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978-1-107-02993-4 - Paul Samuelson on the History of Economic Analysis: Selected Essays

Edited by Steven G. Medema and Anthony M. C. Waterman

Excerpt

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Introduction

Steven G. Medema and Anthony M. C. Waterman

The editors of this volume approached Paul Samuelson during the last year of his life with the suggestion that we might celebrate his unique and influential contribution to the history of economic thought by collecting his many papers in this field and publishing a selection. It seemed to us that the title of the series for which we proposed our book, “Historical Perspectives on Modern Economics,” almost exactly described his heuristic intentions in at least the most formal and best-known of his historical studies.¹ We assured him that the project would not go ahead without his approval. To our delight, Samuelson warmly encouraged our initiative, offered his advice, began to comment on our selection, and interested himself in the thorny question of permissions. But his health began to deteriorate soon after his last communication with us (4 August 2009), and he was unable to write again before his death on 13 December 2009.

Samuelson’s reputation as one of the greatest economists of the twentieth century rests on his classic *Foundations of Economic Analysis* (1947), his pioneering work with Robert Dorfman and Robert Solow (1958) on linear programming as a tool of economic analysis, and on hundreds of articles in almost every branch of economic theory, many of which are regarded as seminal. Why then did he bother with the history of economic thought? How did it relate to the rest of his intellectual enterprise? How did he go about doing the history of economic thought? What did he tell us about what he was doing? What did others think of his historiographic method? And what of permanent value can we identify in his historical writing? We

¹ Some would undoubtedly argue that Samuelson’s research in this vein was more of a “modern perspective on historical economics.” Our position is that this volume shows that the reality is much more complex than that.

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address these questions in what follows, and we conclude with an explanation of what we have selected and why.

1. The Historian

Paul Samuelson once referred, self-disparagingly, to “the 5 per cent of my published papers that deal with the history of economic science” (54, 3).² But D.P. O’Brien (2007, 336) regards this as a “significant underestimate.” Nearly 140 articles, essays, or memoirs listed at the end of this volume, appearing over a period of forty-four years from 1946 to 2009 and comprising perhaps 20 percent of his scholarly publications, are clearly identifiable as studies of the history of economic thought. Many full-time specialists in this subdiscipline have achieved far less. We have selected seventeen of what we believe to be the most important of Samuelson’s contributions to history of economic thought for inclusion in this volume.

Samuelson’s earliest journal articles in history of economic thought were published in the 1950s. But *Foundations* (1947), based on his doctoral dissertation, refers to nearly forty of his more famous forerunners over the previous two centuries, ranging from Barone, Bastiat, Bentham, Böhm-Bawerk, and Bortkiewicz to Adam Smith, Thünen, Veblen, Viner, Walras, Wicksell, and Allyn Young, and including such relatively unexpected authors as Engels, Paley, and Sidgwick.³ It is characteristic that he should have chosen to illustrate a purely mathematical conception, that of “one-sided stability-instability,” with Malthus’s population theory (1947, 296–299), so formulating a Malthusian production function, the germ of his famous “Canonical Classical Model” (29). Among major figures, only Marx is ignored in *Foundations*, for which Samuelson amply made up in later years.

Even in the 1940s, such attention to his predecessors by the author of a work of pure theory was unusual. When Samuelson began his graduate studies in the 1930s, “history of thought was a dying industry” (48, 51).

But it was still a presence in the required curriculum to be reckoned with. Jacob Viner was cracking the whip at the University of Chicago. Edwin Seligmann[sic] at Columbia and Jacob Hollander at Johns Hopkins occupied their professorial chairs. (ibid.)

² Unless otherwise noted, all references are to the numbered list of Samuelson’s publications in the history of economics, which appears at the end of this volume.

³ O’Brien (2007) provides an excellent discussion of the use of the history of economics in Samuelson’s *Foundations*.

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O'Brien (2007, 339) believes that "the importance of the influence of Jacob Viner on Samuelson's intellectual development cannot be overrated." Moreover, at Harvard in the 1930s, graduate students facing general oral exams were often expected to elucidate developments in analysis made by important figures back to Adam Smith and even before (48, 51). And at many other universities around the world, possibly because of "the decadence of literary economics from 1919 to 1930" (ibid.), the study of theory was intertwined with that of the great theorists to a far greater extent than is now the case. Thus the sixteen-year-old Paul Samuelson was "born again"

at 8:00 a.m., January 2, 1932, when I first walked into the University of Chicago lecture hall. That day's lecture was on Malthus's theory that human populations would reproduce like rabbits until their density per acre of land reduced their wage to a bare subsistence level where an increased death rate came to equal the birth rate. So easy was it to understand all this simple differential equation stuff that I suspected (wrongly) that I was missing out on some mysterious complexity. (Samuelson 2003, 1)

To this "accidental, blind chance" did Samuelson attribute his decision to study economics.

Samuelson himself believed that the science of economics "burst to life" shortly after this date in the eruption of four revolutions: "the *monopolistic competition* revolution, the *Keynesian macro* revolution, the *mathematicization* revolution, and the *econometric inference* revolution" (48, 52). The attention of the best economists therefore became focused on the exciting present. The study of past doctrinal controversy was left to those whom Donald Winch (1996, 421) later referred to as "incompetent or retired" practitioners. How then to explain that fascination with economic analysis of the past, and commitment to its elucidation, that Samuelson evinced to the end of his life?

As he was mastering the existing corpus of economic theory during the 1930s, he came to understand that "some unity of method and logic" underlay most of his researches. Two hypotheses – maximization by rational individuals and stability of market equilibrium (Samuelson 1947, 5) – were sufficient to unify "much of current *and historical* economic theory" (64, 1377; emphasis added). The analytical framework of *Foundations* thus had the effect of foreshortening the temporal distance between Samuelson's "heroes in economics" – Walras, Cournot, Edgeworth, Pareto, Fisher and Wicksell – and his fellow "working economists" such as J.R. Hicks and Ragnar Frisch (64, 1381). Perhaps for this reason, Samuelson sometimes wrote as though all the neo-classical masters were in fact contemporaries with whom he was engaged in dialogue. As for earlier generations from David Hume and Adam Smith to

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J.S. Mill and Karl Marx, he habitually maintained that “within every classical economist there is to be discerned a modern economist trying to be born” (e.g., 29, 1415). His discussions of Heinrich von Thünen (43, 47, 75) recognize “a prophet way ahead of his own times” who “anticipated the kind of mathematics later employed” by Jevons, Walras, Edgeworth, and Pareto (75, 1).

Samuelson’s understanding of the conceptually unitary foundations of economic analysis, and his confidence in the power of mathematics to lay bare those foundations, allowed him to analyze a wide range of seemingly disparate problems with a common technical apparatus, and thus to think of himself as “the last generalist in economics” (e.g., Samuelson 1985, 52). His 1979 essay on “Land and the Rate of Interest” (45) is typical. In applying Modigliani and Ando’s (1963) life-cycle model to appraise an off-the-cuff insight of Keynes (1936, 242), it invokes (and formalizes) Turgot’s (1766) analysis and alludes to Henry George, Frank Ramsay, Böhm-Bawerk, Wicksell, Fisher, and Cannan. One of his last papers, “Where Ricardo and Mill Rebut and Confirm Arguments of Mainstream Economists Supporting Globalization” (73), affords striking testimony to Samuelson’s perception of the essential contemporaneity of all good economics. His penchant for hyphenating chronologically distant authors, as in “Minkowski-Ricardo-Leontief-Metzler matrices” (8, 1) or “Smith-Allyn Young-Ohlin-Krugman trade paradigms” (73, 143), though sometimes half-humorous, was always a true index of his unique vision.

Lastly, and closely related to his view of the conceptual unity of economic analysis, Samuelson’s long-standing devotion to history of economic thought may have been in part simply a consequence of his insatiable appetite for hard work, a characteristic noted by O’Brien (2007, 336–338). For seventy years he produced scholarly articles at an average rate of nearly one a month, not to mention several books and hundreds of popular pieces in newspapers and magazines. When he was awarded the Nobel Prize in 1970, he remarked that “it was nice to be recognized for hard work.” At the Nobel banquet Samuelson listed five necessary conditions for academic success in his discipline: the fourth condition, “an important one from a scholarly point of view,” was that “you must read the works of the great masters.”⁴ Few other economists have “read the works of the great masters” from Adam Smith to Kenneth Arrow with the diligence and penetration of Paul Samuelson. Fewer still have written of this literature so widely, or with such insight into its analytical core.

⁴ http://nobelprize.org/nobel_prizes/economics/laureates/1970/samuelson-speech.html.

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2. Historiography

Nevertheless, Samuelson occupies a controversial place among historians of economics. Because of his vision of the conceptual unity of all economic analysis, his historiographic method when reaching deep into the past was to formalize the analysis of his predecessors (and he saw them as such) using modern mathematical tools and theoretical constructs. Contextual elements such as historical background, influences, and ideology – important to most other historians – were ruthlessly ignored. When we move closer to the present, however, and witness Samuelson analyzing the work of contemporaries and near-contemporaries through what he describes as a historical lens,⁵ we see a different approach, one that brings in the role of personalities, contexts, and scholarly communities in the creation of path-breaking ideas – that is, invoking elements of history that, as we shall see in this volume, he tended to dismiss in certain of his commentaries on writing the history of the economics of the distant past.

Samuelson sometimes identified his work as “Whig history,” albeit with qualifications. Others have described it, privately, as an illustration of its worst excesses. This strikes us as excessively harsh. Samuelson himself hoped that “When meeting St Peter my worst crime will be the espousal of a Whig-History approach to the history of science” (54, 3). To understand Samuelson’s contributions to the history of economics, and to apprehend their nature and import, we must appreciate the perspective that motivated his work – though, as we shall see later, that perspective does not wholly account for the historiographical importance of those contributions.

The starting point is Samuelson’s orthodox view of economics as a *science*, and thus of the history of economics as the *history of science*. But his conception of history of science was very different from the contemporary approach that focuses on the production of scientific knowledge and its background conditions, sometimes to the exclusion of scientific knowledge itself. Samuelson labeled that approach, rather unkindly, as “antiquarian.” His own view was roughly 180 degrees opposed:

When I read a Smith or a Keynes, it is the system that they are formulating that first interests me – the system discernible there and not primarily *their* understanding or misunderstanding of it. . . . The historian of science is interested primarily in the history of various scientific models and understanding. (54, 7)

⁵ Many of these essays appear in his *Collected Scientific Papers* under headings such as “Essays in the History of Economics.”

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Samuelson found “antiquarian” approaches to the subject problematic. “History as it happened,” he wrote, “is neither attainable in principle nor, where the history of a *cumulative* science is concerned, is it a desirable end” (54, 5). Sometimes he associated the antiquarian approach with “gossip” (e.g., 8, 2). Elsewhere, however, he took a more charitable view: “I applaud those who study history of scholars – their writings, ideologies, influence, and changing reputations.” But his own special interest was “in the history of economic theories, models, paradigms, measurements, hypotheses, etc.” (55, 149), which is “in many ways . . . easier to write precisely because it need not involve the determination of social influencings” (8, 5).

But how can one claim to be faithful to the ideas of the past when merely translating them into modern mathematics, without attending to what may be crucial contextual elements? Mark Blaug (1990, 32) has charged Samuelson with passing off “rational reconstructions” as “historical reconstructions,” against which Samuelson often defended himself. He was well aware that we cannot know with certainty what an author was thinking when he formulated an idea. However, he observed that writers of the past often “imperfectly understood their own theories” (48, 56), not least because of limitations of the literary form in specifying relations and implications. As he wrote to Patinkin in 1990, “I agree that we humans are often imperfect logical machines – particularly in the early stages of discovery and exploration” (55, 150). But for Samuelson this simply required care to avoid overreaching in one’s analysis, not evading the analysis of what might be implicit, conceivably even present, in a past author’s mind:

Truly, I would not want to write, “If A implies B and Cohen asserts that A obtains, then he asserts (and understands) that B does obtain.” . . . But also, in pursuing my study of the history of (A, B , logical relationships, empirical relevances), I’d reproach myself for failing to recognize when the A ’s do imply the B ’s.⁶ (55, 150)

This may not yield true history, but it gives us “the best-case understanding” of that model (54, 8).⁷ One can then investigate consistency and logical correctness in the model, and how it relates to other ideas past and present.

⁶ Patinkin, for his part, responds that he does not quarrel with Samuelson’s demonstration that ideas are implicit in the work of particular scholars; his quarrel goes to “the cerebral distance between the implicit and the explicit, the width of the synaptic gap between the two” (55, 152–153).

⁷ Samuelson goes on to advocate that the historian of science “should also work out [the author’s] worst-case understandings” (54, 8).

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Samuelson seemed to recognize that there is a fine line between exploring the ideas of the past and abusing them, and was highly critical of “attempts to read into” the ideas of the past “formalisms that are *not* there” (67, 333; see also 54, 10; 48, 56). His own goal was to determine by rigorous probing what *could have been* there. Of course, such probing can expose problematic elements in past ideas; and though he considered it “idle to castigate 18th-century writers for not being 20th-century virtuosi,” Samuelson also considered it “mandatory to point out their errors in describing their own systems and scenarios.” This is particularly important given that “top modern commentators (Schumpeter, Sraffa, Stigler, Blaug, S. Hollander, Kaldor, Wicksell, . . .) sometimes share their errors of omission and commission” (67, 333). If we are “standing on the shoulders of giants,” we must be sure that the giant’s skeletal structure is not deformed. The work of Ricardo and the treatment and use of it by Marx and by subsequent commentators is a case in point for Samuelson.

While there can be no doubt that Samuelson’s approach to analyzing the economics of the eighteenth and nineteenth centuries has a certain Whiggish character, his is not a classic form of Whig history. Traditional formulation of the concept, owing to Herbert Butterfield (1931), paints the present as “the latest and best and *final* thing” (54, 6). Samuelson did not reject the Whig label – and at times even gloried in it – but he saw himself as putting forward a different and better version of Whig history: “That part of the past which is relevant to the present – that is, relevant to one *or more* of today’s competing paradigms – is to be an object of special historical interest.” He further suggested that instead of labeling this “Whig history,” we might better speak of “history that is given special importance and attention because of its relevance to the present” and that “A good, if ugly, title might be ‘Presentistic history’” (54, 6).

Although cognizant of the idiosyncratic nature of his approach, Samuelson was evangelistic about it, arguing in his 1987 keynote address to the History of Economics Society that the adoption of his method by other historians of economics offered a possibility of rescuing the field from the professional wilderness.⁸ In doing so, Samuelson located the study of the history of economics, and the audience for such studies, in the economics profession rather than among historians. In effect, the market tests the interest and sensibilities of *economists* rather than historians, and with significant import. For Samuelson considered science, including economics,

⁸ A contrasting view is found in Schabas (1992). See also the responses to Schabas’s argument in the same volume.

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as progressive, and saw a characteristic difference between science and the humanities in explaining the fact that the ideas of period A are eventually replaced by those of period B:

A later [scientific] paradigm is likely to be a better paradigm and be more lasting. Einstein's 1915 system does not so much reject Newton's 1687 [sic] system as give it a *dominating* generalization. In the creative arts, Shakespeare is not better than Homer because he comes later. To think that economics is merely like poetry is both to downgrade it as a serious attempt to be a science and is to grossly misdescribe economics. (54, 6)

This has implications for the professional acceptance of work done by historians of economics, since "working scientists have some contempt for those historians and philosophers of science who regard efforts in the past that failed as being on par with those that succeeded, success being measurable by latest-day scientific juries who want to utilize hindsight and ex post knowledge" (54, 52–53).

Samuelson came to see that his approach has not served to raise the profile of the history of economic science in the larger economics profession:

When Samuelson (48) proclaimed a manifesto for Whig history of economic science, the argument was made that old-fashioned antiquarians had lost their market and maybe something different would sell better. Kurdas reminds me that empirical experience showed that the market for history of economics remains small despite the shift toward using present-day tools in that area. (54, 12; see Kurdas 1988)

Yet he remained unrepentant, and was happy to abandon the market test and instead make the case for his approach on its intellectual merits: that "economics is in some degree a cumulative science," and that "If the study of the past is worth doing, it is worth doing as well as we can." (54, 12) And that, for Samuelson, meant forsaking antiquarian approaches in favor of what he considered to be a more scientific approach to history.

But as we move closer to the present, a rather different Samuelson emerges – one that further belies his self-proclaimed Whiggishness and which relies increasingly on historical elements that he labeled antiquarianism and gossip when referring to the more distant past. Though the image of Samuelson as a historian of economics has been formed almost exclusively through his work on figures of the eighteenth and nineteenth centuries, he wrote an amazing amount on the history of twentieth-century economics. Here, the subjects were his contemporaries and near-contemporaries, and his analyses ranged from close-in-time retrospectives on individuals and ideas to the development of particular "schools" of economic analysis,

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and he did so for outlets ranging from scholarly journals to *festschriften* to funeral orations.

Samuelson's output on this front, too, is staggering: more than eighty articles spanning a period of roughly fifty years.⁹ The topics covered a range from early assessments of the monopolistic competition and Keynesian revolutions to reflections on fellow Nobel Laureates, teachers, and colleagues. The perspective evinced throughout these works is historical – not judging by the standards of the present, but focusing on the place of ideas at their inception and their relationship to those of the past – attempting to bring out the role of personalities, perspectives, relationships, and contexts in the generation of ideas and in the development of economists and communities of economists.

The obvious question that this raises is how one reconciles this work with Samuelson's own comments to the effect that concern with such matters amounts to little more than antiquarianism and gossip. One possible answer is that he did not see himself doing "history" when he penned such works. But that claim is refuted by multiple elements of the record, including the fact that he or his editors (but presumably with his approval) specifically classified these essays as in the "history of economics" in his *Collected Scientific Papers* and the publication of many of these works in the same outlets as his more mathematically oriented articles. It is difficult to imagine that Samuelson would be so inconsistent as to offer prestigious academic journals and *festschriften* puff pieces in certain situations and serious scholarly work in others. A more reasonable conjecture, we believe, is that Samuelson's attitude toward contextual elements and the like was very different when he was considering the work of contemporaries and near-contemporaries than it was when he was working with the ideas of the distant past. When it comes to the history of twentieth-century economics, Samuelson was writing as a participant-observer, an observer, or as one who was at least in some way (e.g., as a student) connected to nearly all of his subjects. In this respect Samuelson's work as an author resembled that of Winston Churchill in the latter's history of *The Second World War* (1948–1953). The historical "facts" that he laid out, then, were the facts as he knew them, and in many cases had observed them; the impressions formed by professional and nonprofessional interactions at the office, at professional conferences, or based on things said to him by individuals on

⁹ There are, in addition, roughly three-dozen speeches and items written for the popular press on these topics.

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whom he felt he could rely. It may be that Samuelson believed that he was on much firmer ground in talking about the role of contextual elements in the genesis of modern economic ideas and was unwilling to make such leaps when it came to discussing the ideas of earlier centuries and with generations of scholars with whom he had no first- or even second-hand acquaintance.

Of course, this is only a conjecture, one that may be refuted, validated, or supplemented by the work of others who attempt to assess Samuelson's contributions to the history of economics in the coming decades. But whatever the explanation, the fact remains that these essays on modern economists and modern economics are far more than puff pieces written by a long-retired professor celebrating the great advances of economics during his salad days. They are both historical analyses in and of themselves and the source of a treasure trove of data for other historians of modern economics. They also reveal that there is far more to Samuelson as a historian of economics than the translation of the ideas of the past into modern mathematics. To get the proper scope of the man, both as economist and as historian of economics, requires coming to grips with the totality of this work. This introductory essay and the articles reprinted in the present collection represent only a partial view, one limited by multiple factors, including the highly selective nature of this volume and the fact that only time will allow us to digest the magnitude and import of Samuelson's contributions.

3. Mathematics and the History of Economic Analysis

For intellectual historians who attempt to get inside the minds of our predecessors in order to understand what they were trying to do in their day, "Whig History" is a term of abuse. Spinoza's dictum, *Non ridere, non lugere, nequedetestari; sedintelligere* (Not to laugh, not to lament, not to deprecate, but to understand), is their motto (Cassirer 1951, x). Context is all-important. It is therefore regrettable that Samuelson's provocative flaunting of his red rag should have induced a violent allergic reaction in many other historians of economic thought, blinding them to the important fact that Samuelson's method – whatever he himself may have said or thought about it – though certainly not able to produce genuine *history*, is a valid and often valuable *tool* of intellectual history. For it is undoubtedly the case that one of the reasons, perhaps the chief reason, why we are interested in Adam Smith and other "economic" thinkers of the last three