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History of Economic Ideas is an international peer-reviewed journal and it is indexed and abstracted in Current Contents/Arts & Humanities, Art & Humanities Citation Index, Social Science Citation Index and Journal Citation Report/Social Science Edition (ISI · Thomson Reuters). The eContent is archived with Clockss and Portico.
PAUL A. SAMUELSON (1915-2009): THEORIST, HISTORIAN AND INTELLECTUAL

Edited by
Riccardo Faucci
Soon after hearing of Paul Anthony Samuelson’s death, this journal decided to dedicate a number of papers to his towering figure as an eminent theorist, a historian of economic analysis, and a political thinker, spanning through seven decades. To this aim a call for papers was launched on the shoe List of the History of Economics Society.

The aim of the present sub-section is therefore that of offering a first, necessarily sketchy, but, hopefully, non-trivial appraisal of Samuelson’s intellectual and scientific impact on contemporary social sciences. Also on behalf of HEI’s co-editor, Roberto Marchionatti, we wish to thank the managers of the shoe List and the scholars who promptly answered our call for papers and contributed the following works.

R. F.
Paul Anthony Samuelson made wide-ranging contributions to scholarship in the history of economic thought over the course of his long professional career, doing so using a distinctive approach to the subject that was both a source of significant insights into the ideas of the past and the subject of controversy. This article discusses the motivations behind Samuelson’s work in the history of economic thought, both in terms of subject-matter and approach, with a view toward enhancing our understanding of his place in this field.

1. The Historian

Paul Anthony Samuelson once referred, self-disparagingly, to «the 5 per cent of my published papers that deal with the history of economic science» (54, 3). Yet this amounts to more than seventy articles, essays or memoirs clearly identifiable as history of economic thought (HET), appearing over a period of fifty-seven years from 1952 to 2009. Many full-time specialists in this sub-discipline have achieved far less.

Samuelson’s earliest journal articles in HET were published in the 1950s. But Foundations of Economic Analysis (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, Bortkiewicz… to Adam Smith, Thünen, Veblen, Viner, Walras, Wicksell and Allyn Young; and including such relatively unexpected authors as Engels, Paley and Sidgwick. 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 (Samuelson 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 graduate study at Chicago in 1935, «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. (*Ibidem*)

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 before (*ibidem*). And at many other universities around the world, possibly because of «the decadence of literary economics from 1919 to 1930» (*ibidem*), 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 focussed on the exciting present. The study of past doctrinal controversy was left to those whom Donald Winch later referred to as «incompetent or retired» practitioners (*Winch 1996, 421*). 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; our italics). 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 neoclassical masters were in fact contemporaries with whom he was engaged in conversation. As for earlier generations from Hume and Smith to J. S. Mill, he habitually maintained that «within every classical economist there is to be discerned a modern economist trying to be born» (e.g., 29, 598). His discussions of Heinrich von Thünen (43, 47, 75) recognise «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 «one of the last generalists in economics» (e.g., CSP, 5, 356, 800). His 1979 essay on «Land and the Rate of Interest» is typical. In applying Modigliani’s 1963 life-cycle model to appraise an off-the-cuff insight of Keynes (1936, 242), it invokes (and formalises) Turgot’s (1766) analysis, and alludes to Henry George, Frank Ramsay, Böhm-Bawerk, Wicksell, Fisher and Cannan (CSP, 5, 344). 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, 1499) 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 het may have been in part simply a consequence of his insatiable appetite for hard work, a characteristic noted by O’Brien (2007, 336-338) in an article which is complementary to our own. 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, is that you must read the works of the great masters». 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 so well.

2. Historiography

Nevertheless, Samuelson occupies a unique and controversial place among historians of economics. Because of his vision of the conceptual unity of all economic analysis, his historiographic method was simply to formalize the analysis of the past using modern mathematical tools and theoretical constructs. Contextual elements such as historical background, influences, and ideology – important to most other historians – were ruthlessly ignored.

Samuelson sometimes identified his work as «Whig history», though 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.

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 which focuses on the production of scientific knowledge and its background conditions, sometimes to the exclusion of scientific knowledge itself. Samuelson considered that approach «antiquarian», and 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 discernable 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)

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, 1500). 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, 1503).
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 has charged Samuelson with passing off «rational reconstructions» as «historical reconstructions», against which Samuelson often defended himself (Blaug 1990, 32). 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 over-reaching, not evading analysis of what might be implicit, perhaps even present, in the 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.1

This may not yield true history, but it gives us «the best-case understanding» of that model (54, 8).2 One can then investigate consistency and logical correctness in the model, and how it relates to other ideas past and present.

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 virtuos» (67, 333), 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

1 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).

2 Samuelson goes on to advocate that the historian of science «should also work out [the author’s] worst-case understandings» (54, 8).
skeletal structure is not deformed. The work of Ricardo and the treatment and use of it by subsequent commentators is a case in point for Samuelson – a subject to which we return in the next section.

While there can be no doubt that Samuelson’s approach has a certain Whiggish character, his is not a classic form of Whig history. Traditional formulation of the concept, owing to Thomas Macaulay, paints the present as «the latest and best and final thing» (54, 6). Though Samuelson did not reject the Whig label – and at times even embraced it – 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 a 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).

Though 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.1 In doing so, Samuelson located the study of the history of economics, and the audience for such studies, in the economics profession rather than within the historical community. In effect the market tests the interest and sensibilities of economists rather than historians, and with significant import. For Samuelson considered science, including economics, 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 1558 [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 hind-sight and ex post knowledge» (54, 52-53).

1 A contrasting view of found in Schabas 1992. See also the responses to Schabas’s argument in the same volume.
Samuelson was well aware 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.

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 more scientific approach to history.

3. History as polemic

Though economics deals with much that lies at the center of political controversy, it is orthodox to suppose that economists apply their analyses without regard to their ideological commitments. If a strongly ‘left-wing’ economist (e.g., Nancy Folbre) and a strongly ‘right-wing’ economist (e.g., Jennifer Roback Morse) analyze the same problem, they arrive at the same conclusions (see Waterman 2003). In this respect, economists sometimes contrast themselves complacently with other social scientists.

Paul Samuelson both deeply approved of, and was strongly committed to, this view of economic science. Yet his writing sometimes suggests doubt. For example, «It was the tragedy of my teachers’ generation that, as they grew older, economics grew more liberal, and even radical. It has been the comedy of my era that, as we grew older, our profession has become more conservative» (51, 108). But how can economics possibly be ‘liberal’ or ‘conservative’? We must assume that this was a slip: for ‘economics’ and ‘our profession’ we should read ‘economists’ and ‘the members of our profession’.

Now if economics be truly scientific, then at any one time all economists ought to be equally ‘conservative’ or ‘liberal’, since all have the same access to the latest and best economic theory, and relevant facts about the real world, with which to rationalize their normative preferences (Schumpeter 1954, 35-36). Yet this is manifestly not the case, as Samuelson, who never made any secret of his own ‘liberal’ political sentiments, was well aware.
...around 1970 when I accepted a lecture engagement at the Western Economic Association, I never encountered so unanimous a gang of free marketeers. It was a pleasure to see those fellows losing their chips at the rigged gaming tables.

(Ibidem)

Writing in 1983, he reported that

For a decade now mainstream economics has been moving a bit rightward. But I have not been tempted to chase it... I have a dream of a human economy that is at the same time efficient and respecting of personal (if not business) freedoms...

(CSP, 5, 355, 790-791)

The movement in «mainstream economics» that Samuelson was least tempted to chase was the «new classical school that believes in full employment and neutral money even in the short run of a few years. Like herpes, this condition caught us unawares» (57, 109).

It follows from the orthodox view that ideological differences among economists must be a consequence either of the fact that some are imperfectly acquainted with the best current theory, or because there is disagreement about the relevant facts, or both. Samuelson was never oblivious to the latter, and his objections to Chicago School macroeconomics were based on the obvious dissonance between its predictions and what often seems to happen in the real world. But he did very little empirical work, and his strongest interest always lay in theory. The element of ideological polemic in his historical writing therefore, consists chiefly in demonstrating the theoretical inadequacy of all attempts to show that an ‘alternative’ economics might better serve the political needs of left-wing causes than ‘mainstream’ economics. His two principal targets were Marx and Sraffa. But Dobb, Meek and Morishima, as also Stigler, Hollander and Blaug – all of whom, in various ways, failed or refused to see the vacuity of much of the Ricardo-Marx analyses – also drew his fire (62, 192). «The real objects of Samuelson’s more heated criticisms were his own time’s contemporaneous historians of economic thought, rather than ancient heroic scholars» (67, 333).

In view of the bitter attack mounted against him and his economics by ‘radical’ political economists after 1969, one might have conjectured that Samuelson’s sustained critique of Marxian economics was the injured response of a man who had prided himself as a progressive and whose famous textbook was reviled as subversive in 1948, but who was now suddenly become «the personification of all that was bad about the running jackals of capitalism» (63, 159). But his response was temperate, and he saw the New Left as «the continuation of an important strand in the development of economics» (Samuelson 1971, i). Moreover he had begun his studies of Marx «around 1955» (62, 190), twenty-two years before the unwelcome appearance of Anti-Samuelson (Linder 1977), per-
haps stimulated by Joan Robinson’s brief *Essay on Marxian Economics* (1942) which he admired and often cited.

No fewer than sixteen out of Samuelson’s seventy-six het papers concern Marx (1, 2, 11, 12, 13, 19, 20, 21, 22, 23, 24, 25, 41, 42, 57, 62), three of which are replies. At the end of his life their author thought this had been too much: «thirteen articles on Marx is ridiculous. He was a terrible economist and a poor prophet» (P. A. S. to A. M. C. W., 7 July 2009). Nevertheless, one of the most elaborate of these essays expounds and pays generous tribute to Marx’s analyses of simple and extended reproduction in volume II of *Capital*: «Marx’s advance on Quesnay’s *Tableau* should win him a place inside the Pantheon» (19, 232). However, although this achievement gets occasional mention in several of the other papers their general thrust is strongly critical. «The labor theory of value has much mischief to answer for – particularly for how it has served to obscure analysis and understanding of inequality» (57, 159). Marx’s «1867 novelties concerning rates of surplus value and exploitation represented a sterile detour that renders zero or negative insight into the laws of motion of real-life capitalism and into the forces that create inequality of wealth and income» (54, 5). Bortkiewicz’s «transformation» of Marxian values to competitive prices is «logically of the form: ‘Anything’ equals ‘anything else’ multiplied by ‘anything/anything else’» (CSP, 3, 154, 309). A socialist planner maximizing steady-state consumption must eschew Marxian «values» and use «bourgeois prices» (CSP, 3, 155, 312). And in his «Foreword» to the Japanese edition of *Collected Scientific Papers* he declared that «No real light of understanding has ever come from the Marxian novelties of analysis» (CSP, 5, 367, 874). Aside from his one genuine contribution, Marx as an economist was but a «minor post-Ricardian» (1, 368).

Samuelson was more than willing to attribute some of the blame for Marx’s analytical failures to Ricardo, «the most overrated of economists» (8, 1507). Samuelson was unimpressed by «the backward and forward gropings of a scholar who from his 1814 entrance into microeconomics until his 1823 death makes almost no progress in solving his self-created ambiguities and problematics» (48, 53). Like Marx, Ricardo was an «autodidact», but Marx had the excuse of having been «cut off in his lifetime from proper criticism and stimulus» (1, 368) quite unlike Ricardo, who blandly ignored the cogent criticisms of his friend Malthus and others. Ricardo did get some things gloriously right, notably the theory of comparative advantage and his highly controversial conclusion that a viable invention can reduce wages and national income. But his confusion about value theory muddied the analytical waters for decades. «How could Ricardo and the classicals have missed understanding how the complication of land is as logically damaging to the
ltv as the complication of time and interest is?» (66, 35). Moreover, where there are many goods, «there are no ‘natural’ prices definable independently of how consumers choose to spend their incomes» (70, 13332). Yet «A deep student of Ricardo will gain insight into the simple truth that David did not operationally pursue a model in which the labour theory of value obtained, even though his twentieth century editor chose not to stress this substantive fact» (48, 55; our italics).

Ricardo’s twentieth-century editor was Piero Sraffa, praised by Samuelson both for his exemplary Ricardo scholarship and for his famous 1960 monograph; but not for the elucidation of Ricardo’s analysis in his «Introduction» – which had been extolled by some for its «reconstruction of Ricardo’s surplus theory» and for «the analytical role of the labor theory of value», thereby underwriting Marx’s critique of capitalism (Eatwell 1984). Samuelson was dissatisfied with Sraffa’s unwillingness to admit either the uselessness of «surplus theory» or the impossibility of any labor theory of value outside Smith’s «early and rude state of society», and was tempted to attribute this to ideological bias: «A deep student of Piero Sraffa will not find it irrelevant information that he was more prone ideologically to Karl Marx than to Ludwig von Mises» (48, 55). Consequently Samuelson «perceived the need to audit objectively the claims for David Ricardo generated and augmented by» Sraffa; and «by comparison to dispel the Schumpeterian and other disparaging of Smith…» (67, 329). To «the fascinating question whether classical political economy does, or can be made to, offer an ‘alternative paradigm’ to modern mainstream economics» (29, 1415), Samuelson’s answers were «no» and «no».

4. Samuelson’s Contributions to het

Of the seventy-six papers we have identified and listed as «Samuelson’s Publications in History of Economic Thought», ten are biographical or autobiographical (e.g., 35, 64), seven deal with aspects of Keynes’s contribution to contemporary economics (e.g., 9, 61), six are reviews or short articles for works of reference (e.g., 6, 70), and four are about historiography (8, 48, 54, 55). A further three are primarily addressed to international trade theorists though with significant reference to past authorities (e.g., 5), two are replies, two more deal with very recent figures (32, 43), one is an historical survey of wage theory (10) and another criticizes libertarian ideas (7). The remaining forty, most of which are fully developed analyses of the «works of the great masters» from David Hume (1711-1776) to J. B. Clark (1847-1938), may be classified chronologically:

1. Before Adam Smith – Hume (31), Quesnay (44) and one other,
2. *Wealth of Nations* and the «Canonical Classical Model» – Smith, Malthus, Ricardo (29), (28), (60) and one other,
3. David Ricardo (70), (3), (4), (49), (53), (74),
4. Johann Heinrich von Thünen (43), (47), (75),
5. Karl Marx (1), (19), (62) and thirteen others,
6. Post-Classical Political Economy – Böhm-Bawerk (59), (72), Fisher (59), Cassel (58), Wicksell (50), (52), Clark (68) and one other.

4. 1. *Before Adam Smith*

Though ungrudging in his admiration for David Hume, Samuelson was conscious of a serious analytical error in Hume’s famous account of the «specie-flow» mechanism. Although international gold flows can indeed bring about equilibrium, they cannot do so by altering relative prices of traded goods because of the law of one price. Their effect is rather through relative quantities produced and traded. Though Samuelson’s «Corrected Version» (31) was first published in 1980 and appeared in *csp*, 5 (1986), it has yet to affect the leading textbooks (e.g., Hollander 1987, Blaug 1996, O’Brien 2004). Samuelson found Hume «more to [his] personal taste» than Quesnay (44, 632); and felt that the explanation of the *Tableau* by Eltis (1975) and others did «not satisfy [his] professional conscience» (44, 633). He therefore constructed a complete model «to indicate why Quesnay’s zig-zags never did fulfil a useful purpose in the analysis of his own system». Important features of this model are a classical production function which Quesnay «definitely glimpsed» and a three-sector input-output table (44, 636, 639-641). Only a Cornfield-Leontief matrix multiplier can generate a temporal process that superficially resembles the zig-zags, but «It seems gratuitous to read this interpretation into Quesnay himself» (44, 647).

4. 2. *Wealth of Nations* and the «Canonical Classical Model»

Samuelson considered the canonical classical model (29) to be one of his two «deepest contributions to the understanding of the classical economic system» (67, 330). It is based on an aggregate production function similar to that specified for Quesnay in (44) above, with fixed land and a variable «labor-cum-capital» composite factor subject to diminishing returns. Labour supply grows when the wage exceeds a ‘Malthusian’ subsistence rate, and capital grows comparably when the rate of return exceeds some zero-net-investment rate3 0. As population and capital grow, diminishing returns bring down wages and the profit rate and the system converges on the stationary state with output and rent maximized and factor shares determinate. Adam Smith, Malthus, Ricardo
Steven G. Medema and Anthony M. C. Waterman

and J. S. Mill shared this model, and also Marx when land scarcity is added to his model (29, 1415).

The inclusion of Smith among the ‘classical’ economists has been contested by some (e.g., Hollander 1980; Waterman 1999, 2009) because land scarcity and diminishing returns, though evident in Wealth of Nations, are not integrated into his analysis nor were recognized in that work by his successors. In his «Vindication» of Smith as a theorist (28) Samuelson ignored such objections. In the stationary state, wages, rents and profits are determined. A non-linear programming model is used to show that Smith’s «value-added» accounting, and «his pluralistic supply-and-demand analysis in terms of all three components of wages, rents and profits is a valid and valuable anticipation of general equilibrium modeling» (28, 42). Takashi Negishi (1989, 77-83) has provided a simplified (linear-programming) account of Samuelson’s argument.

«The Classical Classical Fallacy» was identified by Samuelson as the doctrine that «Fixed capitals are prejudicial to wages and the demand for labor; circulating capitals … are allegedly favorable to the real wage rate and to the demand for labor» (60, 620). He presented a largely literary refutation, supported by a mathematical appendix concentrating on Ricardo; and suggested that many «modern commentators» including Hollander, Blaug, Sraffa, Stigler and Hicks had been «brushed by the classical fallacy» (60, 624-627).

4.3. David Ricardo

«Ricardo had perceived what he considers faults in Adam Smith. Posterity can be glad that this perception tempted Ricardo into economics, though it can be argued convincingly that Smith is mostly the more correct thinker in cases where Ricardo criticizes him» (70, 13331-13332). Samuelson’s generally less than flattering opinion of Ricardo has been noted in section 3 above. «Like the twentieth-century economists Friedrich Hayek and Milton Friedman» he «pushed voters and public opinion towards libertarian laissez-faire» (70, 13333), no virtue in Samuelson’s ‘liberal’ eyes. And because «Ricardo did not coherently understand the intricacies of his own classical scenario, he never realized how unattainable was his hankering to have all relative prices determinable purely by objective technology and cost data» (ibidem).

Samuelson’s extended, two-part «Modern Treatment of the Ricardian Economy» (3), (4) dates from 1959 and these are among the earliest, as they are among the most detailed, of all his papers in het. «David Ricardo propounded a number of what we today should call linear programming problems. Except in the simplest cases he was not able to give complete and correct answers» (3, 373).
In Part i of his «Treatment» (3) Samuelson investigated the pricing of goods and of labour, and land services, in «Ricardo-like» models, much of it in terms of simple algebra, arithmetical examples and diagrams. His most important conclusion was that Ricardo’s expedient of «going to the external margin where labor works with free, no-rent land» to evade the problem of rent was an «illusion». Moreover, «having demolished labor as an absolute standard of value we can turn Ricardo upside down and find in his simplest, long-run model a ‘land theory of value’» (3, 390, 391). Part i abstracted from time. In Part ii (4) Samuelson considered capital and interest aspects of the pricing process. Elementary algebra shows that the labor theory of value is untenable save in the special case where the time-intensities of industries are exactly equal – as Ricardo himself realized; and Samuelson’s equations (17) and (18) have found their way into the textbooks (e.g., Blaug 1996, 92). A final section shows that the «Ricardian land theory of value» can survive the introduction of time and capital.

Papers (49) and (53) were written contemporaneously in 1987-1988 and address an issue considered eight years later in the appendix to the «Classical Fallacy» (60) noted above: the analytical justification of Ricardo’s notorious chapter «On Machinery» in the third edition of his Principles. In this matter, at any rate «Ricardo was Right!» (53). Simple algebra and diagrams are sufficient to show that whereas «net» product (rent plus interest) is increased by an invention, «gross» product (net product plus wages) will fall if labour is elastic at the subsistence wage-rate, since the induced decline in population and wages will dominate. Ricardo’s critics – Wicksell, Kaldor, Stigler, Schumpeter and others – were wrong.

In one of his last het papers (2006) Samuelson collaborated with Errko Etula (74), using «the new tools of intertemporal Pareto optimality» to shed light on «early-nineteenth-century dialogue among the Masters» (74, 12). Two possible ‘scenarios’ are offered for a conjectural, as yet undiscovered Ricardo manuscript of 1814.

4.4. Johann Heinrich von Thünen

Samuelson (43) «for the first time puzzled out the general equilibrium system implicit in Thünen’s Natural Wage as the Geometric Mean between workers’ Average Productivity and their subsistence level» (75, 1). Thünen generalized marginal productivity in land to all other factors and formulated the neoclassical theory of distribution forty years before J. B. Clark (43, 1468). For Samuelson, Thünen «was a genius, a neoclassicist before there was neoclassicism» (47, 1784).

Yet he ended his bicentenary essay, which fully explicated Thünen’s «timeless land-and-labor model, a magnificent edifice» (43, 1469), «with
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a dismissal of his natural wage obsession» (47, 1777). However, Robert Dorfman (1986) later showed that there is «buried in Thünen’s works one formal scenario that correctly achieves his geometric mean formula» (47, 1777). Samuelson conceded (47); but he questioned «whether Thünen’s natural wage serves a useful purpose in ethics or welfare policy analysis» (47, 1784).

In the case analyzed by Samuelson (43) capital is produced by labour alone. At Samuelson’s suggestion Erkko Etula (2008) investigated «the more complicated Thünen case where K as well as corn gets produced by K and Labor» (75, 2). As a result, «A cogent Thünen-inspired answer can finally be given to David Ricardo’s fundamental query; ‘how does a competitive regime answer the puzzle of how society’s total harvest gets divided among (1) the rent to landowner, (2) the wages of laborers, and (3) the interest yield of (vectoral!) K’s’» (ibidem).

4.5. Karl Marx

Samuelson’s fifty-year-long engagement with Marx has been noted above. Here it is sufficient to indicate three of the most substantial and characteristic essays of his thirteen. 1, his earliest het publication, was part of a larger study of «Ricardo-like systems». He concluded that Marx’s economics was «Ricardo without diminishing returns» (1, p. 341). 19 is a vast elaboration of (1, part iii) and much more flattering. «Marx’s Volume ii models of simple and extended reproduction have in them the important germ of general equilibrium, static and dynamic» (19, 232) as Samuelson proceeds to show. Of (62) the title, «Isolating Sources of Sterility on Marx’s Theoretical Paradigms», speaks for itself.

4.6. Post-Classical Political Economy

Though Irving Fisher appeared to have disagreed with Böhm-Bawerk’s Positive Theory of Capital, it now seems that Böhm pioneered «the only valid agio theory of the interest rate» which was later «perfected by Fisher» and others (59, 223). However, this theory, and indeed «modern capital theory», contains «a vital normative flaw» (72): there is no necessary trade-off in stationary state between the interest rate and consumption per capita (72, 305-306). In effect, (72) is a belated tribute to Joan Robinson and Piero Sraffa, whose dissatisfaction with neoclassical capital theory led them to this discovery.

«Gustav Cassel did not … anticipate … ‘the theory of revealed preference’» (58, 515); and most of (58) reports Samuelson’s own contributions. But it ends with a tribute to Cassel’s other claims as a theorist: on countercyclical policy he was «leagues ahead of 1992 Milton Friedman or Robert Lucas»; he understood the necessity of interest under social-
ism; his borrowing of Ricardo’s «purchasing-power parity» was fruitful; he anticipated the Harrod-Domar model in 1918. The article ends with a succinct statement of Samuelson’s historiographic principles: «The past is usefully viewed and ordered in terms of the latest cumulative contents of economic science» (58, 525).

«Of all the neoclassicisists Wicksell is the most humanitarian, the least conservative» (50, 909) – and was a lifelong, though civil, rival of the socially prominent Cassel. (50) supplies a compendious account of Wicksell’s considerable analytical achievements. But in dispute with Cassel, «often it is Wicksell who must be judged to have the weaker case» (50, 910). However Samuelson’s «Foreword» to Wicksell’s Finanztheoretische (52) locates him in «the inner circle of the immortals … of Adam Smith, Léon Walras and John Maynard Keynes».

«Clark’s imperishable contribution to … economic theory was his magisterial paradigm of distribution according to the principle of marginal productivities of labour, land and capital» (68, 1). This essay presents an extended commentary on Clark’s – and in general, neoclassical – distribution theory, acknowledging «pitfalls in heterogeneous capital goods» analyzed in (72) above. But an ingenious use of the «Ramsay-Solow-Samuelson model» can show that Clark «is not always wrong» (72, 6).

Acknowledgements

The authors wish to acknowledge the helpful cooperation of Mrs Janice Murray of the Massachusetts Institute of Technology, editor of volumes 6 and 7 of Collected Scientific Papers of Paul A. Samuelson now in press. They are grateful to John Singleton for valuable research assistance.

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in the History of economic Thought

Republished in Collected Scientific Papers of Paul A. Samuelson (csp), vols 1-5:


To be republished in csp, vols 6-7, not yet available:


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