

MONOPOLISTIC COMPETITION AND MACROECONOMIC THEORY. BY ROBERT SOLOW. CAMBRIDGE, U.K.: CAMBRIDGE UNIVERSITY PRESS. 1998.

Robert Solow's *Monopolistic Competition and Macroeconomic Theory* is a short collection of three essays based on lectures that he gave at the University of Rome in 1996. Solow introduces the essays by asking: "How can one explain the fact that Keynesian ideas and methods have survived for sixty years despite their theoretical weaknesses and analytical crudities?" (p. 1). Good question. Solow offers two reasons for the survival of Keynesian ideas: (1) The "usual alternatives ask us to believe things that appear not to be true; and (2) Keynes's *General Theory* was premised on the assumption of perfectly competitive goods markets, and economists tend to be enamored of such models.

Reason number 1 seems farcical. It was *Keynesianism*, was it not, that argued that the state can spend us into prosperity, ignoring the concept of opportunity cost; taking a dollar from a citizen and having government spend it will magically turn it into more than a dollar (the "balanced-budget multiplier"); there is a tradeoff between inflation and unemployment; savings and capital accumulation are impoverishing because of the "paradox of thrift"; deficit spending is desirable; government debt is nothing to worry about because "we owe it to ourselves"; macroeconomic central planners will faithfully serve the "public interest" by "stabilizing" the economy and ignoring all political pressures; and countless other absurdities and falsehoods?

Solow doesn't name names when he criticizes "alternatives" (to Keynesian) theories other than to disparage "misperceptions stories." He could well be referring to the Austrian business cycle theory, however, with its emphasis on government-induced malinvestment. If so, it takes a great deal of *chutzpah* on his part to criticize this, or any, theory on the grounds that it is less realistic than Keynesianism. The author pathetically defends Keynesianism on the grounds that there are still economists who spend their time estimating "consumption functions, demand-for-money equations, and slow-price adjustment relations" (p. 3).

Solow may be on more solid footing in arguing that macroeconomists have invested a great deal of time in "modeling" goods markets as perfectly competitive and are hesitant to abandon their old ways. But he completely ignores what is perhaps the main reason why Keynesian ideas and methods have survived despite their "theoretical weaknesses and analytical crudities"—the rational self-interest of economists and policymakers.

As explained by Keynes's biographer, Roy Harrod (1951, pp. 192–93), Keynes himself believed that “the government . . . was and could continue to be in the hands of an intellectual aristocracy. . . . Keynes tended till the end to think of the really important decisions being reached by a small group of intelligent people, like the group that fashioned the Bretton Woods plan.” Arthur Smithies (1951, pp. 493–94) also discussed the authoritarian–central planning mindset of Keynes when he wrote that “Keynes hoped for a world where monetary and fiscal policy, carried out by *wise men* in authority, could ensure conditions of prosperity, equity, freedom, and possibly peace.”

Solow himself championed this view of the economist as central planner in a very influential article co-authored with Paul Samuelson in the *American Economic Review* (1960) which laid out the Phillips Curve framework of having an enlightened elite choose for the country the “appropriate” mix of inflation and unemployment. Macroeconomic stabilization policy, guided by Keynesian dogma, catapulted interventionist-minded economists into positions of power and prominence as apologists for state power. These economists, and the academic institutions that employed them, have also benefited financially from this relationship through generous government funding of their academic research. As Rothbard wrote in *The Ethics of Liberty* (1998, p. 172), the state subsists by “engineering the support of a majority . . . through securing an alliance with a group of opinion-moulding intellectuals whom it rewards with a share in its power and pelf.”

Yet another reason for the popularity of Keynesian ideas and methods that Solow completely ignores is the rational self interest of politicians. Politicians have always viewed Keynesianism as providing intellectual cover for what they always want to do anyway: spend as much of the taxpayers' money as possible on programs that benefit special-interest groups (in return for the groups' political support) while dispersing and disguising the costs with debt finance.

Solow is such a thoroughgoing interventionist that he seems incapable of acknowledging that government intervention is ever a cause of economic instability. In his introductory chapter he ponders the question of why price reduction does not always seem to occur during recessions or depressions and presents five possible reasons, based on pure speculation about the possible motives of business firms (the words “might,” “could,” and “perhaps” proliferate in his prose). One obvious reason for price inflexibility that is nowhere mentioned in Solow's book is minimum wage and other labor laws that have been designed specifically to artificially prop up compensation levels, especially in unionized industries. In this regard, Professor Solow would do well to read *Out of Work* (1993) by Richard Vedder and Lowell Gallaway, but I am not optimistic. Solow's writing is extraordinarily narrow in that his references include almost exclusively his M.I.T. colleagues and former students who all share the same overly mathematical methodology.

As with so much of mainstream economic “model building”—especially in macroeconomics—the sometimes bizarre assumptions Solow makes virtually

guarantee that the models will give him the mathematical “results” that he desires. For example, on page 44 he assumes that “nothing changes from one year to the next.” On page 34 he assumes away the price system: “There is no force that automatically works to eliminate excess supply of or excess demand for labor.” “Investment goods are produced by labor alone” and they “disappear completely after one period of use” (p. 34). As he candidly admits at the end of the introductory chapter, “the particular results depend on those assumptions” (p. 5).

The essence of Solow’s three essays is to replace the fundamentally flawed equilibrium model of perfect competition (flawed from an Austrian perspective, that is) with an equally flawed equilibrium model of “monopolistic competition” in his macroeconomic model building. He is deeply distressed over the fact that “economists have been scared away from Keynesian-sounding conclusions over the past fifteen years or so” (p. 25) and believes that incorporating long-ago discredited theories of monopolistic competition will somehow salvage Keynesian economics.

Solow’s rigid refusal to consider government intervention as the source of economic instability leads him to pose a number of questions which he believes are profound puzzles but which in reality are quite elementary, if not simplistic. “When a modern industrial economy is struck by a real disturbance, like a sudden reduction in the perceived profitability of investment or a sudden increase in the propensity to save,” he asks, “why does it take so long to return to full-employment equilibrium?” (p. 15).

Of course, a “reduction in the perceived profitability of investment” is what is caused by monetary expansion and the malinvestment that it breeds, but Solow mentions nothing of this. Such “disturbances” are simply assumed to occur, like magic, without explanation. Moreover, an “increase in the propensity to save” is nothing to be alarmed about and usually just reflects individual choices guided by rates of time preference. But Solow, like all other Keynesian central planners, believes that he knows the “correct” rate of time preference for all citizens as well as the “correct” or “optimal” savings rate and that the state should use its coercive powers to force us into this state of economic bliss.

There is a mountain of history that would help Solow understand why it sometimes takes an economy so long to “return to full employment.” I would start with Murray Rothbard’s *America’s Great Depression* (1963), which explains how, in economic crises prior to the Great Depression of the 1930s that were *not* responded to with massive government intervention, recovery was quite rapid. Government intervention—especially in labor markets—is the reason why recessions and depressions last longer than they otherwise would. Solow ignores history and pretends that such events are a Great Mystery that can only be unraveled by a garble of exquisite mathematical trivia. In a sense, mathematical economics is but a branch of hermeneutics—the “truth” is whatever these mathematical hermeneuticists say it is regardless of facts, history, and even logic.

At one point Solow seems ready to temporarily suspend his reliance on unrealistic mathematical models that admittedly explain very little about the economy and embark upon a path that will actually improve our economic understanding. Such a passage occurs on page 19, where he makes the Kirznerian statement that “it is very important to know something about the forces that lead the economy to ‘choose’ one equilibrium configuration rather than another.” But one is quickly disappointed. He says nothing about entrepreneurship—or about human action for that matter. Instead, he talks about mysterious “historical, accidental, sociological, and even psychological factors” without offering any hint as to just what these “factors” might be. Not a reference, not a footnote. Again, there is no mention at all of how government itself affects the market system other than as a benevolent tool of “stabilization policy”: “One function of economic policy may be to help or induce the economy to choose a better rather than a worse equilibrium.” This really is a naive child’s view of government.

Solow at times descends into what Mises might have called the “cloud cuckoo land” when he presents theories of how “animal spirits” supposedly guide investment behavior (“What we have . . . constructed is a theory in which animal spirits . . . drive investment,” he says on page 25). He also makes a mountain out of a mole hill in referring to a “literature” that describes markets as “thick” (there are lots of trading partners) or “thin” (there are fewer trading partners). In Solow’s theoretical world, if one wants to sell one’s used car and is not immediately deluged with hundreds of offers, then there is a “thin-market externality” that needs to be “corrected” by state intervention. No such “imperfections” are ever hinted at with regard to the behavior of politicians and bureaucrats.

Solow considers his major “contribution” in this selection of essays to be his introduction of monopolistic competition, rather than perfect competition, into a macroeconomic model. In keeping with what appears to be an M.I.T. tradition of never reading anything but pure mathematical models and studiously ignoring history, philosophy, most alternative economic theories or schools of thought, and real-world economic events, Solow proceeds as though there have never been any criticisms of monopolistic competition theory.

But, of course, there have been so many criticisms that the theory has become an historical artifact (Kirzner 1973; Rothbard 1993). Monopolistic competition is a static equilibrium model that claims to distinguish itself from the equally absurd, static equilibrium model of perfect competition. But in perfect competition there is no genuine competition, as price cutting, advertising, product differentiation, and other genuinely competitive actions are simply assumed away. It is therefore not a valid “benchmark,” regardless of how often economists may assume that it is.

Even Edward Chamberlin, the “father” of monopolistic competition theory, admitted that his theory was useless if one assumes that product differentiation—the key assumption of the theory—occurs in response to a desire on the part of businesses to cater to diverse consumer demands. Competition is a dynamic,

rivalrous, discovery procedure that helps facilitate plan coordination among market participants, and advertising and product differentiation are essential elements of that process. The “excess capacity” story that is told by monopolistic competition theorists is contrived nonsense, but is nevertheless the keystone of Solow’s model.

Just how absurd it is to label product differentiation as “monopolistic” is amplified by the development in recent years of what the management literature refers to as “mass customization” (Pine 1993). Thanks to computer technology, manufacturers have been able to create an ever-increasing variety of goods and services tailored to the individual preferences of consumers. Just since the early 1970s the number of new car models has expanded from 140 to 260; soft drinks from 20 to 87; and television channels from 5 to 185. There are now 340 kinds of breakfast cereals, 3,000 different beers, and even 50 brands of bottled water.

All of this (and much more) has resulted from the combination of a fiercely competitive global marketplace and the application of information technology to manufacturing. Economists like Solow who still insist that product differentiation is a sign of “monopolistic” behavior are mired in an interventionist mindset that is not even remotely in touch with reality.

Murray Rothbard wrote in *Man, Economy, and State* (1993, p. 643) that “we must never let reality be falsified in order to fit the niceties of mathematics” but, of course, that is exactly what Solow and most other mathematical economists do. It is the coin of their theoretical realm. In Solow’s case, he ignores or falsifies reality so that his contrived model can help him arrive at the conclusion that central bank monetary expansion will have nothing but benign effects. “If the money supply is increased,” he writes, “the output of consumption goods increases more than proportionally, . . . output per worker increases, and so does the real wage” (p. 49). Can the cure for the common cold be far behind?

Equally contrived is the model discussed in chapter 3 which concludes, in complete contradiction to sound economic logic, that “wage cutting in the presence of unemployment would be a step in the wrong direction” if one wishes to increase employment (p. 59). No evidence is offered in support of this conclusion, only the gobbledygook of mathematical economics. The model is said to be “a version of the Hotelling–Lancaster–Salop model of monopolistic competition with a circular product space, and the equilibrium concept is symmetric Chamberlinian” (p. 60). (Attention graduate students: Talk like this in job interviews and you could land a faculty spot at M.I.T.!).

Solow seems to have no conception of human action as a process of plan coordination, although he uses Austrian-sounding language at one point in discussing “coordination failure” in the marketplace. He sees the job of the economist as the construction of obtuse mathematical theories to ostensibly explain this alleged “failure,” but not to inquire how market participants act to overcome coordination problems. He doesn’t appear to be the least bit interested in how

markets actually work; only to “model” them as inherently “flawed” in order to stroke his own ideological predilections.

It speaks ill of the economics profession that one of its gray eminences, Robert Solow, who has been “building models” for some fifty years, offers a macroeconomic model that fails to incorporate a discussion of asset and labor markets. Deep down, I suspect that he must recognize the absurdity of it all. On page 35 he confesses: “I can talk about unemployment in this model, but I have not earned the right to talk about it seriously.”

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