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PAUL ANTHONY SAMUELSON (1915–2009)

On 13 December 2009 Paul Anthony Samuelson died. One of the most influential economists of the 20th century, he supported *Metroeconomica* in many ways, as an author, referee and adviser, and he was in close contact with some of the members of the editorial boards of the journal.

Metroeconomica pays tribute to Samuelson, the man and the scholar, in terms of the following assessments and personal reminiscences by members of the editorial boards of the journal.

H. D. K. & N. S.

I

To me, the death of Paul Samuelson marks the closing of an important chapter in the history of economics.

Paul Samuelson was a major influence in my training as an economist. Like many others who began their study of economics towards the end of the third quarter of the last century, I was introduced to economics through Samuelson's *Economics*. Like others, I quickly graduated to his *Foundations*, and was seduced by its rigor and elegance. As an undergraduate there was hardly a field of economics I studied—microeconomic theory, macroeconomic theory, international economics, public economics, the history of economic thought, to name just a few—in which I did not read a seminal contribution by him. I wanted to pursue my graduate studies at Massachusetts Institute of Technology (MIT) in large part because I wanted to meet him and to take his classes. At MIT, I took a course in the micro sequence with him, where he mostly covered welfare economics. I feel very fortunate to have been his student literally, as well as figuratively. Since then barely a month has gone by in which I have not read or reread papers by him, even though my own work has been in what may be called the heterodox tradition, while he is known as one of the leading lights of mainstream neoclassical economics.

For me Paul Samuelson's legacy lies in his interest in alternative perspectives to economics, his love for rigorous yet pragmatic mathematical modeling, his broad interest within and beyond economics, and his ability to go beyond technical economics to serve in the policy arena and as public intellectual. Despite his calling Marx a minor post-Ricardian, Samuelson had great interest in alternative approaches to economics. He contributed greatly to the development of Keynesian economics, creating the simple income–expenditure (45° diagram) model (see Samuelson, 1939), still the simplest and arguably most effective tool for teaching macroeconomics. He took seriously, and engaged with, the work of Sraffa and other heterodox Cambridge economists. He was a willing teacher of the history of economic thought to MIT students. I felt, when we talked, that he was quite appreciative of my pluralist Indian background. Although in the *Foundations* Samuelson (1947) developed a unified structure with the optimizing agents at its heart, he believed in building small, rigorous, mathematical models which could solve practical problems and address specific analytical issues. Unlike Friedman, his work did not betray an overarching 'vision', a trait that, I believe, contributed to his open-mindedness. Samuelson's breadth in economics is reflected not only in his contributions to so many fields of economics, but also in the fact that in the information sheet we were given describing the interests of the MIT faculty to help us choose thesis advisors, he was described as specializing in 'economics'! While he was a technical economist of the highest order, he was also a compassionate policy advisor who believed strongly in society's obligation of reducing unemployment, and a public intellectual who wanted to engage the layperson through his news-media columns.

I and many others are saddened by Paul Samuelson's death and the end of an important chapter in economics. But it would be sadder still if, despite the great recession, economics rejects Samuelson's legacy and becomes less open to alternative approaches and knowledgeable about the history of economics, more fixated with mathematical technique for its own sake, more dogmatic in policy matters, and more narrowly specialized by digging deeper into less and less, and hides behind technical jargon unintelligible to the world outside the economics profession.

Amitava Krishna Dutt

II

Although I did not know Paul Samuelson, I was privy to a letter that he wrote to Joan Robinson when she was visiting the University of Waterloo (Canada) in 1971. In it, Samuelson responded to a query about capital theory, evidently eager to set forth his views as clearly as possibly even though I detected hints of exasperation at what had obviously been an ongoing correspondence. Samuelson's position had long been clearly stated and has long since become firmly established within mainstream economic theory. It was reiterated in Samuelson (2010) but in such abbreviated form that it may be worth looking back at a few of his more detailed arguments in order to detect occasional and intriguing doubts about the relevance of mainstream capital theory.

In their influential text, *Linear Programming and Economic Analysis*, Dorfman *et al.* (1958) known as DOSSO, developed a theory of the relationship between intertemporal efficiency and prices of (real) capital assets. Just as modern methods of backwards induction make clear, it is a *necessary* condition for optimization that 'society as a whole [needs] vision at a distance' (DOSSO, p. 321). By implication, *initial* asset prices must be *pre-set* in order for the economy to follow, as in the theory of optimal control, a convergent, but by its very nature, an *unstable* saddle path. The conclusion that 'the intertemporal invisible hand . . . requires only the most myopic vision' (DOSSO) depends crucially on this *pre-setting* of asset prices at just such values as will then evolve under continuous market clearing to ensure that '*the value of the capital stock at current efficiency prices, discounted back to the initial time, is a constant,* equal to the initial value' (DOSSO, p. 322, italics in the original). Had this point concerning the constancy of the *value* of an evolving vector of capital goods (the quantities of which are initially given in arbitrary amounts) and the concomitant requirement for 'vision at a distance' been more frequently and forthrightly stated during the course of the capital controversy, there would surely have been less heat and more light shed on the topic—if not any more agreement on the appropriate theory.

I do not know if my efforts to explain Samuelson's theory had any effect, but when Robinson came to reprint part of the exchange with him on reswitching, she included a lengthy footnote (Samuelson, 1975, pp. 44), wherein one finds a hint of doubt concerning the relevance of mainstream capital theory and an implicit reference to 'knife-edge' problems in general. Addressing Geoffrey Harcourt, Samuelson writes: 'he does have the right to ask me why a system might move along the warranted paths I so skillfully built into my discussion' (Samuelson, 1975). Robinson would have answered, 'And so much the worse for them'.

Such doubts had been stated forthrightly in an earlier paper (Samuelson, 1967) written for a volume of essays which were presented as seminar papers at MIT during the academic year 1965–66 when debate surrounding questions in capital theory attracted so much interest. In a delightfully written conclusion, Samuelson asks where 'a system gets its guidance'—its 'vision at a distance'. He finds a metaphor for 'ideal futures markets' in a (presumably skilled) cyclist.

> The rider of the bicycle is the bulk of the market, a somewhat mystical concept to be sure—like its analogue, the well-informed speculator who gets his way in the end because his way is the correctly discerned way of the future; and those who think differently are bankrupted by their bets against (him and) the future. (Samuelson, 1967, p. 229)

Samuelson would no doubt have seen the relevance of a cartoon by Edward Gorey (1983) which shows a woman on a bicycle with an urn balanced on her head about to head off down a rope anchored precariously to each side of a canyon at the bottom of which swirl dark and dangerous waters. A variation on Gorey's caption, 'Innocence, on the bicycle of Propriety, carrying the urn of Reputation safely over the abyss of Indiscretion', might read: 'Capital Theory, on her optimally controlled Bicycle, carrying an urn of Assets, over the abyss of Bankruptcy'.

Despite misgivings, Samuelson remained true to his earlier contributions as he pushed the arguments in new directions, often drawing on a lifelong interest in developing economic theories isomorphic to the laws of physics. One paper begins: 'A released body falling to the earth travels a distance proportional to the second rather than the first power of its terminal velocity. This fact, belatedly recognized by Galileo, denied by Descartes, established by Huygens and Newton, culminated in the law of conservation of (mechanical) energy' (Samuelson, 1990, p. 57). A few lines later, a new theorem on the invariance of the capital–output ratio along an optimal path of accumulation is presented. That an argument drawn from physics concerning the evolving future of a capitalist economy under conditions of uncertainty could be central the thinking of an economist who was also a standard bearer of Keynesian economics will remain something of an enduring mystery. It may not be too much to claim that Samuelson's position on many other questions in economics was equally complex.

Harvey Gram

III

Paul Samuelson once remarked, 'In this age of specialization, I sometimes think of myself as the last "generalist" in economics' (Lindbeck, 1992). As is clear from this remark, Samuelson was by no means given to false modesty. Yet, after the disappearance of Hicks there were very few contenders for the title. Indeed, Ken Arrow who might also qualify for that description generously described Samuelson as the greatest living economist. The comparison between the two is both striking and revealing. Arrow is the inheritor of the Walras Pareto tradition and although he has made contributions in an extraordinary variety of fields in economics he will continue to be thought of as the creator of the Arrow–Debreu model which, in a sense, completed the Walrasian enterprise and firmly established the axiomatic approach to modern economics.

Samuelson, however, was an economist in another tradition, and can be considered as having stuck much more closely to the inheritance from physics which economics had received initially from classical mechanics. Samuelson asserted that he saw his work in the *Foundations* as an adaptation of the analysis of thermodynamics to economics. Thus he can be thought of as in the line of Gibbs, who, of course with the rise of econophysics, is now back in vogue. In fact he was a direct heir of Gibbs for in his own words, 'I was able to become the sole protegé of the polymath Edwin Bidwell Wilson, who had himself been the sole protegé of Yale's great physicist Willard Gibbs' (Samuelson, 2003). But apart from his theoretical point of view Samuelson was an economist in the true sense of the word. His aim was not to build more and more sophisticated models but use models to explain economic phenomena and he did not hesitate to enter the arena and to engage in the building of policy and to take strong positions on social problems. He, like many other leading economists, had faith in the capacity of economics to understand and resolve the major problems of society. When Lucas (2003) said that 'the central problem of depression prevention has been solved' there were echoes of Samuelson's phrase 40 years before, cited in the lines attributing his Nobel prize, 'I think the '60s will give us the potentiality of very good growth. More and more of our social problems of the past are, in fact, being licked. So I would face the '60s not complacently, but optimistically' (Lindbeck, 1992).

But what of his impact on the field? To quote Bob Solow, 'If you did a time and motion study of what any modern economist does at work, you would find that an enormous proportion of standard mental devices trace back to Paul Samuelson's long lifetime of research' (Frost, 2010). However, rather than add to the thousands of words on his work that have been written about Paul Samuelson, I will just add a couple of personal recollections of him.

I first spent some time with him when I was invited to MIT as a job candidate and as a young and rather naive PhD student sat at the famous table with him and other gods of the profession such as Solow. It was intimidating but fun, and Samuelson teased me unmercifully about having started my undergraduate studies in geography, with quips about space men and with a clear indication that this was not a major avenue for research.

I have never asked Paul Krugman what Samuelson's reaction to his work on the 'new' economic geography was. I had done my thesis on applying non-cooperative game theory to international trade, and at the time all the people who were to be taken seriously in economics argued that there was not future for non-cooperative game theory and that furthermore it was not appropriate for trade theory which was essentially 2×2 although the factor-price equalization (FPE) theorem (1948) and the famous Stolper–Samuelson result (Samuelson and Stolper, 1941) could, with difficulty, as my advisor Harold Kuhn had shown, be generalized. We had a brief discussion about the FPE theorem since he was engaged in an arcane discussion with Ivor Pearce at the time. I remember clearly that he was quite prepared to listen to a youngster and to conduct an argument without imposing his overwhelming authority.

The second time that our paths crossed was much later, some 20 years later, when he visited the European University Institute in Florence where I was the head of the economics department. I suggested that he might like a game of tennis, knowing that he was a long time adept at the game, and that it was a subject for many stories at MIT, but hoping that in his seventies he might have slowed down enough for me to give him a decent game. It turned out that although he was not terribly mobile whenever he was in striking distance he would play devastatingly accurate shots and particularly drop shots. I enjoyed it and I believe he did.

Off the court, he would talk on almost any subject and was a fountain of wisdom on all aspects of economics but always with an eye to what was going on in the world. He was not prepared to indulge in what he once described as the 'mental gymnastics of a peculiarly depraved type', practiced by the profession, and that many academic economists were like 'highly-trained athletes who never run a race' (Samuelson, 1947). He was fundamentally committed to using economics to move society in what he considered to be the right direction. He also, unlike many economists today, admitted that economists had a responsibility for the current crisis and he said, 'Mea culpa, mea culpa. MIT and Wharton and University of Chicago created the financial engineering instruments, which, like Samson and Delilah, blinded every CEO—they didn't realize the kind of leverage they were doing and they didn't understand when they were really creating a real profit or a fictitious one' (Samuelson, 2009).

But with all the recent discussion about the crisis, my favorite quote from Paul Samuelson is that which figures in his Nobel autobiography, 'My Chicago-trained mind resisted tenaciously the Keynesian revolution; but reason won out over tradition and dogma' (Samuelson, 2003). As his nephew, Larry Summers said, 'Through his research, teaching, and writing he had more impact on the economic life of this country and the world than any government economic official and many presidents. We will not see his likes again' (PBC Newshour, 2009).

Alan Kirman

IV

We met Paul Samuelson, individually or together, several times, beginning in the early 1980s and (one of us) for the last time on the occasion of his 90th birthday. We corresponded with him during the same period and beyond. We had the privilege of interacting with him most intensively while working on our book, *Theory of Production. A Long-period Analysis*, in the late 1980s and early 1990s. One of us was then teaching at the Graduate Faculty of the New School for Social Research and the other was a frequent visitor in New York to collaborate face to face.

Paul encouraged us to keep him informed about the progress of our work, and whenever we sent him material, he would swiftly get back to us with remarks and comments. Each time both of us were in New York we contacted Paul. He then typically invited us to join him at MIT and discuss with him. On such occasions he had carefully read what we had sent him and gave us his criticisms and suggestions. But our conversations never stuck narrowly to some particular idea or argument: they were always enlightened by his breadth and depth of knowledge and erudition. Paul was remarkably well read, not only in economics but far beyond, and we wonder whether there was a subject he was not interested in and on which he had nothing useful to say. He was, in Adam Smith's words, a most impressive exemplar of those 'philosophers or men of speculation, whose trade it is, not to do any thing, but to observe every thing; and who, upon that account, are often capable of combining together the powers of the most distant and dissimilar objects' (Smith, 1976, I.i.9). Paul's ability to combine and relate to one another pieces of knowledge coming from very different fields was astounding and often left us behind, open-mouthed.

We were surprised, and of course extremely pleased, to see how much he cared for our (non-orthodox) work and how open-mindedly he followed our attempts to come to grips with Piero Sraffa's published and unpublished work. Despite several critical essays he had written over the years on Sraffa's contribution, there was no doubt that he was fascinated by Sraffa, the man and the scholar, and esteemed him highly. He was curious to learn what there was in Sraffa's papers and manuscripts. We never had the feeling that he discriminated against us because of our intellectual leanings. On the contrary, he was always kind to us and supported our efforts. When in the early 1990s we sent him a paper in which we showed that the Non-substitution Theorem (an issue to which he had contributed as early as 1949) does not apply in the case in which the rate of interest equals its maximum value (corresponding to a zero wage rate), we exchanged several letters. In the end he agreed with us and advised us to submit the piece to a prestigious journal; he was sure that the paper would be accepted for publication. However, the editor sent us a stock rejection saying that our results were not unfamiliar to the ordinary graduate student at his university. When Paul saw the letter he was furious and wrote a bitter letter to the editor. (We published our paper in another journal.) And when a little later we were drawn into a controversy with a distinguished mathematical economist who had put forward an interpretation of Ricardo and criticism of Sraffa, Paul in a letter dated 13 June 1996 suggested the following formulation to be used in our reply to our opponent: 'Everyone recognizes the perfections of Sraffa's compilations of Ricardo's writings, and among commentators and analysts of Ricardo's scenarios, reasonings, and exposition, he is universally regarded as a giant. For a [. . .] to slight Sraffa's views is to do a disservice both to the literature and himself.'

Paul was deeply interested in the history of economic analysis and very knowledgeable. He contributed important papers on major economists, including Adam Smith, Ricardo, Johann Heinrich von Thünen, who was one of his all-time favorites, Karl Marx, Eugen von Böhm-Bawerk, Knut Wicksell, Irving Fisher, etc. He was keen to put their arguments into mathematical form, which served him as a device to assess their consistency. We could not agree with all the things he wrote, especially about the English classical economists, but his papers were always challenging and never dull. Given his familiarity even with minute biographical details and scholarly footnotes of the authors he studied, it was surprising to see him advocate what he himself called the 'Whig' history of economics. Perhaps this was just another provocation with which he occasionally loved to tease his audience. Did he really mean it? As he wrote in a letter of 25 November 2005 to us: 'Anything I state is, of course, rebuttable'. In a number of cases, we are inclined to think, he was eagerly waiting for his statements to be rebutted.

When editing *The Elgar Companion to Classical Economics* (1998) we were keen to include an entry on Paul's interpretation of the classical authors. Alas, all his major students declined our invitation on the ground that they were insufficiently competent to assess his wide-ranging contributions. In our distress we approached Paul, asking him whether he would be prepared to write the entry. He answered overnight in the affirmative and delivered the entry long before the deadline.

On 18 May 2005 Paul asked us in our capacity as editors of *Metroeconomica* to publish a paper of one of his former students and then collaborator. He garnished his letter with the following remark: 'To be candid, were it not for the usual time pressure, I might have warned him [the author] against *Metroeconomica* if I had known that the latter did not have the status of *RES* or *JPE* or *QJE* or. . . .' This kind of statement was characteristic of Paul: he had so many things to say that each sentence had to carry several meanings.

Re-reading our correspondence with Paul and remembering the meetings with him brings back the *Gestalt* of a warmhearted, kind, helpful, generous, open-minded, curious scholar with a tremendous breadth of knowledge and originality.

Heinz D. Kurz and Neri Salvadori

V

There were three in the spacious room: Paul Samuelson, a student and myself. I was Research Associate at Harvard in the Winter 1973–74 in order to get some international experience before taking up the chair offered to me at the University of Frankfurt, and so I had decided to add Samuelson's seminar at MIT to the courses I was taking at my host university. *Tres faciunt collegium*—our number just sufficed. He basically improvised the sessions or invited someone to give a paper. His comments were so rich that I could not understand why not all of MIT was sitting at his feet to listen. Part of my future job was to teach Marxian economics, and Samuelson was eager to force me to admit mistakes in the Marxian arguments. He disliked dialectic escapes but he allowed me to give a paper on the Marxian forms of value, i.e. on the first chapter of the first volume of *Das Kapital*. He remarked a week later that the argument was interesting but that it was impossible to retain in what it consisted. On another occasion, he invited Dorfman who gave a paper on 'Malthus's theory of growth'. Samuelson looked at the equations and said: 'This is Ricardo's model!' I tried to protest, arguing that Ricardo and Malthus disagreed, that it was necessary to grasp their intentions and to interpret the equations, but Samuelson got up and demonstrated methodically, pointing at one equation after the other, that the models were formally the same, and he concluded not that Dorfman had misread, but that Ricardo and Malthus must have erred, when they thought to be in disagreement.

The characteristic traits which became visible in Samuelson on this and similar occasions were: He had a vision of economics in which the relevant models belonged to one and the same fundamental theory. The range of variation of these models was broad and he made a sophisticated use also of the simpler versions, but he never would admit that there could be a co-existence of two serious theories, and it was the structure that mattered, not the verbal exposition. The paradox was that he was excellent also at providing verbal and metaphorical explanations. He was a brilliant polemicist and did not hesitate to show it, if he felt that it was necessary to defend his basic beliefs in his theory, in moderate reforms and in welfare.

But he was fascinated by Marx and felt challenged by Sraffa. When I returned in the 1980s on several occasions, I again had first to admire his method and his orderliness, which was not pedantic, but necessary in order to pursue his interest in many subdisciplines of economics simultaneously. Did he not have a special file on this or that on his desk? Was there not a paper to be found in a specific drawer? The discussions therefore also were organized. I was related, so to speak, to two of his files: Marx and Joint Production in Sraffa. He would bypass my curious questions about his opinions on monetary policy, say, showed at most some curiosity himself for the political situation in Germany or Switzerland, and came quickly to what he regarded as the heart of the matter, as far as the two of us were concerned. He had made a remark about the high rate of crimes in the United States, and that the possession of weapons should be restricted. I dared to recall that there were far fewer murders per capita in Switzerland, although all Swiss males had their army guns and some ammunition at home, so that social cohesion was what mattered, according to me, but that rendered him impatient. What were the conditions for the wage curve to be monotonically falling in the presence of joint production? Which proofs did I have for monotonicity? He invited me to give a paper on square joint production systems at MIT and I illustrated how by-products, if they become commodities with positive prices, give rise to the emergence of new processes, so that the system of prices tends to be overdetermined, rather than underdetermined, using examples taken from the energy sector. But was underdetermination not generically possible, if one admitted neoclassical indifference curves, determining the rates of substitution and hence prices from the subjective demand side? I argued that a theory that did not need indifference curves was more general, because it needed fewer assumptions, but he did not move and treated utility theory like an axiom which was not to be touched. Sraffa was to be integrated into *his* theory.

These discussions resulted in a written exchange, following on the Sraffa conference held in Florence in 1985. Samuelson (who had not been present at the conference) later contributed a paper to the proceedings of the conference, entitled 'Revisionist findings on Sraffa', which was commented upon by John Eatwell, Pierangelo Garegnani and myself. This is not the place to argue who was right in what and why. Samuelson replied to my comment, 'From no one have I learned more than from Bertram Schefold'—a phrase that I could not help feeling proud of. But, when I visited Samuelson at MIT next time and asked him what he had meant by it, he answered that I was less in contradiction with him than the other two and so I had to conclude that his praise was not only a recognition of constructive arguments, but also a polite way of weakening his adversaries by pretending that we were 'at bottom . . . in agreement', apart from what he considered as 'casuistry' (Bharadwaj and Schefold, 1990, pp. 323–30).

There was a third phase in our collaboration. I had become the editor of the 'Klassiker der Nationalökonomie', a collection of 100 classical texts of economics, of all ages and not only European or American, but including also an Asian and a North-African Classic (Ibn Khaldun). Each volume was reproduced in the form of a rather luxurious reprint of the original edition or manuscript, and each was accompanied by a volume of commentaries with comments from some of the most famous economists of the time. Samuelson contributed on several occasions, and he never failed to add an original idea to the analytical reconstructions. He was proud and delighted, when we chose his *Foundations* as the only text of an author still living; he wrote that he considered this honor as still greater than his Nobel Prize. He was human and generous, ready to give time till he was overcome by the feeling that he had to do some real work and to write down an important idea. I regretted that certain differences in points of view could not be overcome. He thought that there was only one theory, the others being wrong. I thought that there were several, all justifiable, and that there was one important mistake, residing in the neoclassical theory of distribution. As I grow older, I have come to appreciate Samuelson's tolerance in listening to what he regarded as the errors of the young. He was, if the use of the ancient comparison is permitted, more like the fox with many brilliant devices than like the hedgehog with one grand idea, and it may be that this will result in a decline of his fame in the very long run, but he will remain one of the most amazing economists not only for his contemporaries.

Bertram Schefold

VI

This is not the place for any supposedly objective and authoritative assessment of the work of Paul Samuelson; what follows simply hints at some of the ways in which he impressed the present writer.

With an early interest in both capital theory and trade theory, I was impressed by the fact that in his famous papers on factor-price equalization (Samuelson, 1948, 1949) Samuelson was careful to name his factors as homogeneous labor and *land*, thereby sidestepping any capital-theoretic difficulties. Even in the earlier Stolper–Samuelson paper on tariffs (Samuelson and Stolper, 1941), which did reason in terms of labor and capital, it had been carefully noted that, 'It might possibly give rise to less confusion if instead of capital the second factor were called land because of the ambiguities involved in the definition of capital. The reader who is bothered by this fact is invited to substitute mentally land for capital in all that follows'. The influential DOSSO volume on linear theory took a disaggregated approach to capital, of course, but I was always puzzled by Samuelson's renowned 'surrogate production function' paper (Samuelson, 1962); why did he publish it, when he understood very well that 'the surrogate case is so special' (see his footnote citing Garegnani)? Easier to understand was his summing-up of the *Quarterly Journal of Economics* (Samuelson, 1966) symposium on capital theory and his observation that 'scholars are not born to live an easy existence. We must respect, and appraise, the facts of life'.

Samuelson could be helpful in a very down-to-earth way. Stan Metcalfe and I once had a trade theory paper rejected by a respected journal. Our argument involved two commodities, labor, land and a positive rate of interest and the rejection was based on the 'grounds' that, with two goods and 'three factors', our results were unsurprising. Samuelson, who had seen an earlier version, eventually asked us what had happened to the paper and we told him. Not long after that, the editor of the journal wrote to us 'out of the blue', asking whether we might like to resubmit the paper. We did—and it was published.

There was an admirable breadth to Samuelson's interests and he would take seriously and write about authors and issues not central to the current mainstream. What he wrote on Sraffa, say, was not always agreed to by Sraffians. And his forays into the history of economic thought were too Whiggish, too instrumentalist, greatly to endear him to genuine historians of ideas. Yet the very fact that he did enter such fields could only be welcomed, given his standing in the profession and given the narrowness of some contemporary conceptions of economics.

Since this is a personal reminiscence, not a detached assessment, it is perhaps permissible to recall that, when asked whether he would like to contribute an essay to my recent *Festschrift*, Samuelson not only said 'Yes' but insisted that he would like to be an editor and wrote a Preface. Sadly, Samuelson did not live to see the published volume but that he could, in his closing years, be so enthusiastic and friendly towards someone he had never met face-to-face surely shows that he could be a most generous spirit.

Ian Steedman

VII

Paul Samuelson was a larger-than-life figure. His path-breaking contributions spanned across all the fields of economics, ranging from consumer theory, capital theory, international trade, general equilibrium and welfare economics, public expenditure theory, to dynamics, where he developed the first 'overlapping generations model'. He was without doubt the most influential economist of the second half of the 20th century and was the key person to introduce the application of formal mathematical methods to economic analysis, thereby transforming the discipline from being largely literary to being amendable to formal mathematical methods. Much of this was accomplished in his *Foundations of Economic Analysis*, written as his Harvard PhD thesis and published in 1947. This was a real *tour de force* and established in no uncertain terms how the formalization of economics using standard mathematical techniques leads to great gains in terms of transparency and our understanding of economic principles and processes. It truly did lay the foundations for economic analysis for future generations of economists.

Although as a graduate student at Harvard University in the 1960s I had only very limited contact with Paul Samuelson, he nevertheless had a profound impact on my career. In the first place, exposure to his *Foundations*, as a mathematics student in New Zealand, was eye-opening to me in terms of illustrating the potential role for mathematics in other disciplines, and was a key element in my decision to switch to studying economics. Subsequently, as a student, and later as a professional economist, the way that Professor Samuelson applied mathematical techniques in developing his theoretical models was a great source of inspiration to me. He was never high powered or overly abstract, but at the same time his analyses were always very rigorous and offered great insights. It is an approach that I have sought to emulate in my own research.

I had two main points of contact with Professor Samuelson. The first was through my good friend Ed Burmeister, who was one of Paul Samuelson's most successful students in the 1960s. Ed and I wrote many joint papers in the 1970s, some of which reflected Paul Samuelson's view via his feedback to Ed. The other point of contact occurred in the Fall of 1966, after spending the summer working at MIT, I audited Professor Samuelson's course on advanced economic theory. It was a treat. To this day, I remember one of his quotes, which I often repeat to my own students. He said: 'You don't have to be smart to use mathematics, because the mathematics is smart'. I found this not only to be a rather delightful expression, but also to contain a lot of wisdom, offering reassurance to graduate students who might be struggling in applying these analytical tools.

All of us who received our economics training in the Samuelsonian era are in his debt. I hope that we can pass on his wisdom, insights and humility to future generations of economic students.

Stephen J. Turnovsky

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