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How Foundations Came To Be

PAUL A. SAMUELSON¹

1. Introduction

F THERE ARE PEOPLE born under a Llucky star, there must be books that are lucky too. Foundations of Economic Analysis (Samuelson 1947) was one such. Unlike a biological embryo, this work had no definite moment of conception. Gradually, over the period 1936 to 1941, it got itself evolved. As I was mastering the existent corpus of economic theory, I recognized that a limited number of qualitative truths obtained, along with a greater number of indefinite relationships. Puzzled to understand this, I ransacked the mathematical libraries of Chicago and Harvard to explain to myself what made the difference. Reading widely, I was a child of my time, but it was the *internal* logic of the economic puzzles that guided Foundations' growth.

When you read biographies and obituaries of scholars, they fall into a few familiar patterns. The most interesting—like Albert Einstein's or Knut Wicksell's—are cases of early adversity and then final irresistible triumphs. My autobiography is of the duller kind. I began as a precocious infant, with unusually early conscious memories. Parents and two brothers were congenially supportive. Though it was fashionable to hate school, I loved it. Early bloomers develop ridiculous heights of selfconfidence, not realizing that even the duller academic scholars have aboveaverage I.Q.'s.

By accident, the public schools I attended-in Gary, Indiana and Chicago-were unusually good ones that turned out many future scholars and scientists. By chance of geography, I went to the University of Chicago at a young age, and under its experimental New Hutchins Plan I got a deep and wide undergraduate education. Bv chance, my freshman courses included under economics Aaron Director (1901–), who was later to be founder of the Second Chicago School of Milton Friedman, George Stigler and Gary Becker. In that academic year of 1931-32, although I didn't know it at the time, Chicago was the leading world center for neoclassical economics: next in the hierarchy would probably have come the London School of Economics, Cambridge University, and Columbia University. Harvard, then as now, was the greatest university in the world; but with the 1929 death of Allyn Young and the aging of Frank Taussig, its theory was then in a lean period.

The stars at Chicago's First School in economics were Frank Knight, Jacob Viner, Henry Schultz, Paul Douglas and Henry Simons. As an undergraduate I came to know them all well. A legend grew about an unself-conscious teenager who used to correct the omniscient Jacob Viner on the topography of

¹ Massachusetts Institute of Technology.

his graduate seminar blackboard diagrams.

Given my way, I would have stayed at Chicago forever. Why leave Nirvana? However, by chance, that happened to be the year when the Social Science Research Council began an educational experiment: they would scour America for the eight best economics undergraduates and generously underwrite their several years of graduate study. (Later I learned from the late Frank Fetter-Fetter the younger-that he had been the examiner who discovered me: success, President Kennedy observed, has a thousand fathers; failure is an orphan.) There was just one catch to the awards: you could not stay where you had done your undergraduate study. So my choice reduced effectively to Columbia or Harvard. My Chicago mentors recommended the Columbia of Wesley Mitchell, John Maurice Clark and Harold Hotelling. Never one to follow slavishly the advice of mentors, I opted for Harvard. (Joseph Schumpeter and Wassily Leontief were not my magnets: at Chicago Schumpeter was known as the eccentric who believed the rate of interest would be zero in the stationary state. Edward H. Chamberlin of monopolistic competition fame, by my miscalculation, attracted me to Harvard; but more important was my naive notion that Cambridge, Massachusetts would be a peaceful green village where book learning could explode.)

As mentioned, again by good chance, Harvard economics was just then awakening from a fallow period of sleepiness. New European blood—Schumpeter, Leontief, Gottfried Haberler—plus soon-to-come Alvin Hansen, the "American Keynes," was beginning to make Harvard the mecca for advanced economic research. Perhaps most relevant of all for the genesis of *Foundations*, Edwin Bidwell Wilson (1879–

1964) was at Harvard. Wilson was the great Willard Gibbs's last (and, essentially, only) protégé at Yale. He was a mathematician, a mathematical physicist, a mathematical statistician, a mathematical economist, a polymath who had done first-class work in many fields of the natural and social sciences. I was perhaps his only disciple: in 1935– 36, Abram Bergson, Sidney Alexander, Joseph Schumpeter, and I were the only students in his mathematical economics seminar. (Our ages were 21, 19, 52, and 20.) Aside from the fact that E.B. knew everything and everybody, his great virtue was his contempt for social scientists who aped the more exact sciences in a parrot-like way. He detested pseudo-learning and debunked many a pretentious theory (such as the Pearl-Verhulst infatuation with the *logistic curve* in demography). I was vaccinated early to understand that economics and physics could share the same formal mathematical theorems (Euler's theorem on homogeneous functions, Weierstrass's theorems on constrained maxima. Iacobi determinant identities underlying LeChatelier reactions, etc.), while still not resting on the same empirical foundations and certainties.

Publications flowed merrily from my pen. It was my good luck to be appointed as the first proper economist to Harvard's prestigious Society of Fellows—24 youthful princes in all fields, free to work on whatever they liked, but forbidden for three years to work toward any degree or Ph.D. dissertation. For me, those 1937–40 years were veritable heaven, and had I been offered the Faustian bargain of staying a Junior Fellow forever, I would have joyfully embraced it.

This explains how citations of my journal articles had won me in my early twenties an international reputation as a comer. Their topics were diverse: capital theory, lifecycle saving, utility theory, international trade, Keynesian multiplier-accelerator dynamics, revealed preference, and much else. Miraculously, it dawned on me that there was some unity of method and logic underlying much of these researches as well as much of current and historical economic theory.

In mid-1940 my Society of Fellows prohibition against writing a Ph.D. dissertation expired. Harvard's original Senior Fellows-President A. Lawrence Lowell, Alfred North Whitehead, John Livingston Lowes, and Lawrence I. Henderson-had launched the Society as a vendetta to reform the mediocre American Ph.D. system. My colleagues as Junior Fellows-Willard van Quine, the mathematical logician; George Birkhoff, the founder of lattice theory mathematics; Harry Levin, the youthful doyen of comparative literature—each deigned not to become Doctors of Philosophy. Their Harvard careers never suffered from this. My Protestant wife, Marion Crawford, and I decided to follow the prudent course of taking a Ph.D. degree. Lucky that we did, or perhaps Foundations might have been pushed off my agenda by the outflow of new publishable ideas.

From mid-1940 to January 1941, I composed and rearranged at fever pace; some got dictated to Marion, all got typed in first draft by her even though she was a graduate-school economist in her own right. Although I have always insisted that Foundations was formulated on Harvard grounds, before degree time I was an assistant professor at MIT. With or without a Ph.D., I was not to get an early tenure offer from Harvard and, without malice, I revealed a preference for MIT. To everyone's surprise, including my own, that turned out to be the happiest decision of my life.

A learned treatise, like a poem, stands on its own bottom or text. Ad hominem gabble about its author is at best secondary. The times were ripe for Foundations. Nature abhors a vacuum, and Foundations helped fill the vacuum. I have written elsewhere about how much there was back in the 1930s waiting to be discovered, and aching to be codified. I was like a fisher for trout in a virginal Canadian brook. You had only to cast your line and the fish jumped to meet your hook.

Let me give a few examples. Jacob Viner (1931) made a famous error, when his draftsman Y.K. Wong refused to draw a family of descending Ushaped cost curves with a lower envelope that went through their bottoms. Viner was ever after sensitive, but he conceded his error. That same Viner (1929), in a discussion of his hero David Ricardo, had discovered that the domestic price ratio of cloth to wheat, P_C/P_W , was as much equal to their respective (Marginal Cost in $Land)_{\rm C}/({\rm Marginal \ Cost \ in \ } Land)_{\rm W}$ as it was to (Marginal Cost in Labor)_C/(Marginal Cost in Labor)_W. Viner sensed that this in the deepest sense invalidated the hoary Labor Theory of Value, a truth not understood by David Ricardo, Piero Sraffa, or George Stigler. But Viner never connected this insight with what I waggishly called in Foundations the Wong-Viner Envelope Theorem. Actually, that theorem is a kaleidoscope which yields multiple insights: the direction of change of a maximum system when an external parameter gets perturbed; the LeChatelier theorem on constrained variables; the duality property that makes Lagrange-multipliers measure optimizing prices (and marginal costs or utilities).

In postwar years I used to receive letters from all over the world reporting groups of students who met in teams to puzzle out the contents of *Foundations*. Memorable is the London–Cambridge cell that included Jan de Van Graaff, Harry Johnson, Will Baumol, and Frank Hahn: soldiers of destiny on their way to destiny.

Even the book's mistakes generated a history. When I was apprised of a double error of sign in a LeChatelier-Jacobi determinant, I would write back: "Congratulations! You are the seventeenth non-Japanese to notice this."

The book won Harvard's David A. Wells Prize in 1941 for best publishable thesis. Because an earlier winner had pocketed the money but never revised his manuscript, I was required to submit my revised draft. Alas, World War II came to U.S. shores via Pearl Harbor. Nights and Sundays, while working on radar and mathematical fire control at the Radiation Laboratory, I toiled over revisions and expansions. By 1944 I handed in the finished draft. Harvard's long-time economics department chairman was no admirer of me; long before, he had counseled me against working in economic theory before I had reached (his) ripe old age of 50+. Once a month I checked that the manuscript still gathered dust in the anteroom of Economics headquarters. That was an unintentional boon to me: a wartime publication would have been an anticlimax before any climax.

Less lucky was the department chairman's decision to have a first printing of only 500 copies. I objected. We compromised on 750 copies. But he had the last word. His orders were to destroy all that beautiful mathematical type after the first run. When the first printing sold out immediately, all subsequent printings had to be done by photo offset. This turned out to be just as well for a busy author who had no relish for proofreading complicated mathematics.

Young authors expect a respectable

demand for their brainchild. I was no exception. But I never dreamed of the repeated printings that were to come, the paperback editions, or the many translations into foreign languages. I decided not to revise the text, instead merely correcting any errors brought to my attention. (They were relatively few and trivial: reversed algebraic signs; an occasional treacherous double limit. However, early in 1997, one new one got reported to me: it was an inexplicable error alleging that when one of many independent utilities involved a permissible *rising* marginal utility, then that good could have negative income elasticity. The gentle Leontief would have horsewhipped me in my first Harvard year for so crude a slip!)

When Foundations was 35 years old, I finally agreed to an enlarged edition. Rather than tamper with the original text, I added a second part that almost doubled the book's length. I wrote compactly to cover three decades of exploding new results. It was good stuff. A good deal of it was deeper than much of the original. But the result was dramatic confirmation of my suspicion that Foun*dations*' success came from its being the needed exposition for its time. The new stone caused no great ripples in the pond of modern mainstream economics. By 1983 we were all, so to speak, mathematical economists; and several hundred specialized books were available to cover each corner of up-to-date economics.

This is as it should be. Soft and hard sciences are cumulative disciplines. We each bring our contributions of "value added" to the pot of progress. In Max Planck's much-quoted words: Science progresses funeral by funeral. Inside tomorrow's physics treatise will be the lasting truths of Isaac Newton and also of the professor who works down the hall from you. Something of the same goes for economics, where often the dance must proceed Two Steps Forward and One Step Back.

Some fool (it was Henry Ford) said History is bunk. Actually, good history does debunk, by means of detailed reporting, the mystiques of scientific biography. "The lone genius toiling in a garret, and producing the Mona Lisa," that kind of gush. What was the background knowledge that led to Foundations? John Livingston Lowes, in his classic The Road to Xanadu, perused the books Samuel Taylor Coleridge was known to have read. The library withdrawal records at Chicago's Harper Library and Harvard's Widener and Baker libraries would be an unreliable source for my 1932–41 readings. I lived and breathed economics much of those days' 24 hours. What this autodidact learned (belatedly) came from auditing math lectures and reading while standing up deep in the stacks of Widener Library. Edward Gibbon had the seat of an historian; I had the feet of a zealot.

By the time I came to Harvard, though I was still too young to vote, I had taken more varied courses in economics than my fellow students would attend in all their graduate study: labor economics, economic history (both European and American), public finance, money and banking (there was no macro then), business cycles, statistics, everything but agricultural economics. Fellow students—our true teachers—included, at Chicago, George Stigler, Albert Hart, Allen Wallis, Milton Friedman, Jacob Mosak; at Harvard Abram Bergson, Shigeto Tsuru, Robert Triffin, Wolfgang Stolper, Richard Musgrave, Sidney Alexander, Joe Bain, Alice Bourneuf, Lloyd Metzler, John Lintner, Robert Bishop, Paul and Alan Sweezy, Richard Goodwin, Henry Wallich, James Tobin, Evsey Domar, Walter and William Salant, Emile Despres, Robert

Solow, . . . ; the list is endless. As I later wrote: "Yes, Harvard made us. But it is we who made Harvard." The Cambridge, Massachusetts which had begun as Keynes-hostile ended up as the chosen place to spend your postwar sabbatical year. It came to supplant Oslo and Rotterdam, the London School of Economics and Cambridge, England. Had I remained on the Chicago Midway, I might well have missed out on the three revolutions that remade mainstream economics: the Keynesian Revo-**Imperfect-Competition** lution, the Revolution, and the Mathematical-Economics Revolution. Instead I had a front-row seat. In races, once you get a lead you have only to run as fast as the rest to stay up front. (In my own sober self-audit, my 500-odd collected scientific papers outweigh for me all textbook bestsellers or Newsweek columns or governmental testimonies.)

2. The Revealed Preference Story

In 1938 I had proposed a novel paradigm of "revealed preference." For the 2-good case this could provide a com*plete* description of *all* the observable (and testable, and refutable) empirical (price, quantity) data of a coherent demand system. As an example, it could prove, for as many as n-1 out of n goods, there could be *negative* income elasticity at any specified income and prices; and that an own price elasticity, $(\Delta \log q_i)$ $\Delta \log p_i$, p_i , could be Giffen *positive* for any such "inferior" good and *only* for such. Also, necessarily $\partial q_i / \partial p_i \leq q_i (\partial q_i / \partial I) \geq 0$ for all goods. It could also deduce cogently that q_i demands remain invariant when all prices double (or halve) at the same time that nominal expendable income doubles (or halves). These stated results are essentially the *only* and the *exhaus*tive empirical content of received preference-maximization formulations. As I have reported, my revealed-preference innovation came from a marriage between Haberler-Könus index number theory and Gibbs finite-difference formulations of classical phenomenological thermodynamics of the 1870s.

My approach looked backward in summarizing "economically" (in the Mach-Vienna Circle sense) the "meaningful" (testable and, in principle, refutable) core of constrained-budget demand theory. It could do so without mention of "mind" or "brain" or "introspection." It had no explicit need for a utility metric by which beans could be judged to yield twice the utils of peas; or for statements like "My love for Mary exceeds that for Jane in exactly the degree that my love for Jane exceeds that for Fifi." In non-stochastic conditions, I cared not whether the observed demander was risk-neutral or risk-averting or risk-relishing.

My good day's work was well rewarded. My Master, Professor Schumpeter, oh-ed and ah-ed. But in Foundations I chose to downplay this paradigm. One reason was substantive. For more than two goods, $n \geq 3$, my socalled Weak Axiom was recognized to be necessary but not to be alone sufficient to deduce transitivity of preferences. It could not rule out "Jones chooses (3 rye, 2 corn, 2 peas) over (2 rye, 2 corn, 3 peas) and chooses (2 rye, 2 corn, 3 peas) over (2 rye, 3 corn, 2 peas); but also (!) chooses (2 rye, 3 corn, 2 peas) over the initial (3 rye, 2 corn, 2 peas)!" For n > 2 goods, always satisfying the Weak Axiom as applied pair by pair could, in the most general case, decidely tolerate a *transitivity* contradiction-if not after three comparisons then after 3,333 comparisons. (This could not occur if Jones really did adhere to a transitive partial preference ordering.) What was needed to be

added to the Weak Axiom so that (p_i,q_i) observable-data tests could be strengthened to their maximal extent? Young Hendrik Houthakker (1950), on his own in Holland, later supplied the needed Strong Axiom. Already in 1938, before I could know of Houthakker, I had conjectured that it might be enough to specify that "no chain of weak-axiom rulings can ever lead to a contradiction like (A weak-revealed better than B, B weak-revealed better than C, . . . , Y revealed better than Z-but Z weak-revealed better than A!)." I propounded this conjecture to two of the best young mathematicians anywhere: Stan Ulam and Lynn Loomis, Harvard Junior Fellows of my time. (Ulam later invented for Teller the American hydrogen bomb at Los Alamos; Loomis, knowing nothing of game theory, after hearing John von Neumann issue a challenge for an algebraic, non-topological proof of his (1928) two-person, zero-sum game theorem, went home and found the needed proof that night.) But, as I have discovered in life, the great pure mathematicians I have known, except for John Nash, sensibly resist concentrating on puzzles arising from esoteric fields like economics. At the time Foun*dations* went to press, the revealed preference paradigm still lacked completion.

The second reason I soft-pedaled claims for it went deeper and remained after Houthakker's tour de force. The Samuelson-Houthakker paradigm, α , properly exposited, became equal in empirical meaning to the indifference field approach of Pareto-Allen-Hicks-Hotelling, β ; and equal to the ordinalutility formulation of Eugen Slutsky (1915) or John Hicks (1931), γ ; and equal to a Kenneth Arrow (1959) lattice-theory partial ordering, δ . When members of a class are equal, each is *first* among equals. Also, each is *last* among equals. Why debate the different merits of essential equals? Since the δ version is the most "intuitive" one, be satisfied when you demonstrate that α does exhaust *all* the valid empirical content of (α , β , γ , δ).

Professor E. Roy Weintraub (1983) has astutely recognized that Samuelson's (1938) Weak Axiom was already in Abraham Wald's (1934-35) second paper on determinateness of Walras-Cassel-Schlesinger general equilibrium. But I did not in 1938 "refine" that: as often mentioned, my approach arose from considering Haberlerian restrictions that apply to Laspeyres and Paasche quantity index numbers for a "rational consumer." Wald's arose from a search for a set of sufficient conditions to guarantee uniqueness of equilibrium; if I had known Wald's paper and had had the wit to see in it the Weak Axiom, I would certainly have invoked the prestige of Wald's name to help sell the Weak Axiom to readers.

It has been said that no good deed goes unpunished. As a result of my (uncharacteristic) modesty in playing down revealed preference in *Foundations*, some writers have suspected some failure in the paradigm. On reflection, thanks to Houthakker, all I hoped for (or could rationally have hoped for) was attained by it.

Revealed preference, aside from looking backward to consolidate and elucidate received doctrines, inadvertently looked ahead into the finitemathematics of inequality-duality relationships that formed the modern Age of Debreu in economics. Gibbs led me to the promised land before there was a promised land. Indeed, among my alternative 1938 formulations (with respect to ">" or " \geq "), can be found valid relations applicable to admissible specifications of *non-convex* sets. (Example: If a good *never* displays *negative* income elasticity, it can never display a *positive* [Giffen] own-price elasticity—even if indifference contours cease to be convex and demand functions are neither single-valued nor uniquely invertible.)²

3. Admired Influences

Who were the major writers influencing *Foundations*? They were many and various. E. Roy Weintraub (1989, 1991) suggests that there is some mystery here, even maybe some cover up. Interested readers will want to compare the following paragraphs with his account and with earlier memoirs by me.

First, my heroes in economics were scholars such as Léon Walras, Antoine Augustin Cournot, Francis Edgeworth, Vilfredo Pareto, Irving Fisher, and Knut Wicksell. (This was after my infantile infatuation with Frank Knight simmered down to measured respect for a brilliant but erratic economist and theologian.) Among working economists during the 1930s, John Hicks and Ragnar Frisch (two very different egoists)

2 Prior to Houthakker, for n > 2, I tended to side with Roy G.D. Allen rather than with Hicks's insistence upon integrability. Why not be general and be happy to posit non-integrability and global non-transitivity? In those cases, only the Weak Axiom could be validly posited as a constraint on empirical demand observations. My reading of Grif-fith Evans (1930), Allen (1932), and Nicholas Georgescu-Roegen (1936) softened me up for such a half-way house compromise. But, Freudianly, that was perhaps making a virtue of necessity. Once Houthakker delivered me from such necessity, in a 1950 exchange with Herman Wold I lost my tolerance for global intransitivity, which came to smack of uninteresting formalism for its own sake. See Stanley Wong (1978) for related discussions. A reader of Philip Mirowski (1989) may find some difficulty in reconciling remarks there and remarks here. Remark: The deep points raised by Dr. Wong can, I believe, be argued out in the 2-good case without prejudice to their evaluations. Presence or lack of presence of Giovanni Antonelli's (1886) observable integrability conditions introduce interesting technical points but Wong's preoccupations will remain to be addressed even when integrability is assured (as in the 2-good case).

got the most attention from me. (In my 1932 Chicago freshman year, my tutor Eugene Staley told me that John Maynard Keynes was then the world's greatest economist. This was after the Treatise on Money (1930) but before The General Theory (1936). I had no reason to disagree. In 1936 at Harvard, Assistant Professor John M. Cassels told me that John Hicks was the world's leading younger economist. I could believe that, based on the Theory of Wages (1932) and the Hicks-Allen (1934) collaboration. Allen, who visited Harvard, became a good friend, and I always thought he received too little credit. Jan Tinbergen was an admired role model, and I approved when Frisch and Tinbergen shared the first Nobel Prize in 1969.)

In 1935, Alfred Marshall still ruled the roost in fame. What goes up too far comes down too low. Like Gustav Cassel's, his textbook filled a real need; but, like Isaac Newton, he had an inhibiting influence on two generations of followers. Marshall never lived up to his potential, for reasons of health and temperament. Before 1890 he knew the defects in his own constructs (consumers surplus, partial equilibrium, . . .) but never did he follow up with the needed improvements. As Whitehead said to me, "Marshall was more Popish than saintly. We liked Mary Paley Marshall better."

When I came to know John and Ursula Hicks well, I said to him: "I have the best of both worlds. I know your work and know my own, too." In this relative neglect of other scholars Hicks was even-handed. For him the sun rose when he opened his eyes. He wrote well and lectured badly. In Britain, Hicks's originality and breadth never received its full due, perhaps in part for reasons of personality and of "political incorrectness." Hicks's Value and Capital (1939) was an expository tour de force of great originality, which built up a readership for the problems *Foundations* grappled with and for the explosion of mathematical economics that soon came.

4. Mysteries Deciphered

In this English version of my published German essay (Niehans, et al., 1997), I can be brief on certain queries that over the years have arisen about Foundations. (1) Why does it seem to say nought about use of a Lyapunov Function that ever decreases towards a zero rendezvous at the equilibrium asymptote of a dynamic system, and thereby deprive itself of a classic method of proving damped stability of an economic system? (2) Herbert Simon (1959) and Joel Cohen (1987) have noted that Paul Samuelson was an admirer of Alfred Lotka, the mathematical biologist; but did he covertly owe more to Lotka than is explicitly acknowledged in footnotes and bibliographic references? (3) How important an influence on Samuelson was the Gibbsian biochemist L.J. Henderson at the Society of Fellows and in connection with his Pareto cell at Harvard? E. Roy Weintraub (1989, 1991) has nominated questions like these and assayed in a lengthy article on *Foundations*' dynamics to provide them with critical answers. Interested readers may be referred to my German text's detailed attempts to provide dialogue on these topics.

1. I did not use the *name* "Lyapunov Function" in stability analysis for a simple reason: not until World War II did I know that *name* for this 1892-and-earlier technique. But repeatedly *Foundations* did use such Lyapunov Functions, roses by whatever name. Thus, from J.W.S. Rayleigh's monumental *Theory* of Sound (1870), I had early learned to prove stability in the following fashion:

$$\begin{aligned} x + a\dot{x} + x &= 0 \to \frac{1}{2}\dot{x}x + \frac{1}{2}a\dot{x}^2 + \frac{1}{2}\dot{x}x = 0\\ \to & (d/dt)[\dot{x}^2 + x^2] = -\frac{1}{2}ax^2 < 0 \text{ for } a > 0. \end{aligned}$$
(1)

The above bracketed expression is a Lyapunov Function that ever declines toward zero, thereby entailing $[(x(t)\dot{x}(t)] \rightarrow [0 \ 0]$ as $t \rightarrow \infty$.

Similarly, *Foundations* repeatedly studied gradient motions:

$$\dot{x}_i = a \partial F(x_1, \dots, x_n) / \partial x_i$$

$$i = 1, 2, \dots, n; a > 0.$$
(2)

For F a *strictly* concave function with a maximum at $(\bar{x}_i) = (0)$,

$$\lim_{t \to x} [x_1(t) \dots x_n(t)] = [0 \dots 0]$$
(3)

by virtue of strict concavity's entailing the following Lyapunov Function relations:

$$\dot{F}(x_1,\ldots,x_n) - \dot{F}(\overline{x},\ldots,\overline{x}_n) = \sum_{1}^n x_j \delta F / \delta x_j < 0.$$
(4)

Again, Foundations innovated the then-new concept of quasi-definiteness for an n^2 matrix $[a_{ij}]$, and demonstrated by Lyapunov Function reasoning that

$$\dot{x}_j = -\sum_{1}^{n} a_{ij} x_j, \ [a_{ij} + a_{ji} = a^s, pos.def.$$
 (5)

is locally and globally stable. My German text gives page references that dispel mystery about a failure in Foundations to employ the rudimentary Lyapunov technique. My actual 1947 explicit references to Alexander Lyapunov, Emile Picard, and Birkhoff had mostly to do with the more delicate cases of borderline stability related to measure-preserving conservative Hamiltonian and more general systems; prior to the 1960s breakthroughs in chaos theory, I in the late 1930s was too unsophisticated to grapple with Henri Poincaré, George Birkhoff, Edward Lorenz, and Stephen Smale subtleties.

2. My major benefit from Lotka came

in connection with dynamics (as for example his autonomous one-sex population growth model, and his Lotka-Volterra predator-prey model). My three Lotka references came appropriately late in the book because dynamics came late. See the German text for more detail on my admiration for Lotka. Here I need only stress that it was his physical-reductionist biology that interested me, and this is far removed from Marshall's palaver about a biological paradigm in economics. A century on, it is Darwin-Lotka-Fisher-Haldane-Wright-Hamilton mechanisms that have made some progress in economics.

3. My relation to the Pareto-Henderson-Homans-Curtis coterie at the Society of Fellows can be simply put. These turned out to be purely social. I went but once to the famous Henderson sociology seminar. That was either once too many or many too few. When I would want to talk about Gibbs to Henderson, he would prefer to enumerate the shortcomings of Franklin Delano Roosevelt. In 1937 it was a case of Henderson's being too old or Samuelson's being too young, or both. My guarded admiration for Pareto the economist has been great; but during the vogue for his sociology, I stayed out to lunch.

5. The Road Not Taken

Foundations, for the most part, had a unifying theme: how and why one could predict with qualitative certainty the direction of change for an optimizing maximal variable when its exogenous price in the bilinear product . . . — p_jq_j — . . . is perturbed upward. Chapter 3 outlined the general deductive logic; Chapter 4 applied it to cost minimization and supply response; Chapters 5–7 handled the implications for constrained-budget demand optimization. Had I been a strict constitutionalist, I might well have stopped there. The result would have been a shorter 200-page book with one fullyintegrated theme.

Young men in a hurry are prone to want to tell all they know (and sometimes we overshoot!). It seemed a pity not to add an eighth chapter which would explicate, à la Bergson, for the great pioneers in "modern" welfare economics—Arthur C. Pigou, Lionel Robbins, Hotelling, Hicks, Nicholas Kaldor, Pareto, Tibor Scitovsky, John Stuart Mill, Ian Little, John Harsanyi, . . . (note the admixture of nineteenth century savants, 1930s pioneers, and writers not yet born in the antebellum status quo)-their own meanings. Having, in King Alphonso's words, "been present at the creation," I understood precisely the clarification achieved for welfare economics by Bergson's (1938) magisterial synthesis. Using the words the poet Alexander Pope addressed to Newton, I later wrote,

Ethics and Ethics' laws lay hid in night: God said, "Let Bergson be!" And all was light.

Bergson's Welfare Function of Individualistic Type made clear for Hicks and Kaldor—had they deigned to pay heed exactly how their Mill-Pareto Optimality calculus (in which winners can overcompensate losers) fits in with exogenously prescribed interpersonal value norms. Responsive to Geoffrey Chaucer's "And gladly teach," I composed Chapter 8's exposition of Bergsonian welfare economics even though that strayed from early chapters' central theme. Ralph Waldo Emerson and Einstein would have approved: consistency can be a hobgoblin, and elegance is indeed for tailors rather than serious scholars.

One liberty begets another. I could not resist the temptation to add Part

Two on dynamics, even though much of my focus there was on "macroeconomics" (a word not yet coined by Erik Lindahl, Peter de Wolff, or Lawrence Klein). No one associates a Keynesian system with a maximizing single mind or even to an as-if-pretend maximizing system. Yet from consideration of The General Theory's "stable" dynamics, one could predict that a rise in the propensity to invest would increase, not lower, underemployment equilibrium output and GNP. Why that might be possible needed to be researched in the late 1930s. Catastrophe theory and chaos theory were not yet born or reborn in the math literature, but I caught a look at heuristic "correspondences" between dampening in dynamics and qualitative direction of (comparative-statics) equilibrium responses to exogenous perturbations. "If one's reach cannot exceed one's grasp," what is innovating for?³

In retrospect, I have never regretted not taking the road not taken. *Foundations* came a bit too late to be responsi-

3 In reading this essay, Robert Solow remarks on the surprising absence of general equilibrium as an explicit topic in Foundations. Why? I reconstruct memory of my then state of mind as follows. When many folks are alike with (quasi-concave, smooth) tastes, and like initial endowments of goods, it is easily provable that a general equilibrium would exist and be unique. From 1879 on, Marshall and I knew that the exchange equilibrium need not be unique when tastes and/or endowments differed; but I sensed topologically that at least one equilibrium existed. (Only later, from Gerard Debreu, did I realize that "almost always" the number of possible equilibria would be finite.) From Irving Fisher (1892) and Wicksell (1893), Franz Zeuthen (1933), Hans Neisser (1932), Heinrich v. Stackelberg (1933) and Hicks (1931), I understood the Schlesinger-Wald duality relation, according to which a good's non-negative price would be zero when its supply was redundant. And from early Pareto I had learned that a competitive equilibrium could support any defined feasible normative optimum à la Adam Smith. So with the naive optimism of youth I didn't know enough to miss the fixed-point theorems of Debreu-Arrow (1954) and Lionel McKenzie (1959).

ble for the first J.B. Clark Medal of the AEA in 1947. But it is a safe guess that it did accelerate a 1970 Nobel Prize that came, if anything, a little too early for Platonic justice.

6. Finale

The above memoir was written before I could read the many kind words of Schefold, Niehans and von Weizsäcker. As Dr. Samuel Johnson observed, those who communicate at a birthday party are not strictly under oath. Nonetheless, among the compliments were just qualifications: thus, Niehans and Schefold are right to hint that the heuristic "correspondence principle" failed to develop the scope of the "maximizing principles." And von Weizsäcker, himself a deep analyst of Marxian and non-Marxian capital theories, is all too corin the observation that rect my demonstrations on the confusions and sterilities of the Marxian noveltiesequalized rates of surplus values, the Transformation Problem as simply an initial return by Marx from his Mehrwert cul de sac back to the bourgeois square one-has had few converts on the Left. Still, like Galileo, I mutter under my breath the symbolic, "But it does move."

Worse than an ungrateful child is an ungrateful parent. And I have many times been a grateful parent. At its fiftieth birthday, I hail Foundations of Economic Analysis:

* * *

Parens gratvs filio pergrato gratias agit.

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