Erasmus Journal for Philosophy and Economics,
Volume 17, Issue 1,
Summer 2024, pp. aa–aa.
https://doi.org/10.23941/ejpe.v17i1.863

Obituary: Robert Solow and Economic Modeling

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Robert Merton Solow (August 23, 1924 – December 21, 2023) was one of the most influential economists in the second half of the twentieth century. In 1987, he was awarded the Nobel Prize in Economic Sciences for his work on the theory of economic growth. Among economists he was a popular choice. Not only had the Solow growth model become one of the main conceptual tools used in the discipline but Solow was also highly regarded for his work in other areas of economics. He exemplifies what is often considered the MIT (Massachusetts Institute of Technology) way of doing economics: using simple mathematical models tailored to the problem in hand, taking account of all available evidence.

Solow also played an important role as a teacher and hence in the emergence of MIT as one of the most influential economics departments in the world. He was a very popular supervisor, always attending to the needs of students. His conscientiousness as a teacher is illustrated by his habit of discarding his lecture notes each time he delivered a course, so that the process of rewriting them every year would ensure that his teaching did not become stale. (There was one year when he kept his notes because he wanted to use them as the basis for a textbook, which was never written.) The list of his students he supervised reads like a 'Who's who' of modern economics, including, among many others, Joseph Stiglitz (growth and distribution), George Akerlof (wages and capital), William Nordhaus (technological change), Peter Diamond (optimal growth) and Martin Weitzman (indicative planning). The list is also remarkable because, from 1954 to 1997, he supervised theses on a great variety of subjects.[1] If we included students who took his courses but did not have

[1] Other topics included measuring investment, employment and unemployment, inflation dynamics, income distribution, international trade, welfare economics, and urban economics.

AUTHOR'S NOTE: I am grateful to Bradley Bateman, Verena Halsmayer, Gideon Frey and an anonymous referee for helpful comments on an earlier version.

him as a supervisor, the list of eminent economists he taught would be even longer.

To understand Solow's achievements, it is necessary to understand the juncture at which he entered economics. He entered Harvard in 1940 but, as soon as he was old enough, he enlisted in the US Army, which made use of his ability to speak German by assigning him to a communications unit, serving in North Africa and Italy. After demobilization in 1945, he married Barbara Lewis, an economic historian, who suggested that he would find economics interesting. He did. After graduation he went on to graduate work, also at Harvard, working as a research assistant on Wassily Leontief's input-output project, for which he calculated capital coefficients. Leontief, a mathematical economist who had a keen interest in applying theory, was an inspiration for him. In 1949 he was appointed an Assistant Professor at MIT. After a year at Columbia, studying mathematical statistics with Harold Hotelling, and drawing on the statistics he learned there, he submitted his doctoral thesis to Harvard in 1951, applying Markov processes to the dynamics of the interpersonal income distribution (Solow 1951). As with the Leontief project to which he had contributed, this involved combining theoretical arguments with the analysis of data, albeit dealing with personal incomes, not production.

The decade when Solow entered the field was one in which American economics changed significantly. Mathematical economics, represented by Paul Samuelson's *Foundations of Economic Analysis* (1947), was becoming more important than it had been; wartime provided the context in which Keynesian ideas about macroeconomic management, propagated in Samuelson's *Economics: An Introductory Analysis* (1948), were developed. MIT's Department of Economics and Social Science was at the forefront of these changes. In 1940 it was primarily a service department in an engineering school, providing the economics and management courses that scientists and engineers were required to take but, after the appointment of Samuelson, it established a doctoral program in economics, the first graduate, in 1945, being Lawrence Klein (supervised by Samuelson) who went on to dominate the field of macro econometric modeling. On his arrival at MIT, Solow shared an office suite with Samuelson, with whom he was to work very closely for nearly sixty years. Samuelson believed that, by 1950, MIT was already attracting the best graduate students, but it was several years before it began to be top of the rankings of US economics departments. Solow arrived at the right time to play a major role in that transition through both his research and his teaching.

ECONOMICS AS A TECHNICAL DISCIPLINE

Solow's view of how economics should be approached is described in an article in *Daedalus*, "How did economics get that way and what way did it get?" (Solow 1997a). Though written much later, he was responding to criticisms of economics, perhaps voiced most strongly during the economic turmoil of the 1970s, when economics was widely criticized, even by economists, for becoming distant from reality—for becoming too formalistic, with the construction of abstract theories that had no empirical foundation. Those making the accusation included his teacher, Leontief (1971) and a close friend and future collaborator, Frank Hahn (1973). Solow's defense of economics was that, rather than becoming formalistic—pursuing mathematical rigor for its own sake—it had become technical. It had moved on from being a discursive subject, pursued by "gentleman-scholars", to being a technical subject centered on the building of models. Models abstracted from reality in order to isolate certain problems, on which they could, if appropriately constructed, convey important insights. He conceded that there were certainly economists whose work could be described as formalist (Solow had been one of the pre-publication readers of Debreu's *Theory of Value* (1959), written by someone who explicitly advocated the approach of the formalist school of mathematics), but they were a tiny minority. Being technical was different: it was a strategy for tackling important economic problems.

This article was written both as a defense of a discipline under criticism and an explanation of how economics should be done and of the principles underlying his own work. It was a time when economics was changing from a discursive subject, dominated by verbal reasoning to one dominated by the construction of what came to be known as models: mathematical structures that were constructed by precisely specified abstractions from reality and that could be manipulated to show rigorously what conclusions did and did not follow from those assumptions. This change had begun before the Second World War but gathered pace afterwards, at just the time that Solow became an economist. His teacher Leontief, who taught mathematical economics at Harvard and was constructing a model of the United States economy, was at the forefront of this movement, as was Paul Samuelson, with whom Solow came to work very closely soon after arriving at MIT. An important point about models was that they were not descriptions of reality but constructions—artifacts—that were created for a purpose (see Halsmayer, forthcoming).

One consequence of the rise of modeling was that a clear distinction emerged between theory and application. Whereas in the interwar period it was often hard to tell whether authors were making statements they believed to be true of the real world, or constructing an artificial world, in the postwar world such ambiguity became much rarer (Backhouse 1998). Models were objects that were intended to be used, not statements of beliefs about the world. A good model would reflect important characteristics of the world, hopefully relevant to the problem in hand, but theoretical models were analytical tools, as were the quantitative methods, mostly statistical. One implication of this is that an economist might work with models that were inconsistent with each other. Consistency is desirable but it is more important that models are workable and that the assumptions made are appropriate for the problem in hand.

The rise of modeling did not mean that verbal analysis was redundant. To the contrary—if they were to be useful models had to be interpreted. The mathematics, whether algebra or geometry, would be meaningless without a story to accompany it (see Morgan 2012). In general, it was at this point in the argument that philosophical differences between economists emerged. Economists might use the same mathematics but one might treat the model's predictions as a truth about the world whereas another would see them as statements that needed careful qualification and analysis of their applicability.

MODELING GROWTH

The model for which Solow is best known, the so-called 'Solow growth model' is a simplified, aggregated model in which there is a single commodity that can either be consumed or invested and which is characterized by perfectly competitive markets (Solow 1956).[2] It shows how, starting from an arbitrary initial point (which in this model means an arbitrary capital-labor ratio) the economy will converge on a growth path corresponding to the rate of population growth plus the rate of growth of productivity. It abstracts from numerous features of the world—the variety of goods produced and consumed, complex consumption and saving patterns, heterogeneous labor and capital, connections between the rate of investment and technological progress, imperfect competition, and no doubt many others—but it is believed that this makes it possible to focus on a single problem. That problem is how competitive markets, including

[2] For a historical account of the Solow model that probes much more deeply into its context and significance, see Halsmayer (forthcoming).

those determining the rate of profit, cause the capital-labor ratio to change until equilibrium is achieved. It shows that under this highly abstract scenario the equilibrium growth path is stable.

One thing the model does not explain is what determines the growth rate on which the economy converges. It would be natural to expect that investment (and hence the rate of capital accumulation) would affect the rate of technical progress, for newer capital (newer plant, machinery, buildings, vehicles) is typically more productive than old capital goods, but this problem, important as it may be, is ignored in the interests of tackling a smaller problem more rigorously. What was that problem? Solow framed it as the question of whether capitalist economies were inherently unstable, presenting his model as a better answer to this than the growth model produced by Roy Harrod (1939) and Evsey Domar (1948).[3] His claim was that standard 'neoclassical' processes of price adjustment would stabilize a capitalist economy. Of course, this was subject to numerous qualifications, notably that 'Keynesian' problems of deficient aggregate demand did not arise—that savings would automatically be translated into capital accumulation—and that it was possible to ignore issues related portfolio choices between money, other financial assets, and capital.

As influential as this growth model was, Solow's attempt to operationalize a key concept—the rate of technological progress—in a paper published the following year was just as influential (Solow 1957). Drawing on the growth model, this paper proposed a procedure for estimating the rate of technical progress that would work if all the conditions of the Solow model were satisfied—the Solow residual. It acquired that name because of the way it was calculated. Using the assumption of competitive labor and capital markets in which prices of factors equaled their marginal products, the contributions of capital accumulation and growth in the labor force could be calculated. The residual is what is left when these are deducted from the actual growth rate. It is commonly interpreted as a measure of technical progress. As with the original growth model, the legitimacy of this interpretation of the data is completely dependent on the abstractions being made. For example, if there are variations in capacity utilization due to fluctuations in the level of aggregate demand relative to potential output, the residual will include this as well as technological progress.

[3] See Halsmayer & Hoover (2016) and Halsmayer (forthcoming), who argue that such an interpretation is based on a misunderstanding of Harrod.

However, Solow's interest in growth was not confined to this model. Three years before the publication of the aggregative neoclassical model, he had finished a multi-sector model with Samuelson, and two years after the famous growth model he co-authored a book, *Linear Programming and Economic Analysis* (Dorfman, Samuelson, and Solow 1958) that analyzed optimal paths of capital accumulation in a much more disaggregated, multi-sector model. This provides the justification for Samuelson's observation that "One might almost say that there are two Solows—the orthodox priest of the MIT school and the busman on a holiday who operates brilliantly and without inhibitions in the rough-and-ready realm of empirical heuristics" (Samuelson 1962, 193). It was the first Solow who, together with Samuelson, was responsible for multi-sector models and for the derivation of 'turnpike theorems' concerning optimal growth in such models. These theorems were certainly highly abstract, and possibly it was wishful thinking to suggest that they could have practical application, but the hope was to draw useful normative conclusions about capital accumulation. Indeed, part of the motivation for the project out of which *Linear Programming and Economic Analysis*, the book in which turnpike theorems first appeared in print, emerged was the search for methods that could be applied in an age when modern computing was in its infancy and the computation of anything other than a linear model would be highly problematic.

Another case where Solow pursued abstract theories that had little relation to reality was his contributions to the theory of capital, for they involved comparisons of steady states (growth paths on which an economy was unchanging apart from a change in scale) that could never be observed in reality. However, the aim of his work was to challenge claims made by an eminent and respected economist (Joan Robinson) and to defend the use of models he believed to be useful. He and his colleagues on the MIT side of the controversy knew that one-sector models were not as rigorous as multi-sector models, but they believed that there were problems for which this did not matter. Thus, Solow later dismissed the controversy as "all smoke and no fire" (Solow 2004, 167).

Solow is best known for the 'Solow model' in which growth can continue indefinitely because in the long run, there are no scarce resources other than labor, which is growing at a constant rate. Given enough time, any level of capital can be created. In other words, it is a model in which the availability of land or other natural resources does not constrain growth. In the 1970s, the question of whether limited resources would

constrain growth came to the fore, because of the forecasts produced by the Club of Rome and the oil crisis that erupted in 1973. Solow consistently considered 'Doomsday Models' produced by bodies such as the Club of Rome to be "worthless as science and as guides to public policy" but he conceded that his own theory's neglect of natural resources "a careless mistake" (Solow 1972a, 3832; 1982, 250). During the 1970s, he turned repeatedly to the problem of natural resources, focusing not on what should be done (for he believed that there was much that was unknown) but on how to think about the problem. This included thinking not just about whether growth would have to stop but also problems of inter-generational equity. In some of this work he used simple one-commodity models, as with the Solow model, because his belief was that a more complicated model would add nothing of interest to the discussion.[4]

In the 1990s, as public concerns changed, Solow adjusted his focus to discuss the more general issue of sustainability and, eventually climate change. As with his earlier discussion of natural resources, he focused on his economist's view of how the problem should be approached, which was necessary because the notion of sustainability was vague and had become a buzzword (see, for example, Solow 1991). He was critical of claims that we have a moral obligation to leave the world as we found it because that was not feasible. Instead, we should assume an obligation to behave in such a way that future generations could be as well-off as we are: we should not become wealthy through impoverishing our successors. This involves making best guesses, which may be wrong, of how technology will develop and how alternative resources can be substituted for each other. He saw no reason to believe that intergenerational equity could be achieved without government intervention and, when climate change emerged as an important issue, he argued that it should be addressed through market-based policies such as a carbon tax or tradable emissions permits (Arrow et al. 1996).

UNEMPLOYMENT AND INFLATION

Although responsible for creating a neoclassical model of economic growth, Solow became widely known as one of the leading Keynesian economists of his generation. Ignoring the many book reviews he wrote during the 1950s, his move away from growth and problems related to technology came at the end of the decade. He was commissioned, with

[4] For Solow's own assessment of his work on natural resources, see Solow (2009).

Samuelson, to write a major survey of inflation theory. This was never completed but the work was the basis for a paper, longer than most conference papers, presented to the American Economic Association in December 1959. This paper (Samuelson and Solow 1960) has become notorious for introducing the Phillips curve into American debates over how to control inflation. The background was that during the late 1950s there had been concern that inflation was rising despite the absence of war or other obvious cause of excessive demand. There was talk of the 'new inflation' and 'cost-push' inflation. Samuelson and Solow used the Phillips curve, calculated for the United Kingdom in an article published the previous year (Phillips 1958) as a device around which to organize their discussion of the relationship between wage inflation and unemployment. Phillips had shown that the curve appeared to have been stable for around a century, despite the dramatic changes in the economic situation during the first half of twentieth century. Solow and Samuelson calculated such a curve for the United States.

The most interesting aspect of this paper is the way it is argued. Although they calculated a curve, working out the unemployment rate consistent with stable prices, they expressed doubts as to whether there really was a Phillips relation in the United States. They proposed reasons why attempts to exploit the curve would cause it to shift. They used the curve as a way to organize their verbal argument about different causes of inflation: they described their estimates of the unemployment rate consistent with stable prices and of the inflation rate implied by three per cent unemployment as guesses (Samuelson and Solow 1960, 192). They concluded that it was not possible to settle the issue of whether inflation was cost-push or demand-pull and that in the long run the Phillips curve might shift up or down.

In 1961-62, following the election of John Kennedy as President of the United States, Solow served on the staff of the Council of Economic Advisers, chaired by Walter Heller. During the 1960s he continued to publish extensively on topics relating to technology and economic growth. However, alongside this work he began to tackle a broader range of issues, with articles on inequality and the distribution of income (Solow 1960), employment policy (Solow 1962), monetary policy (Kareken and Solow 1963), and macroeconomic policy more generally (Solow 1966).[5] In much of this work we see the second of the Solows identified by Samuelson,

[5] The publications cited here are merely examples of his work during this period.

"operating brilliantly in the rough-and-ready realm of empirical heuristics". He was increasingly focused on short-run problems as well as long-run growth. As inflation began to rise at the end of the 1960s, and following Milton Friedman's (1968) claim that the Phillips curve must be vertical if people anticipate inflation correctly, he turned his attention to the problem of inflation (Solow 1969; Gordon et al. 1970)

Although it has been cited surprisingly little, a significant paper was written with a student, Joseph Stiglitz (Solow and Stiglitz 1968). The reason for its neglect was probably that it was framed as an attempt to reconcile the neoclassical (marginal productivity) theory of wages and profits with the 'Cambridge' theories being proposed by Nicholas Kaldor, Joan Robinson, and others—a topic of little interest to most economists in the United States. It was, however, a model of short-run equilibrium that anticipated much of the literature on microfoundations of macroeconomics that emerged during the 1970s, beginning with the far more highly cited paper by Robert Barro and Herschel Grossman (1971). It was theory that took account of the possibility that markets might not always be in equilibrium between supply and demand.

A further significant paper that took Solow away from the growth theory with which his name has become associated was written with another student, Alan Blinder (Blinder and Solow 1973). This generalized the widely used IS-LM model by recognizing that government deficits needed to be financed either by increasing the money supply or by borrowing from the public, with the result that either the stock of money or the stock of government debt had to increase. In addition, investment would raise the capital stock. Their conclusion was that, provided the economy is stable when deficits are funded purely by selling government debt to the public, fiscal policy will still be effective. If equilibrium were unstable with debt financing, then fiscal policy would be worse than useless, because any deficit would drive income down towards zero.

During the 1970s and 1980s, the consensus approach to macroeconomic theorizing, insofar as it is possible to talk of a consensus, shifted to one that presumed optimizing agents and markets that cleared (markets in which supply equals demand). This took the field in a direction in which Solow did not wish to go. He sought to develop an alternative, more realistic view of the labor market. The basis for this was outlined in a lecture delivered in 1979, in which he argued that models of sticky prices were better than any alternative model of the labor market. His reasoning was that optimizing behavior could lead to unconventional results, with

even small deviations from conventional utility functions and perceived constraints (Solow 1979). He repeated this argument in his presidential address to the American Economic Association a year later, when he argued that social customs, rooted in concerns such as fairness, could reasonably explain why wages were sticky in downward direction (Solow 1980). In a paper with Frank Hahn (Hahn and Solow 1986), written near the beginning of a long research project on the subject, he returned to Keynes' notion that flexibility of the nominal wage might be harmful. Their joint work culminated in *A Critical Essay on Modern Macroeconomic Theory* (1995) in which they argued that, whatever normative properties the competitive market model might have, it did not describe real world labor markets and that this meant that, even if workers were rationally responding to the constraints they faced, nominal wages would not be completely flexible and the result would sometimes be unemployment.

SOLOW AS AN ECONOMIST

The previous two sections have done no more than scratch the surface of Solow's work; even on the topics discussed here—economic growth and the macroeconomics of the short and medium run—he wrote far more than could even be alluded to in a short essay. A major issue is that his work was not confined to theory but involved using theory to solve analytical, empirical and policy-related problems that arose during a career spanning over half a century. As significant as that, there were other fields to which he made important contributions. For example, he contributed to the emerging literature on urban and regional economics (for example, Solow 1972b; Solow 1973). However, despite these limitations, the material outlined here is sufficient to make the crucial point that Solow was much more than a growth theorist; he wrote an enormous amount on growth, technological change, and environmental issues, but this was a long way from being all he wrote about. Moreover, even within the field of economic growth, his work was far wider than just the 'Solow model'. And on top of his academic papers, he wrote substantial and perceptive book reviews on a wide range of subjects—revealing an ability to make perceptive appraisals of many economic problems on which he did not publish elsewhere and to present his discussions in a way that was accessible to readers of the *New York Times* and the *New York Review of Books*, for which he wrote frequently.

Solow's career spanned a period during which economics changed dramatically. Growing up during the Great Depression, it is not surprising

that he became interested in economics, even if it was not until after 1945 that he made the decision to specialize in it. At this time economics was being transformed by a combination of more formal and arguably more problem-focused theorizing, represented by Samuelson's *Foundations of Economic Analysis* (1947) with its emphasis on generating operational theorems, and Jan Tinbergen's econometric modeling (see, for example, Tinbergen 1939) which was about the testing and quantification of economic relationships. Much of this more technical economics—which came to be known as macroeconomics by around 1960—was shaped by the framework provided in Keynes's *General Theory of Employment, Interest and Money* (Keynes [1936] 1972). Technical economics was about model building and many of those models were Keynesian.[6]

Solow's approach to economics was widely accepted during the three decades that came to be known as the age of Keynes It was eclectic and pragmatic: he preferred small models that were tailored to throw light on some part of the macroeconomic mechanism, not comprehensive models that claimed to capture the whole of a complex system. Unlike Milton Friedman (1953), who argued that the realism of assumptions did not matter, Solow favored models that were plausible accounts of the world, which led him to look for "non-Walrasian" equilibrium concepts to understand modern industrial capitalism (Solow 2004, 165). His reasoning was that such models correspond to "our intuitions and observations". He therefore saw no inconsistency in being the author of both his growth model and his attempt, with Stiglitz, to provide a model of the short run, even though these were based on contradictory assumptions, the former assuming competitive equilibrium and the latter disequilibrium. The growth model abstracted from short run problems in order to explain long run trends; the model developed with Stiglitz was a short run model to explain movements around that trend. His time in the Council of Economic Advisers taught him one of the main uses of economists was as "intellectual sanitation workers", holding out against nonsense (Solow 1997b, 108).

Economics changed in the 1970s, when many economists rebelled against what Solow later described as models "held together by duct tape" (Solow 2004, 165). Although Solow was not personally involved in the large-scale, multi-equation modeling that were the main target of that metaphor, he was close to those, such as Franco Modigliani, a colleague

[6] I leave aside the question of whether these models were true representations of Keynes' views, which would take us too far from the subject of this note.

at MIT, and numerous students, who were.[7] Instead, the fashion became to develop models based on simple, consistent assumptions such as utility maximizing individuals and perfectly competitive markets. He believed that these models were neither plausible nor supported by evidence. These assumptions included using the Ramsey model of optimization over an infinite time horizon, an idea central to the models that dominated the field by the 1990s; Solow thought that although this provided apparent intellectual unity, like "wearing school colors or singing the Notre Dame fight song", such an objective was "hardly worthy of grown-ups" (Solow 2008, 245). This was clearly a strongly held point on which he elaborated:

> There has always been a purist streak in economics that wants everything to follow neatly from greed, rationality, and equilibrium, with no ifs, ands, or buts. Most of us have felt that tug. Here is a theory that gives you just that, and this time "everything" means everything: macro, not micro. The theory is neat, learnable, not terribly difficult, but just technical enough to feel like "science". Moreover it is practically guaranteed to give laissez-faire-type advice, which happens to fit nicely with the general turn to the political right that began in the 1970s and may or may not be coming to an end. (Solow 2008, 245)

Like those he was criticizing, Solow accepted that behavior depended on the beliefs, perceptions, and expectations of economic agents. Where he differed was in his belief that this was impossible to model rigorously because the necessary data were not available. Fudging the problem in a way that made sense for specific problems was the only answer. Thus, he described as "tendentious and misleading" the notion held by a significant number of macroeconomists, that "modern macro" had "the special virtue of following the principles of economic theory" (244). Much of it was based on Panglossian assumptions that bore little relation to the real world and found little support in the data. In short, he denied that there could be an economic theory of everything: economics could not be "a Science with a capital S", though economists should think logically and respect facts (they should be scientific with a small s) (Solow 1995, 168).

[7] This metaphor was no doubt chosen for dramatic effect, but it should be taken seriously. The rationale for such modelling and the appropriateness of the duct tape metaphor is explained in Backhouse and Cherrier (2019).

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