Keynote Address to the 2003 *HOPE* Conference: My Keynesian Education

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I have mixed feelings about Bob Byrd[1] saying he's looking forward to receiving my papers. He's probably only going to get them when I'm gone: I don't seem to be able to give up anything out of my file drawers. But when that does happen, my papers will be in the best library for the history of economic thought they can find anywhere, so they will have a happy home.

Well, I'm not here to tell people in this group about the history of monetary thought. I guess I'm here as a kind of witness from a vanished culture, the heyday of Keynesian economics. It's like historians rushing to interview the last former slaves before they died, or the last of the people who remembered growing up in a Polish shtetl. I am going to tell you what it was like growing up in a day when Keynesian economics was taught as a solid basis on which macroeconomics could proceed.

My credentials? Was I a Keynesian myself? Absolutely. And does my Chicago training disqualify me for that? No, not at all. David Laidler [who was present at the conference] will agree with me on this, and I will explain in some detail when I talk about my education. Our Keynesian credentials, if we wanted to claim them, were as good as could be obtained in any graduate school in the country in 1963.

I thought when I was trying to prepare some notes for this talk that people attending the conference might be arguing about Axel

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1. Director of Duke University's Rare Book, Manuscript, and Special Collections Library.

Leijonhufvud's thesis that IS-LM was a distortion of Keynes, but I didn't really hear any of this in the discussions this afternoon. So I'm going to think about IS-LM and Keynesian economics as being synonyms. I remember when Leijonhufvud's book[2] came out and I asked my colleague Gary Becker if he thought Hicks had got the *General Theory* right with his IS-LM diagram. Gary said, "Well, I don't know, but I hope he did, because if it wasn't for Hicks I never would have made *any* sense out of that damn book." That's kind of the way I feel, too, so I'm hoping Hicks got it right.

Today I'm going to reminisce about my macro courses at Chicago and a little bit about what I learned teaching macroeconomics at Carnegie Mellon, which is where the Keynesian phase of my career ended. And then I would like to talk about what I now think, not as a graduate student but as an adult, about Keynesian economics, both as a political force in the years during and after the Depression and as a scientific influence. But I do think those are two different questions. And then, since I love the reference to the "strange persistence" of IS-LM in the conference title, in the end I'm going to take a crack at that, too. Because it *has* persisted.

I started graduate school in the history department at Berkeley in the fall of 1959. As a Chicago undergraduate in history, I had been excited by writings like Marx and Engels's *Communist Manifesto* and the work of the Belgian historian Henri Pirenne. I was interested in ancient history in those days, and Pirenne had an economic interpretation of the end of the Roman Empire in Western Europe and the advent of the Dark Ages that was exciting for me. So I wanted to learn some economics, but hadn't got around to actually doing so.

In those days, Keynes's standing was kind of like Einstein's—everyone knew he was important—this was among undergraduates, but I suppose it was true everywhere; but no one understood what he meant. In high school, they told us that only six people in the world understood the theory of relativity. So I don't know—the *General Theory* maybe would have had sixteen or something. I remember Alvin Hansen had actually written a watered-down version—you had to have an intermediary to get close to the *General Theory*. Somebody had to help you get at it. But I had no idea what was actually in Keynes's book.

2. *On Keynesian Economics and the Economics of Keynes: A Study in Monetary Theory* (New York, 1968).

At Berkeley, I took economic history courses from Carlo Cipolla and David Landes, which in hindsight was amazing good luck. Landes taught a seminar course for first-year graduate students that was sort of a bibliographical boot camp where you had to pick a topic off a list of his and go to the library and find out everything that was known on this topic and come back and report to the seminar. One student came into the seminar with a single piece of paper that he just unfolded and unfolded until it covered the whole seminar table; we were all lost in admiration for this guy! It was a fun seminar; people were having a lot of fun. For me, history courses had been other people handing me things and saying, "Read this," so it was a new experience to be in a seminar where our job was to find out what was worth reading and to tell other people about it.

One of the topics on Landes's list was nineteenth-century British business cycles. I chose this one, since I wanted an excuse to learn some economics. That's where I met Anna Schwartz, although she doesn't know this. I read the monograph by Gayer, Rostow, and Schwartz[3]—yes, that's *Anna* Schwartz and *W. W.* Rostow—I mean this is really a team, right? And they were the *junior* authors in this book! The senior author was A. D. Gayer at Queen's College. This book was a mix: it went over British economic history in the first part of the nineteenth century. It included a kind of a year-by-year history. There was NBER Mitchell-type stuff, and then there was a sort of Keynesian diagnosis, episode by episode. It was an amazingly ambitious and exciting mix of history and theory. Anna [also at the conference] later told me she was embarrassed by the Keynesian theory in the book, but as a student I thought it was very exciting.

I decided I had to take some real economics courses or I was always going to be on the sideline even of economic history, and Landes encouraged me in this view. But Berkeley wasn't going to support me to study economics. At Christmas break I moved back to Chicago: I had passed an exam as a history undergraduate at a high enough level that I was automatically admitted as a graduate student in social science. So I just showed up at the economics department and said, "Here I am."

I started by taking remedial courses, like undergraduate courses in economics. I took some price theory. At Chicago price theory—micro—is always at the center, but my first macro course was from Carl Christ, who introduced me to Patinkin's work. We never used Patinkin in a

3. *The Growth and Fluctuation of the British Economy, 1790–1850: An Historical, Statistical, and Theoretical Study of Britain's Economic Development* (Oxford, 1953).

course. And then I had a fabulous course from Martin Bailey. Christ's course was a step-by-step model-building course, making sure you had the same number of equations and unknowns. Just what I needed. We read some of the Keynesian classics. That's where I first read Hicks's "Mr. Keynes and the 'Classics'" and Modigliani's 1944 paper.[4] I think this was the basis for IS-LM theory, those two papers. Christ also assigned us Klein's book *The Keynesian Revolution*, which is a pretty nice little book.[5] Another book that influenced me a lot was Samuelson's *Foundations*—I'm part of the Samuelson generation that Mark Blaug [who had been mentioned in the introduction to this talk] talked about—which I started reading on my own.

After class one day, I asked Christ about what Hicks thought was going on in labor markets, because there's not much on it in "Mr. Keynes and the 'Classics.'" That's when Christ told me to read Patinkin's *Money, Interest, and Prices*,[6] and I tried to do it. It's such a beautiful book physically, even the pictures. I just loved looking at that book. It made me feel like I was in touch with something elevated. Also, Patinkin's scholarly style, his erudition, I liked that, too. I still do. But the main thing I liked about Patinkin's book was that it was full of supply and demand, of people maximizing, of markets. There's a lot of micro in the book. That was the objective Patinkin had stated in his subtitle: to unify value theory and monetary theory. I liked his high aspirations. They were inspiring to me. But the book doesn't quite come off, does it? I mean, the theory is never really solved. What are the predictions of Patinkin's model? The model is too complicated to work them out. All the dynamics are the mechanical auctioneer dynamics that Samuelson introduced, where *anything* can happen.

There's an interesting footnote in Patinkin's book. Milton Friedman had told him that the rate of change of price in any one market ought to depend on excess demand and supply in all markets in the system. Patinkin is happy about this suggestion because he loves more generality, but if you think about Friedman's review of Lange, of Lange's book,[7]

4. "Liquidity Preference and the Theory of Interest and Money," which appeared in the January 1944 issue of *Econometrica*. Hicks's article appeared in the April 1937 issue of that same journal.

5. Published in 1947 by Macmillan.

6. Published in 1956 by Row, Peterson of Evanston, Ill. The book was subtitled *An Integration of Monetary and Value Theory*.

7. *Price Flexibility and Employment* (Bloomington, Ind., 1944). Friedman's review appeared in the September 1946 regular issue of the *American Economic Review*.

what Friedman must have been trying to tell Patinkin is that he thinks the theory is empty, that anything can happen in this model. And I think he's got a point.

If you look at Rapping's and my paper on labor markets[8]—which I'll come back to, because that's a Keynesian paper—we have a cleared labor market at every point in time, and we were a little self-conscious about that because people didn't think that was the right way to do things. Going back to Patinkin's book, and even though Patinkin says that all the dynamics is some auctioneer moving prices, you can see from his verbal discussion that he's reading a lot of economics into these dynamics. What are people thinking? What are they expecting? He's too good an economist to take the Samuelsonian dynamics literally. He's really thinking about intertemporal substitution. He doesn't know *how* to think about it well, but he's trying to. So in some sense Patinkin's book is less mechanical than it looks.

I think Patinkin was absolutely right to try and use general equilibrium theory to think about macroeconomic problems. Patinkin and I are both Walrasians, whatever that means. I don't see how anybody can not be. It's pure hindsight, but now I think that Patinkin's problem was that he was a student of Lange's, and Lange's version of the Walrasian model was already archaic by the end of the 1950s. Arrow and Debreu and McKenzie had redone the whole theory in a clearer, more rigorous, and more flexible way. Patinkin's book was a reworking of his Chicago thesis from the middle 1940s and had not benefited from this more recent work.

In the spring quarter that year, I took Martin Bailey's course. He was then writing his book *National Income and the Price Level*.[9] It wasn't out then, but it was in draft and this was the basis for the course. Bailey's book moves right along. He's got a Keynesian cross in nine pages. He's got a well-motivated IS-LM diagram by page 20. He's got a production sector and a labor market by page 35. It took Patinkin to page 343 to get to that point! So, Bailey is speeding things up by a factor of ten. And he's getting the mathematical structure of the model clear. You can count equations and unknowns. You can see what the predictions of Bailey's model are. You have to make some assumptions, but you can work with the model.

8. "Real Wages, Employment, and Inflation," which appeared in the September-October issue of the *Journal of Political Economy*.

9. Published in 1962 by McGraw-Hill.

When I think of IS-LM, I think of what I learned from Bailey, where you have IS-LM and then this production sector that he took from Modigliani's paper and put them all together with some additions. For example, Bailey put us on to the fact that it's a *nominal* interest rate in the LM curve and a *real* interest rate in the IS curve. You are making use of the vertical axis for two different things. You have to do something about that. So Bailey's book was good training and was the basis for preparing for the core exam at Chicago and Carnegie Mellon for ten or fifteen more years after that.

I mentioned Samuelson's book [*Foundations*]. You'll see the IS-LM model in Samuelson's chapter when he introduces the correspondence principle: the idea that you can learn about comparative statics by looking at the stability properties of a model. Example 1 is the IS-LM model. (Maybe that's example 2. Maybe supply and demand is example 1.) That's an example of how standard IS-LM was at that point.

So that's my first year [1959–60] of graduate school, as an unsuccessful history student and then as a student in remedial courses in economics. And then by the next fall, I was ready to take Friedman's course, which was the high point of everyone's education at Chicago.

But in my day, Friedman taught price theory; he didn't teach macro. I don't know if Mike Bordo [also at the conference] may have had him—["I had him for money and macro," Bordo said]. I had a neighbor in Chicago, Sue Freehling, who was an MBA student at Chicago and had taken Friedman's course in money and macro. Sue was an active liberal Democrat, and I wondered how she liked the course. She said, "Oh, I loved Friedman. He's such a wonderful guy. But he had us read this awful book by Keynes." I don't know if that's how it was for you [speaking to Bordo], but Sue thought Friedman took that book way too seriously and she wished he'd just talked more about his own ideas.

Anyway, I didn't have any macro from Friedman. What I had that was exciting in macro in my first year was Harry Johnson's first course at Chicago. He had just arrived in Chicago, and he was full of the controversy stemming from Patinkin's book, Archibald and Lipsey's criticism,[10] and so on. This stuff was *way* over the heads of anyone in the class as far as I could tell. Except for Neil Wallace. I remember Neil asking him—I can't imitate his voice but he just calls out without raising his hand: "Wait a minute, Harry! *That's* not what you want to say."

10. "Monetary and Value Theory: A Critique of Lange and Patinkin," published in the October 1958 issue of the *Review of Economic Studies.*

Things didn't happen this way in England, and nobody called him Harry. [*Laughter.*] Somehow I got nothing out of the course. Too much detail. I think I thought I knew everything after Bailey's class, so I basically bailed out and got a C in the course. Which was probably an overstatement of what I actually learned. And Harry never really had a high opinion of me after that.

Johnson's heyday as a teacher at Chicago came when Mundell arrived a few years later, and then he and Mundell trained Frenkel, Dornbusch, Mussa, Razin—people who just transformed and kind of Keynesianized international macro. That was a great period, but it hadn't even started when we were students, and I missed out on that.

Johnson's was the last macro class I took at Chicago. My fields were econometrics and public finance, so I didn't take any advanced macro, I just took the core courses. But public finance in those days was half macro. If you remember, Musgrave's book[11] was divided about equally into a macro part and a micro part. Sort of a Ramsey part and a Keynesian part. Arnold Harberger taught a course—a public finance course on macro policy, and this was really a nice thing. It was based on a multiplier-accelerator model he had calibrated to U.S. national income and product accounts. He really got into the nitty-gritty of all the leakages and the multipliers. It was the only place at Chicago where I saw a dynamic model, I mean with time subscripts, and he actually ran a system of difference equations out, trying to see what kind of shocks it would take to produce a recession. That was an exciting course. In terms of dynamics, until Uzawa showed up—again, after I left—Chicago was a backwater then. The growth theory that was starting at MIT and Stanford and Yale at that point had not yet got under way at Chicago, even though all the students had read Solow's paper[12] and were excited about it.

So what about Milton Friedman and the monetarist counterrevolution? That's what you think of when you think of Chicago in the 1960s. I was even the draftsman for Friedman and Meiselman's paper for the Commission on Money and Credit where they criticized Keynesian models.[13] But I thought of it as just drafting—it was a job. I didn't really

11. *The Theory of Public Finance: A Study in Public Economy* (New York, 1959).

12. "A Contribution to the Theory of Economic Growth," which appeared in the February 1956 issue of the *Quarterly Journal of Economics*.

13. "The Relative Stability of Monetary Velocity and the Investment Multiplier in the United States, 1897–1958," which was published in the commission's *Stabilization Policies* (Englewood Cliffs, N.J., 1963).

know what was going on in the paper. His consumption book was published in 1957.[14] His project with Anna Schwartz on monetary history was just getting going.[15] There were a lot of things that Friedman was doing in macroeconomics in my day, but he didn't talk about any of this in his price theory courses. In fact, Friedman didn't spend much time plugging his past work or talking about it. The only way you would have been in on this monetarist counterrevolution was to be writing your thesis with Friedman and be a member of the money and banking workshop, but that was an invitation-only thing. I was not working with him. The first money and banking workshop I went to was in 1974 when I was a visiting faculty member. I remember learning about the consumption study from my classmate Glen Cain, who was using it in his thesis and saw how important it was going to be, but I can't remember Friedman mentioning that book. It would not have been out of place to talk about it in his price theory course, but I can't remember his doing it. Maybe David's memory differs from mine. He was there.

["I'm trying to remember; I don't remember," said Laidler.

"You were probably more with it than I was," said Lucas.

"He did Archibald and Lipseyian price theory, though," said Laidler.

"He did?" asked Lucas.

"Yeah, he did," said Laidler.

"God, I missed it. I had two shots at Archibald and Lipsey and whiffed both times," said Lucas.]

Everyone from Chicago is a Friedman student in some very basic sense, but in terms of macro, I claim that the credentials I'm describing are true-blue Keynesian.

When I was done with my graduate education, how did I think of Keynesian economics? I didn't think about it very deeply, to tell you the truth. It wasn't my field. I didn't picture myself as doing research in the area. But I certainly thought of myself as a Keynesian. Kennedy was elected in 1960. I remember the Kennedy tax cut. We, meaning students, were excited with the Council of Economic Advisors that Kennedy appointed, the tax cut—it seemed like the theory we were learning about in class was being put in place. We were definitely excited about that. I also remember that the cost-benefit analysis was explicitly introduced in the Department of Defense back in the Kennedy administration when

14. *A Theory of the Consumption Function* (Princeton, N.J., 1957).

15. The results of which were published in 1963 by Princeton University Press as *A Monetary History of the United States, 1867–1960*.

McNamara became secretary—and that was another exciting thing for economics. It seemed like everything we were learning in class, micro and macro, was being put to work in U.S. economic policy. It all went down the drain in Vietnam, so all we remember now about McNamara is how he got us into that awful war, but, at the beginning, it was much more promising.

When I began to teach at Carnegie, I took Bailey's book [*National Income and the Price Level*], his version of IS-LM, as kind of standard stuff. This is the theory, the accepted theory that everyone should know, that it was my job to teach to graduate students, and did. I also held on to Patinkin's ambition somehow, that the theory ought to be microeconomically founded, unified with price theory. I think this was a very common view. Ed Burmeister. [Burmeister attended the talk.] Where's Ed? Ed can remember this, I'm sure. Nobody was satisfied with IS-LM as the end of macroeconomic theorizing. The idea was we were going to tie it together with microeconomics and that was the job of our generation. Or to continue doing that. That wasn't an *anti*-Keynesian view. You can see the same ambition in Klein's work or Modigliani's.

The first macroeconomics work I ever did was my work with Leonard Rapping on Phillips curves and labor markets.[16] This was an ambitious move for us. We wanted to contribute to Keynesian economics, and in particular to the econometric models that were being based on Keynesian economics. Our models—the examples we wanted to follow—were Friedman and Modigliani's work on consumption or Jorgensen and Eisner's work on investment or Meltzer's and Friedman's work on money demand.[17] These were the people who staked out an important equation for macroeconomics and were trying to estimate its parameters. We were going to go after the production and labor side of that model. There was a lot of really bad work being done on labor unions: people regressing wages in this industry on wages in some other industry and getting

16. See Lucas and Rapping, "Real Wages" (cited in footnote 8) and "Price Expectations and the Phillips Curve," published in the June 1969 issue of the *American Economic Review*.

17. See Friedman, *A Theory of the Consumption Function*; Modigliani and Richard Brumberg, "Utility Analysis and the Consumption Function: An Interpretation of Cross-Section Data," in *Post-Keynesian Economics*, edited by Kenneth K. Kurihara (New Brunswick, N.J., 1954); Dale W. Jorgenson, "Capital Theory and Investment Behavior," in the May 1963 "Papers and Proceedings" issue of the *American Economic Review*; Robert Eisner and Robert H. Strotz, "Determinants of Business Investment," in *Impacts of Monetary Policy* (Englewood Cliffs, N.J., 1963); Allan H. Meltzer, "The Demand for Money: The Evidence from the Time Series," in the June 1963 issue of the *Journal of Political Economy*; and Friedman and Schwartz, *A Monetary History of the United States*.

R-squares of .99. Really junk. There was a paper by George Perry that had a respectable theory of wage determination with a Phillips curve in it, but it was all based on labor unions.[18] Rapping and I knew that something like a fifth of the U.S. labor force was in labor unions. It didn't make any sense to have a model of the whole labor market that pretended everybody was a union member. So we thought we'd write down a competitive model.

If you look back at Rapping's and my *JPE* paper, the introduction to that paper, it's a Keynesian introduction, very much so. It's an IS-LM introduction, not that we have an IS-LM sector—somebody else had worked that stuff out—but we were going to try and work out a compatible production side and then put it all together. That was the general idea. Remember the Brookings model from those days? It was like a church supper, the way I think about it, where somebody's bringing the consumption function and somebody else is bringing the investment function. It's like Mrs. Smith is bringing the potato salad and Mrs. Jones is bringing the ribs. Somehow—you just trusted dumb luck that there was going to be the right balance of desserts and salads and God knows what. It's not a good way to design a menu, and it's a completely crazy way to put together a general equilibrium model of the whole economy. Nobody's thinking about the whole thing.

Well, that takes me up to the end of the Keynesian phase of my career. What went wrong? I'm not going to talk about this. It's a complicated story, the story of what's happened in macroeconomics since the late 1960s. It's pretty interesting. I've written about it elsewhere and so have lots of other people. So I'm just going to fast forward. This is complete hindsight. It has nothing to do with what I thought in '63 or '68, but how I think about it now. What happened? What did in Keynesian economics? I'm just going to sketch an outrageously simple view of how I think economic thought evolves, and then I'm going to try and apply it to the models that I've been talking about.

I think the basic view of economics that Hume and Smith and Ricardo introduced, taking people as basically alike, pursuing simple goals in a pretty direct way, given their preferences, where you are trying to explain differences in behavior by differences in the situation people are finding themselves in rather than differences in their culture, their inner wiring, inner workings, their race, whatever, their class, just thinking

18. "The Determinants of Wage Rate Changes and the Inflation-Unemployment Trade-Off for the United States," published in the October 1964 issue of the *Review of Economic Studies*.

about people as people and then trying to account for their behavior in terms of how they are responding to their environment, that this is it for economics. We got that view from Smith and Ricardo, and there have never been any new paradigms or paradigm changes or shifts. Maybe there will be, but in two hundred years it hasn't happened yet. So you've got this kind of basic line of economic theory.

And then I see the progressive—I don't want to say that everything is in Smith and Ricardo—the progressive element in economics as entirely technical: better mathematics, better mathematical formulation, better data, better data-processing methods, better statistical methods, better computational methods. I think of all progress in economic thinking, in the kind of basic core of economic theory, as developing entirely as learning how to do what Hume and Smith and Ricardo wanted to do, only better: more empirically founded, more powerful solution methods, and so on. So I don't think there *was* a Keynesian revolution in a scientific sense, in the sense of a new paradigm or a bifurcation of economic theory into two different directions. I'll tell you what I think did happen, but it wasn't that.

In the twentieth century, which I think was a pretty good century for economics, important technical developments included mathematically rigorous general equilibrium theory, which can be analyzed in modern mathematical terms in a rigorous and clear way, and a language for talking about dynamics, difference equations, differential equations, shocks. The latter tradition I think of as due to Slutsky, Frisch, Tinbergen. It's sort of a statistical language, not an economic language. You think of Slutsky's paper on stochastic difference equations, it's just a purely statistical model that he simulates using results from some Russian lottery and then generates a time series and says, "Hey, this thing looks like pictures I saw in Mitchell's book."[19] People started putting the economics into it, and I think of Keynesian theory as having this excitement because it breathes some economic life into these difference equation systems. So when I think of Keynesian economics or at least the Keynesian economics *I* signed on for, it was part of this econometric model-building tradition. We didn't really treat much of this when I was a student at Chicago, but I certainly moved into it at Carnegie Mellon.

Now what happened is that this statistical way of thinking about dynamics failed. It got replaced by the Arrow-Debreu model, which shows

19. Slutsky's paper, "The Summation of Random Causes as the Source of Cyclic Processes," appeared in the April 1937 issue of *Econometrica*.

how you can take what seems to be a static general equilibrium model and talk about markets for contingent claims, talk about any kind of dynamics you'd like, coming right out of the economics. No auctioneer, or the auctioneer works very quickly. Everything is accounted for in terms of preferences and technology in this model, and everything can include as much dynamics as you can get from a Tinbergen model or Slutsky's model. Patinkin or Bailey or their students, we didn't know this theory existed back in 1960, although it did. But now its potential is getting realized. It has completely succeeded in taking over growth theory, most of public finance, financial economics. Now it's coming in use in macroeconomics with real business cycle theory; certain kinds of monetary variations have been introduced with success. So when I teach macro now, that's all that I teach: variations on these models. Of course, I specialize them and try to apply them to particular economic questions: I'm not a mathematician. But I don't teach any IS-LM. I don't even mention it. I tell them to go somewhere else. Take the course from somebody else. In that sense, for me, it's over.

But I want to come to this persistence of the IS-LM model, because it *isn't* over.

The problem is that the new theories, the theories embedded in general equilibrium dynamics of the sort that we know how to use pretty well now—there's a residue of things they don't let us think about. They don't let us think about the U.S. experience in the 1930s or about financial crises and their real consequences in Asia and Latin America. They don't let us think, I don't think, very well about Japan in the 1990s. We may be disillusioned with the Keynesian apparatus for thinking about these things, but it doesn't mean that this replacement apparatus can do it either. It can't. In terms of the theory that researchers are developing as a cumulative body of knowledge—no one has figured out how to take that theory to successful answers to the real effects of monetary instability. Some people just deny that there are real effects of monetary instability, but I think that is just a mistake. I don't think that argument can be sustained. I do think that most of the post–World War II fluctuations of GDP about trend can be accounted for in real terms. I've estimated that would be something on the order of 80 percent. People can argue with that. But that's not because money doesn't matter. That's because monetary policy in the postwar United States has been so good.

So that's I think where Keynes's real contribution is. It's not Einstein-level theory, new paradigm, all this. I am in agreement with my neighbor

Sue Freehling, that's just so much hot air. I think that in writing the *General Theory*, Keynes was viewing himself as a spokesman for a discredited profession. That's why he doesn't cite anyone but crazies like Hobson. He knows about Wicksell and all the "classics," but he is at pains to disassociate his views from theirs, to overemphasize the differences. He's writing in a situation where people are ready to throw in the towel on capitalism and liberal democracy and go with fascism or corporatism, protectionism, socialist planning. Keynes's first objective is to say, "Look, there's got to be a way to respond to depressions that's consistent with capitalist democracy." What he hits on is that the government should take some new responsibilities, but the responsibilities are for stabilizing overall spending flows. You don't have to plan the economy in detail in order to meet this objective. And in that sense, I think for everybody in the postwar period—I'm talking about Keynesians and monetarists both—that's the agreed-upon view: We should stabilize spending flows, and the question is really one of the details about how best to do it. Friedman's approach involved slightly less government involvement than a Keynesian approach, but I say slightly.

So I think this was a great political achievement. It gave us a lasting image of what we need economists for. I've been talking about the internal mainstream of economics, that's what we researchers live on, but as a group we have to earn our living by helping people diagnose situations that arise and helping them understand what is going on and what we can do about it. That was Keynes's whole life. He was a political activist from beginning to end. What he was concerned about when he wrote the *General Theory* was convincing people that there was a way to deal with the Depression that was forceful and effective but didn't involve scrapping the capitalist system. Maybe we could have done it without him, but I'm glad we didn't have to try. Thank you.