



Robert M. Solow

The Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel 1987

Autobiography



I was born in Brooklyn, New York on August 23, 1924, the oldest of three children. My parents were themselves the children of immigrants. They had to earn a living as soon as they finished secondary school. So my sisters, my cousins and I were the first generation of our family to attend a university. I was educated, and well educated, in the neighborhood public schools of New York City. I was good at school from the very beginning, but not very intellectual until my last year in high school. Then one of those teachers who make a difference taught me to read the great 19th century French and Russian novelists, and to take ideas seriously. I won a scholarship to Harvard College and arrived there in September 1940.

Like many children of the Depression, I was curious about what made society tick. My first studies were in sociology (with Talcott Parsons) and anthropology (with Clyde Kluckhohn) as well as elementary economics. By the end of 1942, when I had turned 18, it seemed that there were more urgent and exciting matters than what I was doing, so I left the university and joined the U.S. Army. I served briefly in North Africa and Sicily, and then from the beginning to the end of the war in Italy, until I was discharged in August 1945.




I think that those three years as a soldier formed my character. I found myself part of a tight-knit group, doing a hard job with skill and mutual loyalty, led by one of the most remarkable men I have ever known, who never wavered from the path of humor and decency. Twice again I have had similar experiences: in Walter Heller's Council of Economic Advisers (along with James Tobin, Kermit Gordon and Arthur Okun) and, for most of my adult life, in the Economics Department at M.I.T. Day in and day out, that is the best sort of environment. The only other thing one could ask for is a warm and happy family life, and that I have certainly had.

Upon returning to Harvard in 1945, now married to the reader and writer of all those V-letters, I chose, almost casually, to go on with economics. By a piece of good luck, Wassily Leontief became my teacher, guide and friend. I learned from him the spirit as well as the substance of modern economic theory. He was also responsible for my introduction to empirical work: as his research assistant I produced the first set of capital-coefficients for the input-output model.



Somehow - the memory is lost - I became interested in statistics and probabilistic models. In those days, the teaching of statistics at Harvard was, to put it kindly, eccentric. I learned a lot from Frederick Mosteller, whose appointment was in the Department of Social Relations. Eventually he advised me to study more intensively in a place where that was possible. So, in 1949-50, I spent a fellowship year at Columbia University, in the lectures of Abraham Wald, Jacob Wolfowitz and T.W. Anderson, along with my fellow student and friend, Jack Kiefer. During that year I was also working on my Ph.D. thesis, an exploratory attempt to model changes in the size distribution of wage income using interacting Markoff processes for employment-unemployment and wage rates. The thesis was awarded the Wells Prize at Harvard, which offered publication in book form and \$500 (in 1951 prices!) upon completion. When I reread the thesis, however, I thought that I could do it better. But I never returned to that work and the thesis remains unpublished (and the check uncashed).

Just before going off to Columbia I was offered and accepted an Assistant Professorship in the Economics Department at M.I.T. I have never had or wanted any other job. M.I.T. hired me primarily to teach courses in statistics and econometrics. In the beginning I fully intended to make my career along those lines. It did not turn out that way, probably for a geographical reason. I was given the office next to Paul Samuelson's. Thus began what is now almost 40 years of almost daily conversations about economics, politics, our children, cabbages and kings. That has been an immeasurably important part of my professional life. I suppose it was inevitable that I should drift back into "straight" economics, where I discovered an instinctive macroeconomist struggling to get out.

The M.I.T. Economics Department has been a wonderful place to teach and work. It has

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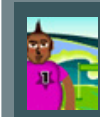
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Robert M. Solow

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provided me - and not only me - with sharp and delightful colleagues, and with a long line of spectacular students. I estimate that if I had neglected the students, I could have written 25 percent more scientific papers. The choice was easy to make and I do not regret it.

From *Les Prix Nobel. The Nobel Prizes 1987*, Editor Wilhelm Odelberg, [Nobel Foundation], Stockholm, 1988

This autobiography/biography was written at the time of the award and later published in the book series *Les Prix Nobel/Nobel Lectures*. The information is sometimes updated with an addendum submitted by the Laureate. To cite this document, always state the source as shown above.

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Addendum, May 2005

The original autobiography left me teaching at MIT in 1987. I retired in 1995, although Samuelson and I still occupy neighboring offices there, and still compare notes. By 1995 there was no compulsory retirement age, but I thought the Department would be better off with another young teacher and researcher.

During the past decade I have continued to work – though inevitably more slowly and less energetically – on targets of opportunity, especially a long collaboration with Frank Hahn on macro-theory. We found ourselves disaffected – to put it mildly – with the assumptions and methods that had become standard in short-to-medium-run macroeconomics. Most of all we rejected the representative-agent models that more or less impose optimal properties on observed trajectories. Our own try at proposing alternative models has not attracted attention, possibly because it tends to be opaque and messy.

Thirty years ago I was one of the founding directors of the Manpower Demonstration Research Corporation, a non-profit research group that has done pioneer work in the rigorous experimental testing of policy interventions that aim at improving the employment and earning power of disadvantaged groups like high-school drop-outs, welfare mothers, etc. The organization has flourished, both intellectually and practically, as the prime source of validated information about the actual effectiveness of such policies. I am currently chairman of its Board of Directors.

I have also participated in several studies conducted by the McKinsey Global Institute aimed at explaining the sometimes large international differences in performance of certain industries. The idea was to combine the use of the usual data with information gleaned from acquaintance with individual firms in the industry. Martin Baily and I have written an article summarizing this work.

More recently I have succeeded the great sociologist Robert K. Merton as Foundation Fellow of the Russell Sage Foundation. My first project there was an attempt to account for the extraordinary success of the U.S. economy during the years 1995-2000. This resulted in a book (*The Roaring Nineties*) edited and introduced by Alan Krueger and me. My current activity is as one of the originators, planners and advisors of an absorbing comparative study of the nature and institutional background of low-wage work in the U.S. and a few European countries, and their differing labor-market outcomes. It is too soon to know what generalizations will emerge.

One of the by-products of involvement in these projects is a reinforced skepticism about the way that labor and product markets are handled by the currently fashionable style in the macroeconomics of the short and medium run. The traditional assumptions are sometimes useful, or even necessary, but have to be treated very gingerly and not simply given the benefit of the doubt. The long-run growth-theory enterprise strikes me as being in better shape. It went through a phase of uncritical acceptance of "easy" routes to endogeneity, but may be coming back to a more historical view. There seem to be many opportunities for further progress.

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