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Revisiting Samuelson's *Foundations of Economic Analysis*

(Version 3)

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Abstract

Paul Samuelson's Foundations of Economic Analysis played a major role in defining how economic theory was undertaken for many years after the Second World War. This paper fills out Samuelson's account of the book's origins and corrects some details, making clear his debt to E. B. Wilson and establishes that turning the thesis into a book was a long process. The contents of the book and its reception are then reviewed.

JEL codes: B2, B3, C0, C6

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1. Preliminaries

Samuelson and modern economics

The formative period in the emergence of modern economics was the decade centered on the Second World War. This decade witnessed the take-off in the idea that mathematical modeling was central to both economic theory and empirical work, two facets of economics that were increasingly separated. Whereas some of the most prominent theorists of the inter-war period (e.g. Frank Knight, Jacob Viner, Allyn Young, John Maurice Clark, or Edward Chamberlin) were were uncomfortable about using what would now be considered basic mathematics, after the Second World War, economic theory became progressively more mathematical. There was a similar transition in empirical work, where there was a change from the empirical work represented by Wesley Mitchell and Gardner Means—data intensive but involving no formal statistical inference—to the methods of modern econometrics, though limitations of the computing power available to economists held back the spread of formal statistical modeling and inference for many years. Symbolic of this change in economics was the rise to prominence of the economics department at the Massachusetts Institute of Technology (MIT). A department of social science that in 1940 was still focused on providing service teaching for engineers became, by the 1960s, one of the most important economics graduate schools in the world. Economics at MIT was part of not just an engineering school, but the engineering school that had received the lion's share of wartime science funding and, with its massive research laboratories and had become one of the pre-eminent locations of "big science".[2]

[2] On economics at MIT see the articles in 2014, in particular Cherrier 2014.

One of the leading figures in this transition was the winner of the AEA's first (1947) John Bates Clark Medal, Paul Anthony Samuelson. As a Junior Fellow at Harvard from 1937 to 1940, he wrote a series of papers that changed the basis on which theoretical debates were conducted in several fields: consumer theory (revealed preference), the theory of international trade (the Heckscher-Ohlin-Samuelson model), Keynesian economics and the business cycle (the multiplier-accelerator model). He formalized the problem of dynamic analysis in economics, bringing these ideas together, along with a codification of welfare economics, in a book, *Foundations of Economic Analysis* (1947). For decades, this book was a core graduate microeconomics textbook and, for many economists, defined the way to do economic theory. For example, in 1960, having read in one of his textbooks that it was "the most important book in economics since the war", Robert E. Lucas (1995) taught himself economic theory by reading its early chapters, applying the techniques he learned from it to the problems that Milton Friedman posed in his Chicago price theory course.[3]

Samuelson's own career illustrates the way economics changed during this period. As a graduate student at Harvard he was considered a specialist in a field called "mathematical economics", in which posts were so difficult to find that his teachers feared that he might never land an academic position. At MIT, his mathematical skills were more highly valued because every student in the Institute was required to study math and physics, but he moved very reluctantly because he wanted to remain in Harvard's much stronger department. Even this opportunity presented itself only because a vacancy arose at short notice when staff were lost through preparations for war (see Backhouse Backhouse 2014). In contrast, by 1948, he was a full Professor who was being sounded out by most of the the leading departments,

[3] Friedman's treatment of these problems was largely intuitive and graphical.

including Chicago which came closer than any other university to luring him away.[4] He committed himself to MIT, where he spent the rest of his long career. According to President James Killian, the success of MIT's economics department was due to Samuelson and the distinguished economists who found satisfaction in being his colleagues.[5]

Clearly, other works were important in the rise of mathematical modeling in economics, notably John Hicks's *Value and Capital* (Hicks 1939), which introduced many economists to more formal general equilibrium analysis, and *The Theory of Games and Economic Behavior* by John von Neumann and Oskar Morgenstern (von Neumann, & Morgenstern 1944). However though *The Theory of Games and Economic Behavior* was well known among mathematical economists, game theory did not acquire its present status in the discipline until much later, and when it did, it was the non-cooperative theory of John Nash to which most economists turned rather than to von Neumann's and Morgenstern's model of coalition formation. And though Hicks might have been first, it was Samuelson's *Foundations*, with its focus on providing the mathematical and conceptual tools with which comparative statics results could be generated, that came to define the way most economic theorizing was done. Samuelson's best-selling elementary textbook (Samuelson 1948), the increasing prominence of the MIT economics department, his prominent involvement in debates over public policy, and a succession of widely-cited theoretical innovations all served to confirm his dominance of economics worldwide, and with it the status of *Foundations*.[6] In considering the origins of

[4] See Maas (Maas 2014). Harvard did not try because they knew he would prefer to stay at MIT.

[5] Killian 1985.

[6] These include the consumption-loan model (Samuelson 1958), the pure theory of public goods (Samuelson 1954), turnpike theory (Dorfman et al 1958), the use of the Phillips curve (Samuelson, & Solow 1960) and the theory of efficient markets (Samuelson 1965a).

Foundations, we come closer to seeing the origins of modern, technical economics than we do by looking at any other work.

"How Foundations *came to be"*

Given the importance of the book, it is not surprising that when Samuelson offered "How *Foundations* came to be", an abridged version of a paper first published in German (1997), John Macmillan immediately accepted it for the JEL, telling Samuelson, "How one of the great economics books came to be written makes a wonderful JEL topic".[7] In this article, he reflected on the book's origins as a Harvard PhD thesis (1940) and the reasons for the long delay between his first having the main ideas and the book's eventual publication. The thesis was not written until 1940, because he was precluded from working towards a higher degree by the conditions of his Junior Fellowship at Harvard; publication was then delayed, partly because of the Second World War and partly because of the anti-theory prejudices of Harold Burbank, the Head of Harvard's economics department, on whose desk it sat gathering dust before being sent to Harvard University Press.

This paper re-examines the origins of *Foundations* using evidence from Samuelson's papers (archived at Duke University) and the archives of the Harvard Economics Department and Harvard University Press. Samuelson's own account remains indispensable but needs re-examination. As people reflect on past events, points that seem significant in in view of their later interpretation of their own lives get elaborated and modified so that their stories become more coherent (see Tribe 1997, pp. 6-9; Weintraub 2007). Samuelson was no exception to this rule. He was a great raconteur who was forever recalling events from his own past. The

[7] McMillan J, January 26, 1998, Letter to Samuelson, AEA Papers, Duke University 2004-0007, Box 3 (Samuelson).

inevitable result was that the details he recalled most clearly were those that fitted his own perception of his life.

A good illustration is provided by the account of the publication process of *Foundations* in “How *Foundations* came to be”. Late in life Samuelson paid much more attention than he had done earlier to the anti-semitism that pervaded Harvard in the 1930s, department chair Harold heading a “roll of dishonor” he sent to his friend Henry Rosovsky in 1989.[8] That animosity could explain why he blamed Burbank for holding up publication of *Foundations*, for trying to limit the print run to only 500 copies, and for destroying the type after the first print run. He also remembered Burbank as critical of him as a young theorist, writing, “Harvard’s long-time department chairman [Harold Burbank] 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+” (Samuelson 1998, p. 1378). Later, in the midst of a published denunciation of his teachers’ anti-semitism, Samuelson (Samuelson 2002, p. 51[9]) recalled Burbank saying to him,

> “Samuelson, you are narrow. Keynes and Hawtrey are narrow. Don’t take up economic theory until after you are fifty. This is what our great Allyn Young used to say.” Alas, I had already lost my heart, and aspired to become even more narrow; and furthermore, Young had died young, just before his rendezvous with greatness. … I was always a young man in a hurry.

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[8] Samuelson, September 26, 1989, Letter to Henry Rosovsky, PASP Box 63 (Rosovsky).

[9] The presence of non-Jewish students in this list could have been one reason why Stolper believed that Samuelson was rejected because of his ability rather than because he was Jewish.

The now-puzzling ageism-in-reverse reported here reflects the different, non-mathematical conception of economic theory then prevailing, at least in Burbank's mind. Theory was not thought something that required mathematical agility but the mature reflection and experience of an older scholar. Samuelson was in a hurry because, having been diagnosed as having high blood pressure, he expected to die young like his father.

In contrast, the archival record shows that, whilst the book did sit on Burbank's desk for a few months, (presumably, as Department Chair, he had a responsibility to review it before sending it to the publishers) it is clear that the timing of the book's publication was determined the availability of typesetting capacity (not all printers could handle the complex mathematics involved), and ultimately by the availability of paper (a rationed commodity in wartime). It also establishes that negotiations with Harvard University Press were handled by Abbott Usher, not Burbank, that the initial order was for 1200 copies (it is a mystery why only 887 were eventually shipped, the likely explanation being the availability of paper), and that the type was "killed" at the instigation of the printers, who pleaded with Harvard University Press that the department had no understanding of the shortage of type metal, then selling for almost double its normal price.[10] When the type was killed, this was on the assumption that the War Production Board would soon revoke controls on the use of paper and it would be easy to scale up production quickly.[11] Burbank's anti-semitism had affected Samuelson whilst he was a student, and there were ample grounds for resentment which

[10] Wilson, C.W., April 18, 1947, Letter to Alfred V. Jules, HUP; Jules, A.V., June 18, 1947, Letter to C. W. Wilson, HUP.

[11] Wilson, C.W., October 2, 1947, Letter to Alfred V. Jules, HUP. Email from Michael Aronson to the author, April 9, 2012.

could have affected his later memory, but the delay in the publication of *Foundations* can be fully explained by production difficulties.[12]

In addition, because we are effectively tracing the roots of modern economics back to the inter-war period, there is also a need to tie down the intellectual climate out of which *Foundations* arose. Samuelson was extremely generous in giving credit to others, notably his teacher and mentor, E. B. Wilson, with whom he remained in lifelong contact, but he was often vague about what they contributed to *Foundations*. This may be the result of a certain defensiveness in "How *Foundations* came to be", for one of his goals in that article was to contest the history of dynamics provided in Roy Weintraub's *Stabilizing Dynamics* (1991), about which he had been upset for several years[13]. Though this chapter sought to place *Foundations* in the intellectual climate in which it was written, Samuelson took him to be suggesting that there was "some mystery, and maybe even some cover up" (Samuelson 1998, p. 1381). Thus Samuelson (1998, p. 1383) stressed the extent to which he was self-taught through hours spent in the Widener Library, asserting that he found nothing of interest in Lawrence Henderson's sociology, and recalling that when he tried to discuss Willard Gibbs with him, Henderson preferred to enumerate the failings of Franklin Roosevelt. There is no reason to doubt this, but his remark hardly addresses the question of whether three years of weekly dinners at the Society of Fellows, dominated by Henderson, may, directly or indirectly, have left more of a mark on his thinking than Samuelson remembered.

For these and other reasons, then, it is instructive to revisit the process whereby *Foundations* came to be.

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[12] The issue of anti-semitism is discussed in more detail in Weintraub 2014 and Backhouse 2014.

[13] Weintraub says that this was reported to him by Robert Summers (Samuelson's brother) and Lawrence Klein (Samuelson's first Ph.D. student). The original version of the JEL article, published in German, discussed Weintraub at length.

2. Graduate study at Harvard

Chicago and Harvard

Samuelson began his account of the origins of *Foundations* by telling stories about his childhood, speculating on the link between being precocious and having “ridiculous” levels of self-confidence, and his undergraduate education at Chicago. Aaron Director introduced him to economics with a very traditional economic principles course, but one in which he drew attention to the equation for general equilibrium in Gustav Cassel’s *Theory of Social Economy* (1923). In his final year he took Jacob Viner’s graduate price theory course in which, to the delight of the graduate students, many of whom lived in terror of Viner’s socratic method combined with a three-mistakes-and-you-are-out rule, Samuelson used to point out mistakes on the blackboard. It was in Chicago that Samuelson decided that learning mathematics would help his economics, something that was far less obvious then than it is today. A graduate student, George Stigler, had exposed Samuelson to the joys of large determinants, showing him notes on a course taught by Henry Schultz, and Viner explained that calculus was a prerequisite for a proper understanding of indifference curves and production possibility frontiers, though admitting that he did not understand it himself (Samuelson 1972b, p. 911). Having taken Elementary Mathematical Analysis at the end of his junior year, Samuelson appears to have taken as much mathematics as he could in his senior year, with courses in statistics, algebra, plane analytic geometry and calculus. After graduating he took a summer course in differential equations.

However, though it was at Chicago that he had decided to become an economist and that he had realized the importance of mathematics, the most important part of Samuelson's education took place at Harvard, where he took courses from 1935-7, before becoming a Junior Fellow, turning out paper after paper, from 1937-1940. By the time his thesis was submitted, he had published 20 articles covering consumer theory, capital theory, international trade, unemployment and business cycle theory and was widely considered to be the leading young economic theorist. He was using mathematical techniques that left most of his teachers behind. Though he acknowledged the importance of Joseph Schumpeter, Wassily Leontief, Gottfried Haberler and Alvin Hansen in teaching him modern economic analysis (see also Samuelson 1976, p. 27), he made it clear that the main influence on *Foundations* was Edwin Bidwell Wilson. He took Wilson's courses in mathematical economics and mathematical statistics, and spent hours talking to him after lectures (Samuelson 1972a). In "How *Foundations* came to be", he confined himself to saying that Wilson had vaccinated him to understand that economics could use the same mathematics as physics without resting on the same empirical foundations and certainties (Samuelson 1998, p. 1376). Wilson had, he claimed, contempt for social scientists who aped the natural sciences "in a parrot-like way".[14] To find more detailed statements about what he learned from Wilson, it is necessary to look elsewhere in his writings (e.g. 1947, p. 81; Samuelson 1972c, p. 254; Samuelson 1965b [CSP], p. 438]; Samuelson 1967 [CSP], p. 655, 673)

Schumpeter acted as Samuelson's mentor for many years. He taught the main graduate economic theory course, which Samuelson later praised for the range of topics and authors it covered (Samuelson 1951, p. 102). They became close and Schumpeter continued to support

[14] This counters the claims of Mirowski 1989 whose work he considered completely mistaken.

him, commenting on his papers and giving feedback on his first presentation to an AEA meeting (in a session Schumpeter chaired). Schumpeter was an enthusiast for mathematical economics but his command of mathematics was weak. He inspired many students and it is possible to see Samuelson as having achieved the breakthrough in mathematical economics that Schumpeter sought but could not achieve himself. It was Leontief, better trained in mathematics and, by 1935, teaching the course that Schumpeter had set up to improve the teaching of mathematical economics, who provided Samuelson with a more rigorous training in formal economic theory than Schumpeter was able to provide.

Though Samuelson disparaged Edward Chamberlin (placed just below Burbank in his roll of anti-semitic dishonor), claiming that at the age of 35 he was already at the zenith of his scholarly career (Samuelson 2004, p. 4), his publications show that he thoroughly absorbed the content of Chamberlin's *Theory of Monopolistic Competition* (Chamberlin 1933), covered extensively in a course Samuelson took from him in 1935 and also covered in Schumpeter's economic theory course. Haberler, whom Samuelson remembered much more positively, was important because Samuelson's theory of revealed preference (1938c; 1938b) grew out of Haberler's theory of index numbers.

> My own work in this direction grew out of a remark made to me by Professor Haberler in his I936 international trade seminar at Harvard. "How do you know indifference curves are concave?" My quick retort was "Well, if they're not, your whole theory of index numbers is worthless". Later I got to thinking about the implications of this answer (disregarding the fact that it is not worded quite accurately). Being then full of Professor Leontief's analysis of indifference curves, I suddenly realised that we could dispense with almost all notions of utility: starting

> from a few logical axioms of demand consistency, I could derive the whole of the valid utility analysis as corollaries. (Samuelson 1950, pp. 369-70)

Samuelson's theory of revealed preference resulted from seeing that Haberler's own theory of index numbers could be used to show that Leontief, who used indifference curves in his trade theory, was right to assume that indifference curves were concave.

Edwin Bidwell Wilson

Edwin Bidwell Wilson is not well known among economists, but his importance to Samuelson and hence to *Foundations* cannot be overstated. Wilson was one of the two protégés of the great American physicist, Willard Gibbs, leading Samuelson repeatedly to express his admiration for Wilson by claiming Gibbs as his intellectual grandfather (this analogy would, of course, imply that Irving Fisher, Gibbs's other protégé, was his uncle) (e.g. Samuelson 1969, p. 10). Wilson had been trained in mathematics at Yale, where he co-authored a book with Gibbs on vector analysis, establishing what has since become standard notation, and the Ecole normal supérieure in Paris. His first publication, "The so-called foundations of geometry" (Wilson 1903) took on the leading pure mathematician of his day, David Hilbert. After a brief spell at Yale, he joined MIT as Professor of Mathematical Physics in 1907, where he wrote textbooks on calculus and aeronautics. In 1922 he moved to Harvard as Professor of Vital Statistics in Harvard's School of Public Health, where his publications included articles on confidence intervals in statistics. His contribution to the Economics Department's teaching was courses mathematical economics and mathematical statistics that he taught in alternate years. In the course on mathematical statistics taken by Samuelson in the spring of 1936, Wilson spent several weeks on the first 12 pages of Arthur

Bowley's *Mathematical Groundwork of Economics* (1924), his reason for proceeding so slowly being that "some of our high-powered mathematical economists did not know their fundamental definitions and would read right over a pair of statements that were contradictory and assume that both were right".[15] The previous year he had published an article in which be pointed out that, though Bowley referred to the marginal utility of money in discussing consumer theory, it was only its rate of change that entered into his proof.

However, whilst it seems certain that Wilson will have raised questions about the significance of the notion of utility to consumer theory, on which he had himself just published (Wilson 1935), Samuelson's main debt to him lay elsewhere. It was Wilson who stimulated Samuelson's lifelong interest in thermodynamics and the realization that economists could learn from physics (see Samuelson PA, 1992, [Answers] by Paul A Samuelson, PASP Box 152 (Donald A Walker), p. 3). Though politeness no doubt lay behind the remarks, there is no reason to doubt Samuelson's observation, made to Wilson when he left Harvard,

> I have benefitted from your suggestions, perhaps more than from anyone else in recent years, and even chance remarks which you have let fall concerning Gibb's [sic] thermodynamical systems have profoundly altered my views in corresponding fields of economics.[16]

For Wilson, the main things that economists had to learn from physics were how to build up an exact argument and how, when faced with difficult factual problems, to apply existing

[15] Wilson, July 14, 1936, Letter to John D. Black, EBWP Box 27 (B 1935-6).

[16] Samuelson, October 9, 1940, Letter to Wilson, EBWP Box 35 (S).

mathematical tools and develop new ones. After attending his lectures, Schumpeter wrote to Wilson, explaining why he found the lectures valuable.

> I was strongly impressed with the immense value to the economist of such lectures as you gave in the first part of the course. I perfectly agree with those who object to the practice of some economists, simply to copy out what they believe is an economic argument from textbooks of pure mathematics or of theoretical mechanics or physics, and I hope you will not interpret what I am about to say in the sense of that practice, which sometimes comes near being ridiculous.
>
> But it is one thing to copy and another thing to learn how to apply existing, and to derive the stimulus for constructing new, mathematical tools in the face of difficult factual patterns. We must not copy actual arguments but we can learn from physics how to build up an exact argument. Moreover we can learn to understand the relation of mathematics to the reality to which it is applied. Most important of all is the consideration that there are obviously a set of concepts and procedures which, although belonging not to the field of pure mathematics but to the field of more or less applied mathematics, are of so general a character as to be applicable to an indefinite number of different fields. The concepts of Potential or Friction or Inertia are of this kind, and the problems and difficulties to which they give rise are formally much the same, whatever the subject matter.[17]

The examples Schumpeter cited (potential, friction, inertia) were presumably ones that Wilson had discussed.

[17] Schumpeter, J.A., May 19, 1937, Letter to, EBWP Box 29 (S 1937).

This view is precisely the one that informed Samuelson's use of mathematics. He learned from Wilson that there were similarities between the structures of economic problems and certain problems in physics and that, even though the evidential basis for the two disciplines was different, he could make use of these similarities to solve economic problems without implying that there was any deeper relation between the physical and the economic concepts. The main example of this was the Le Chatelier principle, the idea for which he attributed specifically to Wilson's lectures:

> In particular, I was struck by his statement that the fact that an increase in pressure is accompanied by a decrease in volume is not so much a theorem about a thermodynamic equilibrium system as it is a mathematical theorem about surfaces that are concave from below or about negative definite quadratic forms. Armed with this clue, I set out to make sense of the Le Chatelier principle. (Samuelson 1972c, p. 254)

Though the status of the Le Chatelier principle in physics and chemistry has been much debated, for Samuelson it was a result that held in any equilibrium system, whether physics, chemistry or economics. This was an important idea in his later work. It should be noted that though accounts of it are often couched in terms of differential calculus, the principle is much more general and applies, as Samuelson showed, equally to systems in which discrete choices mean that there is no smooth substitution between variables.[18]

Samuelson learned from Wilson the importance of basing economic theory on a firm axiomatic framework and of analyzing the general case where functions were not necessarily

[18] Milgrom 2006 discusses subsequent attempts to generalize the conditions under which the principle holds, including ones by Samuelson.

smooth and differentiable. Their correspondence makes it clear that Wilson pushed Samuelson to analyze finite changes and, as Gibbs had done, to base conclusions on inequalities linked to generalized notions of convexity. Directing Samuelson to the types of mathematics on which he was to rely in much of his work, Wilson recommended *The Calculus of Observations* (1926) which discussed many of the techniques that Samuelson was later to use in *Foundations* and which marked his book out from previous work on the subject. It focused on methods for obtaining numerical solutions, beginning with interpolation, difference equations, determinants, linear equations and statistical theory (linear regression and correlation analysis).

Wilson was also important in stimulating Samuelson's views on dynamics and comparative statics. Crucial evidence comes in a letter dated December 30, 1938, in which Wilson wrote detailed comments on a paper Samuelson had recently shown him.[19] Even if Samuelson's paper were an early attempt at tackling the relations between dynamics and comparative statics, the letter showed that he had not yet sorted out the material to Wilson's satisfaction.[20] Wilson thought that Samuelson's analysis might not be "so general in some respects as Willard Gibbs would have desired". Samuelson had written of certain "secondary conditions" and of "a certain form being positive definite". This caused Wilson to tell Samuelson that Gibbs used to talk of "non-negative quadratic forms" and that he used to lay great stress on the fact that it was important "to remain within the limits of stability". Wilson appears to have been telling Samuelson not only that he had not stated the second-order conditions for optimization correctly (positive definiteness was sufficient but not necessary)

[19] Wilson, December 30, 1938, Letter to Samuelson, EBWP Box 31 (S 1938).

[20] It not clear which paper this letter referred to, though it seems most likely that it was either this one or a draft of the corresponding chapter of his thesis.

but also that second-order optimization conditions and stability were related, an idea that was to become central to Samuelson's work.

> Now if one wishes to postulate the derivatives including the second derivatives in an absolutely definite quadratic form one doesn't need to talk about the limits of stability because the definiteness of the quadratic form means that one has stability. (Ibid.)

In an optimizing system, second-order conditions implied stability and *vice versa*.

It is also significant that in the same letter Wilson also reminded Samuelson of the need to consider the more general case where functions were not continuously differentiable.

> He [Gibbs] doesn't use derivatives but introduces a condition which is equivalent to saying that his function has to be on one side or in a tangent plane to it. He doesn't even assume that there is a definite tangent plane but merely that at each point of his surface it is possible to draw some plane such that at each point of his surface it is possible to draw some plane such that the surface lies except for that point and some other points entirely to one side of the plane. (ibid.)

The mathematics of convex sets and separating hyperplanes, which is what Wilson was describing, was not well developed, and he went on to say "Just how general a theorem one can get I don't know because I have never worked it out as carefully as I ought to have done". However, it remained the case that Gibbs had pointed out that if one exceeded the limits of stability, the conditions Samuelson assumed might not hold. Wilson's final advice was that he needed to explain himself better: "a little more text and not so many formulae in proportion to the text might make the whole decidedly easier reading", advice that Samuelson did not always take.

Marion Crawford

At Harvard he audited courses in thermodynamics and Fourier analysis. However, given the mathematics he used in *Foundations*, a particularly important course would seem to be one he took in the summer of 1936, at summer school at the University of Wisconsin, Madison. This was the theory of equations, taught by one of the few women in American mathematics, Margarete Wolf. She had obtained her PhD the year before, and two years later was to publish, jointly with her sister, a paper on the problems of deriving necessary and sufficient conditions for the existence of solutions to linear matrix equations and determining the number of solutions (Wolf 1936). Though this material may not have been covered in the course he took, it was very close to the topic of "systems of linear equations and determinants with applications" described in the course catalog. Given his exposure to Leontief's input-output analysis the term before, and his use of the textbook by Whittaker and Robinson, Samuelson could hardly have failed to see the significance of this material.

One reason for his going to Wisconsin that summer was that it was not far from the home of Marion Crawford, an economics major at Radcliffe College, whom he married in 1938. While he was taking courses in the theory of equations and functions of complex variables, she was studying money and banking (they took German together). After graduation she worked as an assistant to Schumpeter, who rated her extremely highly. For her undergraduate dissertation (Crawford 1937), she undertook a mathematical investigation of a concept that had only recently appeared in the literature, the elasticity of substitution, applying to it techniques in linear algebra that would have been beyond most economists of the time (and which she must have discussed with Samuelson). Her only academic publication was on tariff protection in Australia (Samuelson 1939), and Samuelson later suggested that he might have learned more from this than he had acknowledged when he wrote the paper with Stolper on protection and real wages that was the first to use the term Heckscher-Ohlin trade theory (Stolper, & Samuelson 1941; Samuelson 1994, p. 346). They wrote a joint paper on population dynamics, which remained unpublished. It is clear that her role extended beyond the typing, for he wrote that her contributions to the book were "too many" and that "the result has been a vast mathematical, economic, and stylistic improvement" (Samuelson 1940, preface; Samuelson 1947, p. vii). Given her qualifications it seems likely that Samuelson was not exaggerating, though it is impossible to say where, on the spectrum running from copy-editing to co-authorship, her input should be placed.[21]

3. The thesis

In the middle of 1940, as his Junior Fellowship was coming to an end, he took the decision, with Marion, that it would be prudent to obtain a Ph.D.[22] He composed and rearranged material "at fever pace", dictating some of the material to Marion, who typed the entire first draft of the thesis (Samuelson 1998, p. 1377). It bore the subtitle, "The Observational Significance of Economic Theory".[23] His use of the term observational in the title is consistent with his definition of "*operationally meaningful* theorems" as statements about the world that were in principle refutable (ibid., pp. 2, 3). This idea had been completely absent

[21] Samuelson always praised his fellow-students, arguing that it was they, as much as their teachers, who were responsible for what he called Harvard's golden age. However, aside from specific instances, such as his collaborations with Abram Bergson and Gustav Stolper, their precise role in the evolution of his thinking is unclear.

[22] Backhouse (Backhouse 2014) discusses this period of his life in detail.

[23] In *Foundations* (1947, p. 7) he incorrectly claimed that the subtitle of the thesis was "The operational significance of economic theory".

from his publications in 1937, the first hint of it coming in February 1938, in which he referred to his consumer theory, based on index number theory, as being "more directly based on those elements which must be taken as *data* by economic science" and "more meaningful" in its formulation (Samuelson 1938c, p. 71). But this is no more than a hint. His first clear commitment to operationalism came in the paper he presented to the Econometric Society in December 1937 (Samuelson 1938a). It is tempting to link this to discussions he had in the preceding three months in the Society of Fellows, where the weekly discussions over dinner were dominated by physiologist Lawrence Henderson. Samuelson later played down the significance of his contact with Henderson, though he had cited him when discussing operationalism in *Foundations* (1947, p. 5, n. 2). Henderson had used Gibbs's mathematics to apply the idea of general equilibrium to physiology, and when he discovered Pareto's sociology he saw that it could also be applied to social systems. Henderson organized the "Pareto Circle", an inter-disciplinary group exploring Pareto's ideas, though Samuelson claims he found it of no interest and attended only once.[24]

The term "operational" inevitably conjures up the operationalism of the Harvard physicist, Percy Bridgman which was widely known in the 1930s.[25] Even if Samuelson had not attended Bridgman's lectures on thermodynamics,[26] he could hardly have avoided him, given Bridgman's closeness to those running the Society of Fellows. However, the way Samuelson described operationally meaningful theorems, emphasizing "data" and "observation" (a term that is given prominence by its use in the thesis title) has much stronger echoes of the Vienna Circle ideas being developed by Rudolf Carnap, whom Samuelson's

[24] Weintraub (1991, chapter 3) presents the Pareto circle as part of the context out of which *Foundations* emerged.

[25] For example, Harvard's Stanley Stevens argued for operationalism in psychology.

[26] He has stated that he took a course in thermodynamics, and Bridgman taught the only course listed under that title.

friend Willard Quine had helped bring to the United States in 1936, and which Samuelson might also have encountered through the Senior Fellow, Alfred North Whitehead.[27]

Operationally meaningful theorems could be derived through using mathematics. Samuelson opened the thesis with a quotation, reproduced exactly in *Foundations, from* the mathematician, E. H. Moore, "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 these central features" (Samuelson 1940, p. 1). He sought to apply this idea to economics, presenting the theory underlying the theories of production, the consumer, international trade, the business cycle, and income determination. It had long been recognized that these all involved systems of interdependent equilibrium conditions but the novelty was seeing that there existed "formally identical meaningful theorems" in all of these fields. Not only would he derive operationally meaningful theorems but these theorems would be derived from a common set of underlying principles. The first source of results was that equilibria were often the solutions to optimization problems, for propositions about individuals could be derived from the hypothesis that "conditions of equilibrium are equivalent to the maximization (minimization) of some magnitude" (Samuelson 1940, p. 4). This was not an *a priori* truth but a hypothesis. The second was the assumption that an equilibrium must be stable, for otherwise it made no sense to analyze comparative statics results. This was necessary for the analysis of group behavior because at this level nothing was being maximized.[28]

[27] One of Samuelson's friends, Shigeto Tsuru, worked closely with Whitehead and took an interest in psychology.

[28] Note that phrase he coined to describe this, the "correspondence principle" was *not* used at this point.

> 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, without which there is no possibility of reducing the problem to that of a maximum or a minimum. Here the hypothesis is made that the system is in stable equilibrium or motion in terms of an assumed dynamical system. (Samuelson 1940, p. 4-5)

The justification for assuming stability was that "positions of unstable equilibrium, if they exist, are transient, non-persistent states" that will be observed less often than stable ones.

After these methodological and mathematical preliminaries, Samuelson turned to "cost and production", stressing that production, value and distribution were all aspects of a single problem. He was proposing a general theory and was trying to derive "all possible operationally meaningful theorems" (Samuelson 1940, p. 68). Given the traditional emphasis on derivatives and marginal conditions, he emphasized the need to consider finite changes.

> It is curious to see the logical confusion into which many economists have fallen. The primary end of economic analysis is to explain a position of minimum (or maximum) where it does not pay to make a *finite* movement in any direction. Now in the case that all functions are continuous, it is possible as a means towards this end to state certain *equalities* on differential coefficients which will (together with appropriate secondary conditions) insure that certain *inequalities* will hold for finite movements. It is no exaggeration to say that infinitessimal analysis was developed with just such finite applications in view. (Samuelson 1940, p. 92)

Though influenced by Wilson on this point, it meant his arguments applied not just to traditional theory but also to Leontief's input-output models.

This theme, that the existing literature was confused, continued in the next three chapters, in which he sought to show precisely what was contained in traditional utility theory. After a few pages on the evolution of the concept of utility, he turned to a mathematical treatment of the problem, drawing on his early articles. Revealed preference and notions of indifference were used to show how meaningful propositions could be derived. He clarified the meaning of concepts such as cardinal utility, independent utilities, and complementarity, and devoted a whole chapter to the problem of the constancy of the marginal utility of income, important because of the legacy of Marshall's ideas and because of attempts that had been made by Chicago's Henry Schultz to measure demand functions.

When he turned to dynamics, in the last two chapters of the thesis (covering material that also appeared in 1941 and 1943a) he carried on in the same vein, criticizing previous generations of economists for failing to investigate the character of the laws they claimed existed. If nothing is known other than that supply and demand determines prices, without knowing the shape of the functions, he claimed, "the economist would be truly vulnerable to the gibe that he is only a parrot taught to say 'supply and demand'" (Samuelson 1940, p. 192). Economists needed to derive comparative statics results. The first of his chapters on dynamics linked this to the stability of equilibrium. He then argued that economists needed explicit dynamic models that explained how, starting from arbitrary initial conditions, all variables would change over time. This took the mathematical analysis to an even higher level, for it might be modeled using "differential, difference, mixed differential-difference, integral, integro-differential and still more general" sets of equations (Samuelson 1940, p. 196). Even if some of the ideas would have been familiar to economists, he was using mathematical language in a way that only a tiny minority of mathematical economists would have previously encountered. He then proceeded to discuss different types of stability, illustrating this with several economic examples, including the multi-market equilibrium system used by Hicks (1939) and a Keynesian system, such as James Meade, Hicks and Oskar Lange were using. Though these were not the same type of supply and demand system, he treated the Keynesian system as something analogous to the other market systems he had been discussing in his book. Given that it was not based on optimization, this was the example that showed most clearly the importance of dynamics to comparative statics.

Samuelson achieved a number of things with these examples. The first was to show that dynamic processes were implicit in familiar economic examples, implying that economists could not argue that dynamics was unnecessary. They might not talk in terms of explicit dynamic systems, but they were nonetheless using them. The second was to illustrate some of the different types of mathematics that could be used: differential, difference and integral equations. One of his examples also illustrated stability of the second kind and the notion that systems might be subject to random shocks—that they might "take a random walk" (Samuelson 1940, p. 207). He referred to Brownian motion of large molecules undergoing random collisions, though argued that before applying such ideas, with the possibility that movements might become more variable the longer a system was observed, in economics, "some statistical evidence of its possible validity should be adduced" (Samuelson 1940, p. 208). Another example showed, through familiar examples, that stability analysis was not an esoteric matter that economists could ignore but that it was important for deriving comparative statics results. As in his previous chapters, the tone was of showing economists how to do properly the things that they had previously been trying to do, without success.

Dynamic analysis also made it possible for Samuelson to introduce randomness (also in one of his earlier examples) and to relate economic theory to econometrics (as the term eventually came to be understood). He provided a justification for representing economic equilibrium "as simply a statistically fitted trend", implying that the approach to data analysis represented by Henry Ludwell Moore and Henry Schultz, whose work he cited, might have rigorous theoretical justification. Citing Lotka's *Elements of Physical Biology* (Lotka 1925), he noted that a dynamic equilibrium of supply and demand was "essentially identical with the moving equilibrium of a biological or chemical system undergoing slow change" (Samuelson 1940, p. 236). Here, and in his discussions of concepts of stability, he was moving away from economics into the realm of mathematics and physical systems in general. He was sketching directions in which economic analysis might develop.

4. From Thesis to Book

Chronology

Samuelson's thesis was an obvious candidate for the Wells Prize, for the best publishable thesis in Economics, which brought the recipient $500 (equivalent to two months' salary when he was appointed assistant professor at MIT in October 1940) and also covered the costs of publication of the thesis by Harvard University Press. He remembered being awarded the Wells Prize in 1941, saying that he then revised his thesis whilst working on fire control (designing radar-guidance systems for naval artillery) at MIT's Radiation Laboratory, handing it in in 1944 (Samuelson 1998, p. 1378). However, the Harvard Corporation ruled his thesis, examined in December, would not be considered that year. It was not until February

10, 1942, that the department unanimously endorsed the recommendation, made by a committee comprising Seymour Harris, Haberler, Leontief and Hansen, to award Samuelson the prize.

By this time, Samuelson was in his second year at MIT, teaching courses in economic theory, mathematical economics (which had a course in differential equations as a pre-requisite) and business cycles. He had also become involved, through Hansen, as a consultant for the Full-Employment Stabilization Unit, at the National Resources Planning Board, traveling to Washington every other week. His role there was to help assemble and supervise a team that was forecasting consumer demand under varying assumptions about the distribution of income across households. He was also getting involved in debates over the multiplier and the theory of income determination (e.g. Samuelson 1942b,1943b). In June 1943, the NRPB was disbanded, and his teaching commitments at MIT rose.[29] In addition to his economics teaching, he also taught basic mathematics to US Navy officers.[30]

To focus resources on the war effort, MIT closed down much of its economics teaching for academic year 1944-5, and it was at this point, in March 1944, that Samuelson joined the Radiation Laboratory, as a mathematician in the theory group under physicist Ivan Getting. The pressure he was under, working 8.30 a.m. till 6 p.m., six days a week, able to work on his manuscript only in the evenings and on Sundays, no doubt explains why this stage of the process stuck in his memory. However, he had started revising the book much earlier, for on May 29, 1942, he had told Harvard University Press that he was undertaking "fairly extensive revisions" and that, due to his work at the NRPB, he would not be able to complete it in that

[29] This is discussed in Maas 2014.

[30] The exact date and content of this teaching has not been established.

year.[31] The description he provided of the book suggested that he was planning to extend its coverage of business cycle theory, a topic that he had covered only briefly in his thesis.

Progress continued to be slow, despite Samuelson having reporting to Abbott Usher, Chair of the Harvard Economics Department's publications committee, who was responsible for liaison with Samuelson and the Press, that he hoped to be able to finish it by September 1943[32]. In August 1943 he told Walter Salant, a friend from Harvard working in Washington with whom he was discussing the multiplier, that he was "on the home stretch".[33] However, in April 1944, he told Wolfgang Stolper that it was only with great difficulty that he could make any progress on "the remaining chapters of my own manuscript".[34] It would appear that at some point between these dates he had decided to include new material (the changes he made are discussed below). Given what he told Stolper, it seems unlikely that it could have been submitted before the summer of 1944. There was then the delay, discussed above, before the manuscript was submitted to the Press, during which he saw it gathering dust as he checked its progress once a month. (Samuelson 1998, p. 1378). Given that the book was sent to Harvard University Press in February 1945, this delay seems unlikely to have been more than a few months. If, as is possible, it had taken him a long time to overcome the difficulties he described to Stolper, in the limited time allowed by his work in the Radiation Laboratory, it could have been less than that.

New material

[31] Samuelson, June 28, 1942, Letter to David T. Pottinger, PASP Box 34 (H, 1940-57).
[32] Samuelson, July 8, 1943, Letter to A. P. Usher, PASP Box 85 (Foundations).
[33] Samuelson, August 11, 1943, Letter to Walter Salant, PASP Box 67 (Salant).
[34] Samuelson, April 6, 1944, Letter to Abram Bergson, PASP Box 16 (Bergson).

The modifications he made to the thesis were very substantial and are summarized in the Appendix. The most prominent change was the introduction of the "correspondence principle". In the Introduction to the thesis, Samuelson had explained the link between comparative static and dynamic theory by writing, "Here the hypothesis is made that the system is in stable equilibrium or motion in terms of an assumed dynamical system. This implies no ideological or normative significance …" (Samuelson 1940, p. 4). In *Foundations*, that became:

> 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 or normative significance … (Samuelson 1947, p. 5)

The content of his argument had not changed, but Samuelson had introduced a name for the idea and had elevated it to the status of a "principle".[35] Samuelson clearly thought that his work was in the forefront of a radical change that was taking place in economics.

> An understanding of this principle [the Correspondence Principle] is all the more important at a time when pure economic theory has undergone a revolution of thought—from statical to dynamical modes. While many earlier foreshadowings can

[35] The index entry for "Correspondence principle" gives three additional pages. On one, the phrase is not used, and the remaining two are in a new chapter.

> be found in the literature, we may date this upheaval from the publication of Ragnar Frisch's Cassel Volume essay of only a decade ago. *The resulting change in outlook can be compared to that of the transition from classical to quantum mechanics.* And just as in the field of physics it was well that the relationship between the old and the new theories could be in part clarified, so in our field a similar investigation seems in order. (Samuelson 1942c, p. 1; Samuelson 1947, p. 284, emphasis added)

It is hard not to speculate that he had in mind Keynes's implied comparison of the revolution to be wrought by his *General Theory* with that brought about by Einstein in physics.[36]

The new material on business cycle theory (Samuelson 1947, pp. 335-49) was less a survey of business cycle than a survey of the different mathematical methods that could be used to model the cycle. This was a topic on which many economists, several government agencies, were actively working, Samuelson being one of many economists in government service and academia trying to forecast whether the end of the war would be followed by depression, as had happened after the First World War. The basic distinction he made was between endogenous models (which explained the cycle as self-generating, determined by factors within the system being analyzed) and exogenous theories (which explained fluctuations in terms of factors outside the model). The problem of endogenous theories was that they required that there was no damping—that the parameters of the economic system were such as to generate a system where fluctuations neither faded away nor exploded. In physics there were constants that might generate such systems, but there was no reason to assume such constants in economics. Thus he was critical of Michal Kalecki (1935) for

[36] He would be familiar with Pigou's (1936, p. 115) remark that Einstein actually did for physics what Keynes claimed to have done for Economics.

imposing the condition that cycles not be damped. This was a milder version of an even more critical appraisal of Kalecki that he had made privately in a letter to Hurwicz:

> By the way, have you read Kalecki's most recent "Studies in Economic Dynamics"? He has a chapter on "pure" business cycle which, in my humble opinion, hits the low as far as method is concerned. In order to obtain his favorite mixed difference-differential equations he approximates differences by derivatives, but not all the way thru - then he would have had a simple differential equation. Also, he makes it non-linear to maintain stability regardless of coefficients, but he does not integrate the system explicitly, nor even write out the non-linear term.[37]

Samuelson then explained that the problem with linear endogenous models was that they could not explain the amplitude of the cycle: as with a pendulum, the amplitude could be of any magnitude, depending on where the system started. One way round the problem was to drop the assumption of a purely endogenous cycle and to assume that external factors kept the system going (though he did not cite it at this point, this was Frisch's "rocking horse" model of the cycle, according to which a rocking horse hit periodically by outside shocks would exhibit a continuing cycle). The other was to go for a non-linear model, citing several references to the literature on the use of such models in physical systems. An example of such systems was "billiard table" theories, such as Harrod's (1936) in which output moved between a full-employment ceiling and a floor. A problem here was that Hansen had shown that there was "no (relevant) natural bottom to the economic system" (Samuelson 1947, p. 340). He favored mixed endogenous-exogenous systems, citing his own work on the multiplier model (Samuelson 1939b; Samuelson 1939c). His point here was that if the

[37] Hurwicz, L., August 29, 1944, Letter to Samuelson, PASP Box 39 (Hurwicz).

multiplier-accelerator model was augmented with a periodic movement such as a sine wave (though not necessarily a sine wave), the system would, subject to certain conditions, settle down to a periodicity determined by the exogenous factor. He likened this to the physical phenomenon of resonance. In such a system, shocks would break into this, producing shorter cycles.

Finally he turned to "Mixed systems of a linear stochastic type"—linear models subject to random shocks. Such models had been analyzed by the Russian economist, Eugen Slutsky, and Frisch (1933), whose paper he described as brilliant, but Samuelson linked it to Fourier analysis and his MIT colleague, Norbert Wiener. What is significant about this is that he linked this approach to the cycle to the problem of estimation, citing Trygve Haavelmo's "The probability approach in econometrics" (1944) and work by Abraham Wald. Though it was technically much more difficult, he also outlined the problem of modeling the cycle as a non-linear stochastic system.

New material in Part I included a chapter, "Transformations, composite commodities and rationing" that tackled problems that were of great practical importance in wartime (rationing and index numbers) and which he had confronted in his work on consumer demand. This provided him with the opportunity to tackle a range of issues in consumer theory, notably generalizing a theorem from *Value and Capital* (Hicks 1939) on aggregation. However, the most substantial addition to Part I was a fifty-page chapter on welfare economics that became the canonical statement of what came to be known as Bergson-Samuelson social welfare functions. He included this, he explained in a letter to Viner, "because the condition of the current literature seemed to me to be in a rather scandalous state of confusion and ignorance".[38] Though there was nothing on this topic in his thesis, the

[38] Samuelson, April 9, 1948, Letter to Jacob Viner, PASP Box 74 (Viner).

origins of these ideas go back to 1936-7 when Samuelson's fellow student Abram Bergson kept asking him, "What can Pareto mean by this 1898 use of the French singular when he speaks of 'the social optimum'?" (Samuelson 1981, p. 224). The problem was that Pareto referred to a social optimum as if it were a single point, yet the conditions he proposed did not seem to define any unique point. They solved it by making a clear separation between ethical value judgements and empirical propositions. Bergson (1938) used this distinction to sort out confusing claims that had been made about welfare in the 1930s. Though Samuelson (1939a) had discussed welfare in the context of international trade, his first comprehensive discussion of welfare came in this chapter in *Foundations*.

Samuelson's starting point was a function that was even more general, and with less content, than Bergson's, for it began with a social welfare function, $W = W(z_1, z_2, ...)$ where the *z*'s are any variables that are thought relevant to social welfare. Any statement about what the *z*'s were involved making ethical judgements. Unlike Hicks, who tried to eliminate ethical judgments from welfare economics, Samuelson sought to establish what ethical judgements needed to be made in order to derive conclusions about welfare. He began with widely accepted judgements, such as that individuals' preferences "count" (not true, he claimed, for Nazi and Communist "totalitarian" states) before moving on to more controversial ones, such as those involved in assuming that welfare was the sum of cardinal utilities. Assuming no more than that individuals' preferences counted (implying a social welfare function of the form $W = W\left(U_1(.), U_2(.), ...\right)$, and that utilities depended on goods consumed and productive services supplied, he showed that, to use terminology introduced by Ian Little (1950) shortly afterwards, there was an infinity of Pareto optima. All that the production and exchange conditions could do was to establish that there was an equation relating the well-being of different people in the system: a “utility possibility function” that determined the maximum utility any person could achieve given the utilities of everyone else.

Though it rested on different foundations, Samuelson claimed that this analysis of welfare economics completed his static analysis of maximization. Because it was less clear what should be maximized, the nature of the discussion had to be very different, focusing on conceptual, more philosophical problems. Scientific economic analysis might not be able to arbitrate between competing value judgements but it could analyze the implications of different sets of values.

5. The reception of the book

Despite his youth, Samuelson was already well known by 1947, with around 50 academic papers to his credit, and the publication of his thesis was eagerly awaited. Reviewers greeted it with great enthusiasm as a very important contribution to economic theory (for example, Boulding 1948; Tintner 1948; Reder 1948; Allen 1949; Stolper 1949). This may be because most of them were mostly young, as few of the older generation would have been able to follow the book.[39] The mathematical appendices were welcomed as providing much-needed teaching materials, though the book stimulated discussion of the role of mathematics in economics, still a controversial issue (e.g. Boulding 1948, Savage 1948). The teaching of economic theory was still dominated by Alfred Marshall’s *Principles of Economics* (1920)

[39] Reviewers included (ages in brackets) Roy Allen (41), Gerhard Tintner (40), Kenneth Boulding (37), George Stigler (36), Wolfgang Stolper (35), Lloyd Metzler (34), Kenneth May (32), Leonard Savage (30), Charles Carter (28), Melvin Reder (28), William Baumol (25). Samuelson’s fellow students in this list were Stigler (at Chicago), Metzler and Stolper (at Harvard).

which reflected the view that mathematical reasoning should be kept strictly subordinate to verbal explanations. In his Introduction, Samuelson (1947, p. 6) argued that Marshall's position should be "exactly reversed" on the grounds that "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 particularly depraved type". Where Marshall had relegated mathematics to footnotes and appendices, *Foundations* sought to demonstrate what could be achieved through using mathematics in economics. Not surprisingly, some reviewers took up the comparison with Marshall. For example, Reder (1948, p. 516) characterized Marshall as having sought to clarify concepts, using mathematics when it served that purpose, whereas Samuelson "begins with systems of equations and attempts to deduce their empirical or operational implications", concluding that if one follows Samuelson, mathematics is far more likely to be useful. In a similar vein, Allen (1949, p. 111) compared Marshall's use of mathematics to the use of steel to provide scaffolding, whereas Samuelson used it as part of the structure.

Foundations was also compared with Hicks's *Value and Capital* (1939), Reder and Allen providing extensive comparisons. Samuelson (1998, p. 1382) later described Hicks's book as "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". However, few reviewers saw the relationship in this way. According to Roy Allen (1949, p. 112), who had collaborated with Hicks on consumer theory in the early 1930s, Hicks had tried to work out "if not a complete economic theory, at least a full development of one particular line of approach"; in contrast Samuelson tried to do no more than show the common mathematical basis underlying different fields of economics. More critically, William Baumol (1949, p. 159) noted that the book lacked a theoretical unity, some chapters amounting to "collections of his miscellaneous thoughts and brilliant analytical sorties lumped together on the basis of some tenuously established common characteristic". Tintner (1948) considered Samuelson's treatment of uncertainty inferior to Hicks's.

There was disagreement over which was the most original part of the book. For Lloyd Metzler (1948, p. 906), who had used similar methods, the part dealing with dynamics was most important. Samuelson had gone beyond Hicks in constructing formal dynamic models. Yet many reviewers were disappointed by this part of the book (e.g. Savage 1948; Tintner 1948; Stigler 1948; Allen 1949; Baumol 1949). Three major problems were identified. Discussions of dynamics rested on very special assumptions meaning that there could be no confidence in the results; they focused on the mathematics, with very little economic content; and no attention was paid to expectations. Gerhard Tintner (1948, p. 499), at the Cowles Commission, concluded because Samuelson's results were very limited, his methods needed to be combined with econometric methods so as to quantify the theory. Samuelson, though he had undertaken empirical work on consumption and published papers on statistical theory, and though he worked with students (e.g. Lawrence Klein) and co-authors (e.g. Robert Solow) who did econometric work, never went down that route himself.[40]

Several reviewers recognized that Samuelson did not see economics as synonymous with constrained maximization using calculus-based techniques. Metzler (1948, pp. 905, 906) emphasized Samuelson's departures from traditional theory, pointing out that he believed that the most important problems, including those dealing with the economy as a whole, could not be reduced to maximization problems and that he was cynical about consumer theory.

[40] This is a substantial point that is beyond the scope of this paper and will be discussed in detail elsewhere.

Kenneth May (1948, p. 94), a mathematician, was left feeling that the book contained "an implicit basic critique of economic theory" in that "much economic theory turns out to be banal or meaningless when stripped of its vague literary formulation". Allen (1949, p. 113) noted that Samuelson stressed finite changes and discontinuity and Leonard Savage (1948, p. 202), a statistician, noted that the economic theorist needed little math beyond "a few ideas about continuity and convexity".

Though Samuelson clearly attached importance to the Le Chatelier principle, this was not something that impressed his reviewers, whether economists or mathematicians. The only reviewer to mention it (Baumol 1949, p. 160) did so in a footnote pointing out that Samuelson had got a minus sign wrong. The reason was presumably that, though the Le Chatelier principle had been an important step for Samuelson, it was not a necessary step: it was sufficient to begin with results on optimality conditions and the correspondence principle. Though Samuelson had stated (1947, pp. 38, 81) that the Le Chatelier principle played a role in natural science, his reference to the "metaphysical vagueness" with which it was stated will not have encouraged economists to follow this up.

The range of responses to *Foundations* showed that, unlike *Value and Capital*, *Foundations* could be interpreted in many ways. Though Samuelson remained skeptical about much of the "neoclassical" orthodoxy that emerged in the 1960s and 1970s—and his friend Metzler (1948, p. 905) had gone so far as to claim that Samuelson believed that "most of the important economic problems" could not "be reduced to simple problems of maximization"—*Foundations* provided a toolbox to which those who developed that orthodoxy could turn (exemplified by the case of Lucas, cited earlier). The techniques Lucas and others learned from *Foundations* increasingly dominated economic theorizing. For example, Don Patinkin's reformulation of monetary economics, *Money, Interest and Prices*

(1956), central to discussions of macroeconomics in the 1960s and 1970s (see Backhouse, & Boianovsky 2013) made use of Samuelson's theory of revealed preference, the correspondence principle, and his discussion of dynamics.

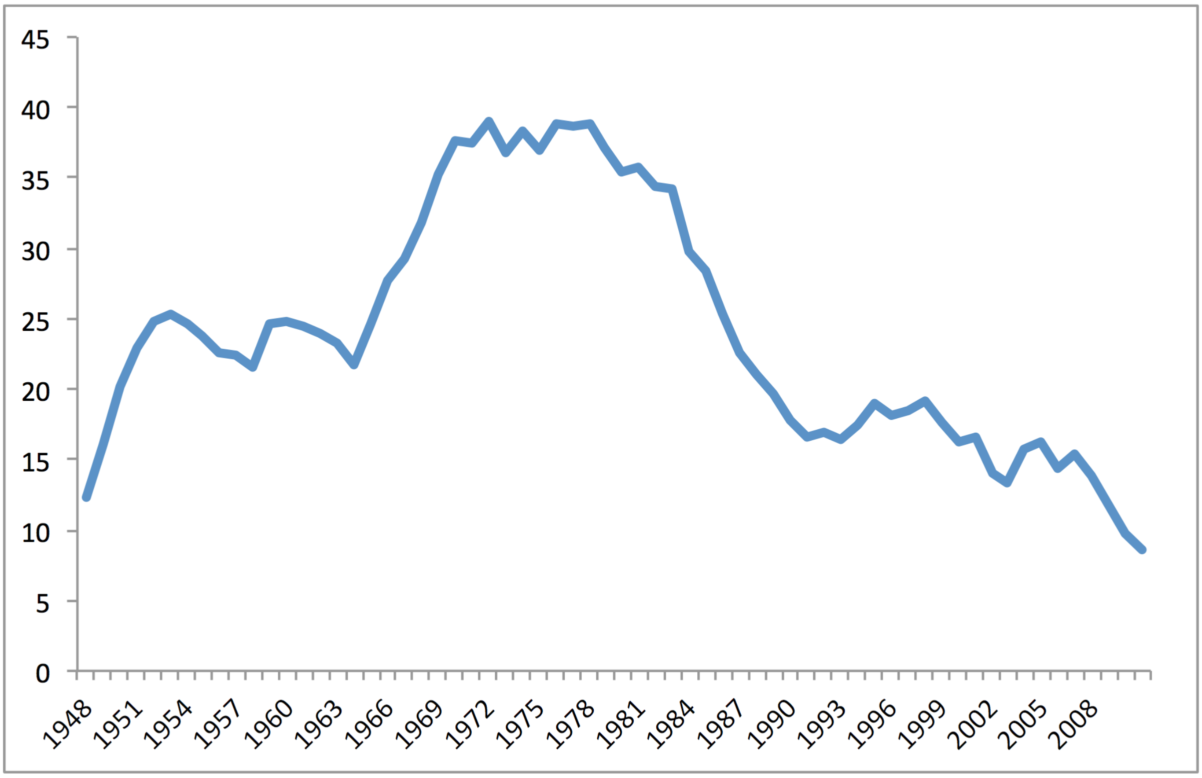
Samuelson had seen the importance of methods that went beyond differential and integral calculus, and from the late 1940s onwards played a major role in the development of linear modeling techniques, clarifying the relation of Leontief and activity-analysis models to traditional theory (1949), and working with Robert Dorfman and Robert Solow to produce a major textbook on such methods, *Linear Programming and Economic Analysis* (1958). *Foundations* came to be associated with what became the standard approach to theoretical modeling relying heavily on the use of Lagrange multipliers and differential calculus, for Samuelson did not become involved in literature on the existence of general competitive equilibrium, and he remained a skeptic about game theory.[41] As such methods came to be more widespread, and as more elegant techniques involving cost and expenditure functions came to be more widely used, the influence of *Foundations* declined.[42] In part, of course, the decline was because methods that were once innovative, came to be routine and unremarkable. This explains the pattern of citations shown in Figure 1: citations reached a peak in the 1970s, the decade by which the use of mathematical methods became firmly established (see Backhouse 1998) but after that *Foundations* was increasingly displaced by books covering newer and different techniques.[43]

[41] This is another story that will be told elsewhere.

[42] Later, when trying to generalize the theorem to apply to Leontief and other models (Samuelson 1960), he admitted that he had failed to derive a result that applied more than locally. However, it seems unlikely that recognition of this played a role in the decline in citations of *Foundations*.

[43] Sales figures for the period for the book's early years tell an interesting story. In 1948-9 it sold over 3000 copies. After the initial demand was met, average sales from 1950-4 were under 300 per year, which rose steadily to 873 by 1962. The background to this steady rise in the late 1950s is no doubt both the increased use of mathematics and the increased

Figure 1: Citations of *Foundations* in JSTOR articles, 1948-2010



Source: http://dfr.jstor.org. Search for "Samuelson" and "Foundations of Economic Analysis", restricted to academic articles.

6. Conclusions

Samuelson's *Foundations of Economic Analysis* did much to define the way economic theorizing was undertaken after the Second World War. It was important both because of its contents—it provided an up-to-date toolbox for doing economics— and because Samuelson,

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number of economists. To put this in perspective, AEA membership rose from 5,329 in 1947 to 11,285 in 1962, with similar expansions in other countries.

despite being only thirty two when the book was published, was already a well known figure. He was widely considered the star of the generation of economists that had come of age during the Second World War, acknowledged by the award of the AEA's first Clark Medal. He was already making his mark at MIT, where his presence was a major factor in the transformation of a service department focused on teaching engineers into one of the leading American economics departments. His articles on fields as diverse as consumer theory, international trade, business cycle theory and the Keynesian multiplier, demonstrated the way mathematics could be used to resolve confusions in literary accounts of economic theory. This gave *Foundations*, the long-awaited revision of his Ph.D. Dissertation, a significance it would not otherwise have had. As economics became progressively more mathematical, with graduate students increasingly expected to construct formal models of maximizing consumers and firms, *Foundations* was widely seen as the canonical exposition of such methods. In addition, it was an important resource for anyone tackling dynamics or welfare economics.

This paper fills out Samuelson's account of the book's origins as well as correcting some details. It makes clear the extent of his debt to E. B. Wilson, who not only drew his attention to thermodynamics but also impressed upon him the importance of looking carefully at all the assumptions being made, the importance of analyzing finite changes and models in which functions were not necessarily continuous and differentiable, and the relationship between comparative statics and stability. It also establishes that turning the thesis into a book was a long process, extending over two to three years, during which it almost doubled in length. The most notable additions included much of the material on dynamics and a completely new chapter on welfare economics, as well as the term "the correspondence principle".

Central to Samuelson's book was the idea that there were common mathematical structures underlying different problems, both within economics and across disciplines. Operational theorems could be obtained by analyzing the properties the appropriate equilibrium systems. For problems involving consumers and the firm, this involved maximization and hence the use of second order conditions. For problems involving aggregates where optimization was not involved, comparative statics results could be derived by assuming that the equilibrium was stable. The paper contends that, contrary to what some commentators have claimed, what mattered to Samuelson was not the analogies with thermodynamics *per se*, but the mathematical structures on which certain physical, chemical and biological theorems were based. Results that might appear to be physical in origin could actually be mathematical implications of a system being in equilibrium. This was an argument for the use of mathematics in economics, at a time when most economic theory was conducted using verbal reasoning.

Contrary to popular belief, as his close friend Metzler noted, *Foundations* reflected the view that there was much more to economics than optimizing behavior. Macroeconomics required different foundations, for aggregate behavior could not be explained as the outcome of optimizations: hence the need for the correspondence principle. Though Wilson left a stronger mark on *Foundations* than did his other Harvard teachers, it is tempting to conjecture that Samuelson's belief that there were strict limits to what could be achieved through the analysis of optimizing behavior reflected the influence of Alvin Hansen, who was emphatically not a mathematical economist. For much of the period when Samuelson was revising *Foundations*, he was working with Hansen on macroeconomic policy problems, notably estimating consumers' expenditure and trying to forecast postwar national income, for which the mathematical apparatus constructed in *Foundations* was of little use. Though the main influence of such work was on his introductory textbook (Samuelson 1948), which expresses a very different view of economics—more institutional and data-driven—it is hard not to speculate that the view on which Metzler picked up reflected this work. His wartime teaching at MIT and interaction with policy makers had no doubt given Samuelson the opportunity to develop his expository skills. This was evident in the material he added, notably on index numbers and welfare economics, though in general he failed to respond to Wilson's advice to expand greatly the explanations of what he was doing with the mathematics, and most of the material taken from the thesis was unchanged. The book might be flawed, but neither the minor mistakes nor the failure to provide an integration of economic theory prevented it from being a resource on which many economists drew heavily during a period that was to witness a major transformation of the subject along lines that Samuelson helped lay down.

Appendix: Foundations in relation to Samuelson's PhD Thesis

Chapters in *Foundations*	**Pages**	**Material added to/removed from thesis**
I. Introduction	5-6	Discussion of correspondence principle
II. Equilibrium systems and comparative statics		
III. The theory of Maximizing behavior	23-9	New section: A calculus of qualitative relations
IV. A Comprehensive restatement of the theory of cost and production	78-80	New section: Indeterminacy in purest competition?
V. The Pure theory of consumer's behavior	117-24	New section: A note on the demand for money
VI. Transformations, composite commodities and rationing	125-71	New chapter

VII. Some special aspects of the theory of consumer's behavior	179	Paragraph on independence added
	184-9	Discussion of *Value and Capital* and complementarity added
	189-202	Three sections, on Constancy of marginal utility of income and consumer's surplus are a completely rewritten version of a thesis chapter, reflecting the content of Samuelson 1942a
VIII. Welfare economics	203-53	New chapter
IX. The stability of equilibrium: comparative statics and dynamics	258	Sentence on correspondence principle added since Samuelson 1941
	266	Figure illustrating offer curves added in Samuelson 1941
	272	Two sentences on imperfect stability omitted since Samuelson 1941
	274-5	Discussion of Lange 1942 and Lange 1944 added since Samuelson 1941
X. The stability of equilibrium: linear and non-linear systems	284-310	New chapter (Samuelson 1942c)
XI. Some fundamentals of dynamic theory	311-7	New Section: Statics and Dynamics. All but one page is included in Samuelson 1943a.
	327	Final paragraph on stability of equilibrium changed
	330-2	Discussion of "what processes shall be described as equilibrium processes?" added
	334	Phrase "physical systems" corrected to "conservative physical systems"
	335-49	Six sections on business cycles added
XII. Conclusions	350-5	New chapter
Appendix A: Maxima and quadratic forms	379	Two clarificatory sentences added
Appendix B: The theory of difference and other functional equations	380-439	New chapter

Note that pages are those on which significant changes have been identified. Only those articles published after the thesis are mentioned.

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