Lives of the Laureates, Seven Nobel Economists
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Beginning with Keynes at Harvard

Rare is the child, I suspect, who wants to grow up to be an economist, or a professor. I grew up in a university town and went to a university-run high school, where most of my friends were faculty kids. I was so unfailing an A student that it was boring even to me. But I don't recall thinking of an academic career. I liked journalism, my father's occupation; I had put out "newspapers" of my own from age six. I thought of law; I loved to argue, and beginning in my teens I was fascinated by politics. I guess I knew that there was economics at the university, but I didn't know what the subject really was. Of course, economic issues were always coming up in classes on history and government — civics, in those days. I expected economics to be among die social science courses I would someday take in college, probably part of the pre-law curriculum.

I grew up happily assuming I would go to college in my hometown, to the University of Illinois. One month before I was scheduled to enroll as a freshman, I was offered and accepted a Conant Prize Fellowship at Harvard. I should explain how this happened. My father, a learned man, a voracious reader, the biggest customer of the Champaign Public Library, discovered in *the New York Times* that Harvard was offering two of these new fellowships in each of five midwestern states. President Conant wanted to broaden the geographical and social base of Harvard College. Having nothing to lose, I accepted my father's suggestion that I apply. University High School, it turned out, had without even trying prepared me superbly for the obligatory College Board exams. Uni High graduates only thirty to thirty-five persons a year, but it has three Nobels to its credit and, once I had broken the ice, many national scholarships.

Thus James Bryant Conant, Louis Michael Tobin, and University High School changed my life and career. Illinois was and is a great university. But I doubt that it would have led me into economics. For several reasons, Harvard did.

Harvard was the leading academic center of economics in North America at the time; only Columbia and Chicago were close competitors. Both its senior and junior faculty were outstanding. Two of the previous lecturers in this series were active and influential members of the community when I was a student, Wassily Leontief on the faculty and Paul Samuelson as a junior Fellow, a graduate student free of formal academic requirements. Of the senior faculty of the 1930s, Joseph Schumpeter would have been a sure bet for a Nobel, Alvin Hansen, Edward Chamberlin, and Gottfried Haberler likely choices. Haberler, still active, remains a possibility. Naturally, Harvard attracted remarkably talented graduate students. That able undergraduates might go on to scholarly careers was taken for granted.

When I arrived at Harvard, I knew I would want to major — at Harvard the word is *concentrate* — in one of the social sciences or possibly in mathematics. By the end of freshman year I was leaning to economics. But I hadn't yet taken any. In those days even Ec A, the introductory course, was considered too hard for freshmen. As a sophomore all of eighteen years old, I began Ec A in a section taught by Spencer Pollard, an advanced graduate student specializing in labor economics and writing a dissertation on John L. Lewis and the United Mine Workers.

Pollard was also my tutor. A Harvard undergraduate, besides taking four courses, met regularly, usually singly, with a tutor in his field of concentration, generally a faculty member or graduate student associated with the student's residential house. Tutorial was not graded. It was modeled, like the house system itself, on Oxford and Cambridge. Pollard suggested that we devote our sessions to "this new book from England." He had recently been over there and judged from the stir the book was creating even before publication that it was important. The book was *The General Theory of Employment, Interest and Money* by John Maynard Keynes, published in 1936.

Pollard was no respecter of academic conventions; that I was only an Ec A student meant nothing to him. I was too young, and too ready to assume that teacher knows best, to know that I knew too little to read the book. So I read it, and Pollard and I talked about it as we went through it. Cutting my teeth on *The General Theory,* I was hooked on economics.

Like many other economists of my vintage, I was attracted to the field for two reasons. One was that economic theory is a fascinating intellectual challenge, on the order of mathematics or chess. I liked analytics and logical argument. I thought algebra was the most eye-opening school experience between the three Rs and college.

The other reason was the obvious relevance of economics to understanding and perhaps overcoming the great depression and all the frightening political developments associated with it throughout the world. I did not personally suffer deprivations during the depression. But my parents made me very conscious of the political and economic problems of the times. My father was a well-informed and thoughtful political liberal. My mother was a social worker, recalled to her career by the emergency; she was dealing with cases of unemployment and poverty every day.

The second motivation, I observe, gave our generation of economists different interests arid priorities from subsequent cohorts dominated by those attracted to the subject more exclusively by the appeal of its puzzles to their quantitative aptitudes and interests.

Thanks to Keynes, economics offered me the best of both worlds. I was fascinated by his theoretical duel with the orthodox classical economists. Keynes's uprising against encrusted error was an appealing crusade for youth. The truth would make us free, and fully employed too. I was already an ardent and uncritical New Dealer, much concerned about the depression, unemployment, and poverty. According to Keynesian theory, Roosevelt's devaluation of the dollar and deficit spending were sound economics after all.

By sheer application, unconstrained by the need to unlearn anything, I came to know Keynes's new book sooner and better than many of my elders at Harvard. Keynes was the founder of what later came to be known as macroeconomics, what his young associate Joan Robinson called at the time "the theory of output as a whole," a phrase I found strikingly apt. The contrast was with the theory of output and price in particular markets or sectors. This — what we now call "micro" — was the main stuff of the theory course we economics concentrators took after Ec A. I liked the methodology of the new subject, modeling the whole economy by a system of simultaneous equations; by now I had calculus to add to my algebra. J.R. Hicks and others showed, more clearly than Keynes himself, how the essentials of *The General Theory*, and its differences from classical theory, could be expressed and analyzed in such models.

Harvard was becoming the beachhead for the Keynesian invasion of the new world. The senior faculty was mostly hostile. A group of them had not long before published a book quite critical of Roosevelt's recovery program. Seymour Harris, an early convert to Keynes, was an exception, especially important to undergraduates like myself, in whom he took a paternal interest. Harris was an academic entrepreneur. He opened the pages of *the Review of Economics and Statistics*, of which he was editor, and the halls of Dunster House, of which he was senior tutor, to lively debates on economic theory and policy.

The younger faculty and the graduate student teaching fellows were enthusiastic about Keynes's book. Their reasons were similar to my own but better informed. A popular tract by seven of them, *An Economic Program for American Democracy,* preached the new gospel with a left-wing slant.

Most important of all was the arrival of Alvin Hansen to fill the new Littauer chair in political economy. Hansen, aged fifty, came to Harvard from the University of Minnesota the same year I was beginning economics. He had previously been critical of Keynes and had indeed published a lukewarm review of *The General Theory*. He changed his mind 180 degrees, a rare event for scholars of any age, especially if their previous views are in print. Hansen became the leading apostle of Keynesian theory and policy in America. His fiscal policy seminar was the focus of research, theoretical and applied, in Keynesian economics. Visitors from the Washington firing lines mixed with local students and faculty; I had the feeling that history was being made in that room. For undergraduates the immediate payoff was that Hansen taught us macroeconomics, though under the course rubric Money and Banking. Hansen was a true hero to me, and in later years he was to be a real friend also.

I wrote my senior honors thesis on what I perceived to be the central theoretical issue between Keynes and the classical economists he was attacking. The orthodox position was that prices move to clear markets, rising to eliminate excesses of demand over supply and falling to eliminate excess supplies. Applied to the labor market, this meant that reductions of wages would get rid of unemployment. Excess supply of labor could not be a permanent equilibrium. Unless wage cuts are prevented by law or by monopolistic trade unions, competition for jobs will lower wages and in turn restore or create jobs for the unemployed. This was just an application of the central thesis of orthodox economics, the Invisible Hand proposition of Adam Smith. Individual agents are selfish and myopic. They respond in their own interest to the market signals locally available to them. Their actions miraculously turn out for the best for the society as a whole. Competition brings this miracle about.

Keynes's heresy was to deny that this mechanism could be counted on to eliminate involuntary unemployment. He didn't say just that the mechanism was slow and needed help from government policy. He said it might not work at all. Instead, the economy would be stuck in an underemployment equilibrium. Orthodox economists thought they could prove that free competitive markets allocate resources efficiently. In saying that willing and productive workers can’t get jobs, Keynes was indicting the market system for a massive failure. After all, there is no greater inefficiency than to leave productive resources idle.

My honors thesis found fault with Keynes’s logic. That may seem surprising. But I didn’t think Keynes needed to insist on so sweeping a theoretical victory on his opponents’ home court. His practical message was just as important whether unemployment was an incident of prolonged disequilibrium or of equilibrium. My first professional publication (1941) was an article in the *Quarterly Journal of Economics* based on my senior thesis; the *QJE* is, of course, edited and published at Harvard. The issue is very much alive today. It has also remained an interest of mine, a subject on which I have published several other papers, including my 1971 presidential address to the American Economic Association (1972).

Tools of the Trade, Theoretical and Statistical

By graduation time in 1939 I had forgotten about law and drifted into the natural decision, to become a professional economist. Harvard has a way of keeping its own: my fellowship was extended and I went on to graduate school. The transition was easy; I had taken courses with graduate students while I was a senior. Now I needed to pick up some tools of the trade. One was formal mathematical economic theory, and another was statistics and econometrics. Harvard was just beginning to catch up to the state of these two arts.

I see in retrospect that our professors left most of our education to us. They expected us to teach ourselves and learn from each other, and we did. They treated us as adult partners in scholarly endeavor, not as apprentices. I am afraid our graduate programs today try too hard to convey a definite and vast body of material and to test how well students master what we know. I wrote my undergraduate thesis under the nominal supervision of my senior-year tutor, Professor Edward Chamberlin. He said he knew nothing about my subject and left me on my own. Our tutorial sessions were nonetheless interesting; we argued about Catholic agrarianism, his vision of economic utopia. The faculty adviser for my doctoral dissertation in 1946–47 was, by my choice, Professor Schumpeter, one of the truly great economists, indeed social scientists, of the century. He had no use for Keynes and little for my topic, the consumption function. He read what I wrote and made helpful suggestions, but mostly he kept hands off. When I saw him, we talked of many other things, to my lasting benefit.

The theory we were taught was largely in the Anglo-American tradition, in which mathematical argument was subordinated to verbal and graphical exposition and relegated to footnotes. The great book was *Principles of Economics* by Alfred Marshall, Keynes’s own mentor in the other Cambridge. Markets were analyzed mostly one at a time — *partial* equilibrium analysis. Little rigorous attempt was made to describe a *general* equilibrium of the system as a whole, with many commodities, many consumers and producers, many markets interconnected with each other.

Mathematical models of general equilibrium were a stronger tradition in continental Europe, to which the French-Swiss economist Leon Walras had made the seminal contribution in 1870. Though F.Y. Edgeworth at Oxford and Irving Fisher at Yale had written in the same vein, they had not greatly influenced the main line of English-language economics from Adam Smith to David Ricardo to John Stuart Mill to Marshall. But in the late 1930s and 1940s the mathematical general equilibrium approach was coming into vogue, thanks to J.R. Hicks and R.G.D. Allen in Britain and Wassily Leontief and Paul Samuelson at Harvard. Joseph Schumpeter fostered this development, believing that Walras had provided economics its "magna charta," even though his own theory of the dynamics of capitalism was wholly different.

I liked the general equilibrium approach; that was one of the great appeals of macroeconomics. But those models of output as a whole were small enough and specific enough to understand and manipulate. I have never been an aficionado of formal mathematical general equilibrium theory, which is so pure and general as to be virtually devoid of interesting operational conclusions. Moreover, I have come to think that its elegance gives many economic theorists today an exaggerated presumptive faith that free competitive markets work for the best. I did use the approach in some articles in the late 1940s on the theory of rationing, all but one of them in collaboration with Hendrik Houthakker.

In statistics and econometrics Harvard was further behind the times. The professors who taught economic statistics were idiosyncratic in the methods they used and quite suspicious of methods based in mathematical statistical theory. Until the 1950s Harvard was pretty much untouched by the developments in Europe led by Ragnar Frisch and Jan Tinbergen or those in the United States at the Cowles Commission. Students like me, who were interested in formal statistical theory, took refuge in the mathematics department. For econometrics we squeezed as much as possible from a seminar on statistical demand functions offered by a European visitor, Hans Staehle. We also discovered that regressions, though scorned by professors Crum and Frickey, were alive and well under the aegis of Professor John D. Black's program in agricultural economics. In the basement of Littauer Center we could use his electromechanical or manual Marchands and Monroes.

I did just that for my second published paper (1942), originally written for Edward S. Mason's seminar in spring semester 1941, on how to use statistical forecasts in defense planning; my example was estimation of civilian demands for steel. The paper was one reason Mason recommended me for a job in Washington with the civilian supply division of the nascent Office of Price Administration and Civilian Supply. So I left Harvard in May 1941, having completed all the requirements for the Ph.D. except the dissertation. I would not return until February 1946. After nine months of helping to ration scarce materials, I went in the Navy and served as a line officer on a destroyer until Christmas 1945.

Statistics and econometrics were important in my research after the war. in my doctoral dissertation (1947) on the determinants of household consumption and saving, I tried to marry "cross-section" data from family budget surveys with aggregate time series, the better to estimate effects of income, wealth, and other variables. In a later study of food demand (1950), I refined the method. This, along with my empirical and theoretical work on rationing, took place in England in 1949–50, at Richard Stone's Department of Applied Economics in Cambridge. I hoped that cross-section observations could resolve the ambiguities of statistical inference based on time series alone. Later my interest in cross-section and panel data led me to the work of the Michigan Survey Research Center, where I spent a fruitful semester with George Katona, James Morgan, and Lawrence Klein in 1953.

My work on data of this type led me to propose a new statistical method, which became known as Tobit analysis (1958). Probit analysis, which originated in biology, estimates how the probabilities of positive or negative responses to treatment depend on observed characteristics of the organism and the treatment. In economic applications, Yes responses often vary in intensity; for example, most families in a sample would report No when asked if they bought a car last year, while those who answer Yes spent varying amounts of money on a car. My technique would use both Yes-No and quantitative information in seeking the determinants of car purchases.

The label Tobit was perhaps more appropriate than Arthur Goldberger thought when he introduced it in his textbook. Perhaps not. My main claim to fame, a discovery enjoyed by generations of my students, is that, thinly disguised as a midshipman named Tobit, I make a fleeting appearance in Herman Wouk's novel *The Caine Mutiny*. Wouk and I attended the same quick Naval Reserve officers' training school at Columbia in spring 1942, and so did Willy, the hero of the novel.

Innovative and seminal work in mathematical economics and econometrics took place at the Cowles Commission for Research in Economics in the years 1944–1954. The commission was then affiliated with the University of Chicago. Its research output over that period is one of the most fruitful achievements in the history of organized scientific inquiry. The leaders were Jacob Marschak and Tjalling Koopmans; Koopmans was awarded a Nobel Prize for his contributions to the theory of resource allocation, including linear programming, during this period. The remarkable teams Marschak and Koopmans assembled included two of the previous speakers in this series at Trinity, Arrow and Klein, and two other Nobel laureates, Simon and Debreu.

When I was a graduate student at Harvard after the war, I stood in awe of the Cowles Commission and of Marschak and Koopmans. I came to know them at meetings of the Econometric Society. For the December 1947 meeting in Chicago I was asked to be a discussant of a paper by Marschak. I didn't get the paper until a few days before the meeting, indeed a day or so before Christmas. I worked hard on the paper — neglecting my wife, Betty, pregnant with our first child, and holiday festivities with our families. I was able to report some important flaws in Marschak's model and to offer some constructive suggestions. One thing led to another. I was asked to join the commission, and in 1954 1 was asked to become its research director, to succeed Koopmans as he had succeeded Marschak.

The offer was flattering, challenging, and tempting. But I was very happy at Yale, and Betty and I had come to like New Haven very much as a place to live and raise a family. It turned out that we could have our cake and eat it too. Koopmans was quite interested in relocating the commission, because of difficulties in attracting staff to Chicago at that time and problems in the relation between the commission and the university. He gave rue not the slightest inkling of this interest until I had definitely declined the offer. The founder and financial angel of the commission, Alfred Cowles, was a Yale graduate; he hoped his creation could find permanent hospitality from his alma mater.

In 1955 the commission moved to Yale, renamed the Cowles Foundation for Research in Economics at Yale University. I became its research director after all. Cowles Foundation Discussion Paper 1 (1955) was a precursor of the Tobit analysis mentioned above. The coming of Cowles was an important factor in the rise of economics at Yale to front-rank stature. I broadened the scope of the foundation's research to include macroeconomics. I was particularly eager to make room for the interests and talents of a young Yale assistant professor, Arthur Okun, who was working on macroeconomic forecasting and policy analysis.

Developing Keynesian Macroeconomics; Synthesizing it with the Neoclassical Tradition

My main program of research and writing after the war continued my early interests in Keynes and macroeconomics. I sought to improve the theoretical foundations of macro models, to fit them into the main corpus of neoclassical economics, and to clarify the roles of monetary and fiscal policies. In this endeavor I shared the objectives of many other economists, notably Abba Lerner, Paul Samuelson, Franco Modigliani, Robert Solow, J.R. Hicks, and James Meade. A new mainstream, synthesizing the Keynesian revolution and the classical economics against which it was revolting, was in the making. I am proud that Paul Samuelson called me a "partner in [this] crime."

The building blocks of the Keynesian structure were four in number: the relation of wages and employment; the propensity to consume; liquidity preference and the demand for money; the inducement to invest. I have already referred to my work on the first. I turn now to the other three.

Keynes's "psychological law" of consumption and saving stated that saving would be an ever-larger proportion of income as per capita real incomes became greater. National income data between the two world wars appeared to confirm his law. Statistical equations, fit to those data, extrapolated to much higher incomes, foretold trouble after the Second World War. Investment would have to be a much larger fraction of national income than ever before to absorb the high saving and avoid recession and unemployment. The extrapolation was wrong. Incomes rose as expected, but consumption was no smaller a proportion than before. This forecasting error triggered an agonizing reappraisal of the consumption function, with fruitful results.

My doctoral dissertation (1947) was on this subject. I thought Keynes's law should be interpreted to refer to the relation of lifetime consumption to lifetime income, not to a relation between those variables year by year. The same considerations implied that wealth, not just current income, determines consumption in the short run. As so often happens, this idea was in the air. Milton Friedman's permanent income theory and Franco Modigliani's life-cycle model were elegant explanations of saving behavior in this spirit. They showed how cyclical data could look "Keynesian" even though saving would be roughly proportional to income in the long run. I have written a number of papers on this subject over the years.

The episode is, I believe, an example of how economic knowledge advances when striking real-world events and issues pose puzzles we have to try to understand and resolve. The most important decisions a scholar makes are what problems to work on. Choosing them just by looking for gaps in the literature is often not very productive and at worst divorces the literature itself from problems that provide more important and productive lines of inquiry. The best economists have taken their subjects from the world around them.

The bulk of my work in the 1950s and 1960s was on the monetary side of macroeconomics. I had several objectives.

First, I wanted to establish a firm foundation for the sensitivity of money demand or money velocity to interest rates. Why was this important? The quantity theory of money, later called monetarism, asserted that there was no such sensitivity, that the velocity of money was constant except for random shocks and for slow, secular changes in public habits, banking institutions, and financial technology. The implication was that fiscal stimulus, such as government spending or tax reduction, could not affect aggregate spending on goods and services unless accompanied by money creation. The same implication applied to autonomous changes in private investment. In this sense Milton Friedman and other monetarists were saying not just that money matters, with which I agreed, but also that money is all that matters, with which I disagreed.

In an empirical paper in 1947 I let the data speak for themselves, loudly in favor of Keynes's liquidity preference curve. But I was not satisfied with Keynes's explanation of liquidity preference. He said people preferred liquid cash because they expected interest rates to rise to "normal" prosperity levels of the past, causing capital losses on holdings of bonds. As William Fellner, later to be my colleague at Yale, pointed out in a friendly debate with me in journal pages, Keynes could hardly call "equilibrium" a situation in which interest rates are persistently lower than investors' expectations of them. Fellner was espousing a principle of model building later called "rational expectations," and I agreed with him.

I found and offered two more tenable sources of the interest sensitivity of demand for money. One (1956) was based on an inventory theory of the management of transactions balances. As I learned too late, I had been mostly anticipated by William Baumol, but the model is commonly cited with both names. The second paper (1958) gave a new rationalization of Keynes's "speculative motive": simply, aversion to risk. People may prefer liquidity, and prefer it more the lower the interest rate on noncash assets, not because they expect capital losses on average but because they fear them more than they value the equally probable capital gains.

I had been working for some time on portfolio choices balancing such risks against expected returns, and the liquidity preference paper was an exposition and application of that work. Harry Markowitz had already set forth a similar model of portfolio choice, and our paths also converged geographically when he spent a year at Yale in 1955–56. My interest was in macroeconomic implications, his more in advising rational investors.

When my prize was announced in Stockholm in 1981, the first reports that reached this country mentioned portfolio theory. This caught the interest of the reporters who faced me at a hastily arranged press conference at Yale. They wanted to know what it was, so I did my best to explain it in lay language, after which they said "Oh no, please explain it in lay language." That's when I referred to the benefits of diversification: "You know, don't put all your eggs in one basket." And that is why headlines throughout the world said "Yale economist wins Nobel for 'Don't put all your eggs...,'" and why a friend of mine sent me a cartoon he had clipped, which followed that headline with a sketch of next year's winner in medicine explaining how his award was for "An apple a day keeps the doctor away.

The fact that one of the available assets in the model of my paper was riskless turned out to have interesting consequences. I felt somewhat uneasy and apologetic that I was pairing the safe asset with just one risky asset to represent everything else. This aggregation followed Keynes, who also used "*the* interest rate" to refer to the common yield on all nonmoney assets and debts. I proved that my results would apply even if any number of risky assets were available, each with different return and risk. The choice of a risky portfolio, the relative weights of the various risky assets within it, would be independent of the decision how much to put into risky assets relative to the safe asset, money. This "separation theorem" was the key to the capital asset pricing model developed by Lintner and Sharpe, beloved by finance teachers and students, and exploited by the investment managers and counselors who compute and report the "betas" of various securities.

The debate about fiscal and monetary policy, as related to the interest-sensitivity of demand for money, went on for a long time, too much of it a duel between Milton Friedman and me. In a Vermont ski line a young attendant checking season passes read mine and said in a French-Canadian accent, "Tobeen, James Tobeen, not ze economiste! Not ze enemy of Professeur Friedman!" He was an economics student in Quebec; it made his day. He let me pass to the lift. This debate, I would say, ended for practical purposes when Friedman shifted ground, saying that no important issue of monetary policy or theory depended on interest-sensitivity of money demand. The ground he shifted to was the basic issue between Keynes and the classics, the contention that the economy is always in a supply-constrained equilibrium where neither monetary nor fiscal policy can enhance real output.

Second, I proposed to put money into the theory of long-run growth. In the 1950s one phase of the synthesis of Keynesian and neoclassical economics was the development of a growth theory along neoclassical lines. Some, not all, Keynesians were ready to agree that in the long run employment is full, saving limits investment, and "supply creates its own demand." The short run was the Keynesian domain, where labor and capital may be underemployed, investment governs saving, and demand induces its own supply. Roy Harrod had started modern growth theory in 1939, followed by Evsey Domar in the 1940s and Trevor Swan, Robert Solow, Edmund Phelps, and many others in the 1950s and 1960s.

I was involved too. My 1955 piece, "A Dynamic Aggregative Model," may be my favorite; it was the most fun to write. It differed from the other growth literature by explicitly introducing monetary government debt as a store of value, a vehicle of saving alternative to real capital, and by generating a business cycle that interrupted the growth process. In three subsequent papers (1965, 1968, and 1985) I showed that the stock of capital in a growing economy is positively related to the rates of monetary growth and inflation.

Third, in a long series of papers I developed, together with William Brainard and other colleagues at Yale, a general model of asset markets and integrated it into a full macroeconomic model. In a sense we generalized Hicks's famous IS/LM formalization of Keynes by allowing for a richer menu of assets. As I already indicated, I had been uncomfortable with that unique "*the* interest rate" in Keynes and with the simple dichotomy of money versus everything else, usually described as money versus bonds. I thought nominal assets versus real capital was at least as important a way of splitting wealth, if it must be split in only two parts, and this is what I did in the growth models cited above.

Portfolio theory suggested that assets should be regarded as imperfect substitutes for each other, with their differences in expected yields reflecting their marginal risks. Our approach also suggested that there is no sharp dividing line between assets that are money and those that are not. The "Yale approach" to monetary and financial theory has been widely used in empirical flow-of-funds studies and in modeling international capital movements.

Our approach also explicitly recognizes the stock-flow dynamics of saving, investment, and asset accumulation, as in my 198 1 Nobel lecture. These dynamics were explicitly ignored in Keynes, who defined the short run as a period in which the change in the stock of capital due to the flow of new investment is insignificant. Stock-flow dynamics are also ignored in IS/LM models. But flows do add to stocks. Investment builds the capital stock, government deficits enlarge the stocks of government bonds and possibly of money, trade surpluses increase the net assets of the nation vis-à-vis the rest of the world, and so on. Without these effects, macro stories about policies and other events are incomplete.

The bottom line of monetary policy is its effect on capital investment, in business plant and equipment, residences, inventories, and consumer durable goods. The effect is not well represented by the market interest rates usually cited, or by quantities of money or credit. Our approach to monetary economics and macroeconomics led us naturally to a different measure, closer to investment decisions. This has become known as "Tobin's *q*." It is the ratio of the market valuations of capital assets to their replacement costs, for example, the prices of existing houses relative to the costs of building comparable new ones. For corporate businesses, the market valuations are made in the securities markets. It is common sense that the incentive to make new capital investments is high when the securities giving tide to their future earnings can be sold for more than the investments cost, i.e., when *q* exceeds one. We see the reverse in takeovers of companies whose *q*s are less than one; it is cheaper to buy their productive assets by acquiring their shares than to construct comparable facilities from scratch. That is why in our models q is the link from the central bank and the financial markets to the real economy.

Policy and Public Service

As must be clear from my narrative, I have always been intensely interested in economic policy. Much of my theoretical and empirical research has been devoted to analyzing and discerning the effects of monetary and fiscal policies. In the 1 950s I began writing occasional articles on current economic issues for general readership, some of them in *The New Republic*, *The Yale Review*, *Challenge*, the *New York Times*.

Some of my friends in Massachusetts were advising Senator Kennedy. They told him and his staff about me. In summer 1960 Ted Sorenson came to see me and arranged for the Kennedy campaign to employ me to write some memoranda and position papers on economic growth. Sorenson signed me up despite the fact that I had felt it necessary to tell him I favored Stevenson for the nomination. I didn't notice any effects of my memos during the campaign, but I was told that they were used by the Kennedy team at the party platform deliberations, mainly to oppose the exaggerated "spend to grow" views of Leon Keyserling and some union economists.

My message at the time was that we needed a tight budget, one that would yield a surplus at full employment, and a very easy monetary policy, one that would get interest rates low enough to channel the government's surplus into productive capital investment. The point was to have frill employment, but by a mix of policies that promoted growth in the economy's capacity to produce. Incidentally, my message is similar today.

After the 1960 election I served on a transition task force on the domestic economy chaired by Paul Samuelson. One day in early January 1961 I was summoned from lunch at the faculty club to take a phone call from the president-elect. He asked me to serve as a member of his Council of Economic Advisers. JT: "I'm afraid you've got the wrong guy, Mr. President. I'm an ivory-tower economist." JFK: "That's the best kind. I'll be an ivory-tower president." JT: "That's the best kind." I took a day or two to talk to Betty and to my colleagues and then said Yes. I served for twenty months.

Walter Heller was the chairman of the council, and Kermit Gordon was the other member. We had a fantastic staff, including Art Okun, Bob Solow, Ken Arrow, and a younger generation whose names would also be recognized as leaders in our profession today. We were all congenial, intellectually and personally, and we functioned by consensus without hierarchy or bureaucracy. We were optimistic, confident that our economics could improve policy and do good in the world. It was the opportunity that had motivated me to embrace economics a quarter century before.

The January 1962 *Economic Report* is the manifesto of our economics, applied to the United States and world economic conditions of the day. The press called it "the new economics," but it was essentially the blend of Keynesian and neoclassical economics we had been developing and elaborating for the previous ten years. The report was a collective effort, written mainly by Heller, Gordon, Solow, Okun, and Tobin. It doesn't appear on my personal bibliography, but I am proud of it as a work of professional economics as well as a public document. The January 1982 *Report* is the comparable document of Reaganomics, likewise the effort of professional economists to articulate a radically new approach to federal economic policy. It is interesting to compare the two; we have nothing to fear.

The Kennedy council was effective and influential because the president and his immediate White House staff took academics seriously, took ideas seriously, took us seriously. JFK was innocent of economics on inauguration day. But he was an interested, curious, keen, and able student. He read what we wrote, listened to what we said, and learned a lot.

Our central macroeconomic objective was to lower unemployment, 7 percent inJanuary 1961, to 4 percent, our tentative estimate of the inflation-safe unemployment rate. That goal was achieved by the end of 1965, with negligible increase in the rate of inflation and with a big increase in capital investment The sweet success turned sour in the late 1960s, when contrary to the advice of his council and other Keynesian advisers President Johnson failed to raise taxes to pay for the escalating costs of the war in Vietnam. Critics looking back on the 1960s accuse the Kennedy-Johnson economists of naïve belief in a Phillips trade-off and of policies explicitly designed to purchase lower unemployment with higher inflation. The criticism is not justified. The council did not propose to push unemployment below what came to be known as the "natural rate." Moreover, beginning in 1961 the council and the administration adopted wage and price policies designed to achieve an inflation-free recovery — "guideposts for noninflationary price and wage behavior" were espoused in the report.

I returned to Yale in September 1962. I loved the job at the council, but I knew my principal vocation was university teaching and research. Fifteen-hour days and seven-day weeks were a hardship for me, my wife, and our four young children. I remained active as a consultant to the council, particularly on international monetary issues that had concerned me as a member. Moreover, I was now more visible outside my profession, so I wrote and spoke more frequently on issues and controversies of the day. But I knew that alumni of Washington often have difficulty getting back into mainline professional scholarship. I determined to accomplish that re-entry, and I believe I did.

Kennedy and Johnson added the war on poverty to their agenda. Walter Heller and the council were very much involved. I became quite interested in the economic disadvantages of blacks and in the inadequacies, inefficiencies, and perverse incentives — penalties for work and marriage — of federal and state welfare programs. I wrote major papers on these matters in 1965 and 1968. This was not macroeconomics, but one implication of the Keynesian-neoclassical synthesis was that welfare and redistributional policies could be, within broad limits, chosen independently of macroeconomic goals. Nothing in our view of the functioning of capitalist democracies says either that prosperity requires hard-hearted welfare policies and small governments or that it requires redistribution in favor of workers and the poor.

I favored a negative income tax. So did Milton Friedman — although his version seemed to me too small to fill much of the poverty gap, and he refused to join a national nonpartisan statement of economists favoring the approach. I helped to design a negative income tax plan for George McGovern in 1972. Unfortunately, he and his staff botched its presentation in the heat of the California primary; I am sure most people to this day think McGovern was advocating a kooky budget-breaking handout. After the election Nixon proposed a family assistance plan pretty much the same as the McGovern scheme he had ridiculed during the campaign.

I have lived long enough to see the revolution to which I was an eager recruit fifty years ago become in its turn a mainstream orthodoxy and then the target of counterrevolutionary attack. The tides of political opinion and professional fashion have turned against me. Many of my young colleagues in the profession are as enthusiastic exponents of the new classical macroeconomics as I and my contemporaries were crusaders against old classical macroeconomics in the 1930s. Many of the issues are the same, but the environment is quite different from the great depression. The contesting factions are better equipped — our profession has certainly improved its mathematical, analytical, and statistical tools. I do not despair over the present divisions of opinion in economics. Our subject has always thrived and advanced through controversy, and I expect a new synthesis will evolve, maybe even in my lifetime. I haven't abandoned the field of battle myself. I hope I learn from the new, but I still think and say that Keynesian ideas about how the economy works and what policies can make it work better are relevant today — not just Keynes wrote them, of course, but as they have been modified, developed, and refined over the last half-century.

Date of Birth
March 5, 1918

Academic Degrees
A.B. Harvard University, 1939

M.A. Harvard University, 1940
Ph.D. Harvard University, 1947

Academic Affiliations
Junior Fellow, Harvard University, 1946–1950
Associate Professor of Economics, Yale University, 1950–1955
Professor of Economics, Yale University, 1955–1957
Sterling Professor of Economics, Yale University, 1957–present

Selected Books
The American Business Creed (with S. E. Harris et al.)
National Economic Policy
Essays in Economics: Macroeconomics
The New Economics One Decade Older
Essays in Econometrics: Consumption and Econometrics