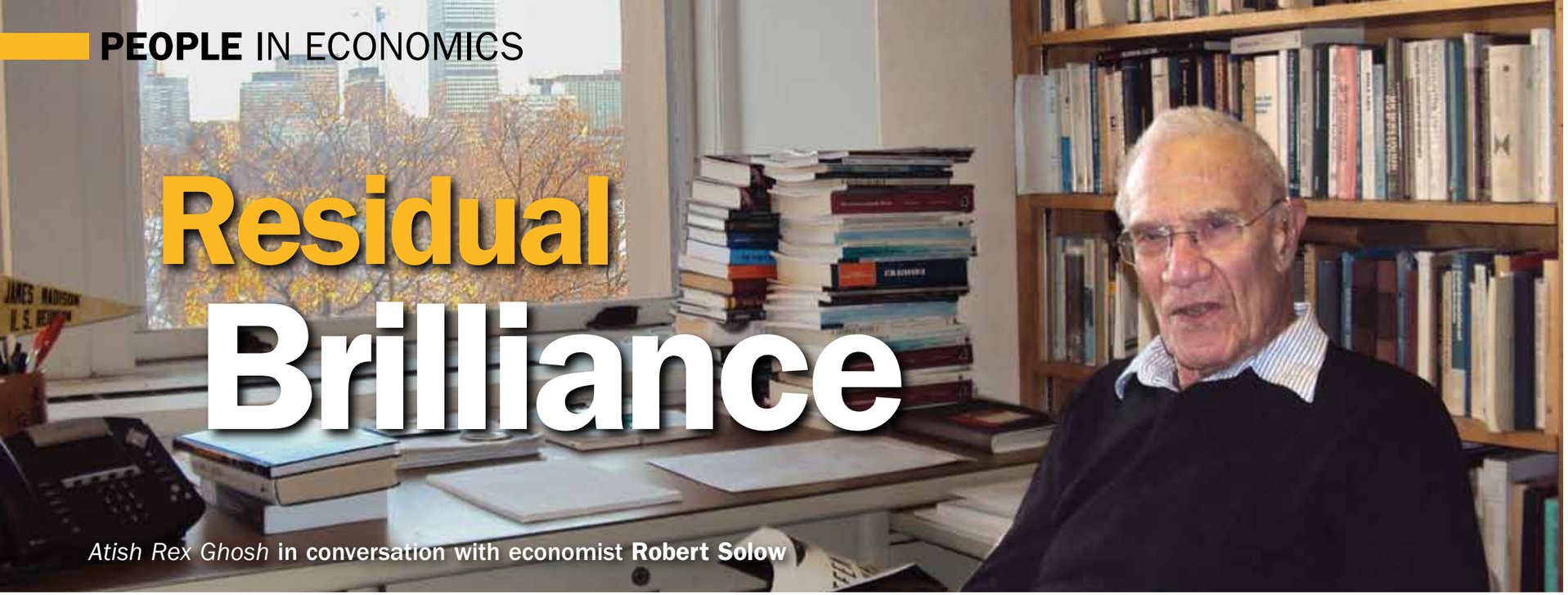
Residual Brilliance



Atish Rex Ghosh **in conversation with economist Robert Solow**

HE doesn't use e-mail—yet his name is inextricably linked with technological progress. An avid sailor who never strays far from shore, Robert Solow is one of the most adventurous minds in economics, but worked in the same university office overlooking Boston's Charles River for more than half a century.

A self-styled solver of puzzles, who eschews grandiose ideas, Solow developed a landmark model that fundamentally changed research on how economies develop and grow. Now Professor Emeritus at the Massachusetts Institute of Technology (MIT), Solow won the Nobel Prize in economics in 1987 for his seminal contributions to growth theory.

"Here is a scholar whose work has left an indelible imprint on his discipline," said Princeton professor Alan Blinder. "Not just a model, mind you, but even a residual bears his name!" (Blinder, 1989).

Child of the Depression

We meet on one of those beautiful, crisp, sunny New England days that are the last gasp of fall before winter sets in. He is a lanky man, with a warm smile. Solow's room in the MIT economics department also has a view of the Boston skyline; it's an office he had occupied for the better part of 60 years, and that he relinquished a few weeks later. "This is the only full-time academic job I've ever had. So I'm not a bird of passage; I settled here."

As an assistant professor he would never have merited such a magnificent office, he hastens to inform me, but when the economics department moved into its new building in 1952, Solow, who had been on the faculty for only a couple of years, was already a close friend and colleague of the late Paul Samuelson, one of the most important economic theoreticians of the 20th century. It was understood that he had to have the office next to Samuelson—who, of course, had to have the best office in the department.

Born in New York in 1924, Solow has lived through both the Great Depression and the Great Recession. The son of a furrier who traded with the Soviet Union, he grew up in Brooklyn. The events of the Depression left an indelible imprint on the minds of many future pioneers in economics, and Solow was no exception. "I was very much aware, even as a kid, that something bad had happened and that it was called the Depression. And it meant that there were a lot of people out of work and a lot of people were poor and hungry, and that stuck with me. It was an important thing in my life and probably has a lot to do with attitudes I have, even now."

After his arrival on a scholarship to Harvard at the age of 16, his interest in the underlying factors behind social upheaval led him to study sociology and anthropology, together with some elementary economics (and some not-so-elementary economic tomes, such as in Wassily Leontief's just-published *Structure of the American Economy*). But the attack on Pearl Harbor in December 1941 prompted him to drop his studies and sign up immediately as a private in the U.S. Army. Had he waited to graduate, he could have enlisted as an officer, but "defeating Nazism was simply the most important thing to do at that time," he said. He joined a signals intelligence unit (he knew both Morse code and German) and saw active duty in North Africa and Italy.

As soon as he got back home, he married his sweetheart, economic historian Barbara Lewis, to whom Solow has been married for more than 65 years.

On his return to Harvard in 1945, Solow decided—at Lewis's suggestion—to study economics, becoming Leontief's pupil, research assistant, and, eventually, lifelong friend. He credits Leontief with his transformation from graduate student to professional economist. As his tutor, Leontief would assign Solow a paper to read each week for discussion during their next meeting.

In those days, economics was not very mathematical, and Solow lacked college-level mathematics, but he got sick of being given only nontechnical papers—one can hear the indignation and determination in his voice: "I wasn't going to allow *that* to happen, read the second-rate papers because I couldn't read the first-rate articles." So he enrolled in the necessary mathematics courses in calculus and linear algebra.

It was a fortuitous decision. Not only did it earn him an assistant professorship at MIT (to teach probability and statistics), it also meant that Solow was able to speak the same language as Samuelson and to keep up with him intellectually—a feat he likens to "running as hard as you can, all the time." Samuelson, in turn, described Solow as the "consummate economist's economist."

They were colleagues and friends for the next 60 years, and whenever Solow was offered a position at another university, he would stipulate that he would move only if Samuelson's office were moved alongside his. This never quite worked out, and was one of the reasons both men ended up spending their careers at MIT.

Reconstruction and decolonization

Post–World War II reconstruction in industrialized countries and economic development in newly independent colonies meant that growth theory was *the* topic for economists in the 1950s. Before Solow's contribution, the field did exist, but it was a somber one. Seminal papers by Roy Harrod in 1939 and Evsey Domar from 1946 onward had postulated that steady long-run growth was a possible but an exceedingly unlikely outcome that teetered on a knife edge in the standard macroeconomic models of the time. For steady growth to prevail, the economy's saving rate had to match exactly the product of the capital output ratio and the rate of growth of the labor force.

But in the Harrod-Domar growth model, these three variables—the saving rate, the capital-output ratio, and labor force growth—were fixed and exogenous—given by assumptions on preferences, technology, and demographics, respectively. There was no reason for the required equality to hold, and if it did not, the model predicted that the economy would be subject to ever-increasing fluctuations.

Solow came into this debate with two valuable insights. First, despite the 1890s recession, Great Depression, and World War II, Solow thought it was historically untenable that the main characteristic of capitalist economies should be explosive volatility (either growing without bound or shrinking out of existence) rather than stable growth (with occasional crises). Nor did he accept predictions that a higher saving rate would lead to increased long-run growth.

Second, of the outside influences of the Harrod-Domar model, Solow's attention was naturally drawn to his research specialty: the production side. This choice made his reputation. In his 1956 "A Contribution to the Theory of Economic Growth," Solow showed that relaxing the production technology to allow a flexible capital-output ratio made steady-state growth not only possible, but a natural outcome. Growth theory could rid itself of reliance on finely balanced configurations. And as all students of economics now know, the long-run growth rate in Solow's model is independent of the saving rate.

He did not stop there. Not satisfied with the prospect of much spilling of ink by growth theorists following his 1956 article, Solow further shook up empiricists with his "Technical Change and the Aggregate Production Function" in 1957. He used his theoretical model to decompose the sources of growth among capital, labor, and technological progress. And he showed that technological change, rather than capital accumulation, was the main driver of long-run growth. This "technical change residual"—so called because it is the part of growth that cannot be explained by identifiable factors such as capital accumulation or labor force growth—would forever bear his name.

The Solow residual

Ironically, Solow himself was surprised by the size of the residual and its importance in accounting for growth, even though a central prediction of his model is that long-run growth can come only from technological progress. His next major paper—on embodied technology—was an attempt to accord capital accumulation a larger role in long-run growth.

Solow's work has strongly influenced governments' policies to augment funding for technological research and development to spur economic growth (see box).

Solow initially thought of his model exclusively in terms of advanced economies like the United States. Later, however, he came to believe it also applied to developing countries, provided the institutional prerequisites are in place. (He attributes China's spectacular growth to the country's very high

investment rates and the government's determination to get the economy on the technological frontier.)

Regardless, he readily acknowledges his intellectual debt to Arthur Lewis's work on growth in labor-surplus countries. He is also quick to give credit to Trevor Swan, who independently arrived at much the same model at almost exactly the same time, but never received as much recognition as Solow for it. The reasons for this are not clear, though Solow says he had "a slightly better mousetrap."

In a 2007 paper, Solow speculates as to why his work attracted more attention. First, Swan presented his model in terms of a specific (the Cobb-Douglas) production function (and only in a posthumously published paper did it become clear he was aware of the more general case all along). This was a case where Solow's more general assumption turned out to be simpler and more transparent. Second, Swan's model, which included an important appendix ("Notes on Capital"), was perceived to be a response to the likes of Joan Robinson and Piero Sraffa, mired in the "Cambridge capital controversy" (a technical and mathematical dispute over how to account for capital in economic models), and therefore lost attention as those controversies lost the profession's interest. And third, Solow was an American publishing in the *Quarterly Journal of Economics*, Swan an Australian publishing 10 months later in the less widely read *Economic Record*. What is clear is that, over the years, Solow has made what Barbara Spencer (Trevor Swan's daughter, and a well-known trade economist) terms "generous efforts" to ensure that Swan's work was not overlooked.

Solow's talent was recognized early. He received the John Bates Clark Award, given by the American Economic Association to the best economists under age 40. He also served on the staff of President John F. Kennedy's Council of Economic Advisers during the 1960s and later was president of the American Economic Association in 1979.

Involving private sector research

Solow believes there should be much greater interaction between cloistered university economists and those working in private sector research laboratories.

His suggestion to economists modeling technological progress is to spend some time in research laboratories to better appreciate the randomness of scientific progress and the interplay between the creative process and the incentives of profit-making firms. Solow should know: he served for eight years on the Science Advisory Committee of General Motors, where the research laboratories are "the size of a small university."

"I'm convinced that the problem is there will always remain for economics an exogenous element in technological progress because there is an exogenous element in science. Any scientist or analytical engineer will tell you that when you work on something, you often end up solving a problem different from the one you thought you were working on. And so, from the point of view of economics, what comes out of science and engineering is exogenous. And there will always be that element, but the endogenous growth literature just doesn't seem to me to be capturing that."

Irresistible combination

The combination of the empirical success and analytical simplicity of Solow's model proved irresistible to economists in a variety of fields looking for a workhorse model, but Solow often disapproved of the way his model was used. And it did not take long for economists working in separate subdisciplines to adopt the model to their own purposes, ranging from explanations of entrepreneurship and business cycles to improvements in product variety and innovation.

Solow emerged as a robust critic of the burgeoning field of *real business cycle theory*, which placed his own model at the heart of an explanation of short-run macroeconomic fluctuations that said recessions were efficient market behavior and were not the result of some market failures. On theories of unemployment, he has argued that labor market failures should be a central component of business cycle analysis, rather than assumed away.

More recently, as most real business cycle theorist do, Solow has welcomed the development of New Keynesian approaches to macroeconomics. In particular, he has held out hope that the introduction of "sticky" (or slow to adjust) prices, monopolistic competition, and other market imperfections into macroeconomic theory would at last help provide a sounder foundation for short-run analysis.

Success in economics is not without its fair share of ironies. Just as Solow was reluctant to project his experience of the Depression and the Second World War directly into an explosive theory of long-run growth, he never thought his growth model adequately depicted short-run fluctuations. Indeed, his 1956 article goes out of its way to emphasize that his model was one of long-run growth, not business cycle movements. However, in the 1960s and 1970s, Solow worked on aspects of business cycle theory with an array of economists, such as Joseph Stiglitz and Blinder, who would make their own names in the field. He related the short-run behavior of the economy to stickiness in prices and wages, especially the downward rigidity of wages, and he defended Keynesian predictions for the effectiveness of fiscal policy against the monetarists' claims that government borrowing would crowd out private sector borrowing. In the process, he emerged as a witty critic of economists who advocated for extreme government intervention in the economy, or none at all. "Everything reminds Milton Friedman of the money supply," he once quipped; "everything reminds me of sex, but I try to keep it out of my papers."

Resurgence in growth theory

As Solow embarked on his journey to Stockholm at the behest of the Royal Swedish Academy of Sciences to receive the 1987 Prize in Economic Sciences in Memory of Alfred Nobel (the formal name for the economics award), a resurgence in growth theory was under way. Among others, Paul Romer and Robert Lucas declared their dissatisfaction with allowing the long-run steady-state growth rate to be determined only by an external "technological process." Solow agrees wholeheartedly. His own theoretical and empirical work had shown the importance of technological progress in accounting for growth—now the profession sought a deeper understanding of what drives that progress, and hence what drives growth.

The explosion of papers that followed proposed theories along three different lines. Some of the earliest contributions, such as Romer's first paper, proposed that steady-state growth was possible even in the absence of technological progress, as long as capital did not have diminishing marginal returns. A second strand of papers added extra accumulable factors such as human capital. The final category of papers decided to model explicitly the process of technological innovation; Solow thinks of this as the most interesting strand, though he also thinks that economists have a lot to learn about how scientific and technological innovation actually comes about. Innovation to produce new varieties of products, or higher-quality products, was modeled as an active business decision of firms. Government policies on capital accumulation, and incentives for research and development, could now, at least in theory, affect the economy's long-run growth rate.

Such results in the new field of *endogenous growth theory,* as it came to be called, were attractive to economists and policymakers, to the extent that in 1994 even Gordon Brown, who went on to become the British Chancellor of the Exchequer and then Prime Minister, could not resist referring to the theory as a cornerstone of his proposed agenda. Although Solow believes this is the most promising avenue for explaining long-run growth, he also considers models that treat technical innovation as simply another product—the mechanical output of a production function—are hopelessly unrealistic.

Learning from the crisis

So where did that leave the state of macroeconomics on the eve of the 2008 global financial crisis? Too much the prisoner of its own (representative agent, real business cycle, frictionless equilibrium) models, in Solow's view. Not that Solow would blame the crisis itself on whether economists were using quite the right models; rather, the crisis resulted from the belief that "if the market for orange marmalade is self regulating, the market for fixed-income securities must also be self regulating." Economists, he says, played a role in furthering that belief, but even without such endorsement, too many people made too much money from that premise for it not to have taken hold anyway.

Two lessons Solow would like to see economists take to heart are that, in the modern world, it is impossible to pursue macroeconomics without taking account of finance; and second, financial markets are not necessarily stable or self-correcting. "You know, I'm getting old. I don't have very long to wait. But I'd like to see the macroeconomics profession learn from the crisis. You're supposed to learn from observation, and big deviant observations should teach you more than little teeny-tiny deviations, and there's not much sign of that."

Power of groups

Armed with a biting wit, Solow says he tries not to take himself too seriously. When asked to contribute to a book on the life philosophies of famous economists, he wrote an essay "on coping" and believes more in the value of group or team accomplishments than individual achievement.

In his own case, he recalls, he was lucky to be part of such a group when he was in the army; when he was in the MIT economics department; and when he worked for the Council of Economic Advisers from 1961 to 1963 with the likes of Walter Heller, Arthur Okun, and Kenneth Arrow. "I think it's really important," says Solow, "if you want to make intellectual progress, to create nice communities that work well together. It's a process. You succeed, your morale is good; you succeed more."

"I think that the most important thing in intellectual success is being part of a high-morale group."

The light is fading, and I take some photos of Solow in his book-lined office. I do not ask him whether he is sad to be vacating it, but I think not. Solow projects a sense of contentment—as though he is confident he has given the profession a running start and is ready now to pass the baton to the next generation.

While we pack up, I ask if he has any last reflections. "Yes," he says. "It's one of the lessons of my work and life. I think that the most important thing in intellectual success is being part of a high-morale group. I think that progress comes from intellectual communities, not from individuals generally. That's what's wrong with Nobel Prizes and all that."

His final words reflect his insistence, recurrent throughout our talk, on crediting others with contributing to his successes. And as he walks down the steps of the MIT economics building toward his wife, I am struck that someone who has accomplished so much in—and for—his profession should be so unassuming. A modest man, who has little to be modest about. ■

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