientists still in the prime of life. Still, the attention that an assistant professor's dissertation cannot command to a discussion of how Helmholtz achieved his scientific contributions may just possibly be attracted by Helmholtz's own account. Again, this suggests a role for autobiography in science. The laboratory notebooks of Michael Faraday are more precious to me than the Domesday Book or the Rosetta Stone.

Autobiography has its pitfalls. No better example of that could be found than in the case of Coleridge. Research shows that his statements to have written this or that poem at the age of seven, or eleven, or forty-three can never be taken at face value—and there is the further danger that the "original" poem in question is in fact a translation from Schiller's German. Coleridge has high claims to the title of greatest plagiarist of all time. Genius is above scandal. But just what there is to be forgiven or praised posterity does want to know.

Watson's account of DNA is Watson's account. It is part of a truth and belongs in the record, even though it does less and more than justice to its narrator. Isaac Newton did not falsify the story of his transcendental achievements. He did not do so either consciously or unconsciously. But he was not generous to rivals and you would not be able to discern from Newton's *Principia* how he arrived at his major discoveries and insights. Even Albert Einstein, the sweetest of human beings and one with so many early achievements to his credit that he had no need for puffery or priority claims, does not seem in late-life accounts to have managed to give plausible accounts of the origins of all his notions. Not to have known of the Michelson-Morley experiment by name is not



the same thing as not to have known of equivalent empirical findings. Innumerable writers Einstein studied did know of Brownian motion; in what sense, then, can one interpret Einstein's claim not to have known of it when writing his 1905 paper on stochastic processes?

Content analysis will confirm that autobiographers cannot help but give themselves the benefit of the doubt. Accounts by scientists of their earlier works are invaluable, but such accounts are only one subset of the data and hypotheses that nominate themselves for inclusion in the adversary procedure that constitutes the corpus of science.

Let the reader beware. I have made a conscious attempt to render correctly my role in the creation of modern theoretical economics. But I urge that the grain of salt I have to apply to the present dish be kept at hand by those who have the objectivity of distance as well as of time.

### APOTHEOSIS OF THE CALCULUS

The first edition of *Foundations of Economic Analysis* was a culmination and also a beginning. It finally achieved for economics a synthesis of Cournot's Newtonian calculus method of maximizing with Walras's equations of general equilibrium. Certain problems were settled for all time: consider, for example, observable demand functions that achieved for a consumer the maximum of an ordinal utility function subject to given income and prices—call them  $q = f(P/\text{income})$  in matrix notation. If they satisfied the Slutsky conditions of having a symmetric, negative semi-definite matrix,  $S = [\partial q_i / \partial p_j + q_j \partial q_i / \partial (\text{income})]$ ,  $S = S^T$ ,  $PS \equiv 0$ ,  $X^T S X \leq 0$  for arbitrary column vector  $X$ , then it was shown that nothing more could be asked of them by the theory. No young Hotelling, Hicks, Allen, Pareto, or Slutsky could later come along and add a new requirement once closure had been attained by rigorous proof.

But beyond the counting of equations and testing of Newton-Weierstrass quadratic forms and bordered determinants, *Foundations* began the systematic use of *finite inequalities* in modern economics. To say that raising price from  $p_1$  to  $p_2$  will lower quantity bought from  $q_1$  to  $q_2$  along a demand curve,  $q = f(p)$ , one need not

be able to say that  $f'(p)$  is almost everywhere negative on the interval  $(p_1, p_2)$ . It will do to know that  $(p_2 - p_1)(q_2 - q_1) = \Delta p \Delta q < 0$  for every two distinct points on the demand curve. Where Newtonian calculus helps, economists are grateful. But where it doesn't apply, as when price can take on only integral (or rational) values, we are even more grateful for more general methods. When I stumbled on the notion of revealed preference in 1937, I was shoved into the task of trying to free classical mathematical analysis from its calculus corsets.

The original version of this book abounded with questions for further research and development. Future Arrows, Houthakkers, Debreus, Shephards, Samuelsons, and McKenzies were to apply to economics the unifying tools of convex set theory, partial orderings and lattices, fixed-point theorems, nonstandard analysis, and all of the paraphernalia of Bourbaki mathematics. Gerald Debreu once commented to me—soberly—that the discipline which most fully uses in its daily work the frontier refinements of mathematical analysis is modern economic theory. That is a sobering thought indeed.

### NEWTONIAN PARADISE REGAINED

So fertile were the analytical methods of A. Wald, J. von Neumann, K. J. Arrow, H. Wold, T. Koopmans, D. Gale, H. Kuhn and A. W. Tucker, G. Debreu, L. S. Pontryagin, R. J. Aumann, and Abraham Robinson that by the time *Foundations* celebrated its official twentieth birthday, its pages of Newtonian calculus were old hat. But then the mathematician Steven Smale wandered into economics, pointing out in effect that if only one added the magic incantation of "almost always," the classic assertions of the calculus were reinstated into respectability. Thus, if a demand function is smooth everywhere on an interval except at a set of points of measure zero, it is necessary and sufficient for it to be strictly monotone-decreasing that "almost everywhere" its derivative,  $f'(p)$ , be negative. Willard Gibbs's phase rule in thermodynamics—which his disciple and my master, E. B. Wilson, pointed out is no more than the economist's counting of equations and unknowns—after all does have a substantive *generic* content even in the land of Bourbaki.

More can be less. Much of mathematical economics in the 1950s gained in elegance over poor old Pareto and Edward Chamberlin. But the fine garments sometimes achieved fit only by chopping off some real arms and legs. The theory of cones, polyhedra, and convex sets made possible “elementary” theorems and lemmas. But they seduced economists away from the phenomena of increasing returns to scale and nonconvex technology that lie at the heart of oligopoly problems and many real-world maximizing assignments. Easy victories over a science’s wrong opponents are hollow victories—at least almost always.

Fortunately, some of the inequalities of revealed preference have a validity even when utility functions are not quasi-concave and do possess indifference contours that reverse their curvatures. Thus, suppose  $Q^1$  is the cheapest way for me to buy at  $P^1$  at least a specified level of well-being,  $\bar{U} \leq U(Q)$ , and suppose  $Q^2$  is the cheapest way to achieve at least that well-being when prices have changed to  $P^2$ . Then, even if  $U(Q)$  is not a well-behaved quasi-concave function, and even if it is not defined for all real numbers in the non-negative orthant  $(q_1, \dots, q_n) = Q \geq 0$ , the inequality of the Weak Axiom of Revealed Preference is still valid:  $\Delta P \cdot \Delta Q = \sum_1^n (p_j^2 - p_j^1)(q_j^2 - q_j^1) \leq 0$ . This follows from the basic logic of maximizing. One of the most joyful moments of my life was when I was led by listening to E. B. Wilson’s exposition of Gibbsian thermodynamics to infer an eternal truth that was independent of its physics or economics exemplification. (A student who studied only one science would be less likely to recognize what belonged to logic rather than to the nature of things.)

### BLESSED LACK OF UNITY

*Foundations* is three books. I am glad that I did not stick to my original thesis intention of concentrating solely on the implications for observable economic data of the hypothesis that individuals or firms act to maximize specifiable functions. Most of Part I did focus on that problem. But every time an interesting topic beckoned, I departed from the narrow path in hot pursuit.

Four instances will suffice. Chapter III begins with “A Calculus of Qualitative Relations.” That is admitted not to fall under the rubric of maximizing behavior. But it is a novel topic, valu-

able for its own sake, and years later it led such economic theorists as K. Lancaster, W. M. Gorman, and T. Quirk to develop and codify what can be said on the basis solely of qualitative relationships (such as algebraic sign of influence of one variable on another). This calculus has had applications even in ecology.

A second valuable digression (or partial digression) was Chapter VI's section "The Economic Theory of Index Numbers." Index number theory is shown to be merely an aspect of the theory of revealed preference. Thirty-five years after that analysis appeared there has been but one major advance in index number theory—namely W. E. Diewert's formalizing concept of a "superlative index number," which is a formula based upon two periods ( $p_j, q_j$ ) data that will be exactly correct as an ordinal indicator of utility for some specified family of indifference contours. (Only a few different "superlative" formulas are known; perhaps the set of simple superlative formulas is a limited set.) What my book's discussion around equations (76) and (77) settled, and what needed still to be settled back in the mid-1930s when the *Review of Economic Studies* rejected the pearl I cast before them, was the fact that no index number would ever be found that would do the exact job that early economists wanted of an index number. Thus, let half the goods double in price and half of them triple. Trivially, the cost of living has increased somewhere between 100 and 200 percent. But, even if we know exactly what you and I spend on each good before and after the price changes, it is impossible in principle for the same scalar formula— $f(P^a, Q^a; P^b, Q^b)$ —to represent correctly for the two of us with different (not yet known!) tastes the exact amount by which our respective costs of living have risen. Knowable in principle from knowledge of our respective complete indifference contours are the ratios of new-to-old dollars each of us needs in the new  $P^b$  price situation to be as well off as we were in the initial  $P^a$  price situation; but—and this is the point of revealed preference—knowledge of but two  $(P, Q)$  situations (or of a limited number of such situations) can at best put bounds on each one of our sought-for ratios.

Before leaving this second digression I ought to mention that my notorious opposition to the usual consumers-surplus palaver was similarly based on objection to nonoptimal revealed-preference inferences. If you know my  $q_1$  responses to changes in

$p_1$ , while  $p_2$  and my income are held constant—and that is what you know when you observe my “demand curve”—you are not able to infer my many indifference contours in the region surrounding the observed ( $Q$ ) points. Therefore, you cannot calculate the deadweight-loss magnitudes needed for various welfare purposes. If you have more knowledge about me—for example, how income changes my  $Q$  purchases—a more accurate mapping of my contours is made possible. Revealed preference is the theory that analyzes just what can be inferred from available ( $P, Q$ ) observations. When consumers surplus is done right, it becomes revealed preference theory and reveals its own redundancy.

A third digression had to do with Chapter V’s “A Note on the Demand for Money.” Young scholars tend to want to tell you all they know. And I was young. Now that I am not young, I am glad that this section got included. For the need existed to bring money into conventional demand theory, and to do so in a way that would deduce as a theorem the essential difference between coffee that you want for its own sake and money that you want only for its ability to enable you to buy and sell goods like coffee. Because the stock of money,  $M$ , enters the ordinal utility function along with the price vector,  $P$ , in the homogeneity form of  $P/M$ , a rational basis for the Quantity Theory is provided free of the gratuitous  $MV = PQ$  formulation. Neoclassical economics is better than Monetarism!

Finally, it is pleasant to note Chapter V’s final paragraphs in which the invention of money-market funds is indicated and the prospect is envisaged in which transaction money comes near to earning the full interest yield of an economy, with all the puzzles for a determinate theory of velocity of circulation that this would imply.

## WELFARE ECONOMICS

The second of the three books contained in *Foundations* is Chapter VIII’s summary of Abram Bergson’s new welfare economics. Until that chapter was available, only those who knew Bergson’s seminal 1938 *Quarterly Journal* article or Oskar Lange’s 1942 *Econometrica* article could find their way through the swamp of assertion, truth, ambiguity, denial, and misunderstanding that went under the name of the “new welfare economics.” In

a 1981 *Festschrift* to Bergson, I have written that his breakthrough of a *weakly separable* individualistic social welfare function "came like a flash of lightning, describable only in the words of the pontifical poet: 'Nature and Nature's laws lay hid in night: / God said, Let Newton be! and all was light.' "

Suddenly one recognized Lerner's marginal proportionality RULE and Pareto-optimality conditions for what they really are—members of the subset of conditions *necessary* for welfare optimization that are statable independently of the form of the interpersonal ethical norms specified for the (separable, ordinal) social welfare function. Just as Hegel is said to have understood his philosophy for the first time when he read its French translation, Vilfredo Pareto could have learned what it was he meant exactly to say when he read Bergson's 1938 classic. That Bergson's approach could stimulate new breakthroughs as well as unify past paradigms is shown by the fact that my 1954 application of it to the theory of public goods both gave new life to the old Wicksell-Lindahl theories and led to new cheat-proof algorithms by W. Vickrey, T. Groves and J. Ledyard, E. Clarke, G. Tullock, and J. Buchanan. Judged by frequency of citation, the 1954 effort succeeded in making its original point that mathematics does have a creative role to play in modern economics.

After four decades there is still misunderstanding concerning an individualistic social welfare function (of what is called Bergson-Samuelson type). Because Kenneth Arrow, in his 1951 classic *Social Choice and Individual Values*, gave the same name ("social welfare function") to the Constitutional Voting Function that he proved could not possibly exist with the properties one wishes for it, economist tyros think the Bergson-Samuelson Social Welfare Function has been proved not to exist. Experts know better. But some of the avant-garde make the curious mistake of thinking that the requirement that a Bergson-Samuelson Individualistic Social Welfare Function be "ordinal" implies that some one person is a dictator whose preferences are to prevail on all interpersonal judgments. This is a misunderstanding, as I have argued in *Economica* (1977) and in the Bergson *Festschrift*. Here is one more try.

Suppose society has one hundred chocolates to allocate between Jane and Dick:  $q_1$  for Jane,  $q_2$  for Dick,  $q_1 + q_2 = 100$ ,  $q_i \geq 0$ . An Individualistic Social Welfare Function, ISWF, that is ordinal

might have ethical indifference contours in the  $(q_1, q_2)$  plane defined completely as rectangular hyperbolae: If  $q_1^a q_2^a > q_1^b q_2^b$ , the allocation  $(q_1^a, q_2^a)$  is ethically better than  $(q_1^b, q_2^b)$ ; if  $q_1^a q_2^a = q_1^b q_2^b$ , the two allocations are ethically indifferent. Then subject to the constraint of one hundred chocolates to divide among sweet-toothed Jane and Dick, neither of whom is envious or altruistic, suppose the ethical judgment is made that the best state of the world is an even division of fifty each.

Everything is ordinal. Agreed? Nothing up anyone's sleeve. Agreed? Then what is the shouting about?

Somehow readers of the social choice literature have associated the good old word "ordinal" with a compulsion to use *rank-order* ratings of *changes*. They've stumbled into an axiom that is unhappily named a "neutrality axiom." It goes something like the following: "If it is ethically better in *some* situation to prefer a move that raises Jane and lowers Dick in individual welfares, then *every* choice that raises Jane and lowers Dick must be ethically preferred." You will say, how stupid an axiom is that entails one person's being an absolute dictator. I am not one to disagree. But wise or stupid, such an axiom has nothing to do with the usage of the word "ordinal" in the Edgeworth-Pareto-Hicks-Samuelson literature. The Bergson ordinal Individualistic Social Welfare Function is alive and well.

John Harsanyi (1955) has made one valuable addition to the Bergson ISWF. He has shown that, after all, there is something to be said for the Bentham form that merely adds different persons' *cardinal* utilities with appropriate ethical weights. Harsanyi deduces this simplification by extending the individualistic ethic—which agrees that ethically one state of the world is better than another if all persons unanimously deem it to be—to the stochastic realm.

Mathematical Appendix C, section 5, will give some details of the argument, but here is a sketch of how it goes. Suppose each of  $N$  persons wants to act in choosing among probability situations in accordance with a few Ramsey-von Neumann axioms of consistency and "rationality." It follows that each will act to maximize the Expected ("mean") Value of his existent *cardinal* utility function. Suppose Bergson's Ethical Observer, choosing ethically among probability situations, wishes to do so in accord with those few Ramsey-von Neumann axioms; then She, or He, will act to

maximize the Expected Value of an existent cardinal Social Welfare Function. Further, suppose that this SWF is an Individualistic Social Welfare Function in the sense that, when all persons agree that State A is better than State B, the ISWF agrees that A is ethically better than B. Harsanyi's point is that it is plausible to let A and B be probabilistic states as well as certainty states. If you grant him this point, then by logic it follows that the ISWF can be written in Bentham's additive form:  $\lambda^1 u^1(q) + \lambda^2 u^2(q) + \dots$ , where only the non-negative ethical weights ( $\lambda^1, \lambda^2, \dots$ ) are at the choice of the ethical system.

Before leaving the topic of welfare economics, I might mention that Chapter VIII throws light on how primitive the pre-1940 understanding was on some basic issues. My teachers, giants of the subject such as Jacob Viner, Frank Knight, and Joseph Schumpeter, could not explain to me or to themselves what was the germ of truth in Adam Smith's doctrine of the Invisible Hand, which was supposed to lead to something or other in a competitive market. Or consider the fashionable belief, then prevailing, that constant returns to scale is incompatible with stability of perfect competition. Even Pierro Sraffa's classic 1926 article, which launched the modern Chamberlin-Robinson theory of imperfect competition, was flawed in its contention that the boxes of increasing cost and of constant cost represented one box too many. Accepted literally, Sraffa's argument would deny validity to the following simplest case of general equilibrium: corn is produced by labor and land by a first-degree homogeneous production function; cloth is produced solely by labor at constant returns to scale. Then a shift of tastes toward food and away from clothing will raise  $P_{\text{corn}}/P_{\text{cloth}}$ , lowering the wage/rent ratio and labor's share in national income. Sraffa's logic can be caricatured to allege that such a model is logically self-contradictory and so (*sic*) we need a new theory of imperfect competition. I remember how delighted—and doubtful!—Schumpeter was with the Chapter IV section "Indeterminacy in Purest Competition?" which set this matter straight.

#### A VIRGIN CONTINENT

This leads to a needed observation. Newton said: Aw shucks, because I stood on the shoulders of giants I saw so far. True. We



all benefit from our scientific predecessors. I in particular pored over the works of Ragnar Frisch, Jan Tinbergen, Harold Hotelling, Griffith C. Evans, E. B. Wilson, Wassily Leontief, Nicholas Georgescu-Roegen—to mention only my then living elders.

Newton might also have added, less modestly, that he raised the platform from which his posterity could climb. And if Newton had not been so solitary a thinker and so transcendent a genius, he could have benefited from the work of contemporaries. Every racing cyclist knows how fast the ride is for the contender who travels just behind the pacesetter, enjoying the vacuum his efforts create as an externality. I pored over the works of J. R. Hicks, R. G. D. Allen, Abram Bergson, A. P. Lerner, Nicholas Kaldor, and all the great theorists of the day. Admittedly, it is easier on the ego to develop what is already in the air while nurturing the self-deception of subjective originality. But early on I decided that the higher prize was to get on with the subject's advancement by utilizing and acknowledging whatever was already to be found in the literature, eschewing what Gunnar Myrdal acidly called “unnecessary Anglo Saxon ‘originality’.” Then, if one could leap a cubit from those ethereal heights, the fulfillment was the greater.

There is something still more important. I was lucky to enter economics in 1932. Analytical economics was poised for its take-off. I faced a lovely vacuum that young economists today can hardly imagine. So much remained to be done. Everything was still in an imperfect state. It was like fishing in a virgin lake: a whopper at every cast, but so many lovely new specimens that the palate never cloyed.

Learned journals were then allergic to mathematics: having the leisure as a Junior Fellow at Harvard to pour out papers, my problem was to avoid saturating any one journal. I doled out the articles to as many different publications as would tolerate them. Again and again editors wrote: “Please shorten and make less mathematical.” I swallowed the temptation to protest: “Which do you want? Both are impossible. And neither is optimal.” The last laugh is to the scientist: the quality of the papers that editors rejected was, if anything, a bit better than the rest.

Being a Junior Fellow was the life for me. The pay was good. I owned a pen and the library was near. I used to think that I would gladly settle for the three-year term's becoming a life sen-