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An Interview with Wassily Leontief

Interviewed by Duncan K. Foley

BARNARD COLLEGE OF COLUMBIA UNIVERSITY

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Wassily Leontief is one of the central creators and shapers of twentieth-century economics. He invented input–output theory and the techniques for constructing input–output tables from economic and technological data and was responsible for making input–output tables the most powerful and widely used tool of structural economic analysis. The theory of input–output matrices played an important role in the clarification of general equilibrium theory in the 1940s and 1950s as well. Leontief has also made fundamental and seminal contributions to the theories of demand, international trade, and economic dynamics. His research interests include monetary economics, population, econometric method, environmental economics, distribution, disarmament, induced technical change, international capital movements, growth, economic planning, and the Soviet and other socialist economies. Leontief has played a vigorous part in formulating national and international policies addressing technology, trade, population, arms control, and the environment. He has also been a well-informed and influential critic of contemporary economic method, theory, and practice. Leontief received the Nobel Memorial Prize for Economics in 1973.

I met Wassily Leontief on April 14, 1997, at his apartment high above Washington Square Park in New York City. Leontief reclined on a sofa in the living room, with Mrs. Leontief going about her business in the

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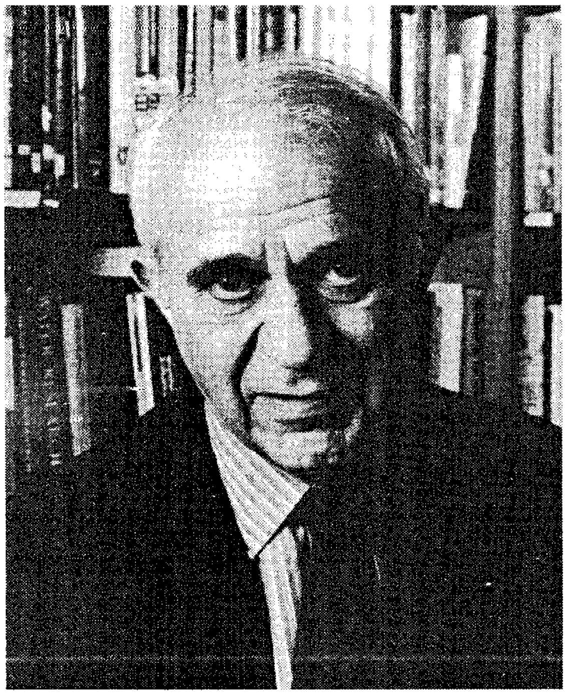

Figure 1.1 Wassily Leontief.

background, occasionally asking after Leontief's comfort. Leontief's voice on the tape ranges from an assertive *forte* to a whispery *piano.* He is by turns animated, thoughtful, puzzled, inspiring, and charming. A chiming clock marking the passage of quarter-hours and characteristic New York street noise occasionally obscure his words on the tape. I have edited the transcript for continuity and clarity.

Foley: There has been considerable discussion about the relation between input–output analysis and Marx's schemes of reproduction from Volume II of *Capital.* What was the role, if any, of Marx in your education as an economist? Were Marx's schemes of reproduction an inspiration or influence on your development?

Leontief: I did my undergraduate work in Russia, and that's where I learned Marx, but I am not a militant Marxist economist. When I developed input–output analysis it was as a response to the weaknesses of classical–neoclassical supply-and-demand analysis. It was terribly disjointed essentially, I always thought. You read my Presidential Address, I think? I felt that general equilibrium theory does not see how to integrate the facts and I developed input–output analysis quite consciously to provide a factual background, to register the facts in a systematic way, so it would be possible to explain the operation of this system.

Foley: So, did the structure of Marx's schemes of reproduction play any role in forming your ideas?

Leontief: No. Not really. No. Marx was not a very good mathematician. He was always mixed up in math, and the labor theory of value didn't make much sense, but essentially I interpret Marx and am interested in Marx only as a classical economist. And it is possibly Quesnay, the ideas of Quesnay, that influenced me. It is very difficult to say what influenced you. I got my training as an economist as an undergraduate. Already I read systematically all economists beginning with the seventeenth century. I just read and read, so I had a pretty good background in the history of economic thought, and my feeling is that I understand the state of the science.

Foley: You were in the Soviet Union in the very first years of the Soviet experiment?

Leontief: I left the Soviet Union in 1925. I got in trouble with the government, actually. I had to go away in order to be able to work.

Foley: Was anyone at that time thinking about a statistical basis for planning in the Soviet Union?

Leontief: No. The first thing which had some relation to it was essentially a national income analysis. Like all national income analyses, it was not very disaggregated. Everything gives you one figure, while I thought that to understand the operation of the system, one figure is not enough. You want to see how it disaggregates. I was not interested in improving the system; I was just concentrating on understanding how it works. Of course, it's nice to understand before you improve, but my feeling is that to understand the economic system is the first job of the economist.

Foley: Then, in 1925, you moved to Berlin?

Leontief: To Berlin. I got my Ph.D. very quickly, and I had two professors. I was research assistant of Professor Sombart, who was a quite interesting historical economist, and Bortkiewicz, who was a mathematical economist. But Professor Sombart didn't understand mathematics.

Foley: Were they particularly interested in the statistical side of input–output tables?

Leontief: No, and economists in their empirical efforts must be factual. But there is a tendency to be abstract, theoretical, particularly among the better economists.

Foley: How long did you stay in Berlin?

Leontief: About two years. I got my Ph.D. very quickly. Then I was in the Institute for World Economics—a big Institute in Kiel—and I was invited to be a member of the staff, and this is essentially where I developed my idea of the input–output approach.

Foley: Were there other scholars at Kiel working on that general line, or anything related to that type of thing?

Leontief: No. I was isolated.

Foley: It must have been a tremendous job to do the statistical groundwork for input–output analysis.

Leontief: Yes, it was. I decided the only thing was practically to show how to do it, and I did this with one assistant. I was invited to the United States by the National Bureau of Economic Research. I received some foundation money and my assistant and I worked very hard, I mean, using all kinds of information—technological information, naturally, beginning with the Census. The U.S. Census was the best statistical record of an economy. From there I was invited to Harvard, where I spent 45 years. When the war began, interest in input–output analysis grew. I was kind of a consultant on economic planning. It was for the Air Force, which of course was very important during the war. The best input–output matrix was computed by the Air Force. They had also an input–output table of the German economy, because it enabled them to choose targets. Usually I'm not very pragmatic, but if you want to do something, you have to understand what you're doing, and for the Air Force that was the committed choice of targets and so on, so input–output analysis was very interesting to them.

Foley: What was your reaction to Keynes's work during the 1930s? Have you changed your mind since then?

Leontief: No, not at all. My attitude was rather critical because I felt that he developed his theory to justify his political advice. Keynes was more of a politician than an analyst. I never became a Keynesian, although I wrote some of the first criticisms of Keynes. If you look at my bibliography you'll find them. But I tried to do it systematically; that is, not so much the political side but just the approach, which was for me too pragmatic. Now, you improve the system, all right, but first describe the system in order to improve it.

Foley: Did you have an alternative theory of the Depression at that time?

Leontief: No. My feeling is that the fundamental theoretical understanding of economic fluctuations is as a dynamic process. I still believe, what explains the fluctuations of economies is some kind of difference, differential equations. Of course, structural change is very important, particularly now. It's always dynamic. It's a system of interrelationships, a system of equations, but still the quantitative approach is important. Since I paid so much attention to the relationship between observation and theory, at the same time I developed a theory of input–output analysis which is really mathematical, and tried myself to collect data. I think I influenced the course of economic statistics.

Foley: Yes. Input–output analysis and national income analysis are the two major systems that came into place in the 1940s and 1950s.

Leontief: Right. I don't think there is really a dichotomy. I think input–output analysis is just much more detailed. Stone, for example, who was commissioned to develop the statistical economic system for the United Nations, assigned a very great role to input–output analysis, as a foundation for the aggregation to national income.

Foley: In a system of that kind, there's usually some attempt to model both supply-side effects and demand-side effects.

Leontief: I was always slightly worried about having demand analysis as a separate thing. My feeling is that households are an element of the system. In a good theoretical formulation, households are just a large sector of the economy.

Foley: This echoes the classical idea that the reproduction of the population is an aspect of the reproduction of the economic system.

Leontief: Exactly. This goes back to Quesnay.

Foley: I've also talked with Richard Goodwin about this theme of the interplay between structural change and fluctuations.

Leontief: Richard Goodwin was my student. He studied with me, he was my assistant. He couldn't get a permanent appointment at Harvard and then went to England. He was a good friend. He was very interesting.

Foley: I talked with Goodwin about this at one time. He did have a job at Harvard in the late 1940s, but it was an untenured job, right?

Leontief: Yes. He couldn't get tenure. And this was the reason why he went to England.

Foley: Yes, that's what he told me as well, but I was somewhat puzzled as to why someone who had been doing the kind of work he was doing in the late 1940s would not have been a shoo-in for tenure.

Leontief: I think possibly it was politics. He was on the left.

Foley: So that shaded the evaluation of his scientific work?

Leontief: Yes. That was it, frankly.

Foley: So you would still now look for the major cause of business-cycle fluctuations in lags, but the impulses in supply-side structural change?

Leontief: Yes, structural changes, but be very careful, because a system, a dynamic system, without structural change would have lags, and latent eigenroots that create fluctuations. Of course, at the present time, technological change is very important. Technological change is the driving force of economic change and the cause of social change.

Foley: To come to a slightly more technical issue, what about the question of whether the fluctuations are damped or undamped?

Leontief: They don't have from a mathematical point of view necessarily to be damped. This raises the problem, why don't we explode? And there are some forces which prevent them from exploding, including economic forces, such as policy and other nonlinear effects.

Foley: In the 1930s, you had a controversy with Marschak over demand analysis.

Leontief: Yes, now I do not remember the details, but I think there was a logical flaw in Marschak's position.

Foley: Did this have anything to do with the development of input–output analysis?

Leontief: That was already after I developed input–output analysis, which I really developed when I was in Kiel, and at the National Bureau. In the National Bureau, I was very subversive, because the National Bureau under Mitchell was extremely empirical, while I on the other side had a very strong theoretical intuition. To understand the process you have to have a theory. I organized an underground theoretical seminar in the National Bureau. It was underground, because it was against the principle of the National Bureau.

Foley: In the 1940s, there was a rather sharp controversy between the Cowles Foundation and the National Bureau around issues of empirical method and theory. Koopmans wrote a very sharp paper at that time.

Leontief: Since I thought mathematics plays a great, important role, I would of course be on the side of the Cowles Commission.

Foley: But you found yourself institutionally associated with the National Bureau.

Leontief: Exactly. Because I always felt, as I explained in my Presidential Address, if you want to really understand an empirical science, you must have the facts. And the problem is how to organize the facts. Essentially, theory organizes facts.

Foley: So your position was a kind of synthesis of these two points of view.

Leontief: Yes. Right.

Foley: The Cowles Commission developed a very characteristic approach to econometrics and measurement problems in the 1940s. Did you find yourself sympathetic to that way of doing it?

Leontief: No. I criticized it very early.

Foley: Did you foresee that there would be a role for input–output analysis in guiding government policy after the Second World War? This is an interesting period because it set the pattern for the next decades.

Leontief: Not only in government, but also in industry. I remember when the question arose much later about the position of the automobile industry in the American economy, there was some kind of association of industrialists who said "go to Leontief," because I published some work using the example of the auto industry. I published empirical work, and my principle always, though I could not always adhere to it, was always, when I made some theoretical observation to use the data—not just to say it, but really to see how it works.

Foley: And so you used input–output analysis, say, to study the future of the auto industry or prospects for specific industries?

Leontief: Right, right. During the Cold War, there was an economist by the name of Hoffenberg. He did a lot of empirical analysis. In constructing an input–output table of the United States, he played a very important role. He was a really excellent intuitive statistician. And, you must understand, it takes a particular knack to understand statistics. When I constructed the first input–output table, which was very early, I often used the telephone. I called up industries, particularly firms which were engaged in the distribution of commodities, and got the data from them.

Foley: So you would ask the distributors what their customers' proportions were in terms of the sectors?

Leontief: Exactly! I just went straight to them.

Foley: Did the U.S. government have a functioning input–output table in 1946?

Leontief: Yes, yes. In the Department of Commerce, in the Bureau of Economic Analysis. National income computations were conducted in the Bureau of Economic Analysis, and they had an input–output table. Although the best input–output table was constructed by the Department of Labor. Roosevelt's Secretary of Labor, Frances Perkins, wrote to me that the President had asked her the question, what will happen to the American economy after the war? She said, we don't know how to do it. We tried to look at the literature, but we don't know how to study this type of thing, and then one of my first articles appeared and they said, all right, we thought possibly you could tell us how to do it. They sent a representative, and I said, get the facts and good theory; and, as a matter of fact, at that time under Roosevelt the government was very active and intelligent. Yes, they told me, all right, collect the facts. Come to Washington and collect the facts. I said, no. One cannot collect facts in Washington. I must do it at Harvard. And they opened a division of the Bureau of Labor Statistics at Harvard, at the Littauer School, and I hired people, not many economists, mostly engineers, and we constructed an input–output table. The next detailed input–output table was constructed with the money of the Defense Department. And they had a lot. Without money it's very difficult to construct an input–output table; it's a resource-intensive activity.

Foley: After the war, was there a competition between Keynesian demand management and a more structurally oriented input–output approach to economic policy?

Leontief: Oh, I think the Keynesian approach definitely took over. I don't think there was much competition. Keynes took over.

Foley: Why did that happen?

Leontief: Because Keynes was very pragmatically oriented. In spirit, he was very much a politician, an excellent politician. I think he developed his theory essentially as an instrument to support his policy advice. He was incredibly intelligent.

Foley: Well, it sounds as if you had your political contacts, too, in the Labor Department and the Defense Department, and the Commerce Department.

Leontief: Oh, yes, but you know it was different. It was much more modest. The Labor Department studied the problem of the supply of labor, different skills and so on. It was much more technical. They still have an input–output division in the Labor Department—the Bureau of Labor Statistics.

Foley: The late 1940s, as we look back on it, seems to be the time when a methodological synthesis took place in economics. How did you view the relation of input–output to the developing methodological consensus in economics and econometric theory? Did you see it as part of it or as a different path?

Leontief: You see, I was somewhat skeptical of the whole curve-fitting notion. I thought of technological information. The people who know the structure of the economy are not statisticians but technologists, but of course to model technological information is very difficult. My idea was not to infer the structure indirectly from econometric or statistical techniques, but to go directly to technological and engineering

Figure 1.2 Members of Professor Leontief's seminar of the August 1948 Salzburg Seminar in American Studies. 1, Friedrich G. Seib (Germany); 2, Bjarn Larsen (?) (Norway); 3, Helge Seip (Norway); 4, Leendert Koyck (?) (Holland); 5, Gérard Debreu (France); 6, Paul Winding (Denmark); 7, Robert Solow (U.S.A.); 8, Mrs. Robert Solow (U.S.A.); 9, Mario Di Lorenzo (Italy); 10, Arvo Puukari (Finland); 11, Jacques D. Mayer (France); 12, Odd Aukrust (Norway); 13, Professor William G. Rice (U.S.A.); 14, Joseph Klatzmann (France); 15, unknown (Germany); 16, Bjarke Fog (Denmark); 17, Professor Leontief Wassily (U.S.A.); 18, Per Silve Tweite (Norway).

sources. I had some proposals to this effect, which could not be realized because there was no money. Empirical analyses are extremely expensive.

Foley: And the input–output type is more expensive than the indirect statistical investigation.

Leontief: Oh yes, much more. It was indirect statistical methods that were used. I think I have a very strong theoretical streak. I am essentially a theorist. But I felt very strongly that theory is just construction of frameworks to understand how real systems work. It is an organizing principle, while for many economists theory is a separate object.

Foley: Some economists think of theory as predictive or behavioral.

Leontief: Yes. I think if one knows, or one agrees, what the formal nature of a mathematical system is, one can do certain predictions because of the general nature of the system. I published a couple of articles on prediction. There are short-run problems and long-run problems in quantitative analysis, and I have a feeling that conventional prediction is good for short-run problems; but technological change, which is the driving force of all economic development, is a long-run process.

Foley: So, from this point of view, specific hypotheses about human behavior, or expectation formation, or preferences would play a subsidiary role.

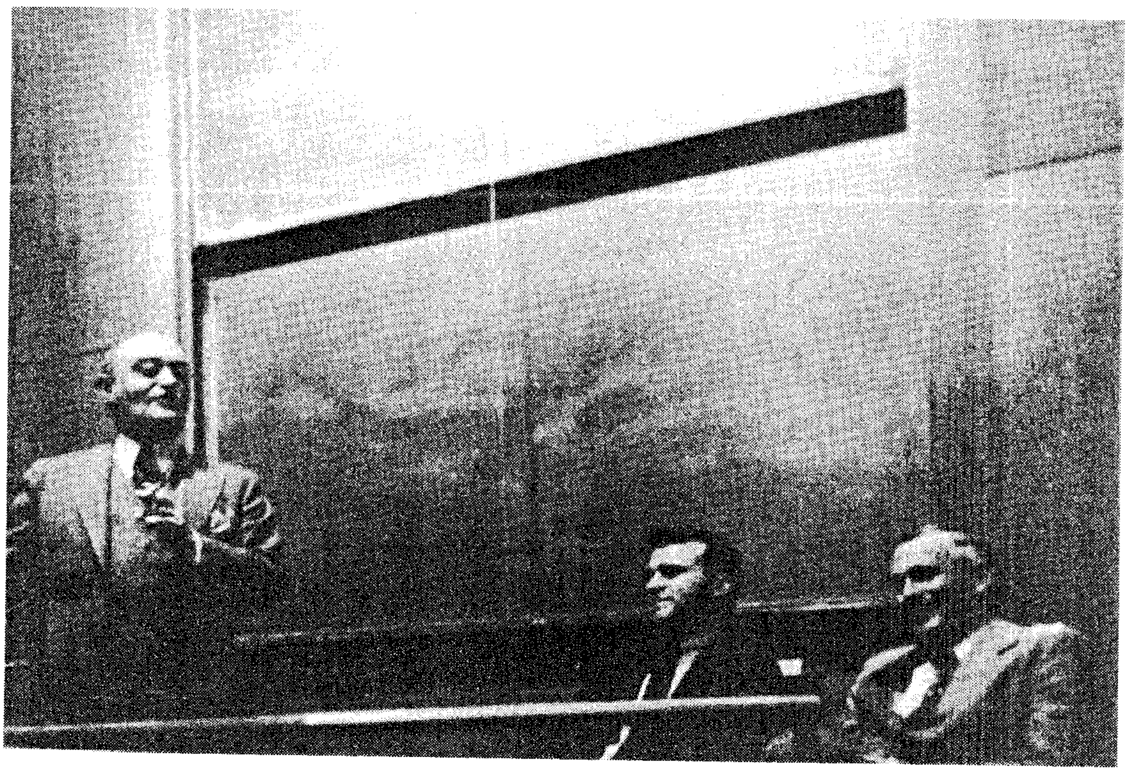

Figure 1.3 Joseph Schumpeter, Wassily Leontief, and Paul Sweezy at Harvard in the late 1940s.

Leontief: A subsidiary role. I think so, because my feeling is that, particularly under a market system, a capitalist system, the big industrialists play a really big role, and they try to make profits, to choose technologies which maximize profits—essentially in the short run. Of course, national policy has to be taken into account, but business is certainly a short-term type of system.

Foley: In sectors like transportation or power generation where you have long-term investments, this can create problems.

Leontief: I agree. There you need long-run engineering, power generation, and—I suppose—environment, which is important now.

Foley: You talked about the money and resource problem. Was there a competition for resources between national income approaches and input–output approaches in the United States during the 1950s?

Leontief: I have a feeling that, at least in the Department of Commerce, they realized input–output was very useful for national income computations. As a matter of fact, there was a period of time—possibly even now—when the national income computation essentially summarized the results of the input–output analysis. Funding is always a problem. For input–output analysis, particularly analysis of technological change, we need more of an engineering understanding, because scientific progress is now the driving force in technological change.

Foley: Do you see any feedback in the other direction, to the priorities in scientific research from economic bottlenecks?

Leontief: Oh, no doubt, no doubt. First of all, it was always true of the war industry. Scientific progress helped the military.

Foley: In the 1960s and early 1970s, there was another major change in economic doctrine, a shift from a Keynesian consensus to what's now called rational-expectations models and the notion of market clearing and perfect foresight. You were a professor at Harvard at that time. How did you see that happening in the profession? What factors determined that change?

Leontief: In the earlier times, Keynes dominated economic thinking. I do not know to what extent the whole expectations revolution made any headway. It is a very delicate thing mathematically, and I do not follow the literature closely now, but I think there was not so much analysis about expectations. There was just talk about it. I did not see any material contribution to the theory of expectations, except the very short run, naturally, for the business cycle, which is important.

Foley: So you don't think that was the result of any empirical superiority of this new approach?

Leontief: No, I don't think so.

Foley: Was it just that Keynesianism ran out of steam?

Leontief: I think so.

Foley: You said that you were not that taken with the Keynesian point of view to begin with, so I suppose you observed this with some equanimity.

Leontief: Yes. I'm not a monetary economist, but I think that deeper analysis of the flow of money might give a little more meat to this field.

Foley: There has been the development of the flow of funds accounts.

Leontief: Yes, but it was very aggregative. For our understanding how the economic system works, disaggregation is very important.

Foley: Would it make sense to try to link flow of funds with input–output analysis at the same levels of disaggregation?

Leontief: Oh, yes, but I didn't see anybody try to do it. Of course, there's no money there, but I think the money flows are important. I sometimes suggested the possibility of aligning money flows from micro up rather than from macro down, because I think in every corporation there is some high functionary who is in charge of money flows—budgets, credit, and so on—and he has to make a plan. The only planning that exists is for money flows, and there is his counterpart in a bank, who is close to operations and, if I'm not mistaken, there is a cooperation between the official in the company who sees to it that they have enough credit, and the credit manager in the bank, who is very often in charge of separate corporations. I made a couple of proposals to work on this. The interesting thing is to have the same figures from two points of view. It might be very helpful to understand how they interact to determine the short-run path of investment.

Foley: There's not been a lot of economic theorizing in that area. Most of the models assume some kind of equilibrium conditions, but you said that it was a mistake to start from equilibrium as opposed to explicit dynamics.

Leontief: Exactly, exactly.

Foley: When you resigned from Harvard in 1974, soon after you received the Nobel Memorial Prize, you sharply criticized the direction of economics as a discipline and called for a reevaluation and redirection of research methodology in economics. Do you think that redirection has taken place?

Leontief: No.

Foley: Would you have the same view on the success of economics as a science now?

Leontief: Yes. My feeling is we require patient, practical economic advice. Our basis of understanding how the economy works is not very strong. Practical advice could and should be more based on understanding how the system works.

Foley: We've mentioned several times this afternoon the role of mathematics. Some people argue that economics has become too dominated by mathematical formalism.

Leontief: I completely agree. Very many mathematical economists were simply mathematicians who were not good enough to become pure mathematicians, so mathematical economics, which had always been dull, gave them a marvelous pretext to become economists.

Foley: But on the other hand, you've strongly supported the role of mathematics in theory in economics. When is mathematics fertile and when does it become just a formalism?

Leontief: My feeling is that mathematics is simply logic. The general insights are the most important. For example, I think mathematics gives us good reason to feel that all fluctuations are due to lags—it's dynamics. This is a real mathematical insight. Mathematicians know it. As a matter of fact, one of the problems I had in my theoretical work was how to avoid explosive fluctuations, because there are so many eigenvalues in those big matrices, and some explode.

Foley: What was your own training as a mathematician?

Leontief: I took mathematics courses, but I tried to improve my range of mathematics very early, when I realized that mathematical argument was of great importance for economics. I read a lot. I took basic courses in the university. My tendency was always to combine the empirical and theoretical. In economics that combination requires mathematical concepts, such as systems analysis.

Foley: But you're also saying that the vision of the economic structure and relations has to come first.

Leontief: I think, together, because if we have only that vision, it never adds up to anything. When I developed input–output analysis before going to the National Bureau of Economic Research, I felt it terribly important to have a good insight into the mathematical relationships. I think that the mathematics which economics used are not of a particularly high order. For example, those who translated neoclassical economics into mathematics didn't develop any very interesting insights. They obviously developed some things, but didn't come to any very interesting insights into how the economic system works, and they were, on the whole, not interested in empirical analysis.

Foley: From having talked with other people about that, I think there were very high hopes that the formalization of economics would yield some substantive insights.

Leontief: Without data you couldn't do it. Absolutely, without data it couldn't work. It can just establish certain principles of equilibrium and nonequilibrium.

Foley: You think that more or less exhausted the real scientific contribution of the program?

Leontief: I think so. They would have made more progress if they really had good, very detailed, empirical information. For example, it would be very interesting to see how modern technological change has affected the demand for labor. It might reduce the demand for labor, and even create a social problem, because labor isn't just one more factor of production. Then you will have to support labor. My speculative intuition is that the government now has to support a large part of income through education expenditure, health expenditure, and of course social security—and possibly a kind of welfare—but social security is more important. My feeling is that ultimately the transfer of income so as to provide people money to buy consumers' goods will become part of social security. It's already very large—I'm amazed how large my family social security is.

Foley: This is a Keynesian theme, the support of demand through government subsidies.

Leontief: Yes, Yes, but it's not only supporting demand. Keynes was supporting employment, which this does not do. Just demand. You feed people. Technology will reduce employment, or certainly not increase employment. Certainly I think that technology competes with labor, ordinary labor: If you produce everything automatically naturally you're not going to employ so much labor.

Foley: So this is an example of your sense that there should be a substantive foundation for the investigation in the real structure of what's going on?

Leontief: Yes. Technological change was always the driving force for economic development beginning in prehistorical time, but now, when technological change has become propelled by scientific investigation, this type of analysis is extremely important. Economists attempted to do it, but mostly by making general statements. The moment energy becomes cheaper, technological change becomes important. Production now requires much more energy.

Foley: Do you think the establishment of the Nobel Memorial Prize in Economics has, on the whole, fostered a better atmosphere for research in economics?

Leontief: You know, there's a problem. I think they'll soon run out of candidates for Nobel Prizes in economics. I think we have already problems now.

Foley: Did the Nobel Prize have any particular impact on your work or your life as a scientist?

Leontief: On my life, some. Not on my work. Naturally, it was easier to get jobs. Not necessarily easier to get financing. Now, for example, I cannot get any financing. So, I suppose my academic life got easier, but, as I said, there is a problem how the Nobel committee can continue. I think they have already begun to shift from theoretical to institutional economists. Now there is a problem because in technical economics at least you can point out some hierarchy, and also major steps forward, breakthroughs, while in institutional economics I don't really see any large breakthroughs. As a matter of fact, I am concerned that economists are not sufficiently interested now in institutional changes brought about by the development of new technologies, which I think is definitely the driving force.

Foley: There's been a lot of discussion about whether economics should take any other science as its model, in particular physics or biology, and, if so, which one. Did you ever think in those terms, that economics should be like a physics of society or a biology of society?

Leontief: I think it doesn't help much. Naturally, mathematical economists like to look to physics. I think it was the Darwinian approach that was really interesting, and I think that in one way the great intellectual revolutionary was Darwin. Incredible revolution, not only in biology, but in the analysis of all living processes. I think Darwin—it was Newton and Darwin who I think were the great contributors to the understanding of social change. Darwinism is very important, although, of course, it is interesting that Darwin was influenced by Malthus. What are you interested in?

Foley: One of the things I've been spending some time on recently is evolutionary modeling of technical change. The issue of global warming evolves over the kind of very long timescale on which changes in technology will be decisive.

Leontief: Oh, yes. I completely agree. Technology is terribly important.

Foley: If you look over a shorter time horizon, substitution of existing technologies might be important, but I think over a long time period, it's going to be the direction of technological change, and the bias of technological change. The question is whether there's any way to control it.

Leontief: Exactly. Not necessarily consciously, but. . . . Now, of course, there is a much closer link between scientific change and technological change. One hardly even can visualize technological change without science, and, with global warming, these things are terribly important. And what can we do? We can do many things. Slow down, for one.

Foley: That's not popular in a world anxious to develop as fast as it can.

Leontief: It is remarkable how technologically backward many less-developed economies are. Once you begin to cut trees, you can do it very quickly.

Foley: If you were encountering a younger scholar who had some innovative but expensive idea like input–output, how would you recommend that person proceed?

Leontief: He has to publish something. I do not know who gets money nowadays. I haven't been following the development of the field recently. I'm, after all, over 90 years old, but, of course, big money is spent not on research, but on data collection. Some people have good ideas and can really do something with the data, but economics has come closer and closer to technology now. To exploit the influence of technological change on economic change, you just can't compute some supply curve; you must really have a mass of information. I wrote up how it can be done, and I nearly succeeded in getting money to do it. My feeling is one could even do some anticipation, prediction, if one had really detailed data. I got in touch with engineering societies, the society of mechanical engineers, and they were ready to provide information. I think this is the future of the work, in the interaction between economics and engineering, science, and the substructure of production.

Foley: Did you ever have any personal or scientific contact with Piero Sraffa, the Anglo-Italian who worked on linear models?

Leontief: No. I never met him. But I think he was a very interesting man. His vision was interesting. In general, I think the input–output analysis is not necessarily linear. I would interpret it as an outgrowth of neoclassical theory. Sraffa was interested in something slightly different, the indirect relationships. I don't insist on linear relationships, only I'm conscious of the fact that dealing with nonlinear systems is terribly complicated; and even in computations, what do mathematicians do? Linearize the system in pieces, and then put it together. This is the way most of us use mathematics in a field in which data is important.

Foley: I was going to ask you about the relationship between production-function analysis and input–output. Production functions seem to have taken over the economics of production.

Leontief: Oh, yes. The production function is too flexible. First of all, continuity is silly. I visualize different methods of production as cooking recipes, including even such things as temperature and so forth, and what must be known in order to be able to cook the dish. This approach might enable us to analyze technological change. The technological production function was essentially an attempt not to go into empirical analysis. You see, given production functions, you don't need that. You guess at a few parameters, instead of having to look in detail at what's happening, and if you try to generalize production functions, it's dangerous, very dangerous.

Foley: What are your thoughts on the advantages and disadvantages of linking sectoral and establishment- or firm-level data?

Leontief: I think the institutional organization of production through the establishment in some way reflects technology, but it is a very delicate situation, because it's not very simple, how economic activities are distributed to different human organizations. It has some relation to what is actually being done, but it's very delicate. Human organizations are very complicated. You can accomplish this linkage in some respects and not some other respects, but I agree with both approaches, particularly since the establishment is not enough. Even establishments are now institutional organizations.

Foley: One practical problem is that, as you disaggregate the input–output structure, you find that one firm begins to appear in several different sectors. This also touches on the theme you were talking about earlier of finance. Finance comes at the firm level.

Leontief: I completely agree, and here, we agree with the need for continuity. Institutional organizations change very easily. So far as top management is concerned, the firm doesn't reflect technology at all. You can have the same corporation making ice cream and making steel. This is, I think, unavoidable, but, possibly because of my interests, I would rather favor establishments first, and then corporations, because the establishment is a homogeneous concept. It is quite an interesting problem.

Foley: Where do you think the future of economics, and macroeconomics in particular, lies?

Leontief: I think problems of income distribution will increase in importance. As I mentioned before, labor will be not so important, and the problem will be just to manage the system. People will get their income allocated through social security—already now we get it through social security, and we try to invent pretexts to provide social security for people. Here, I think, the role of the government will be incredibly important, and those economists who try to minimize the role of the government, I fear, show a superficial understanding of how the economic system works. My feeling is, if we abolished the government now, already there would be complete chaos. Now, planning plays a role, naturally, but I don't emphasize just planning as a role for the government, which is I think extremely important, and its importance is bound to increase because of technological change. If one asks oneself, what will happen to the system if we abolished completely the government, it would be horrible.

Foley: But you think that's particularly true because of the pressures technological change in capitalism are putting on the social fabric, and a weakening of the nexus between labor and income?

Leontief: Oh yes. Absolutely. The labor market is not a sufficient instrument to move from production to consumption.

Foley: I'm just going to ask you one more question. You've been a lifetime participant observer in the subculture of American economics and the larger world of American science and politics. If you were an anthropologist, how would you characterize economists as a tribe or a culture compared to the physicists or biologists?

Leontief: It depends what economists you have in mind. Academic economists are just part of the academic establishment, but I suppose we economists are as indispensable as accountants. In managing a system, you have to have represented the point of view and principle which managers have, and economists are just a particular type of management, if you disregard academic economists, who are a special type.

Foley: Within academics, do you see any difference between economists and their counterparts in science or the engineers that you work with?

Leontief: You see different tendencies in economics. Some prominent economists have just proved a couple of theorems, or codified classical and neoclassical textbooks.

Foley: You're suggesting that economists value classificatory or formal contributions more than finding out something about the world itself?

Leontief: Yes. My observation was a critical one. Particularly since I am interested in society, I see economics as a social science. Certainly economists should contribute something to understanding how human society developed, and here, economists have to cooperate with anthropologists and others.

3

An Interview with Robert E. Lucas, Jr.

Interviewed by Bennett T. McCallum

CARNEGIE MELLON UNIVERSITY

Summer 1998

Bob Lucas is widely regarded as the most influential economist of the past 25–30 years, at least among those working in macro and monetary economics. His work provided the primary stimulus for a drastic overhaul and revitalization of that broad area, an overhaul that featured the ascendance of rational expectations, the emergence of a coherent equilibrium theory of cyclical fluctuations, and specification of the analytical ingredients necessary for the use of econometric models in policy design. These are the accomplishments for which he was awarded the 1995 Nobel Prize in Economic Sciences. In addition, he has made outstanding contributions on other topics—enough, arguably, for another prize. Among these are seminal writings on asset pricing, economic growth and development, exchange-rate determination, optimal fiscal and inflation policy, and tools for the analysis of dynamic recursive models.

Clearly, Bob Lucas is very much a University of Chicago product; he studied there both as an undergraduate and as a Ph.D. student and has been on the faculty since 1975. Also, he has served as chairman of the Chicago Department of Economics and two terms as an editor of the *Journal of Political Economy.* Nevertheless, I and several colleagues at Carnegie Mellon like to point out that Bob was a professor here in the Graduate School of Industrial Administration from 1963 until 1974, during which time he conducted and published the central portions of

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the work for which he was awarded the Nobel Prize. Consequently, I could not resist asking Bob a few questions about his GSIA years in the interview.

Many researchers in the economics profession have been impressed and inspired by Lucas's technical skills, but the clarity and elegance of his writing style also deserve mention, plus his choice of research topics. The latter is reflective of Bob's utter seriousness of purpose. Each of his projects attacks a problem that is simultaneously of genuine theoretical interest and also of considerable importance from the perspective of economic policy. There is nothing frivolous about Lucas's research, as he had occasion to remind me during our interview.

As is well known to those who have been around him, Bob Lucas is a person who never uses three words when one will suffice—but that one will usually be carefully chosen. This characteristic shows up in the interview below. As a departure from standard MD Interview practice, and with the Editor's permission, this interview was conducted at a distance—i.e., via mail and e-mail. It yielded a smaller number of pages than have previous interviews, but I think that readers will find them stimulating. The process of obtaining them was somewhat challenging but highly informative and thoroughly enjoyable for me.

McCallum: Let me begin by asking how and when you got interested in economics, both generally and as the subject for a career.

Figure 3.1 Robert E. Lucas, Jr.

Lucas: When I was seven or eight, my father asked me if I had noticed how many different milk trucks stopped at our block: Darigold delivered to some houses, Carnation to others, and so on. We counted to five or six. He asked me if I thought there were any differences in the milk provided by these dairies. I thought not. He then told me that under socialism only one truck will deliver to all the houses on each block, and the time and gasoline wasted in duplicating routes will be used for something else.

I doubt very much that this was my first discussion of economics, but it is the earliest I can remember. My parents had come of age politically in the 1930s, and the virtues of free markets were not right at the front of their thinking, or mine. We took it for granted that an economic system should be intelligently managed, and we debated every day over the details of how this could and should be done.

As an undergraduate at Chicago in the 1950s, I got the idea that an intellectual career was a possibility, and knew that was what I wanted for myself. In college, these interests and prejudices led me to history. Early in graduate school, I shifted to economics.

McCallum: And how did you happen to go to Chicago as an undergraduate?

Lucas: My alternative was to stay at home and attend the University of Washington in Seattle. Chicago gave me a full-tuition scholarship, which was the ticket I needed to move out on my own. This was something I needed to do.

McCallum: Then as a graduate student in history? Can you tell us a bit about your reasons for shifting to economics?

Lucas: I drifted into economics from economic history, with no idea of what economics is or what economists do. This was just luck, but I soon discovered the essential role that mathematical reasoning played in economics, and it didn't take me long to see that this way of thinking about human behavior was congenial to me.

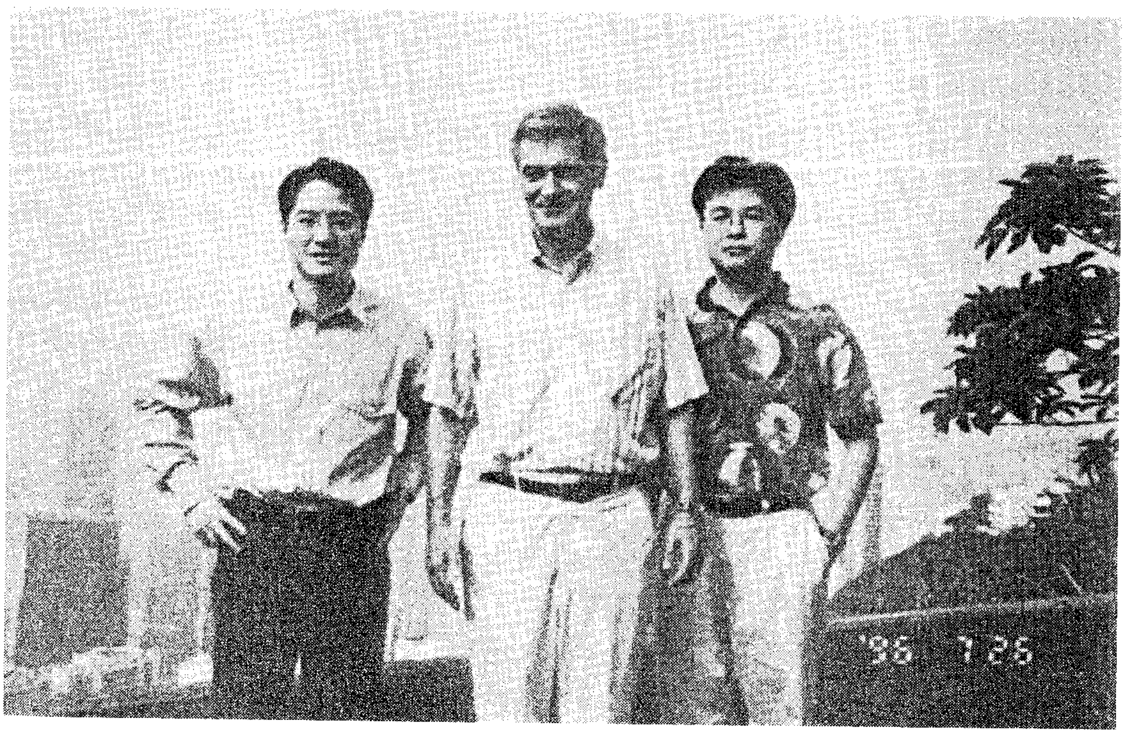

Figure 3.2 Louis Chan, Robert Lucas, and Chi-Wa Yuen at Victoria Peak in Hong Kong.

McCallum: How did you develop your outstanding mathematical tools?

Lucas: It is easy to forget how little math one needed to know to be at the technical end of economics, back in the early 1960s. I had had calculus and differential equations as an undergraduate, before I got into history. Samuelson's *Foundations* taught me (and the rest of my cohort) how people were using math in economics. In my summers as a graduate student, I took a linear algebra course and a rigorous calculus course. I also took the mathematical statistics sequence from Chicago's statistics department. With this background, I have kept learning on my own, and much of the math I use now I picked up since leaving graduate school.

McCallum: While you were a Ph.D. student at Chicago, which faculty members had major influences on your intellectual development? Describe these a bit, please.

Lucas: The biggest influence by far, on me and all my classmates, was Milton Friedman. His two graduate price theory courses were fabulously exciting and valuable: a life-changing experience. But I was a very receptive graduate student and learned a lot from many other people. Al Harberger was doing quantitative general equilibrium modeling then, in a way that still looks quite modern. Martin Bailey, Carl Christ, and Harry Johnson were our other macroeconomics teachers. Gregg Lewis went through his book on unions in an advanced seminar that I learned a lot from.

Among the younger faculty, Zvi Griliches taught econometrics, and encouraged technical types like me. Dale Jorgenson, a visitor in 1962–63, was inspiring to me. Don Bear taught a terrific course in mathematics for economists.

McCallum: Somehow I had the impression that Uzawa influenced you in some way. Is that just completely wrong?

Lucas: Uzawa joined the Chicago faculty the year after I left, so he was not one of my teachers. But I did attend two summer conferences on dynamic theory that Uzawa and David Cass organized, one at Chicago and another at Yale. These involved me in intense interactions with the best young theorists in economics. I liked the idealism and seriousness of the tone Uzawa and Cass set. I was flattered to be included, learned a lot, and gained a lot of confidence.

McCallum: Which workshops did you attend regularly?

Lucas: There were many fewer workshops then than we have now. Everything in econometrics and mathematical theory went on in the Econometrics Workshop. Zvi and Lester Telser ran it, and Merton Miller and Dan Orr from the business school were regulars. I was too. Al Harberger ran the Public Finance Workshop, which all the students working with him (as I was) attended. Gregg Lewis invited me to give a paper at the Labor Workshop, but I was not a regular there.

McCallum: So you did not attend the Money and Banking Workshop?

Lucas: Attendance in workshops then was by invitation, and I was never asked to attend the Money and Banking Workshop. But there was no reason why I should have been. Money and Banking was not one of my prelim fields (those were Econometrics and Public Finance) and I did not work with Friedman.

McCallum: I believe that you became an assistant professor at Carnegie Mellon—then Carnegie Tech—about 1963. Is that approximately correct?

Lucas: Yes. I came to the Graduate School of Industrial Administration —GSIA—in September 1963. Tren Dolbear, Mel Hinich, Mort Kamien, Lester Lave, and Tim McGuire came at the same time. I think we were the first cohort hired by Dick Cyert, then a new dean.

McCallum: How did you get started with rational expectations analysis? Did John Muth have much direct influence on your thinking?

Lucas: Before I left Chicago, Zvi Griliches told me to pay attention to Jack Muth, that he was someone I could learn a lot from. That turned out to be good advice! I learned a lot from Jack, but it was a few years before I appreciated the force of the idea of rational expectations. This happened when I was working on "Investment Under Uncertainty" with Ed Prescott.

McCallum: Do you have any thoughts about the intellectual processes that led Muth to his rational expectations hypothesis?

Lucas: The opening paragraphs of his "Rational Expectations and the Theory of Price Movements" are very informative and interesting. One can see the extent to which Muth was influenced by and was reacting to Herbert Simon's work on behavioral economics, and how this led him to such a radically nonbehavioral hypothesis as rational expectations. (I once tried to discuss this with Herb, thinking of it as an instance of the enormous, productive influence he had on all of us, but he took offense at the suggestion.)

Jack was the junior author in the Holt, Modigliani, Muth, and Simon monograph *Planning Production, Inventories, and Workforce*. This was a normative study—operations research—that dealt with the way managers should make decisions in light of their expectations of future variables, sales, for example. I'm sure it was this work that led Muth to think about expectations at a deeper level than just coming up with regression equations that fit data.

The power of thinking of allocative problems normatively, even when one's aim is explaining behavior and not improving it, was one of the main lessons I learned at Carnegie, from Muth and perhaps even more from Dave Cass. The atmosphere at Chicago when I was a student was so hostile to any kind of planning that we were not taught to think: How

Figure 3.3 Ed Prescott, Tom Sargent, Bob Lucas, and Buz Brock at a conference.

should resources be allocated in this situation? How *should* people use the information available to them to form expectations? But these *should* always be an economist's first questions. My Dad was wrong to think that socialism would deliver milk efficiently, but he was right to think about how milk *should* be delivered.

McCallum: Please describe other aspects of the intellectual atmosphere at GSIA that were important to your professional development.

Lucas: I guess I have already referred to the influences of Herb Simon, Dave Cass, and Ed Prescott in answering your question about Muth's influence. In general, GSIA offered me a nice mix of people whose point of view on economics was pretty close to mine, like Leonard Rapping and Allan Meltzer, and others like Simon, Muth, Cass, and Prescott, to name just a few, who could come at problems from angles I never would have hit on my own.

McCallum: Please describe aspects of the atmosphere at Chicago, after your return in 1974–75, that were important to your continued professional development.

Lucas: At Chicago, I began teaching graduate macroeconomics regularly for the first time in my career. (Allan Meltzer had done this at Carnegie.) This was a stimulus for me. My papers "Understanding Business Cycles" and "Problems and Methods in Business Cycle Theory" came out of the experience of organizing my thoughts on the entire field, the way teaching a graduate course in a top department forces one to do.

McCallum: Your Nobel Prize was awarded for work in reconstructing the fields of macro and monetary analysis so as to incorporate the hypothesis of rational expectations. Before we go on to other interests of yours, are there points regarding this topic that you would like to make? Has the macro profession evolved in a manner that you are pleased with?

Lucas: Like most scientists, I imagine, I tend to be pleased with developments that confirm my prejudices and make my conjectures look good. So I am happy about the successes of general equilibrium theory in macro and sad about the de-emphasis on money that those successes have brought about. Pleasure aside, though, I feel I have learned a huge amount from research in real business cycle theory. I think about the relation of theory to data and about the sources of fluctuations now at an entirely different level from the way I thought 15 years ago.

McCallum: How important quantitatively are technology shocks, in your opinion, in generating business cycles?

Lucas: The answer must depend on which cycles we are talking about. If we are discussing the U.S. Depression in the 1930s or the depression in Indonesia today or Mexico five years ago, I would say that technology shocks are a minor part of the picture. On the other hand, if we are talking about fluctuations in the postwar United States the relative importance of technology and other real shocks is *much* larger, something like 80% of the story.

McCallum: But "technology and other real shocks" would include shocks to preferences, government spending, terms of trade, and possibly other things. How about pure technology shocks—shocks to production functions—in the postwar U.S. context?

Lucas: I don't know how my 80% guess would break down among these and other real shocks. I'm not even sure there is such a thing as a "pure technology shock." I guess for me the central distinction is between shocks that competitive markets can deal with efficiently, without any intervention (all of those on your list, and more) and shocks that need to be offset by a monetary response.

McCallum: In your opinion, is price stickiness an important economic phenomenon?

Lucas: Yes. In practice it is much more painful to put a modern economy through a deflation than the monetary theory we have would lead us to expect. I take this to be what we mean by "price stickiness."

McCallum: There has been some disagreement among monetary economists concerning the most appropriate target variable for the European

Central Bank, with inflation and money growth targets being the leading contenders. What are your views on that issue?

Lucas: That's a classic question for any central bank. I like the policy you've studied of formulating a target for the path of nominal output and then using a slowly reacting feedback rule for the monetary base to keep the system moving toward that target. If you want to replace "nominal output" with "inflation rate," this policy still has a lot of appeal, though less. If you want to replace "monetary base" with "M1," it has even more appeal, to me.

If you replace "monetary base" with "short-term interest rate," you get a version that everyone seems to like nowadays, and I'm willing to get on board myself for pretty much anything that keeps the focus on price stability. But I don't understand how this particular feedback system works, and I am concerned about the kind of bad dynamics that Wicksell, and more recently Peter Howitt, worried about.

McCallum: Do you actually believe that the welfare costs of cyclical fluctuations are as small as indicated in your Jahnsson Lectures, or were these numbers presented mainly as a challenge to the profession to explain?

Lucas: I don't write things I don't believe in just to be provocative! Those estimates may be too small, but if so, it is an honest mistake. The estimates I reported there are the welfare cost of postwar U.S. consumption fluctuations, under the assumption that idiosyncratic risk is perfectly pooled. As I explained in the lectures, the costs of 1930s-level crashes were vastly higher, and were aggravated by the absence of unemployment insurance and other features of a modern welfare system.

The reason these costs came out so small is that they are proportional to the variance of consumption, which is very small in the postwar period in the United States. How can one get large costs from so little variability? No one else has, either, except by assuming enormous risk aversion. Of course, this reduced variability is due at least in part to the sensible monetary policy pursued over these years. My claim is not that monetary instability is incapable of causing great harm, but only that it has not done so over the past 50 years, in the United States.

McCallum: Could you make a few comments on your views regarding microeconomics over the past, say, 25 years?

Lucas: In the past 15 years, microeconomics has come to be synonymous with game theory in many places (not including Chicago!), and that is unfortunate. About 99% of all successful applied economics is still based on the idea of a competitive equilibrium. But game theory *has* given us a language for talking about resource allocation with private information and about issues of reputation that represents a huge advance over anything that you and I learned in graduate school.

McCallum: Some other major contributions of yours have concerned asset pricing theory, economic growth and development, and the role of economic theory in econometrics and policy analysis. Could you please tell us how you were led into each of them?

Lucas: The origin of my asset pricing paper makes the best story. I was interviewing Pentti Kouri, then a job-seeking new MIT Ph.D., in my office in Chicago. Kouri didn't want to waste our half hour talking about Chicago winters, so he asked me: "How would you price assets in the following economy?" and then went on to describe the model that is treated in my paper. I went to the blackboard and began writing Bellman equations and clearing markets, and the fact that you didn't need to know the value function to get a very tractable functional equation for prices fell right out in a few minutes. Kouri was not interested in collaborating, so I wrote up these results and others myself.

McCallum: What about your increased emphasis on growth and development? Did that stem partly from the Jahnsson Lecture numbers or had you been interested in this area all along?

Lucas: I taught an undergraduate elective in economic development at Carnegie Mellon, and have been interested in this area as long as I can remember. But my research is guided more by my hunches as to where I might be able to make some progress than anything else. I found myself slipping into the same old ruts in thinking about business cycles, and thought it would be good to think about something else.

McCallum: Your writing is regarded by many in the profession as quite elegant. Do you work hard at your writing?

Lucas: Thank you for the compliment. I revise a lot, though I think of that more as an effort to get the logic straight than as an attempt at style. I also read a lot of people who are *really* good writers, and I'm sure something rubs off.

McCallum: How did you manage to give up smoking?

Lucas: Well, I started smoking when I was 13 and quit when I was 56, so I'm not ready to set up as an adviser on this problem. I quit cold turkey, with the help of nicotine patches. Fear, nagging, and social stigma were all contributing factors.

McCallum: You and Paul Romer both made outstanding contributions to growth theory during the 1980s. Were you Paul's dissertation supervisor? Could you tell us a bit about your interactions on this topic?

Lucas: In teaching macroeconomics, I have been treating a many-country version of Solow's model as a (tentative) model of development for many years. Paul was certainly exposed to this set of problems in my class. But the increasing returns-externalities model that Paul developed in his thesis was entirely his, and new to me. Sherwin Rosen and Ted Schultz told Paul about Allyn Young's work, but I had never heard of that, either.

The model in Romer's thesis raises novel technical problems, since it does not converge to any steady state or balanced path. Jose Scheinkman helped him on this, and I believe chaired his thesis committee as well.

McCallum: Do you have any interest in working for a few years in an economic policy making position? Do you think that one or two years in such a position tends to improve or worsen an economist's subsequent academic work?

Lucas: Back in the late sixties, when George Schultz was Nixon's Secretary of Labor, Schultz asked me to work as an adviser to him. The job was then held by my friend Jack Gould, and it was an interesting position because Nixon was looking to Schultz for help on a much wider range of economic questions than just labor issues. Later Schultz moved to a more central job at OMB, and if I had taken the job I would have moved with him. Schultz called me in person, impressing my secretary at GSIA enormously, and for that matter (why be blasé?) impressing me too! But flattered or not, I was excited about my research at that time and didn't want to interrupt my work with a stay in Washington. I declined.

Do I regret this decision? When I turned the job down, Arthur Laffer accepted it. You never know about such things, but my guess would be that I, Art, and the U.S. economy were all better off as a result, and I can take some pleasure in my role in helping to locate a Pareto-dominant decision.

McCallum: How about writing a regular column on economics for a newspaper or popular magazine? Would you have any interest in such an undertaking?

Lucas: Maybe someday, but not now. I like the sense of discovery and intellectual progress that I can get from doing technical economics. In order to get this sense, one needs to spend a lot of time facing problems one doesn't understand and will probably never understand. This is hard to do, and as you get older and more famous you get more interesting and pleasant excuses to avoid doing it. The last thing I need is more such excuses.

5

An Interview with Franco Modigliani

Interviewed by William A. Barnett

UNIVERSITY OF KANSAS

and

Robert Solow

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

November 5–6, 1999

Franco Modigliani's contributions in economics and finance have transformed both fields. Although many other major contributions in those fields have come and gone, Modigliani's contributions seem to grow in importance with time. His famous 1944 article on liquidity preference has not only remained required reading for generations of Keynesian economists but has become part of the vocabulary of all economists. The implications of the life-cycle hypothesis of consumption and saving provided the primary motivation for the incorporation of finite lifetime models into macroeconomics and had a seminal role in the growth in macroeconomics of the overlapping generations approach to modeling of Allais, Samuelson, and Diamond. Modigliani and Miller's work on the cost of capital transformed corporate finance and deeply influenced subsequent research on investment, capital asset pricing, and recent research on derivatives. Modigliani received the Nobel Memorial Prize for Economics in 1985.

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Barnett was on the faculty at Washington University in St. Louis when this article was written.

In macroeconomic policy, Modigliani has remained influential on two continents. In the United States, he played a central role in the creation of the Federal Reserve System's large-scale quarterly macroeconometric model, and he frequently participated in the semiannual meetings of academic consultants to the Board of Governors of the Federal Reserve System in Washington, D.C. His visibility in European policy matters is most evident in Italy, where nearly everyone seems to know him as a celebrity, from his frequent appearances in the media. In the rest of Europe, his visibility has been enhanced by his publication, with a group of distinguished European and American economists, of "An Economists' Manifesto on Unemployment in the European Union," which was signed by a number of famous economists and endorsed by several others.

This interview was conducted in two parts on different dates in two different locations, and later unified. The initial interview was conducted by Robert Solow at Modigliani's vacation home in Martha's Vineyard. Following the transcription of the tape from that interview, the rest of the interview was conducted by William A. Barnett in Modigliani's apartment on the top floor of a high-rise building overlooking the Charles River near Harvard University in Cambridge, Massachusetts. Those concluding parts of the interview in Cambridge continued for the two days of November 5–6, 1999, with breaks for lunch and for the excellent espresso coffee prepared by Modigliani in an elaborate machine that would be owned only by someone who takes fine coffee seriously. Although the impact that Modigliani has had on the economics and finance professions is clear to all members of those professions, only his students can understand the inspiration that he has provided to them. However, that may have been adequately reflected by Robert Shiller at Yale University in correspondence regarding this interview, when he referred to Modigliani as: "my hero."

Figure 5.1 Franco Modigliani (formal portrait photo, date unknown).

Barnett: In your discussion below with Solow, you mentioned that you were not learning much as a student in Italy and you moved to the United States. Would you tell us more about when it was that you left Italy, and why you did so?

Modigliani: After the Ethiopian war and the fascist intervention in the Spanish Civil War, I began to develop a strong antifascist sentiment and the intent to leave Italy, but the final step was the close alliance of Mussolini with Hitler, which resulted in anti-Semitic laws, which made it impossible to live in Italy in a dignified way. At that time I had already met my future wife, Serena, and we were engaged. Her father had long been antifascist and preparing to leave Italy. When those laws passed, we immediately packed and left Italy for France. We spent 1939 in France, where we made arrangements to leave for the United States. We left in August 1939 for the United States on the very day of the famous pact between Hitler and Stalin, which led to what was the later attack by Germany on Russia. I came to the United States with no prior arrangements with a university. I wanted very much to study economics, and I received a scholarship from the New School for Social Research, thanks in part to the fact that the school had many prominent intellectual antifascists, and one of them, the renowned antifascist refugee, Max Ascoli, helped me to get the scholarship.

Barnett: Franco, I understand that after you had left Italy you returned to Italy to defend your dissertation. Can you tell us whether there were any risks or dangers associated with your return to defend your dissertation?

Modigliani: Yes, it is true that when we left from Rome to Paris, I had finished all of my examinations to get my degree, but I had not yet defended my thesis. In July of 1939, before leaving Paris for the United States, I wanted to have all my records complete, and I decided to go back to Rome to defend my thesis. That operation was not without dangers, because by that time I could have been arrested. I had kept my contacts with antifascist groups in Paris, so there was the possibility of being harassed or being jailed. Fortunately nothing happened. My father-in-law was very worried, and we had made arrangements for him to warn us of any impending perils by a code. The code was all about Uncle Ben. If he was not feeling well, we should be ready to go. If he was dead, we should leave instantly. We never needed to use that code, but I felt relieved when I was able to complete my thesis, and then late in August we left for the United States.

Barnett: The famous painter and sculptor, Amedeo Modigliani, was born in Livorno, Italy, in 1884 and died in Paris in 1920. Was he related to your family?

Modigliani: There is no known relation.

Solow: Franco, the first thing I want to talk about is your 1944 *Econometrica* paper, "Liquidity Preference and the Theory of Interest and Money." When you were writing it, you were 25 years old?

Modigliani: Yes, about that. I hadn't studied very much in Italy of any use. There was no useful teaching of economics. What was taught there was something about the corporate state. So all I picked up was at the New School of Social Research in New York with the guidance of Jacob Marschak.

Solow: When was that?

Modigliani: That was 1939 through 1941–42.

Solow: So your main guide was Jascha Marschak.

Modigliani: Jascha Marschak was my mentor. We studied Keynes and the *General Theory* in classes with Marschak. I attended two different seminars, but in addition received a lot of advice and support from him. He suggested readings and persuaded me of the importance of mathematical tools, acquired by studying some calculus and understanding thoroughly the great book of the day by R.G.D. Allen, *Mathematical Analysis for Economists*, and studying some serious statistics (attending Abraham Wald lectures at Columbia); and last but not least he sponsored my participation in a wonderful informal seminar, which included besides Marschak people like Tjalling Koopmans and Oskar Lange. But unfortunately, to my great sorrow, Marschak in 1942 left New York for Chicago. He was replaced by another notable mind, Abba Lerner. I had a lot of discussions with him about Keynes. At that time, Abba Lerner was pushing so-called functional finance.

Solow: Yes, the famous "steering wheel."

Modigliani: Functional finance led me to the 1944 article. In functional finance, only fiscal policy could have an impact on aggregate demand. Therefore, it was an economy that belonged to what I later called the Keynesian case. I tried to argue with Lerner and to have him understand that Keynes did not say that. That was the origin of the 1944 article, trying to put Keynes in perspective.

Solow: Now, with Marschak or Lerner, had you read any of the earlier mathematical models of Keynesian economics, such as Hicks's, of course, or Oscar Lange's articles?

Modigliani: Well, I was familiar with the literature, and of course it had hit me, as is visible in my articles. Hicks's article on Keynes and the classics was a great article, and it was the starting point of my article, except that in Hicks the rigidity of wages was just taken as a datum, and no consideration was given to alternatives. It was just the one system, and fixed forever.

Solow: What he later called "a fixed-price model."

Figure 5.2 In Stockholm in December 1991, at a reunion of the Nobel Prize winners. From left to right are Kenneth Arrow, Franco Modigliani, Paul Samuelson, and Robert Solow.

Modigliani: Fixed price so that he could deal in nominal terms as though they were real. Money supply is both nominal and real.

Barnett: Prior to your arrival at MIT, you were at a number of American universities, including the New School for Social Research, the New Jersey College for Women (now Douglas College), Bard College of Columbia University, the University of Chicago, the University of Illinois, Carnegie Institute of Technology (now Carnegie Mellon University), and Northwestern University. Prior to MIT, what were the most productive periods for you in the United States?

Modigliani: The most productive period was unquestionably the eight years or so (1952–60) spent at Carnegie Tech, with an exceptionally stimulating group of faculty and students, led by two brilliant personalities, the dean, G.L. Bach, and Herbert Simon, working on the exciting task of redesigning the curriculum of modern business schools, and writing exciting papers, some of which were to be cited many years later in the Nobel award: the papers on the life-cycle hypothesis and the Modigliani and Miller papers.

Barnett: I've heard that while you were teaching at the New School, you had an offer from the Economics Department at Harvard University, which at that time was by far the best economics department in the United States. But to the surprise of the faculty, you turned down the offer. Why did you do that?

Modigliani: Because the head of the department, Professor Burbank, whom I later found out had a reputation of being xenophobic and anti-Semitic, worked very hard and successfully to persuade me to turn down the offer, which the faculty had instructed him to make me. He explained that I could not possibly hold up against the competition of bright young people like Alexander, Duesenberry, and Goodwin. "Be satisfied with being a big fish in a small pond." Actually it did not take me too long to be persuaded. Then, after my meeting with Burbank, I had scheduled a lunch with Schumpeter, Haberler, and Leontief, who had expected to congratulate me on joining them. But they literally gave me hell for letting Burbank push me over. Nevertheless, in reality I have never regretted my decision. Harvard's pay at that time was pretty miserable, and my career progressed much faster than it would have, if I had accepted the offer.

Barnett: I understand that the great football player Red Grange (the Galloping Ghost) had something to do with your decision to leave the University of Illinois. What happened at the University of Illinois that caused you to leave?

Modigliani: In short it was the "Bowen Wars," as the episode came to be known in the profession. The president of the university brought in a new wonderful dean, Howard H. Bowen, to head the College of Commerce, which included the Department of Economics. But the old and incompetent faculty could not stand the fact that Bowen brought in some first-rate people like Leo Hurwicz, Margaret Reid, and Dorothy Brady. The old faculty was able to force Bowen out, as part of the witch hunt that was going on under the leadership of the infamous Senator Joseph McCarthy. The leader of the McCarthyite wing of the elected trustees was the famous Red Grange. I then quit in disgust with a blast that in the local press is still remembered: "There is finally peace in the College of Commerce, but it is the peace of death." My departure was greeted with joy by the old staff, proportional to their incompetence. But 40 years later, the university saw fit to give me an honorary degree!

Solow: Well, how do you look at the 1944 paper now? Would you change it drastically if you were rewriting it?

Modigliani: Yes! Not really in content, but in presentation. That is what I have been doing in my autobiography. I am revising that paper completely and starting from an approach which I think is much more useful. I am starting from the notion that both the classics and Keynes take their departure from the classical demand for money model, which is one of the oldest and best-established paradigms in economics. The demand for money is proportional to the value of transactions, which at any point can be approximated as proportional to nominal income (real income multiplied by the price level). The nominal money supply is exogenous. Therefore, the money market must reach an equilibrium through changes *in nominal income*. Nominal income is the variable that clears the money market.

Where then is the difference between classical and Keynesian economics? Simple: The classics assumed that wages were highly flexible and output fixed by full employment (clearing of the labor market). Thus the quantity of money had *no effect on output but merely determined the price level, which was proportional to the nominal money supply* (the *quantity theory of money*). On the other hand, Keynes relied on the *realistic assumption* that wages are rigid (downward). That is, they do not promptly decline in response to an excess supply of labor. Workers do not slash their nominal wage demands, and firms do not slash their wage offers, when unemployment exceeds the frictional level. What, then, clears the money market? Again, it is a decline in nominal income. But since prices are basically fixed, the decline must occur in real income and particularly in employment. When there is insufficient nominal money supply to satisfy the full employment demand for money, the market is cleared through a decline in output and employment. As Keynes said, the fundamental issue is that prices are not flexible.

Solow: Not instantly flexible.

Modigliani: That's right. They may very slowly respond, but a very slow adjustment of the real money supply can't produce the expansion of the real money supply needed to produce a rapid reestablishment of equilibrium. What, then, reestablishes equilibrium? Since wages and prices are fixed, the decline in nominal income can only occur through a decline in real income and employment. There will be a unique level of real income that clears the money market, making the money demand equal to the money supply.

Solow: No mention of the interest rate?

Modigliani: The interest rate comes next, as a link in the equilibrating mechanism. In fact, Keynes's unique achievement consisted not only in showing that unemployment is the variable that clears the money market; he also elaborated the mechanism by which an excess demand for money causes a decline of output and thus in the demand for money, until the demand matches the given nominal money supply. In the process of developing this mechanism, unknown to the classics, he created a new branch of economics: *macroeconomics.*

Macroeconomics, or the mechanisms through which money supply determines output (employment), stands on four basic pillars, with which, by now, most economists are familiar: (1) liquidity preference, (2) the investment function, (3) the consumption or saving function, and (4) the equality of saving and investment (properly generalized for the role of government and the rest of the world).

Liquidity preference is not just the fact that the demand for money depends on the interest rate; it brings to light the profound error of classical monetary theory in assuming that the price of money is its purchasing power over commodities (baskets per dollar) and that, therefore, a shortage of money must result in a prompt rise in its purchasing power (a fall in the price level). In reality, of course, money has many prices, one in terms of every commodity or instrument for which it can be exchanged. Among these instruments, by far the most important one is "*money in the future,*" *and its price is money tomorrow per unit of money today*, which is simply $(1 + r)$, where r is the relevant interest rate.

Furthermore, experience shows that financial markets are very responsive to market conditions: Interest rates (especially in the short run) are highly flexible. So, if money demand is short of supply, the prompt reaction is not to liquidate the warehouse or skimp on dinner, forcing down commodity prices, but a liquidation in the portfolio of claims to future money (or a rise in borrowing spot for future money), leading to a rise in the terms of trade between money today and tomorrow—that is, a rise in interest rates. And this starts the chain leading to lower output through a fall in investment, a fall in saving, and thus in income and employment. It is this fall, together with the rise in interest rates, that reduces the demand for money till it matches the supply.

Solow: Yes, so the interest rate is a key price.

Modigliani: Actually it's one plus the interest rate. If you are short of money, and the system does not have enough money, the first thing it attempts is to get more spot money by either liquidating assets or by borrowing, which is borrowing money today against money tomorrow. Interest rates rise, reducing investment, and then comes the great equation: investment equals savings—an identity that is so far from the classical view that in the beginning they would not even believe it.

Solow: Right.

Modigliani: And income then adjusts so that the demand for money is finally equated to its supply. This will result in both a higher interest rate and a lower income. The two together will serve to equate the money demanded with the given supplied. And how much must interest rates rise or income decline? That depends upon the parameters of the system (demand elasticities).

Solow: But exactly! And Keynes's fundamental contribution then was to say that it's not the interest rate and the price level, but interest and real output.

Modigliani: Yes, precisely. I think this is the way to look at it. It is the output that adjusts demand and supply.

Solow: What you just described is maybe a different way of telling a story and saying what's important, but it's not fundamentally different from what's in the 1944 paper or in the IS–LM apparatus.

Modigliani: Absolutely. But I suggest that to think of unemployment not as a transitory disease, but as a variable that clears the money market, is a useful and significant innovation. Unemployment is an equilibrating mechanism. It seems like a dysfunction, since we think that full employment is what an economy should produce. But unemployment is a systematic feature of an economy relying on money to carry out transactions. To avoid unemployment, it takes continuous care by either setting the right money supply or fixing the right interest rate. There is no other way to get full employment. There is nothing automatic about it.

Barnett: Some work on monetary policy has emphasized the possibility that the monetary transmission mechanism works through a credit channel. The implication of this research is that monetary policy may affect consumer and business spending, because it affects the quantity of credit available to agents, rather than the interest rate. This work is often motivated by the observation that the interest elasticity of spending is too low to explain the large impact monetary policy appears to have on real economic activity. Do you think there is an important credit channel for monetary policy?

Modigliani: My attitude toward this question, about which I have done much thinking and some writing, is that in the end it is an empirical question, not an a priori question. It is entirely credible that monetary policy may work, in part, through changing the volume of credit supplied by banks in the form of commercial loans, as well as its cost. That way it may have the same effects as acting through market interest rates, but without necessarily producing large movements in interest rates. I think that future research will help in sorting this out. But the answer will not be perpetual, since the answer depends upon the structure of financial intermediaries and the laws regulating them.

Solow: Now one of the questions I've wanted to ask you, which I think you've already now answered, is what does it mean to be a Keynesian today? But I take it that what you just said is the essence of Keynesian economics, and by that definition you would describe yourself as a Keynesian.

Modigliani: Absolutely. I consider myself a Keynesian. Now as I think it over in this light, I consider Keynesian economics to be a great revolution, having a really tremendous impact, with tremendously novel ideas. Again I consider myself a Keynesian in the very fundamental sense that I know the system does not automatically tend to full employment without appropriate policies. Price flexibility will not produce full employment, and therefore unemployment is always due to an insufficiency of real money. But it must be recognized that there are certain circumstances under which the Central Bank may not be able to produce the right real money supply. For instance, the case of Italy was interesting. Unemployment there was due to the fact that real wages were too high, in the sense that they resulted in substantially negative net exports at full employment. Under those circumstances, if the Central Bank expanded the money supply to create more aggregate demand and employment, the balance of trade would run into nonfinancable deficits, and the Central Bank would be forced to contract. So, you're not always able to increase the real money supply. But that's not the case in Europe, where the money supply could be easily increased and the unemployment is largely due to insufficient real money supply.

Solow: You know I rather agree with you about that.

Modigliani: Interest rates are too high. There is not enough real money being supplied. This is not being understood. Keynes is not being understood. That's the main source of European unemployment. Some improvements in the labor market, such as more wage flexibility, could help, but would not get very far without a significant rise in aggregate demand (which at present would not significantly increase the danger of inflation).

Solow: Right, but you cannot get European central bankers to see that.

Modigliani: In Europe they accept the view that long-lasting unemployment contributes to the current high level because it reduces search by the unemployed, *causing long-term unemployment.* No, sir. That's a consequence of the too restrictive policy.

Barnett: You have argued that stock market bubbles sometimes are produced by misinterpreting capital gains as a maintainable component of current returns (a permanent addition to current profits). Do you believe that that phenomenon is going on now, or do you believe that current stock market valuations are consistent with the fundamentals?

Modigliani: I am very much interested and concerned about bubbles, and I believe that bubbles do exist. They are one of the sources of malfunctioning of the market mechanism. The essence of these bubbles is that indeed capital gains get confused with profits, and this results in the stock becoming more attractive, so people bid up the price, which produces more capital gains, and so on. I believe that indeed the stock market in the United States is in the grips of a serious bubble. I think the overvaluation of stocks is probably on the order of 25% or so, but, by the nature of the process, it is not possible to predict just when the whole thing will collapse. In my view, there will be a collapse because if there is a marked overvaluation, as I hold, it cannot disappear slowly.

Barnett: How does your research help us understand what has occurred over the past few years in the volatile economies of East Asia?

Modigliani: My view is that what has happened in East Asia is very much in the nature of a bubble, where expected high returns have attracted capital. The attraction of capital has held up exchange rates, permitting large deficits in the balance of trade; the influx of capital has supported the exchange rate making capital investment more attractive. So, you have a spiral until people realize that those returns are really not maintainable. I think it is important for the future of the international situation to set up systems under which bubbles cannot develop or are hard to develop, such as requiring reserves against short-term capital movements.

Solow: Now, I want to ask what's your current belief about wage behavior? How would you today model the behavior of nominal or real wages?

Modigliani: This, I think, is one of the fundamental issues that we face today, because in my model the wage and the price level are exogenous. Why is the price level exogenous? Because prices fundamentally depend upon wages, and wages are not flexible. Wages are certainly not responding mechanically to unemployment. So what do we do about wages? Well, I do think that some of this rigidity of wages is historical. It's very likely that in the nineteenth century the situation was different. In that century there was a greater role for competitive industries such as agriculture. In any event, the wage is *the* fundamental component of the price level. What's going to determine wages? Well, we've come to a difficult period, mostly since unions in Europe have been very powerful. They've become unreasonable and pushed for higher and higher wages, nominal wages. But my view is that in the long run we'll have to reach the point at which the wage, the nominal wage, is negotiated in a general simultaneous settlement of wages and prices. Now that's what's happened in Italy.

Solow: Say some more about that.

Modigliani: What saved Italy from the tremendously disastrous situation that existed just before devaluation was the fact that workers agreed to fix nominal wages for three years together with a price program, so that real as well as nominal wages were set. To me, that is the future because I do not know what else to say about the price picture.

Solow: What you're saying is that Keynes's remark that labor cannot determine the real wage may turn out to be false because institutions change and permit bargaining over the real wage.

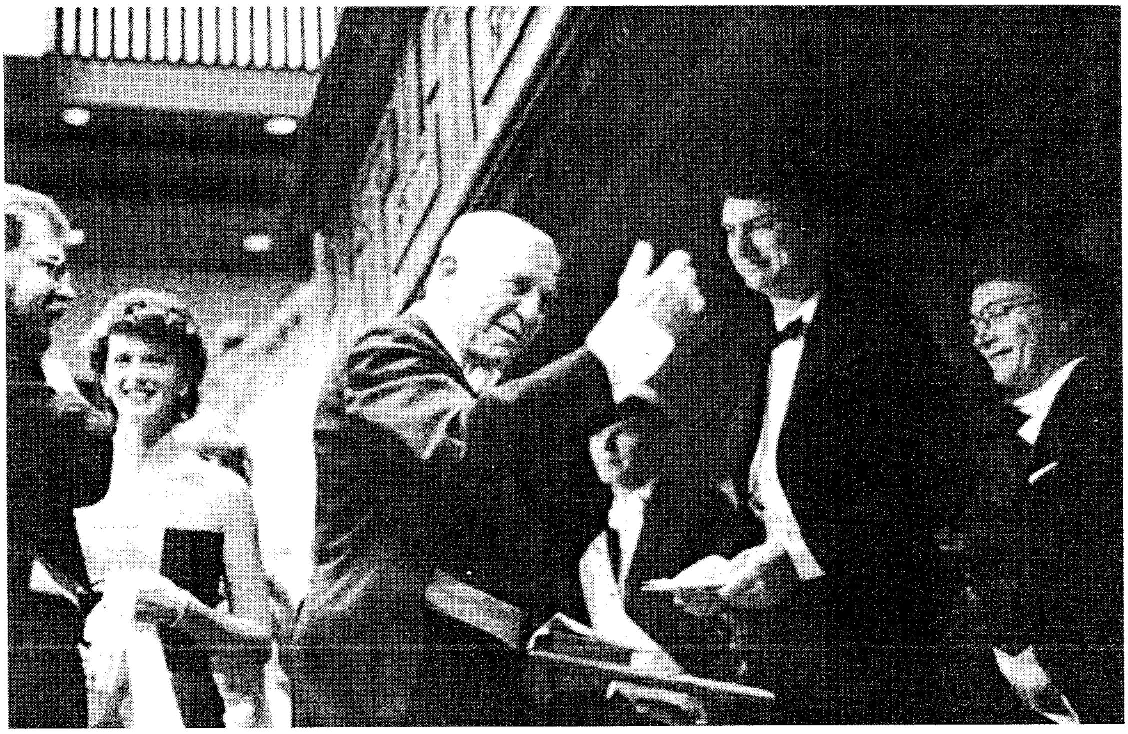

Figure 5.3 In Stockholm in 1985, after receiving the Nobel Prize.

Modigliani: That's right. Yes, yes.

Solow: What's your current feeling about NAIRU, the nonaccelerating-inflation rate of unemployment?

Modigliani: I think it is true that if unemployment gets too low, then you will have accelerating inflation, not just higher inflation but higher rate of change of inflation. But I do not believe that if unemployment gets very high, you'll ever get to falling nominal wages. You may get very low acceleration of wages, or you may get to the point at which wages don't move. But I don't believe that high unemployment will give us negative wage changes.

Solow: We might even get falling wages for a while, but you would surely not get accelerating reductions in wages. No believer in the NAIRU ever wants to speak about that side of the equation.

Modigliani: That's right. So I think that these views are consistent, in the sense that left alone there may be a tendency for the system to be always in inflation. The Central Bank can pursue full employment policy without simultaneously being concerned that it must keep the inflation rate at zero. What I regard as a real tragedy today is the fact that all of a sudden the European banks and many other banks have shifted to the single-minded target of price stability. I think that is one of the sources of the European tragedy, in contrast with the shining performance of the United States. No concern whatever about employment.

Solow: Well, they argue that there is nothing they can do about it, but you and I think that's fundamentally wrong and simply a way of avoiding responsibility.

Modigliani: Exactly. And on the contrary, I think they should say the priority target should be "first unemployment," though price stability is also very important. There are situations in which indeed you may either have to accommodate inflation or stop it at the cost of temporary unemployment. I think *then* I would accept unemployment as a temporary state to stop an inflationary spiral. But to say that price stability is *the* only target, I think is wrong.

Solow: So, you don't believe that the NAIRU in France is 13% today?

Modigliani: Absolutely not, absolutely not. Nor do I believe that here in this country it is as low as 4%. I have great doubts about the stability of NAIRU but even more about the appropriate way to estimate it.

Barnett: While I was an undergraduate student at MIT, I was permitted to take your graduate course in corporate finance. I shared with the graduate students in the class the view that the Modigliani–Miller work on the cost of capital was dramatically raising the level of sophistication of the field of corporate finance. What motivated you to enter that area of research, and what earlier research inspired you?

Modigliani: Ever since my 1944 article on Keynes, I have become interested in empirical tests of the Keynesian structure. As everybody knows, one of the key components of that structure is the investment function, which explains investment in terms of the interest rate, seen as the cost of capital, the cost of funds invested. I was then under the influence of the views of the corporate finance specialists that the cost of funds depended upon the way in which the firm was financed. If you issued stock, then the cost of that would be the return on equity, which might be 10%, but if you used bonds, the cost would be their interest rate, which might be only 5%. That sort of answer didn't seem to me to be very convincing. In the end, what was the cost of capital: 5% or 10%? To an economist it could not be rational to say that the required return was 5% if you chose to finance the project by debt and 10% if you chose equity. After listening to a paper by David Durand suggesting (and then rejecting) the so-called "entity theory" of valuation, I gradually became convinced of the hypothesis that market value should be independent of the structure of financing, and was able to sketch out a proof of the possibility of arbitraging differences in valuation that are due only to differences in the liability structure. This result later became part of the proof of the Modigliani–Miller theorem. In essence, the market value of liabilities could not depend on its structure, because the investor could readily reproduce any leverage structure through personal lending or borrowing (as long as there was no tax impediment). As a consequence, there was no difference between the use of equity and debt funds. Even though debt had a lower apparent cost, it increased the required return on equity, and the weighted average of the two would be unaffected by the composition. I unveiled my proof in a class in which Miller happened to be an auditor. He was convinced instantly and decided to join me in the crusade to bring the truth to the heathens.

The theorem, which by now is well known, was proven very laboriously in about 30 pages. The reason for the laboriousness was in part because the theorem was so much against the grain of the teachings of corporate finance—the art and science of designing the "optimal capital structure." We were threatening to take the bread away, and so, we felt that we had to give a "laborious" proof to persuade them. Unfortunately, the price was paid by generations of students that had to read the paper; I have met many MBA students that remember that paper as a torture, the most difficult reading in the course. It's too bad because, nowadays, the theorem seems to me to be so obvious that I wonder whether it deserves two Nobel Prizes. All that it really says is that (with well-working markets, rational-return-maximizing behavior for any given risk, and no distorting taxes) the value of a firm—its market capitalization of all liabilities—must be the value of its assets. The composition of the claims can change (equity, debt, preferred, convertible preferred, derivatives, and what not), but the aggregate value of the claims cannot change. It is the value of the assets. Of course, it is true that this conclusion implies that the way that you finance investment is immaterial. It follows that in estimating the required return, the cost of capital, we do not have to bother with the details of the composition of the financing. In that sense, Jorgenson is right.

In later years, the Modigliani–Miller theorem has provided the foundation for the work on derivatives, such as options. All of that work assumes that the underlying value of the firm is independent of its current liability structure. But let me remind you of the assumptions needed to establish the theorem and, in particular, the assumption of no distorting effects of taxation on the net-of-tax amount received by an individual from one dollar of before-interest corporate earnings. If there are such effects, then the situation is more complicated, and in fact in this area there is a disagreement between Miller and me. I believe that taxes can introduce a differential advantage between different kinds of instruments, while Miller thinks not. But I should add that even though, in principle, taxation could affect the comparative advantage of different instruments, Miller and I agree that, with the current system of taxation, the differences are unlikely to be appreciable.

Solow: Now I want to make room here for you to make a brief comment about real business-cycle theory. If you look at macroeconomic theory today, what has replaced the Keynesian economics that you and I both accept is, in the minds of young people, real business-cycle theory.

Modigliani: I have no difficulty in believing that business cycles can exist in the real economy. You don't need money, and I myself built models of that kind, when it was fashionable. There was Hicks's article on the business cycle, and then Sidney Alexander had a very interesting article on the introduction of a bound that can permit you to get a cycle without money. But I think that has little to do with Keynesian unemployment. In the thirties, for instance, there was a tremendous depression that I think was caused by an insufficiency of real money. That was a horrible error made by the Federal Reserve, a point on which Milton Friedman and I agree. There was a serious shortage of real money and irresponsible behavior in letting the money supply shrink. I think that unemployment is mostly due to the rigidity of wages and to the shifting conditions. Therefore, there is the need for adjustment by the Central Bank, and the adjustment must be fast enough.

Solow: What's distinctive about real business-cycle theory is not just that it says that the monetary mechanism has nothing to do with cycles, but that business cycles, as we observe them, are optimal reactions of the economy to unexpected shocks to technology and tastes and things like that.

Modigliani: Yes, yes. Well, of course, much of this goes back to rational expectations, and my attitude toward rational expectations is that it is a wonderful theory. It is indeed the crowning of the classical theory. The classical theory spoke of optimal response to expectations. Lucas and company add optimal formation of expectations. From that point of view, I am satisfied that that is what economic theory would say; and I am proud because I contributed an important concept, which is, I think, at the essence of rational expectations, namely, the existence of expectations that map into themselves.

Solow: Self-validating.

Modigliani: Self-validating. No, not "self-validating"—"internally consistent."

Solow: That's what I meant by self-validating. There is one set of expectations that is self-validating, not that every set is self-validating.

Modigliani: That's right, because usually self-validating means that it happens because you expect it. This is not the case. In addition, I believe that it is *not* a description of the world. I don't believe that the world is behaving rationally in that extreme sense, and there are many circumstances under which the model will not apply. In particular, I do not believe that that model justifies the conclusion that anything the government does is bad.

Solow: It adds variance and the mean is already right, so discretionary policy is bad.

Modigliani: It creates noise, so therefore whatever government does is bad. Wage rigidity to me is a perfect example contradicting the above conclusion. Nor can you dispose of wage rigidity with the hypothesis of staggered contract. If that contract is rational, then wages *are* rigid and one better take this into account in theory and policy; or the staggered contract is not rational and in a Chicago world, it should have long ago disappeared.

Barnett: Robert Barro, who I understood was a student in some of your classes, advocates a version of Ricardian equivalence that appears to be analogous in governmental finance to the Modigliani–Miller theorem in corporate finance and in some ways to your life-cycle theory of savings with bequests. In fact, he sometimes speaks of one of your classes at MIT that he attended in 1969 as being relevant to his views. But I understand that you do not agree with Barro's views of government finance. Why is that?

Modigliani: Barro's Ricardian equivalence theorem has nothing in common with the Modigliani–Miller proposition, *except the trivial relation that something doesn't matter*. In the Modigliani–Miller theorem, it is capital structure, and in the Barro theorem it is government deficit. In my view, Barro's theorem, despite its elegance, has *no* substance. I don't understand why so many seem to be persuaded by a proposition whose proof rests on the incredible assumption that everybody cares about his heirs as if they were himself. If you drop that assumption, there is no proof based on rational behavior, and the theorem is untenable. But that kind of behavior is very rare and can't be universal. Just ask yourself what would happen with two families, when one family has no children and another family has 10. Under Ricardian equivalence, both families would be indifferent between using taxes or deficit financing. But it is obvious that the no-children family would prefer the deficit, and the other would presumably prefer taxation. Indeed, why should the no-children family save more, when the government runs a deficit? I am just sorry that any parallel is made between Modigliani–Miller and Ricardian equivalence.

I have in fact offered concrete empirical evidence, and plenty of it, that government debt displaces capital in the portfolio of households and hence in the economy. My paper is a bit old, though it has been replicated in unpublished research. But there is an episode in recent history that provides an excellent opportunity to test Barro's model of no burden against the life-cycle hypothesis measure of burden—the displacement effect. I am referring to the great experiment unwittingly performed by Reagan cutting taxes and increasing expenditure between 1981 (the first Reagan budget) and 1992. The federal debt increased $3\frac{1}{4}$ times or from 7% of initial private net worth to about 30%. In the same interval, disposable (nominal) personal income grew 117% [all data from the

Economic Report of the President, 1994, Table B-112 and B-28]. According to my model, private wealth is roughly proportional to net-of-tax income, and hence it should also have increased by 117%, relative to the initial net worth. But net national wealth (net worth less government debt, which represents essentially the stock of productive private capital) should have increased 117% minus the growth of debt, or 117 − 23 = 94% (of initial net worth). The 23% is the crowding-out effect of government debt, according to the life-cycle hypothesis. The actual growth of national wealth turns out to be 88%, pretty close to my prediction of 94%. On the other hand, if the government debt does not crowd out national wealth, as Barro firmly holds, then the increase in the latter should have been the same as that of income, or 117% compared with 88%. Similarly for Barro the growth of private net worth should be the growth of income of 117% *plus* the 23% growth of debt, or 140%. The actual growth is 111%, very close to my prediction of 117% and far from his, and the small deviation is in the direction opposite to that predicted by Barro.

Figure 5.4 In Stockholm in 1985, after receiving the Nobel Prize. Left to right are Sergio Modigliani (son), Leah Modigliani (granddaughter), Franco Modigliani, Queen Silvia of Sweden, King Gustav Adolph of Sweden, Serena Modigliani (wife), Suzanne Modigliani (wife of Sergio), Andre Modigliani (son), and Julia Modigliani (granddaughter).

Why do so many economists continue to pay so much attention to Barro's model over the life-cycle hypothesis?

Solow: Okay, let's move on. I think the next thing we ought to discuss is your Presidential Address to the American Economic Association and how, in your mind, it relates both to the 1944 paper that you've been talking about and your later work.

Modigliani: As I said before about Keynes, I stick completely to my view that to maintain a stable economy you need stabilization policy. Fiscal policy should, first of all, come in as an automatic stabilizer. Secondly, fiscal policy might enter in support of monetary policy in extreme conditions. But normally we should try to maintain full employment with savings used to finance investment, not to finance deficits. We should rely on monetary policy to ensure full employment with a balanced budget. But one thing I'd like to add is that it seems to me that in the battle between my recommendation to make use of discretion (or common sense) and Friedman's recommendation to renounce discretion in favor of blind rules (like 3% money growth per year), my prescription has won hands down. There is not a country in the world today that uses a mechanical rule.

Solow: It's hard to imagine in a democratic country.

Modigliani: There is not a country that doesn't use discretion.

Solow: You know, I agree with you there. How would you relate the view of your Presidential Address to monetarism? It was stimulated by monetarism, in a way. How do you look at old monetarism, Milton Friedman's monetarism, now?

Modigliani: If by monetarism one means money matters, I am in agreement. In fact, my present view is that *real* money is the most important variable. But I think that a rigid monetary rule is a mistake. It is quite possible that in a very stable period, that might be a good starting point, but I would certainly not accept the idea that that's the way to conduct an economic policy in general.

Solow: And hasn't Milton sometimes, but not always, floated the idea that he can find no interest elasticity in the demand for money.

Modigliani: I've done several papers on that subject and rejected that claim all over the place. Anybody who wants to find it, finds it strikingly—absolutely no problem.

Solow: You had a major involvement in the development of the Federal Reserve's MPS quarterly macroeconometric model, but not lately. How do you feel about large econometric models now? There was a time when someone like Bob Hall might have thought that that's the future of macroeconomics. There is no room for other approaches. All research will be conducted in the context of his model.

Modigliani: Right. Well, I don't know. I imagine that, first of all, the notion of parsimoniousness is a useful notion, the notion that one should try to construct models that are not too big, models that are more compact in size. I think that at the present time these models are still useful. They still give useful forecasts and especially ways of gauging responses to alternative policies, which is most important. But under some international circumstances, there is no room for domestic monetary policy in some countries. In such a country, an econometric model may not be very helpful. But an econometric model would be somewhat useful in considering different fiscal policies.

Barnett: Has mentoring younger economists been important to you as your fame grew within the profession?

Modigliani: My relation with my students, which by now are legion, has been the best aspect of my life. I like teaching but I especially like working with students and associating them with my work. Paul Samuelson makes jokes about the fact that so many of my articles are coauthored with so many people that he says are unknown—such as Paul Samuelson himself. The reason is that whenever any of my research assistants has developed an interesting idea, I want their names to appear as coauthors. Many of my "children" now occupy very high positions, including Fazio, the Governor of the Bank of Italy, Draghi, the Director General of the Treasury of Italy, Padoa Schioppa, a member of the Directorate of the European Central Bank, and Stan Fischer, Joe Stiglitz, and several past and current members of the Federal Reserve Board. All have been very warm to me, and I have the warmest feeling for them.

Solow: Now if you were giving advice to a young macroeconomist just getting a Ph.D., what would you say is the most fertile soil to cultivate in macroeconomics these days?

Modigliani: I think that these days, in terms of my own shifts of interest, I've been moving toward open-economy macroeconomics and especially international finance. It's a very interesting area, and it's an area where wage rigidity is very important. Now the distinction becomes very sharp between nominal wage rigidity and real wage rigidity.

Solow: Explain that.

Modigliani: With nominal wage rigidity, you will want floating exchange rates. With real rigidity, there's nothing you can do about unemployment. I've been looking at the experiences of countries that tried fixing exchange rates and countries that tried floating exchange rates, and I am finding that both experiences have not been good. Europe has been doing miserably.

Barnett: You have been an important observer of the international monetary system and the role of the United States and Europe in it, and I believe that you have supported the European Monetary Union. Would you comment on the EMS and the future of the international monetary system, in relation to what you think about the recent financial crises and the role that exchange rates have played in them?

Modigliani: Yes, I have been a supporter of the euro, but to a large extent for its political implications, peace in Europe, over the purely economic ones. However, I have also pointed out the difficulties in a system which will have fixed exchange rates and how, for that to work, it will require a great deal of flexibility in the behavior of wages of individual countries having differential productivity growth and facing external shocks. I have also pointed out that the union was born under unfavorable conditions, as the role of the central bank has been played, not legally but *de facto*, by the Bundesbank, which has pursued consistently a wrong overtight monetary policy resulting in high European unemployment. It has reached 12% and sometimes even higher, and that policy is now being pursued to a considerable extent by the European Central Bank, which is making essentially the same errors as the Bundesbank. This does not promise too much for the near future.

Solow: What we're going to do now is switch over to talking about the life-cycle theory of savings, and what I'd like you to do is comment on the simplest life-cycle model, the one that you and Albert Ando used for practical purposes, with no bequests, et cetera.

Modigliani: Well, let me say that bequests are not to be regarded as an exception. Bequests are part of the life-cycle model. But it is true that you can go very far with assuming no bequests, and therefore it's very interesting to follow that direction. The model in which bequests are unimportant does produce a whole series of consequences which were completely unrecognized before the Modigliani–Brumberg articles. There were revolutionary changes in paradigm stemming from the life-cycle hypothesis. Fundamentally, the traditional theory of saving reduced to: the proportion of income saved rises with income, so rich people (and countries) save; poor people dissave. Why do rich people save? God knows. Maybe to leave bequests. That was the whole story, from which you would get very few implications and, in particular, you got the implication that rich countries save and poor countries dissave, an absurd concept since poor countries cannot dissave forever. No one can. But from the life-cycle hypothesis, you have a rich set of consequences. At the *micro* level, you have all the consequences of "Permanent Income," including the fact that consumption depends upon (is proportional to) *permanent* income, while saving depends basically on transitory income: The high savers are not the rich, but the temporarily rich (i.e., rich relative to their own normal income).

The difference between life-cycle and permanent income is that the latter treats the life span as infinite, while in the life-cycle model, lifetime is finite. For the purpose of analyzing short-term behavior, it makes no difference whether life lasts 50 years or forever. So you do have fundamentally the same story about the great bias that comes from the standard way of relating saving to current family income. But, in fact, in reality it does make a difference what the variability of income is in terms of short term versus long term. The marginal propensity to save of farmers is much higher than that of government employees, not because farmers are great savers, but because their income is very unstable. Other consequences that are very interesting include the fact, found from many famous surveys, that successive generations seem to be less and less thrifty, that is, save less and less at any given level of income. These conclusions all are consequences of the association between current and transitory income.

Then you have consequences in terms of the behavior of saving and wealth over the lifetime, and here is where the difference between life cycle and permanent income become important. With the life-cycle hypothesis, saving behavior varies over the person's finite lifetime, because with finite life comes a life cycle of income and consumption: youth, middle age, children, old age, death, and bequests. That's why there is little saving when you are very young. You have more saving in middle age, and dissaving when you are old. With infinite life, there is no life cycle. Aggregate saving reflects that life cycle and its interaction with demography and productivity growth, causing aggregate saving to rise with growth, as has been shown with overlapping generations models. All that has been shown to receive empirical support.

Solow: Dissaving and old age, as well?

Modigliani: Right. Now let me comment on that. Some people have spent a lot of time trying to show that the life-cycle model is wrong because people don't dissave in old age. That is because the poor guys have just done the thing wrong. They have treated Social Security contribution as if it were a sort of income tax, instead of mandatory saving, and they have treated pension as a handout, rather than a drawing down of accumulated pension claims. If you treat Social Security properly, measuring saving as income earned (net of personal taxes) minus consumption, you will find that people dissave tremendous amounts when they are old; they largely consume their pensions, while having no income.

Solow: They are running down their Social Security assets.

Modigliani: In addition to running down their Social Security assets, they also are running down their own assets, but not very much. Somewhat. But, if you include Social Security, wealth has a tremendous hump. It gets to a peak at the age of around 55–60 and then comes down quickly. All of these things have been completely supported by the evidence. Now, next, you do not need bequests to explain the existence of wealth, and that's another very important concept. Even without bequests, you can explain a large portion of the wealth we have. Now that does not mean there are no bequests. There are. In all my papers on the life-cycle hypothesis, there is always a long footnote that explains how to include bequests.

Solow: How you would include it, yes.

Modigliani: In such a way that it remains true that saving does not depend upon current income, but on life-cycle income. That ensures that the ratio of bequeathed wealth to income tends to remain stable, no matter how much income might rise. It is also important to recognize the macro implications of the life cycle, which are totally absent in the permanent-income hypothesis, namely, that the saving rate depends not on income, but on income growth. The permanent income hypothesis has nothing really to say; in fact, it has led Friedman to advance the wrong conclusion, namely that growth *reduces saving*. Why? Because growth results in expectations that future income will exceed current income. But with finite lifetime, terminating with retirement and dissaving, growth generates saving.

Consider again the simplified case of no bequests. Then each individual saves zero over its life cycle. If there is no growth, the path of saving by age is the same as the path of saving over life: it aggregates to zero. But if, say, population is growing, then there are more young in their saving phase than old in the dissaving mode, and so, the aggregate saving ratio is positive and increasing with growth. The same turns out with productivity growth, because the young enjoy a higher life income than the retired. Quite generally, the life-cycle model implies that aggregate wealth is proportional to aggregate income: hence the rate of growth of wealth, which is saving, tends to be proportional to the rate of growth of income. This in essence is the causal link between growth rate and saving ratio, which is one of the most significant and innovative implications of the life-cycle hypothesis.

Barnett: There has been much research and discussion about possible reforms or changes to the Social Security System. What are your views on that subject?

Modigliani: The problems of the Social Security System are my current highest interest and priority, because I think its importance is enormous; and I think there is a tragedy ahead, although in my view we can solve the problem in a way that is to everyone's advantage. In a word, we need to abandon the pay-as-you-go system, which is a wasteful and inefficient system, and replace it with a fully funded system. If we do, we should be able to reduce the Social Security contribution from the 18% that it would have to be by the middle of the next century, to below 6% using my approach, and I have worked out the transition. It is possible to go from here to there without any significant sacrifices. In fact, it can be done with no sacrifice, except using the purported surplus to increase national saving rather than consumption. And given the current low private saving rate and huge (unsustainable) capital imports, increasing national saving must be considered as a high priority.

Barnett: Are there any other areas to which you feel you made a relevant contribution that we have left out?

Modigliani: Perhaps that dealing with the effects of inflation. At a time when, under the influence of rational expectations, it was fashionable to claim that inflation had no *real* effects worth mentioning, I have delighted in showing that, in reality, it has extensive and massive real effects; and they are not very transitory. This work includes the paper with Stan Fisher on the effects of inflation, and the paper with Rich Cohen showing that investors are incapable of responding rationally to inflation, basically because of the (understandable) inability to distinguish between nominal and Fisherian real interest rates. For this reason, inflation systematically depresses the value of equities.

I have also shown that inflation reduces saving for the same reason. Both propositions have been supported by many replications. In public finance, the calculation of the debt service using the nominal instead of the real rate leads to grievous overstatement of the deficit-to-income ratio during periods of high inflation, such as the mid-seventies to early eighties in the presence of high debt-to-income ratios. In corporate finance, it understates the profits of highly levered firms.

Figure 5.5 At the Kennedy Library in Boston in the spring of 1998, talking with the King of Spain.

Barnett: Your public life has been very intense, at least starting at some point in your life. I presume that you do not agree with Walras, who believed that economists should be technical experts only, and should not be active in the formation of policy. Would you comment on the role of economists as "public servants"?

Modigliani: I believe that economists should recognize that economics has two parts. One is economic theory. One is economic policy. The principles of economic theory are universal, and we all should agree on them, as I think we largely do as economists. On economic policy, we do not necessarily agree, and we should not, because economic policy has to do with value judgments, not about what is true, but about what we like. It has to do with the distribution of income, not just total income. So long as they are careful not to mix the two, economists should be ready to participate in policy, but they should be careful to distinguish what part has to do with their value judgment and what part with knowledge of the working of an economy.

Barnett: You have been repeatedly involved in advocating specific economic policies. Were there instances in which, in your view, your advice had a tangible impact on governments and people.

Modigliani: Yes, I can think of several cases. The first relates to Italy and is a funny one. Through the sixties and seventies, Italian wage contracts had an escalator clause with very high coverage. But in 1975, in the middle of the oil crisis, the unions had the brilliant idea of demanding a new type of escalator clause in which an x% increase in prices would entitle a worker to an increase in wages not of x% of *his* wage but of x% of the *average* wage—the same number of liras for everyone! And the high-wage employers went along with glee! I wrote a couple of indignant articles trying to explain the folly and announcing doomsday. To my surprise, it took quite a while before my Italian colleagues came to my support. In fact, one of those colleagues contributed a "brilliant" article suggesting that the measure had economic justification, for, with the high rate of inflation of the time, all real salaries would soon be roughly the same, at which time it was justified to give everyone the same cost-of-living adjustment! It took several years of economic turmoil before the uniform cost-of-living adjustment was finally abolished and its promoters admitted their mistake. It took until 1993 before the cost-of-living adjustment was abolished all together.

A second example is the recommendations in the 1996 book by two coauthors and me, *Il Miracolo Possibile* [The Achievable Miracle], which helped Italy to satisfy the requirement to enter the euro. This, at the time, was generally understood to be impossible, because of the huge deficit, way above the permissible 3%. We argued that the deficit was a fake, due to the use of inflation-swollen nominal interest rates in the presence of an outlandish debt-to-income ratio ($1\frac{1}{4}$), but the target was achievable through a drastic reduction of inflation and corresponding decline in nominal rates. This could be achieved without significant real costs by programming a minimal wage and price inflation through collaboration of labor, employer, and government. It worked, even beyond the results of the simulations reported in the book! And Italy entered the euro from the beginning.

A third example is my campaign against European unemployment and the role played by a mistaken monetary policy. "An Economists' Manifesto on Unemployment in the European Union," issued by me and a group of distinguished European and American economists, was published a little over a year ago. Although it is not proving as effective as we had hoped, it is making some progress.

Finally, I hope that our proposed Social Security reform will have a significant impact. Here the stakes are truly enormous for most of the world, but the payoff remains to be seen.

6

An Interview with Milton Friedman

Interviewed by John B. Taylor

STANFORD UNIVERSITY

May 2, 2000

"His views have had as much, if not more, impact on the way we think about monetary policy and many other important economic issues as those of any person in the last half of the twentieth century." These words in praise of Milton Friedman are from economist and Federal Reserve Chair Alan Greenspan. They are spoken from a vantage point of experience and knowledge of what really matters for policy decisions in the real world. And they are no exaggeration. Many would say they do not go far enough.

It is a rare monetary policy conference today in which Milton Friedman's ideas do not come up. It is a rare paper in macroeconomics in which some economic, mathematical, or statistical idea cannot be traced to Milton Friedman's early work. It is a rare student of macroeconomics who has not been impressed by reading Milton Friedman's crystal-clear expositions. It is a rare democrat from a formerly communist country who was not inspired by Milton Friedman's defense of a market economy written in the heydays of central planning. And it is a rare day that some popular newspaper or magazine around the world does not mention Milton Friedman as the originator of a seminal idea or point of view.

Any one of his many contributions to macroeconomics (or rather to monetary theory, for he detests the term macroeconomics) would be an extraordinary achievement. Taken together, they are daunting:

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- permanent income theory;
- natural rate theory;
- the case for floating exchange rates;
- money growth rules;
- the optimal quantity of money;
- the monetary history of the United States, especially the Fed in the Great Depression, not to mention contributions to mathematical statistics on rank-order tests, sequential sampling, and risk aversion, and a host of novel government reform proposals from the negative income tax, to school vouchers, to the flat-rate tax, to the legalization of drugs.

Milton Friedman is an economist's economist who laid out a specific methodology of positive economic research. Economic experts know that many current ideas and policies—from monetary policy rules to the earned-income tax credit—can be traced to his original proposals. He won the Nobel Prize in Economics in 1976 for "his achievements in the field of consumption analysis, monetary history and theory and for his demonstration of the complexity of stabilization policy." Preferring to stay away from formal policymaking jobs, he has been asked for his advice by presidents, prime ministers, and top economic officials for many years. It is in the nature of Milton Friedman's unequivocally stated views that many disagree with at least some of them, and he has engaged in heated debates since graduate school days at the University of Chicago. He is an awesome debater. He is also gracious and friendly.

Born in 1912, he grew up in Rahway, New Jersey, where he attended local public schools. He graduated from Rutgers University in the midst of the Great Depression in 1932. He then went to study economics at the University of Chicago, where he met fellow graduate student Rose Director whom he later married. For nearly 10 years after he left Chicago, he worked at government agencies and research institutes (with one year visiting at the University of Wisconsin and one year at the University of Minnesota) before taking a faculty position at the University of Chicago in 1946. He remained at Chicago until he retired in 1977 at the age of 65, and he then moved to the Hoover Institution at Stanford University. I have always found Milton and Rose to be gregarious, energetic people, who genuinely enjoy interacting with others, and who enjoy life in all its dimensions, from walks near the Pacific Ocean to surfs on the World Wide Web. The day of this interview was no exception. It took place on May 2, 2000, in Milton's office in their San Francisco apartment. The interview lasted for two and a half hours. A tape recorder and some economic charts were on the desk between us. Behind Milton was a floor-to-ceiling picture window with beautiful panoramic views of the San Francisco hills and skyline. Behind me were his bookcases stuffed with his books, papers, and mementos.

The interview began in a rather unplanned way. When we walked into his office Milton started talking enthusiastically about the charts that were on his desk. The charts—which he had recently prepared from data he had downloaded from the Internet—raised questions about some remarks that I had given at a conference several weeks before—which he had read about on the Internet. As we began talking about the charts, I asked if I could turn on the tape recorder, since one of the topics for the interview was to be about how he formulated his ideas—and a conversation about the ideas he was formulating right then and there seemed like an excellent way to begin the interview. So I turned on the tape recorder, and the interview began. Soon we segued into the series of questions that I had planned in advance (but had not shown Milton in advance). We took one break for a very pleasant lunch and (unrecorded) conversation with his wife Rose before going back to "work." After the interview, the tapes were transcribed and the transcript was edited by me and Milton. The questions and answers were rearranged slightly to fit into the following broad topic areas:

- money growth, thermostats, and Alan Greenspan;
- causes of the great inflation and its end;
- early interest in economics;
- graduate school and early "on-the-job" training;
- permanent income theory;
- the return of monetary economics;
- fiscal and monetary policy rules;
- the use of models in monetary economics;
- the use of time-series methods;
- real business-cycle models, calibration, and detrending;
- the natural rate hypothesis;
- rational expectations;
- the role of debates in monetary economics;
- capitalism and freedom today;
- monetary unions and flexible exchange rates.

Money Growth, Thermostats, and Alan Greenspan

Friedman: [*Referring to the charts in Figures 6.1 and 6.2*] I thought that you'd be interested in these charts. Don't you think it's as if the Fed has installed a new and improved thermostatic controller in the 1990s![1]

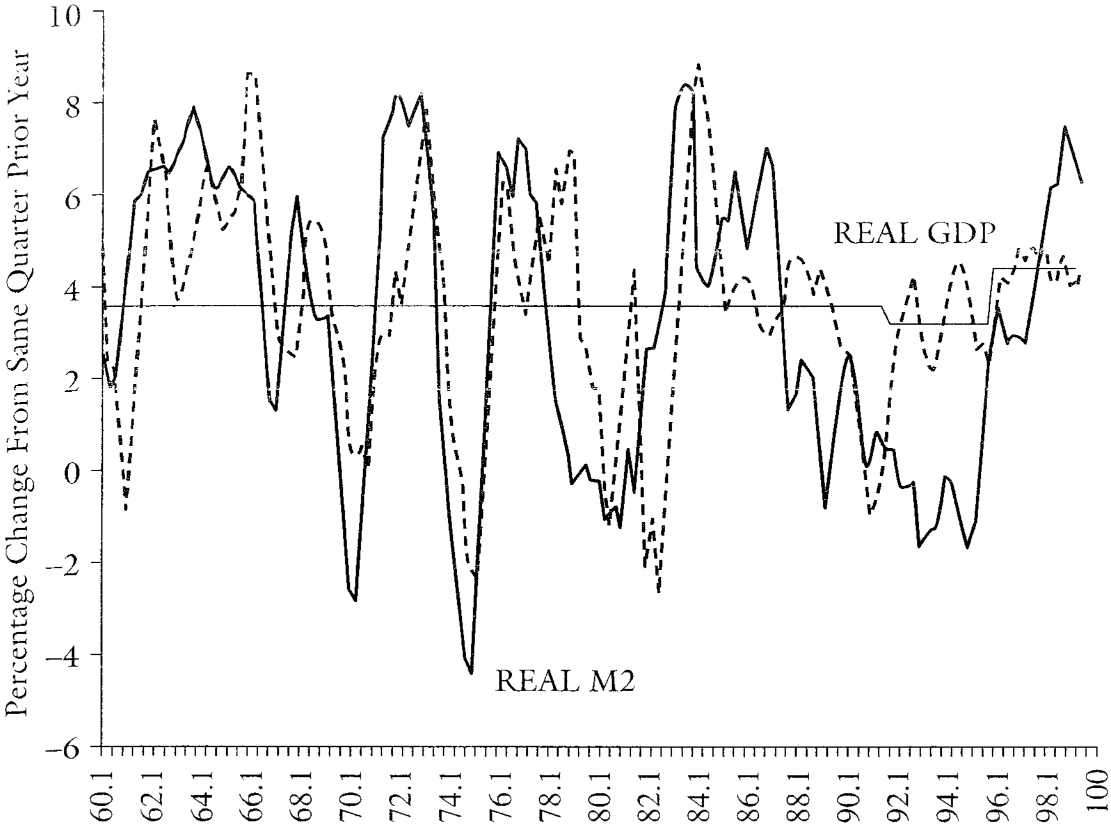

Figure 6.1 Year-to-year change in U.S. real M2 and real GDP, 1960.1–1999.3.
Source: Milton Friedman, February 20, 2000.

Taylor: I can see that there is a change in the relationship between money growth and real GDP and that the size of fluctuations in the economy has diminished greatly. There is much greater stability starting in the early 1980s.

Friedman: The change in stability really comes in 1992.

Taylor: Isn't 1982 the best break point?

Friedman: I think 1992 is the break. [*Referring to the charts in Figure 6.2*] Here are the charts that show the velocity of M1, M2, and M3 against the logarithmic trend.

Taylor: One reason to focus on 1982 is that it was the beginning of an expansion. There are also statistical tests that several people have done to test when the size of the fluctuations changed. Most say that it is in the early 1980s. Since then, the fluctuations in real GDP seem smaller. There is only one recession in 1991 and that is pretty small.

Friedman: [*Pointing to the dip in real GDP growth in 1990–91*] But this looks like a pretty big recession.

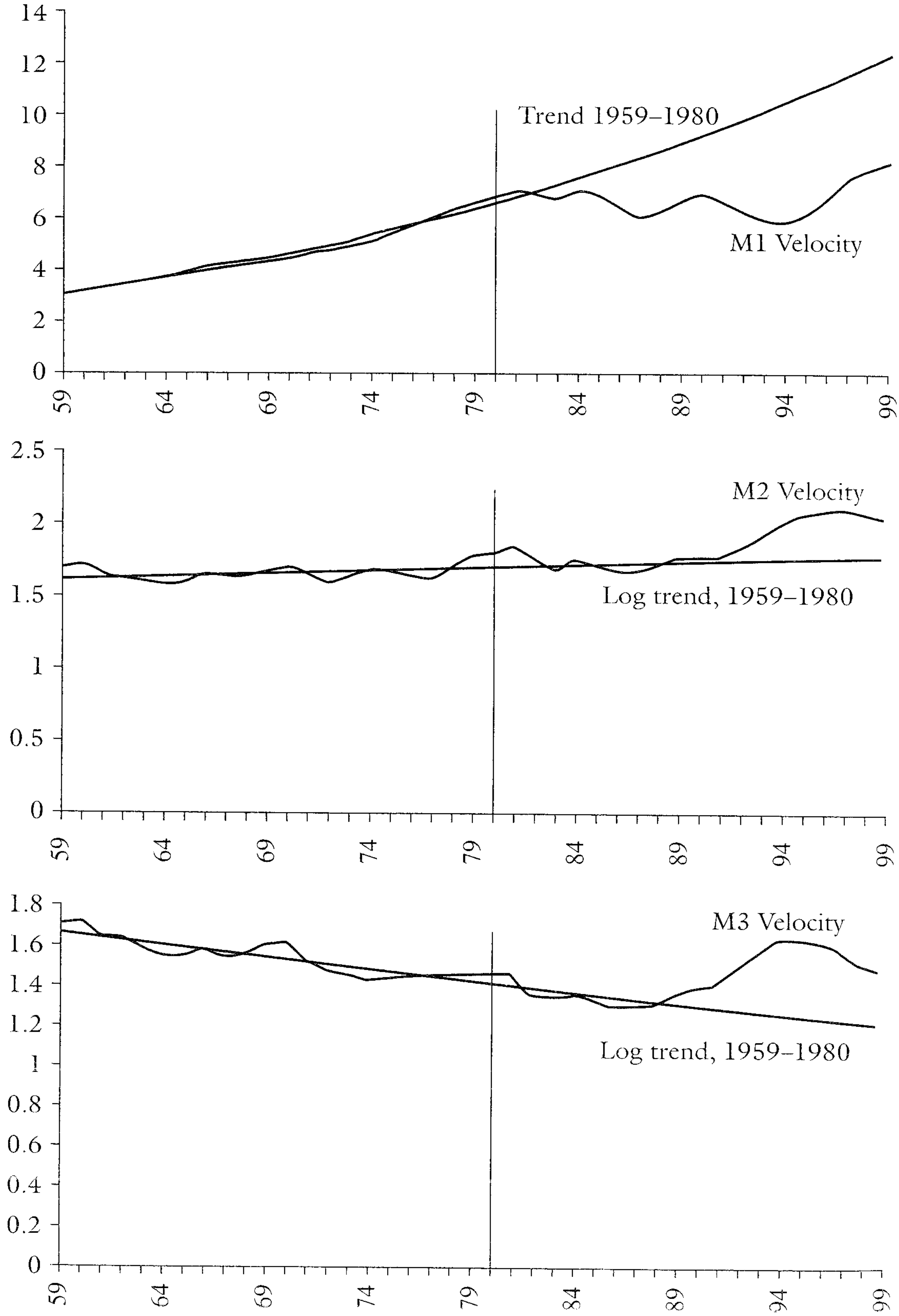

Figure 6.2 Velocity of M1, M2, M3, and log trends based on data from 1959 to 1980, annual data, 1959–99.
Source: Milton Friedman, April 30, 2000.

Taylor: Well, whatever the break point is, why do you think things have changed? Why, as you put it, does the Fed seem to be operating the monetary-policy thermostatic regulator so much better now? What do you think the reason is?

Friedman: I'm baffled. I find it hard to believe. They haven't learned anything they didn't know before. There's no additional knowledge. Literally, I'm baffled.

Taylor: What about the idea that they have learned that inflation was really much worse than they thought in the late 1970s, and they therefore put in place an interest-rate policy that kept inflation in check and reduced the boom/bust cycle?

Friedman: I believe that there are two different changes. One is a change in the relative value put on inflation control and economic stability and that did come in the eighties. The other is the breakdown in the relation between money and GDP. That came in the early nineties, when there was a dramatic reduction in the variability of GDP. What I'm puzzled about is whether, and if so how, they suddenly learned how to regulate the economy. Does Alan Greenspan have an insight into the movements in the economy and the shocks that other people don't have?

Taylor: Well, it's possible.

Friedman: Another explanation is that the information revolution has enabled enterprises to manage inventories so much better, as you pointed out in your recent discussion. But inventories can't be the answer because the same thing has happened to noninventories.

Taylor: I agree with that. If you look at final sales, you see the same change in stability, unless you really want to focus on very-short-term wiggles, such as the quarterly rates of change in real GDP during an expansion.

Friedman: And it may get big again. It may be a statistical artifact. They may have somehow changed their methods. There have been significant changes in estimation.

Taylor: Yes, but going back to the possibility that the Fed has more knowledge, do you think that they have learned more about controlling liquidity or money while at the same time recognizing the fact that there are these shifts in velocity?

Friedman: But then again, if you look at these shifts in velocity, they don't come until 1992.

Taylor: Well, what about this one?

Friedman: That's M1, but, all along, M2 has been the preferred aggregate exactly because of this change, which was the result of eliminating the prohibition on paying interest on demand deposits. So I don't think you can explain it through velocity. It looks as if somehow in

1992—1991–92—they were able to install a good thermostat instead of a bad one. Now, is Alan Greenspan a good thermostat compared to other Fed chairmen? That's hard to believe.

Causes of the Great Inflation and its End

Taylor: Hard to believe, yeah. Well, let's go back to an earlier period when things did not look so good. In recent years, there has been a lot of interest in what caused the Great Inflation of the 1970s and what caused its end. Why did inflation start to rise in the late 1960s and 1970s in the United States?

Friedman: Yes, the Great Inflation. The explanation for that is fundamentally political, not economic. It really had its origin in Kennedy's election in 1960. He was able to take advantage of the noninflationary economic conditions he inherited to "get the economy moving again." With zero inflationary expectations, monetary and fiscal expansions affected primarily output. The delayed effect on prices came only in the mid-sixties and built up gradually. Already by then, Darryl Francis of the St. Louis Fed was complaining about excessive monetary growth. Inflation was slowed by a mini-recession but then took off again when the Fed overreacted to the mini-recession. In the seventies, though I hate to say this, I believe that Arthur Burns deserves a lot of blame, and he deserves the blame because he knew better. He testified before Congress that, if the money supply grew by more than 6 or 7% per year, we'd have inflation, and during his regime it grew by more than that. He believed in the quantity theory of money but he wasn't a strict monetarist at any time. He trusted his own political instincts to a great degree, and he trusted his own judgment. In 1960, when he was advising Nixon, he argued that we were heading for a recession and that it was going to hurt Nixon very badly in the election, which is what did happen. And Nixon as a result had a great deal of confidence in him.

From the moment Burns got into the Fed, I think politics played a great role in what happened. So far as Nixon was concerned, there is no doubt, as I know from personal experience. I had a session with Nixon sometime in 1970—I think it was 1970, might have been 1971—in which he wanted me to urge Arthur to increase the money supply more rapidly [*laughter*] and I said to the President, "Do you really want to do that? The only effect of that will be to leave you with a larger inflation if you do get reelected." And he said, "Well, we'll worry about that after we get reelected." Typical. So there's no doubt what Nixon's pleasure was.

Taylor: Do you think Burns was part of the culture of the times in that he put less emphasis on inflation, or that he was willing to risk some inflation to keep unemployment low, based on the Philips curve?

Friedman: Not at all. You read all of Arthur's writings up to that point and one of his strongest points was the avoidance of inflation. He was not part of that Keynesian group at all. In fact, he wrote against the Keynesian view. However, it did affect the climate of opinion in Washington, it did affect what activities of the Fed were viewed favorably and unfavorably, and therefore it did affect it that way, but not through his own beliefs of the desirability of inflation.

Taylor: Another thing that people say now is that Burns was as confused as other people were about potential GDP, and that he thought the economy was either below capacity or that it was capable of growing more rapidly than it was. Do you think that was much of a factor?

Friedman: I don't think that was a major factor. I think it may have been a factor.

Taylor: Mainly political?

Friedman: Yeah.

Taylor: What about the end of the Great Inflation? It lasted beyond Burns's time. We had G. William Miller and then Paul Volcker.

Friedman: Well, there's no doubt what ended it. What ended it was Ronald Reagan. If you recall the details, the election was in 1980. In October of 1979, Paul Volcker came back from a meeting in Belgrade, in which the United States had been criticized, and he announced that the Fed would shift from using interest rates as its operating instrument to using bank reserves or base money. Nonetheless, the period following that was one of very extreme fluctuations in the quantity of money. The purpose of the announcement about paying attention to the monetary aggregates was to give Volcker a shield behind which he could let interest rates go.

[*Pointing to Figure 6.1*] That's the period, here . . . ups and downs. (The picture of the nominal money supply is very much the same as for the real money supply.) They did step on the brake, and in addition, sometime in February 1980, Carter imposed controls on consumer credit. When the economy went into a stall as we were approaching the election, the Fed stepped on the gas. In the five months before the election, the money supply went up very rapidly. Paul Volcker was political, too. The month after the election, the money supply slowed down. If Carter had been elected, I don't know what would have happened. However, Reagan was elected, and Reagan was determined to stop the inflation and willing to take risks. In 1981, we got into a severe recession. Reagan's public-opinion ratings went down, way down. I believe no other president in the postwar period would have accepted that without bringing pressure on the Fed to reverse course. That's the one key step: Reagan did not. The recession went on in 1981 and 1982. In 1982, finally Volcker turned around and started to raise the money supply and at that point the recession came to an end and the economy started expanding.

Taylor: Your explanations of both the start and end of the Great Inflation are very much related to changes in people in leadership positions, as distinct from changes in ideas. What you seem to be saying is that it was mostly Burns, Nixon, Reagan. Could you comment on that a little bit?

Friedman: I may be overemphasizing Burns's role. I certainly am not overemphasizing Reagan's. And again, in both cases I feel I have personal evidence. I was one of the people who talked to Reagan and there's no question that Reagan understood the relation between the quantity of money and inflation. It was very clear, and he was willing to take the heat. He understood on his own accord, but he also had been told so, that you could not slow down the inflation without having a recession.

Taylor: In the first case, a president didn't take your advice, and in the second case, a president did take your advice.

Friedman: Correlation without causation. They were different characters and persons. Nixon had a higher IQ than Reagan, but he was far less principled; he was political to an extreme degree. Reagan had a respectable IQ, though he wasn't in Nixon's class. But he had solid principles and he was willing to stick up for them and to pay a price for them. Both of them would have acted as they did if they had never seen me or heard from me.

Early Interest in Economics

Taylor: I'd like to change the topic from politics to your work in economics. I hope you can share some personal recollections about your remarkable contributions to economics, especially to macroeconomics. How did you get the ideas? Who influenced you? Which parts of your background, education, or work experience were most important? I know it's a long time ago . . .

Friedman: It is a long time ago! But sure, you go right ahead, but I don't trust my memory that far back.

Taylor: Just to get started, let's go back to when you went to college at Rutgers. At first you were interested in mathematics, but then you got interested in economics. Is that correct?

Friedman: I graduated with essentially a double major of mathematics and economics.

Taylor: You got interested in economics in college though?

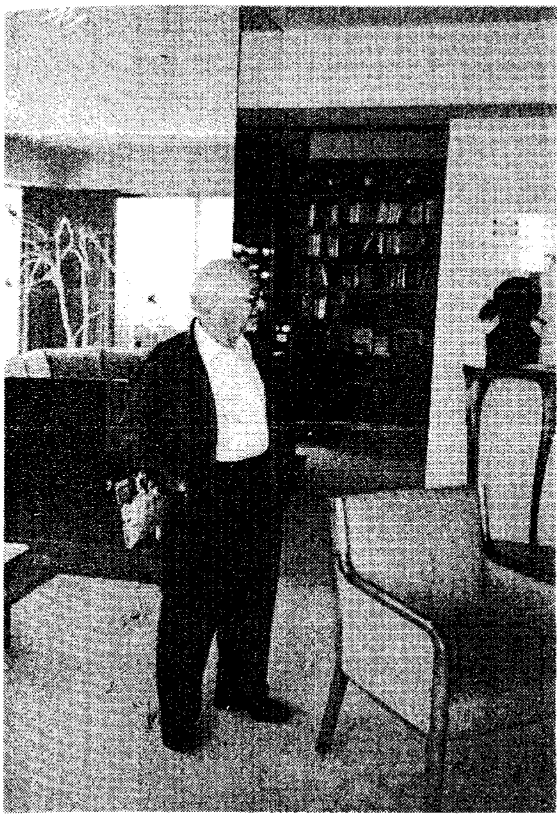

Figure 6.3 In own living room.

Friedman: Yeah.

Taylor: And the two people who you say influenced you early on were two economists: Arthur Burns and Homer Jones. Could you share a little bit about how that occurred? Was Burns teaching you microeconomics, or was he more influential on the macroeconomic side of things?

Friedman: It was much more micro than macro. We had a seminar with Burns in which we went over the draft he had written of his book on production trends in the United States. As we went over his manuscript with him, it was one of the best educational experiences I've ever had, because it gave me a feeling for how to do research. It demonstrated a willingness on his part to accept criticism from people who were not in a way his peers, and so it was a very educational experience.

So far as Homer was concerned, Homer taught a course on statistics and one on insurance. He was a novice himself; he was just keeping one lesson ahead of his students. He clearly was a disciple of Frank Knight of Chicago. He was a member of the Chicago school of economics as it was then. And Homer had a very great influence on me both through his teaching and by getting me to Chicago!

Taylor: He taught you statistics mainly?

Friedman: That, plus the course on insurance, which dealt with economic issues.

Taylor: So you didn't really study macroeconomics or monetary theory much then?

Friedman: I'm sure I had a course in money and banking. It was a standard undergraduate course, no real macro. I didn't get any real training in economics until I went for graduate work in Chicago.

Taylor: It is remarkable that Burns would be working with undergraduates at that level on his own research, that level of detail.

Friedman: Burns at that time was finishing his Ph.D. dissertation. He was a young man; he was not what you think of usually. He had just gotten married and was living in Greenwich Village. He had long hair, long fingernails. You know, he was a different character than he was later on. But he was always an enormously able person intellectually and very dedicated to the research he was doing, to getting it right. And somehow, I'm not sure where, Marshall came in. He was a great student of Marshall and a great admirer of Marshall.

Taylor: So he introduced you to Marshall?

Friedman: Yeah.

Taylor: What about the idea that the free-market system is a good way to organize a society? Was that part of the microeconomics you were learning?

Friedman: Remember, I'm talking about 1928–32; that was before the real change in public opinion, and that really wasn't the kind of issue then that it was scheduled to become. There was, of course, discussion about the breakdown of the economic system, but I graduated in June of 1932 and most of my years there, 1928, 1929, people didn't teach "if markets work well"; they just taught markets. You took it for granted in a sense. Of course, there was a strong intellectual movement toward socialism but it wasn't of the kind that later developed. Norman Thomas was at that time the leading socialist; he was enormously respected, and he got more votes as candidate for president in 1928 than any socialist ever did before or since. The intellectual community in general was socialist, but so far as the department of economics was concerned, I don't think there was much of that.

Taylor: So you wouldn't even have given it a thought?

Friedman: No, I never got involved in politics. I probably would have described myself as a socialist, who knows. When I graduated from college, I wrote myself an essay about what I believed at the time, and I left it in my mother's apartment where I grew up; my father had died when I was in high school. When I went back years later and tried to find it, I never could find it, and I've regretted that very much. That would be a nice document for this purpose.

Taylor: You can't even guess what you wrote?

Friedman: I'm pretty sure I did not have the views I later developed. I probably had the standard views that we needed to do something, but I have no idea what they were.

Taylor: So economics was more technical—supply-and-demand curves, this is how a market works—rather than philosophical?

Friedman: My impression is that it was much less philosophical.

Taylor: So how did Homer Jones encourage you to go to Chicago?

Friedman: He not only encouraged me to go, he made it possible for me to go. People now don't recognize what the situation was then. There were very few scholarships, almost no fellowships of the sort we now take for granted. When I graduated from Rutgers, I applied for graduate work to a number of places, and I received two offers, one from Brown University in applied mathematics, and one from Chicago, thanks to Homer, in economics. Both of them were tuition scholarships, no money beyond free tuition. That was the standard practice at that time. Graduate students mostly paid their own way.

Taylor: Did you have an idea of what you wanted to work on as an economist then?

Friedman: None whatsoever. When I originally entered college, I thought I was going to be an actuary and I took actuarial exams because that was the only way that I knew of that a person could make a living using mathematics. And it is, it's a very skilled job. Only after I got into college and started taking economics courses as well as mathematics courses did I discover that there were alternatives. Of course, the fact that we were in a depression at that time made economics a very important subject.

Graduate School and Early "On-the-Job" Training

Taylor: You were at Chicago for graduate school for a year and then you went to Columbia for a year, and then you went back to Chicago. My understanding is that during this time you developed an interest in mathematical statistics and working with data, with Henry Schultz at Chicago and with Harold Hotelling and Wesley Mitchell at Columbia. And right after graduate school you took a job in Washington working on a new consumer spending survey, and then you moved to New York to work on income survey data with Simon Kuznets. Did working with data and using mathematical statistics interest you a lot?

Friedman: Yes, it did. First of all at Chicago I took Schultz's course in statistics, and when I came back to Chicago after a year at Columbia, I came back as a research assistant to Schultz. Let me go back, and really trace this to Rutgers, to Arthur Burns, because the book that we reviewed, *Production Trends in the United States*, which was his doctoral dissertation, was essentially data analysis. The thesis of the book is that retardation in the growth of each industry separately does not imply retardation in the economy as a whole.

Taylor: My impression is that, at least in your early work with survey data, you put less emphasis on economic models, or formal theories, and more on describing the facts and using mathematical statistics?

Friedman: No, I don't think so. I was trying to explain the data, but not through models, not through multi-equation models, but through more informal stories—basically trying to appeal to microeconomic interactions.

My first year in Chicago really gave me an understanding of economics as a theoretical discipline. In my first year at Chicago, Jacob Viner, Frank Knight, and Lloyd Mints were my main teachers. Both of what's now called micro and macro. I hate those words, I think it's price theory and it's monetary theory. Why the hell do we have to use these Greek words?

Anyway, it seemed to me at that time, spending a year at Chicago first and then a year at Columbia was the ideal combination. Chicago gave you the theoretical basis with which you can interpret the data. Also, there was an empirical slant at Chicago compared with an institutional slant at Columbia. When I went to Washington to work at the National Resources Council in 1935, my work was almost entirely statistical, very little economic theory.

Taylor: Before you went to Washington, you wrote your first published paper, an article criticizing a method proposed by the famous Professor Pigou of Cambridge University. It was published in 1935 in the *Quarterly Journal of Economics*; it must have been written in your first or second year in graduate school. What motivated you to write and publish such an article?

Friedman: Schultz's book that I was working on was on the theory and measurement of demand, the Pigou article was on the elasticity of demand, so it came right out of what I was doing with Schultz. He probably suggested that I publish it, I don't remember.

Taylor: Pigou took the article as a very strong criticism and there was a debate. Did you enjoy that aspect of it?

Friedman: What really happened is this: I sent the article to the *Economic Journal*, where the editor was John Maynard Keynes. Keynes rejected the article on the grounds that Pigou didn't think it was right. I then sent the article to the *Quarterly Journal of Economics*, where Taussig was editor. Fortunately, in submitting it to the *Quarterly Journal of Economics*, I said that I had earlier submitted it to the *Economic Journal* and gave the reason why it was rejected and why I didn't think that was right. I guess it was published in the *Quarterly Journal of Economics* because it was refereed by Leontief. Then Pigou submitted a criticism of it to the *Quarterly Journal of Economics* and Taussig wrote to me and sent me a copy of the criticism. The *Quarterly Journal of Economics* then published both Pigou's criticism and my response.

Taylor: Did that experience whet your appetite for controversy?

Friedman: I really can't say. That's now what, 1935; it's 65 years ago.

Taylor: That story reminds me of referee work you once did for me when I was an editor at the *American Economic Review*. You signed your "anonymous" referee report!

Friedman: I always believed I should be responsible for what I write. I didn't want to go under an anonymous name. And I've never been willing to publish something under my name written by somebody else. You know, I've frequently been asked to, somebody wants propaganda for something or other, but I don't believe that's the appropriate thing to do.

Taylor: I want to ask you about your work at the Statistical Research Group at Columbia University during World War II, but what other experiences were important around that time in your career?

Friedman: So far as your questions about economics versus statistics is concerned, you should note that, for the two years before I went to the Statistical Research Group, I was at the U.S. Treasury Department where it was entirely economics and negligible statistics. We were designing the wartime tax program. Unfortunately, a large part of the income tax today derives from what happened during the war. That was when withholding was introduced, that was when rates were really hiked way up and they were made more progressive, so everyone of the present disputes existed then, even the marriage penalty. In the proposal we made at the Treasury, we eliminated the marriage penalty but our solution wasn't politically feasible. There was a very good group of economists at the Treasury, including Lowell Harris and Bill Vickrey.

Taylor: So that was also part of the war effort?

Friedman: Sure. I went there in 1941 just before we got into the war and the big issue during that period was the argument between the price control people and the people who wanted to hold down inflation through taxation. In the summer of 1941, I participated in a research project with Carl Shoup and we wrote a book, *Taxing to Prevent Inflation*. It's not something I'm very proud of now. It was in the style of a model and it had to do with how much taxation was required to prevent inflation, which I now believe was the wrong issue.

Taylor: You published a paper in the *American Economic Review* in 1942 on the inflationary gap. I want to come back to that, but was it also part of your work at Treasury?

Friedman: Oh yes, it was while I was at the Treasury.

Taylor: Let's discuss your work at the Statistical Research Group in New York during the war. It was heavily statistically oriented, but was there much economics?

Friedman: Oh, entirely statistically oriented; no economics at all. I shouldn't say no economics at all. One of the things that was found out during the war was that social scientists are more effective than natural scientists in dealing with many wartime data problems because social scientists are accustomed to dealing with bad data and natural scientists are accustomed to dealing with good data. And here you have all sorts of problems that arose involving the analysis of data.

Taylor: Do you think that social scientists have a better sense of approximation? What is their advantage?

Friedman: Social scientists have ways of trying to judge the quality of data, to find proxies, to find substitutes, to find ways of evaluating it. Now, in what we did at the statistical research group, that wasn't so evident most of the time.

Taylor: What kind of problems did you work on?

Friedman: We were primarily concerned with such problems as: You've got an antiaircraft missile. It's possible to produce it in such a way that you can control how many pieces it breaks into when it explodes. Should you have a lot of little pieces, so there's a high probability of hitting, but it won't be as harmful to the object hit? Or, should you have a few big pieces, each of which will destroy the plane you're shooting at if it hits it, but the probability of hitting it is less? One of the jobs I worked on was to write a paper on the optimum number of pieces into which to break up a shell. We had data from various test firings on what would be the effect if a fragment of a certain size hit a certain place on a plane, and so on. It was that kind of a problem. Now that's an economic problem.

Taylor: Could you elaborate on that? Why is it an economic problem?

Friedman: I mean it in a broader sense. What we discovered on that project is what you always discover in economics. If you ask people what are the biggest industries in the United States, they'll give you the wrong answer every time. They'll say steel or automobiles. More people are employed in domestic service than in either steel or automobiles and many more still in wholesale or retail trade. That is because those industries consist of a large number of small enterprises. So in this shell project, the naval experts and the military people all came down for a fairly small number of large fragments, so if you hit, you really do damage. Our calculation came out with something different. We showed that there should be a large number of small fragments because the probability of hitting is so much higher than with the large pieces. And that's why I say that's an economic problem—maximization subject to restraints. Again, it always comes down to, should you have one big aircraft carrier or two small ones?

Taylor: Maybe you could say a little about your work on sequential testing. How did you get the idea?

Friedman: Well, Allen Wallis tells the story in an article in the *Journal of the American Statistical Association.* Allen came back to the office one day saying that he had just been with a navy captain who had been observing tests of artillery. The captain said, "You know these statisticians always have to make so many shots, but I know long before the test is done which is the right one." And so Allen came back and said, "You know, there's some sense in that." We agreed and we thought about it and I fixed up an example in which I was able to demonstrate that by having a good stopping rule, you could achieve the same probability of error with a much smaller sample on average.

We knew we didn't have the mathematical competence and could not afford the time to do this ourselves, so we shopped around. But we stated the problem in such a way that statisticians found it difficult to accept. We said, "We know how to construct a test that's more powerful than the uniformly most powerful test." They said, "That's mathematically impossible, you can't do that—we've proved that this is the most powerful test." And so statisticians wouldn't have anything to do with it. Then, we talked to Abraham Wald, and he initially had the same reaction. But then he went home and a day later he called and said, "You are right and I know how to do it and I know what the answer is."

Taylor: A lot of things followed from that important discovery. And you had worked out a little numerical example to show that it would work, at least in some cases?

Friedman: A very simple case, I've forgotten what it was. And then later, one of the jobs we had was to advise the Navy on sampling inspection. So we got up a whole series of sampling inspection programs including sequential analysis using those findings.

One of the other problems, probably the most important one I worked on, had to do with proximity fuses, which are used when firing an antiaircraft gun at an incoming bomber or fighter. A proximity fuse is designed to eliminate the error in timing by being so adjusted that it would go off when it was near the target. The fuse sends out a radio signal that would bounce back from the target; if the target was close enough, the fuse would go off. The radio signal sent out could be adjusted to different angles and different intensities. What was the optimum design of the proximity fuse to maximize the chance of hitting the object? A very interesting problem, and one that we spent a lot of effort on.

Taylor: That sounds like an amazingly complex problem to be working on. Did you write up papers or reports?

Friedman: Oh, sure. I have those reports somewhere.

Taylor: How did you feel about writing important papers that you wouldn't be able to publish, to show to the world?

Friedman: You can't conceive of what the situation was at the time. The war was the most important thing going on and everybody, not me particularly, but everybody was putting aside almost all other considerations to contribute what they could to help in the war. I don't think there was any feeling on the part of any of us that we were concerned about what would happen to our research. In any event, this was in an area that was not of much long-term interest for me.

Taylor: What about the methodology of optimization that you used at the Statistical Research Group. Is that something that you have used later in economic research, perhaps in your research on monetary policy rules?

Friedman: I think it comes the other way. The economic view of seeking an optimum subject to constraints was a way to approach these military problems, rather than the other way around. But I will say that that was very interesting because it was so different from anything we had been exposed to before.

Taylor: Is there anything else that you would like to add?

Friedman: No, I really don't think there is. The Statistical Research Group got me involved with a group of people that I wouldn't otherwise have been involved with. For example, it was the way I got to know Jimmy Savage. He and I wrote a number of papers later together.

Taylor: Do you remember how you happened to write the paper with Savage on utility functions, which gave risk preference at low incomes?

Friedman: I don't know. I honestly don't know. Somehow Jimmy and I must have been talking about it, but I cannot reproduce it. Jimmy Savage was a real genius, there's no question that he was a remarkable character.

Taylor: How did you come to collaborate with him?

Friedman: We got to know one another at the Statistical Research Group. What happened was that at the time he didn't know how to write and I was forced to rewrite some of his papers. He later developed into an excellent writer. You know, he was almost blind, he could only see out of one corner of his eye. He was trained as a mathematician, he had a Ph.D. in mathematics, and then he went on to statistics and really revolutionized statistics. How we got into the risk paper, I no longer have the slightest recollection.

Permanent Income Theory

Taylor: Now let's go on to your research. Let's start with your research on the consumption function. I understand that you think that this is your best purely scientific contribution.

Friedman: I think it is.

Taylor: Could you say a little more about it? Relating to our earlier discussion, did your early work with data and mathematical statistics help you develop the idea?

Friedman: Aside from the work I did on the consumer spending survey in Washington during the 1930s, I also spent several years at the National Bureau of Economic Research working with Simon Kuznets. That ended up in the book, *Income from Independent Professional Practice.* It served as my Ph.D. dissertation. It was largely statistical and empirical, dealing with a whole bunch of questionnaires Kuznets had sent out while he was working at the Department of Commerce. But it also involved the application of economic theory dealing with the explanation for differences of income in different professions. An early venture in the analysis of human capital.

The book on the consumption function was a combination of ideas from the professional income study, from the consumers' spending study, and the work I was doing on methodology (which ultimately appeared in the article I wrote on methodology). What I like about the consumption function book is that it is the best example I know, in my own work, of the methodological principles that are laid out in my essay on methodology. You start with a hypothesis. It has implications. You test whether those implications are correct or not. If the implications are not correct, you try to adjust your hypothesis and readjust.

In this case I started out with a hypothesis that is similar to that which underlies the distinction between real and nominal interest rates. How do people adjust their expectations? How do they decide what fraction of their income to spend? I developed the hypothesis along these lines. I put it in a form in which it could be tested and I derived its implications. I tested those implications and, on the whole, they tended to confirm the hypothesis. I suggested additional tests that should be made to test the hypothesis. So it was, in this way, methodologically pure.

In addition, it produced a hypothesis that seemed to explain the data. As you know, the original pressure for the analysis was the apparent inconsistency between two bodies of data: long time-series data and cross-sectional budget data on consumption and income. The question was: "How could you reconcile those two apparently contradictory bodies of data?" A lot of hypotheses had been offered to reconcile them. The hypothesis I offered, the permanent income hypothesis, seemed to me a much more elegant way to rationalize that difference. And it had, as special cases, almost all of the alternative hypotheses, so it was a consolidation of a lot of empirical evidence as well as theoretical analysis.

Taylor: It seems to me that your signal extraction characterization of the problem, as we call it these days, was quite revolutionary at the time.

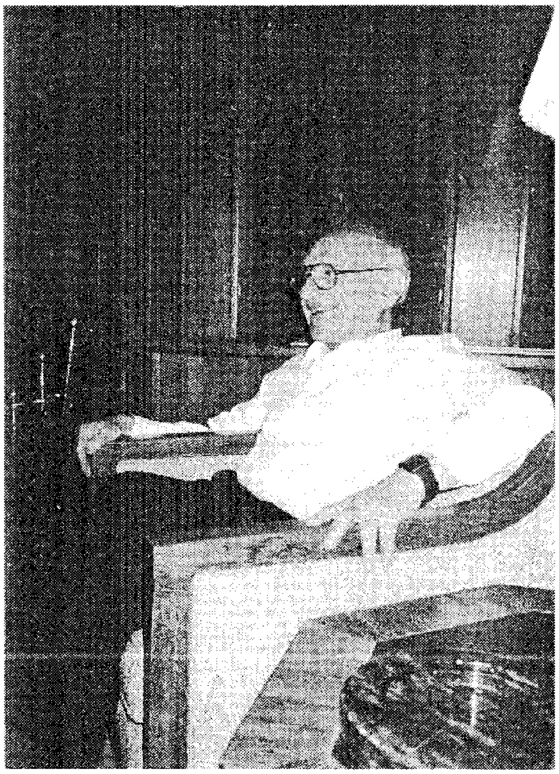

Figure 6.4 Milton Friedman, March 1992.

Friedman: That really came out of the work with Kuznets's data on incomes from professional practice. In that earlier work, I introduced the concepts of permanent income and transitory income in a simplified form, and I just carried that right over. In the professional income data research, I had three categories: permanent, quasi permanent (that's what I called the intermediate one), and transitory. Later I got it down to two.

Taylor: Where did you get the idea to use such statistical decomposition theories in economics?

Friedman: Just from the fact that I was simultaneously becoming an expert in statistical analysis.

Taylor: I guess it is an example of the benefits of a little crossfertilization. Your work on the consumption function got characterized sometimes as kind of an attack on the Keynesian consumption function. Did that motivate you at all?

Friedman: I don't think so, and it isn't an attack, it's just a demonstration that the Keynesian consumption function is not a long-run function; it's a transitory function as he defined it.

Taylor: Did you argue that your theory would imply that a Keynesian model wouldn't be very stable?

Friedman: I think I did argue that in the conclusion.

The Return of Monetary Economics

Taylor: When did your interests in monetary economics begin, exactly?

Friedman: It really began I guess when I was serving in the Treasury Department from 1941 to 1943, because the crucial question was, "What are we going to do to keep down inflation?" Everybody was aware that, during the First World War, taxes had paid for a very small fraction of the war and, during the Second World War, they were determined to raise the fraction paid for by taxes. At the same time, they also had the problem of predicting inflation and that's how I got involved. I was at the Treasury, Division of Tax Research, and our job was to prepare tax proposals for Congress.

The problem—it was interesting from a political point of view and from a scientific point of view—was that a group in the administration who were trying to get a price control statute didn't want us to come up with a tax proposal because they were afraid we would say, "we can stop inflation through taxes, we don't need price controls." They wanted price controls.

We were making estimates of the amount of taxes you would need to stop inflation. Our estimates of how much taxes you would need were much higher than comparable estimates made by those favoring price controls. A month after the price control law was passed, their estimates were much higher than ours. *Now* they wanted all the help they could get from the tax system.

Taylor: Why didn't people mention money through all of this talk about inflation? Was it discussed at all?

Friedman: Hardly. As a result of the Keynesian revolution, money had almost dropped out of the picture. I look back at that and say, how the hell could I have done that? I had good training in monetary theory at Chicago and yet, once the Keynesian revolution came along, everything was on taxes and spending, everything was on fiscal policy, and that's why I was trying to answer the question about the level of taxation needed to stem inflation. With a sufficiently expansive monetary policy, no amount of taxes could do it. It was the wrong question. The right question was, "What monetary policy do we need?" That was the result of the mindset we had.

Taylor: So that's when your 1942 *American Economic Review* article on the inflationary gap was written. When did you go back to basic monetary theory you had learned at Chicago?

Friedman: All I know is from the record. When I republished that article in *Essays in Positive Economics* [published in 1953], I added sections about money and I had a footnote saying that the original article was deficient in this respect. It must have been only a few years before, somewhere in between, that I suddenly realized, or somebody made me realize that money mattered. I no longer remember now.

Fiscal and Monetary Policy Rules

Taylor: Of your two early articles on stabilization policy, the first one is on fiscal policy rules, which had implications for money, of course, and the second one focused more on money growth rules. Could you talk a little about that?

Friedman: Sure. In the earlier paper, I was at the point where I would say money is important but the quantity of money should vary countercyclically—increase when there was a recession and, the opposite, decrease when there was an expansion. Rules for taxes and spending that would give budget balance on average but have deficits and surpluses over the cycle could automatically impart the right movement to the quantity of money.

Then I got involved in the statistical analysis of the role of money, and the relation between money and money income. I came to the conclusion that this policy rule was more complicated than necessary and that you really didn't need to worry too much about what was happening on the fiscal end, that you should concentrate on just keeping the money supply rising at a constant rate. That conclusion was, I'm sure, the result of the empirical evidence.

Taylor: Was part of the reason for the change that the link from deficits and surpluses to changes in money growth were not so tight with changes in the money multiplier?

Friedman: Partly it was that, and partly it was that the link from fiscal policy to the economy was of no use.

Taylor: I remember Bob Lucas saying, in reference to your constant money growth proposals, that they were designed to work in the long run, but that, when you thought about it, they worked well in the short run too. Were you thinking more of the long run? How did you think about the short run?

Friedman: I'm sure I was thinking more of the long run. I've always had the view that you ought to try to design policies for the long run. Given the view that you want the role of government to be stable, that immediately imposes on you a long-run point of view.

Taylor: Did you have a sense that they would work well in the short run?

Friedman: I don't think so.

Taylor: But didn't your first proposal have some of that? If you increase money growth in a recession because of the deficit, and if you retract money growth in a boom because of the surplus, that seems to me to be a short-run consideration.

Friedman: That was short run. That was still the relic of the Keynesian thinking. It was really a waste, I think, trying to reconcile the Keynesian thinking with the monetarist thinking.

Taylor: Was there any relationship between your thinking about these monetary control issues, and your work in statistical analysis? Did you think about these policy problems as regulator problems, thermostats, in any way?

Friedman: Oh yes, I'm sure I did. Thermostatic analysis goes back decades. There were several articles by Levis Kochin, at the University of Washington, on thermostatic analysis of the relation between the quantity of money and the economy.

Taylor: Continuing on the issues of money and monetary policy, in the early 1950s you were one of the very few people who were talking about money, but real controversy developed later, perhaps not until the 1960s.

Friedman: There was no controversy in the sense that I was simply way out in left field. In the 1950s, Chicago and UCLA, maybe, were the only places where anybody was talking about money.

Taylor: Did you think your proposal for a fixed money growth rule or your empirical work on the importance of money in the economy was more responsible for setting off the debate?

Friedman: I'm not sure what you're asking. For the fixed-growth rule to make sense you had to have an empirically supported theory with money in the model. The fixed-growth rule was not original with me; it's a rule that was recommended repeatedly decades ago by different economists.

Taylor: You certainly get the credit for most of it and you deserve it.

Friedman: Perhaps I was a better publicist.

Taylor: But if you explain things more clearly and explicitly than others, you put yourself out further on a limb and therefore you deserve more of the credit when you are right.

Friedman: Certainly the argument that money plays an important role in the economy has been settled. That was the result of the so-called radio AM/FM debates [Ando and Modigliani versus Friedman and Meiselman].

Taylor: Yes, that debate is not going on much anymore.

Friedman: It's over, everybody agrees fundamentally.

Taylor: Agrees with you?

Friedman: In large part, but not wholly. I still have more extreme views about the unimportance of fiscal policy for the aggregate economy than the profession does.

The Use of Models in Monetary Economics

Taylor: In looking back at these monetary versus fiscal debates, it seems that most of your articles are empirical rather than theoretical. Macroeconomic models appear sometimes, but they are not the main focus. Would you agree with that?

Friedman: I believe that one reason the work had whatever effect it has had is because it did have an empirical base. I believe that I can honestly say that I never reached a judgment about monetary or fiscal policy because of my beliefs in free markets. I believe that the empirical work is independent and honest in that sense. If fiscal policy had deserved to play a much larger role, that would have shown up in the data.

Taylor: In your work in consumption theory, for example, there is a more explicit model than in your work in the money area. Is that because you feel it's just too difficult to use models in the latter. Is macro a much more difficult area? Why do you think there is that difference?

Friedman: I really don't know. I think it's partly to do with the use of mathematics in economics in general, and I go back to what Alfred Marshall said about economics: Translate your results into English and then burn the mathematics. I think there's too much emphasis on mathematics as such and not on mathematics as a tool in understanding economic relationships. I don't believe anybody can really understand a 40-equation model. Nobody knows what's going on and I don't believe it's a very reliable way to get results.

Taylor: Didn't the work you did during the war involve complex mathematical models?

Friedman: They very seldom had models of that kind. The one place where you seem to be having that kind of modeling now is in the debate about global warming. And those models seem to be very unreliable and inaccurate. But if you think of physics, they usually have models with only a few equations. In any event, if you have a lot of equations, you ought to be able to draw implications from them that are capable of being understood. You should not present the model and say, now its up to you to test. I think the person who produces the model has some obligation to state what evidence would contradict it.

Taylor: I know that many people who follow the overall economy worry about using models for the reasons you're saying. But do you think the models can be helpful just to keep track of the many relationships?

Friedman: I don't want to say you shouldn't use models. Somebody will come up with one that will prove me wrong. People should do what they want to do. But I think, on the record, you've got to ask yourself whether large-scale modeling is going to continue to exist. You can't do without models—don't misunderstand me. You always have to have some kind of theoretical construct in your mind and that's a model. I think the large models are conceptually different from those with a few equations.

The Use of Time-Series Methods

Taylor: In recent years, you have had some debates with David Hendry about statistical issues relating to your empirical work on money. And that's related to the use of modern methods of statistics and time series. Could you describe your views about various approaches to time-series analysis? Where do you see some advantages and disadvantages?

Friedman: I think the major issue is how broad the evidence is on which you rest your case. Some of the modern approaches involve mining and exploring a single body of evidence all within itself. When you try to apply statistical tests of significance, you never know how many degrees of freedom you have because you're taking the best out of many tries. I believe that you have a more secure basis if, instead of relying on extremely sophisticated analysis of a small fixed body of data, you rely on cruder analysis of a much broader and wider body of data, which will include widely different circumstances. The natural experiments that come up over a wide range provide a source of evidence that is stronger and more reliable than any single very limited body of data.

Let me put it another way. I don't believe that we can possibly understand enough about the economy as a whole to be able to predict or interpret small changes. The best we can hope for is to be able to understand significant larger changes. And, for that, you want a wide body of data and not a narrow body of data. If you have a complex model and then try to extrapolate outside of that model, it will not be very reliable.

I learned that lesson very well while I was at the Statistical Research Group, going back to that. One of the problems I worked on was a metallurgy problem with an application to jet engines. There was a big project during the war of trying to determine the alloy that would have the greatest strength under high temperatures. We were called in as statistical consultants to the various groups working on the problem. I had a lot of data from all their experiments. I computed a multiple regression using these data—data that had been derived by hanging a weight on an experimental turbine blade to see how long it took for the blade to rupture at a given temperature. I regressed the length of time to rupture on the chemical composition and various other variables based on the best metallurgical theory I could find. I got an excellent correlation. So I used my regression to predict what new alloys would have a longer time before rupture. I got wonderful results even though I insisted on restricting every variable separately to the range of values that had been used in the experiment. My equation predicted something like 200 hours until rupture for my constructed alloy. That would have been an enormous success compared to the existing alloys.

Unlike in economics, we could put the prediction to a test. I called some people up at MIT and they constructed this alloy and tested it. And it took an hour, or maybe two hours, to break. It was an utter failure! That taught me that you could not depend on a narrow range of evidence using a lot of variables. I think I had a half-dozen or more variables.

By the way, at that time we did not have our present high-speed computers. So on that occasion I had to use the Mark I or some big machine up at Harvard, which was a collection of IBM sorting equipment. With the desk calculators we had, it would have taken three months to compute the regression. It took 40 hours up at Harvard. That was an enormous achievement. Now it would take five seconds on my Mac.

Taylor: So, did you have to have more discipline in trying out different regressions then?

Friedman: Boy, you sure did! Improvements in computing capacity have made this problem much more serious. It is so easy to fish around for high correlations. I don't have any confidence in a correlation obtained that way. People today pay all too little attention to the quality of data they're analyzing as opposed to the sophistication of the methods they use.

Taylor: As you described earlier, your first few jobs were very data-intensive. Do you think that kind of work is rewarded very much today?

Friedman: No, it isn't rewarded today.

Taylor: And many young economists do not seem to find it as enjoyable as more theoretical work. Did you find it enjoyable?

Friedman: Well, yes. I did and I do. It's kind of fun trying to figure out what's wrong with the data, like these charts we were looking at. Why is this damn thing happening? Is this is a pure data issue? Then we can think of all these great theories we love to try to explain the data, and that's where the fun comes in.

Real-Business-Cycle Models, Calibration, and Detrending

Taylor: A related question on statistical analysis, and on time series in particular, concerns the trend in the economy, whether you come back to a deterministic trend or not. Some real-business-cycle work was generated by the notion that real GDP does not come back to trend. What do you think about the real-business-cycle view?

Friedman: Well, I've always been rather skeptical about the real business cycle, primarily on the grounds of its empirical methodology, which is not to try to fit the data, but rather to calibrate. I think that's not a reliable way to get good results. I think Slutsky proved that years and years ago.

Taylor: Can you elaborate a little bit on that? Why don't you think that's a legitimate way to proceed?

Friedman: It's a perfectly legitimate way to derive hypotheses, but it doesn't test them. If I show you that with this calibration I get results that look like the observed data, okay, that's interesting. But why don't you go test it and use your analysis to see if you can reproduce real data that way and predict it for a period for which you did not have the data when you formulated your hypothesis. Either backward, or for another country, or something.

Taylor: So, just the fact that it looks like a business cycle is not enough?

Friedman: That's what I say. Slutsky proves that with an accumulation of random shocks. Maybe Slutsky's series are right there [*pointing to Figure 6.1*], I do believe that short-run fluctuations in the economy are simply the accumulation of random shocks. I don't believe there is such a thing as a business cycle. I think there are fluctuations and there are reaction mechanisms. Various parts of the economy react systematically to shocks to the system, but in the sense of regularly recurring cycles, the kind of thing that Mitchell was trying to describe, I don't think they exist.

Taylor: What about the notion that the economy returns to a trend after a recession?

Friedman: Well, I don't know what the opposite view is.

Taylor: The opposite view is that if you are at the bottom of a recession, then your best guess is that you're going to have only trend growth from that point onward.

Friedman: Oh, I see what you're saying. Oh, no, no, I think that there is a basic equilibrium position and the economy as a whole will tend to return to it. But that trend may change sometimes. Surely if something has been going on for 100 years, you've got to be a little skeptical in saying it's not going to go on again!

The Natural Rate Hypothesis

Taylor: Let's talk about a concept of equilibrium that you have made famous—the natural rate of unemployment. Your Presidential Address to the American Economic Association in December 1967 was on the Phillips curve and the natural rate hypothesis. It must have been quite an event. Could you talk a little about how that happened?

Friedman: The basic ideas in my Presidential Address were already present in a comment that I made at a conference on guidelines, the proceedings of which were published in a 1966 book edited by George Shultz and Robert Aliber [Solow (1966)].

I'm sure the basic idea grew out of the discussions about guidelines and, in particular, out of the Samuelson and Solow paper on the Phillips curve. I can't say exactly where my ideas originated; all I know is by the time I gave the Presidential Address in 1967, there was nothing new in that compared to what I had earlier published. Arthur Burns was in the chair when I gave the Presidential Address, and he had gone over the Address earlier. Arthur always went over my papers.

Taylor: You're kidding. He would read all your papers?

Friedman: Sure, and I went over his. Despite what I said about his chairmanship of the Fed, Arthur was a first-rate economist. He had a feeling for the English language and an ability to use it, which was unusual. He was always one of the most valuable critics of anything I wrote. He didn't always agree with what I wrote, don't misunderstand me, and I'm not sure on this occasion that he agreed with me, but he was one of the people who had commented on early drafts of the paper. At the time, I never had any expectation that it would have the impact it did. It only had that impact because of the accidental factor that you had a test right after.

Taylor: Yes, very impressive.

Friedman: This was one of the few occasions when something was predicted in advance and confirmed later.

Taylor: Did you think much in advance about whether this would be a good topic for the Presidential Address?

Friedman: You want to talk on what you are working on, and the major focus of my work at that time was monetary policy, so I talked about the role of monetary policy.

Taylor: That work has, of course, generated much work by others. One could argue that the whole rational expectations revolution came out of that research because you focused on expectations.

Friedman: I think the focus on expectations was important. But as for rational expectations, I think you have to give Bob Lucas a lot of credit for that.

Rational Expectations

Taylor: That brings me to the question about what causes the short-run impact of money. Do you feel that it's mainly unfulfilled expectations or do you think that sticky prices and wages play a role?

Figure 6.5 At 80th birthday party in 1992, given by the Frazer Institute in Vancouver.

Friedman: You've mentioned both the things that are no doubt the legitimate causes. After all, a wage agreement is not for a day, it's for a year, two years, three years. It's costly to change prices and so on, but I think the most important single thing is the tendency for expectations to be backward looking and to be adjusted slowly so that it takes time before any expectation is altered by the impact of an event.

Taylor: Does that mean you disagree with rational expectations?

Friedman: I have no basic disagreement with rational expectations. The question is, "how do you form your rational expectations." Let me start over. You are talking about what's going to happen tomorrow. The price is either going to go up or it isn't. If it goes up, the probability that it went up is one; the probability that it went down is zero.

What you are doing with rational expectations is to ask yourself, what is the probability that the movement tomorrow will be up or the movement tomorrow will be down. And now the thing that you have to ask yourself is, "I have an expectation. How do I know after the event whether that expectation was fulfilled or not? I said the probability that the price was going to go up was 60%; now, it actually went up. Does that confirm it? I can't tell. I have to have a lot of similar cases." And so, the notion of "correct rational expectations" is a notion I find very hard to give much content to.

If the idea is that people try to predict what is going to happen tomorrow, then rational expectations, in that sense, certainly makes sense, but on what do they base their rational expectations? They base it on past experiences; there is always going to be a lag in expectations catching up.

The Role of Debates in Monetary Economics

Taylor: In my view the debates in macroeconomics have helped get people interested, and this has motivated more research. Was there some strategy behind your role in generating debate?

Friedman: I don't think so. It just happened. I think most of the things that just happen are likely to be more valuable and interesting than those you plan!

Taylor: How did you get to be such a good debater? Did that just happen too?

Friedman: That just happened, too.

Taylor: You weren't a debater in college?

Friedman: I may have been involved, but that was not a major activity of mine. I just like to talk, that's all! And I like to argue. I enjoy the stimulus of arguments back and forth, but I never did anything special to improve my skill as a debater.

Taylor: Well, I do think it's an effective way to get people interested.

Friedman: It is, I agree with you. What people like is that a person is willing to take positions. He's not hedging all the time. The idea of the one-armed economist, one-handed, I guess.

Taylor: I always have to watch when I say "on the other hand."

Friedman: Right!

Taylor: Is hedging your views something that you strive not to do?

Friedman: No. It's the way I am. You know, somehow or other, people have a tendency to attribute to me a long-term plan; they think I must have planned this campaign. I did no planning whatsoever. These things just happened in the order in which they happened to happen. And luck plays a very large role, a very large role indeed. Take the effect of presidential elections.

Capitalism and Freedom Today

Taylor: Let me ask about your work on capitalism and freedom. *Capitalism and Freedom* was published in 1962 and has influenced people all over the world, but you did not do a second edition. Is there a reason?

Friedman: I think *Free to Choose* is, in a sense, another edition, from a different perspective. But since my main activity was science and economics, this is essentially a secondary activity.

Taylor: You mean to say that *Capitalism and Freedom* was secondary?

Friedman: Oh, sure. It was a series of lectures I gave at Wabash College in 1956 at a summer conference for assistant professors. The organizers wanted me to talk about free markets and those lectures were really the basis for *Capitalism and Freedom*. It was not a book that was conceived from the outset as a book.

Taylor: Did you take much time to write them?

Friedman: I had to spend time preparing the initial lectures and I also spent a lot of time editing the volume, but it was an avocation rather than a vocation. My wife did most of the work of turning the transcripts of the lectures into publishable prose.

Taylor: As your public policy work is in general?

Friedman: It's always been an avocation. I've often had students come up to me and say that they want to promote free markets or they want to get involved in politics and the advice I uniformly gave is, don't do that as a profession. Get yourself established in something you believe in and can work in and which has no necessary ideological component, so you have a little nest. Then go on and get involved in public policy; otherwise the public policy will impose itself on you and will affect what you believe rather than your beliefs affecting it. That's why I think that people stay in Washington too long.

Taylor: I remember one time when I was working in Washington, as a member of the Council of Economic Advisers, you said as much to me. I called to get your support on an important policy issue, and your first answer was, "Why don't you just come back to Stanford. You have been there too long." But how did you manage to have so much impact?

Friedman: I stayed away from Washington.

Taylor: Would you like to see a new *Capitalism and Freedom*, one that would be oriented to where we are now? In many respects the world has moved in the direction that you advocated. Do we need another book? Do you think we have moved?

Friedman: We need another one, but I can't write it. In many ways we are worse off. Government spending as a fraction of income is higher now than when *Capitalism and Freedom* was published. A good deal higher. Unless I'm mistaken, I think it was 30% then and 40% now.

Taylor: That is for the United States?

Friedman: Yeah, just for the United States. And also worldwide; I once got together a list of 10–12 countries and how much they were spending as a fraction of income, and in every single country the fraction of income spent by government had gone up. We're much better off in the realm of ideas. The intellectual climate of opinion is more favorable to a free-market society, but the practical world is less favorable. Just look at the regulations we've got now that we didn't have then.

Taylor: That's true, there is more social regulation, but millions of people around the world have been freed from communism.

Friedman: That's true. In the former communist countries, there's no doubt. In a country like Britain, France, or Germany, I'm sure there are more regulations now than there were 30–40 years ago, so that, far from having moved in the right direction, in practice it's moved in the wrong direction. And that's why, going back to your comment, that's why we need another *Capitalism and Freedom* to start from where we are now.

Monetary Unions and Flexible Exchange Rates

Taylor: Let me ask a question about monetary issues that relates to the global economy. You have Europe's new single currency, and you have Bob Mundell arguing that we should have one world currency. You also have talk about dollarization in Argentina and a greater commitment to floating in Brazil. Where is this all going?

Friedman: From the scientific point of view, the euro is the most interesting thing. I think it will be a miracle—well, a miracle is a little strong. I think it's highly unlikely that it's going to be a great success. It would be very desirable and I would like to see it a success from a policy point of view, but as an economist, I think there are real problems, arising in a small way now when you see the difference between Ireland and Italy. You need different monetary policies for those two countries, but you can't have it with a single currency. Yet they are independent countries; you are not going to have many Italians moving to Ireland or vice versa. So I do not share Bob Mundell's unlimited enthusiasm for the euro. But it's going to be very interesting to see how it works. For example, I saw a study in which somebody tried to ask the question, "What is the effect of having a common currency on the volume of intercountry trade?" And the result was surprising. It was that having a common currency had a surprisingly large effect, about four times the effect of geographical proximity or of flexible exchange rates. Now that was just a small sample.

Taylor: And beware of multiple regressions!

Friedman: Right! At any rate, one thing that I could be leaving out in my evaluation of the dangers of the euro is the effect of a common currency on the volume of trade between the countries. If it has a major effect on trade, it may enable trade to substitute for the mobility of people.

Taylor: Do you think that the depreciation of the euro is bad sign? [It was about $0.90 at that time.]

Friedman: No, not for a second. At the moment the situation is very clear. The euro is undervalued; the U.S. dollar is overvalued. As a result of the undervaluation of the euro, the producing enterprises in Europe are doing very well, the consumers in Europe are suffering, the consumers in the United States are getting a good deal, and the opposite is true for the producers in the United States. And there's very little doubt that within the next few years that's going to come together. Relative to the dollar, the euro will appreciate and the dollar will depreciate.

Taylor: One of your most famous articles is the one advocating flexible exchange rates, though you stressed microeconomic speculation more than macroeconomic issues in that article. Do you want to say something about how that article came about?

Friedman: That article originated from three months I spent in France as a consultant to the Marshall Plan agency in 1950. At the time, the German mark was having balance-of-payments problems and I was asked to analyze proposed solutions. I concluded that the best solution would be to float the exchange rate, but that was so far out of sync with the attitudes of the time that it was summarily rejected.

Taylor: That article, like many others of yours, has been tremendously influential.

Friedman: Yes, I think it has been very influential.

Taylor: Does it surprise you sometimes, the things that are more influential than others?

Friedman: I think it's almost impossible to predict what will be influential. You know that from your own work. You never dreamed when you presented the Taylor Rule that it was going to become worldwide conventional wisdom.

Taylor: I think that's true.

Friedman: It's an accident what happens to get picked up and what doesn't. It depends on the circumstances that develop afterward.

Taylor: Well, that's sounds like a good place to end, but maybe I should just ask one more question: Is there anything else you want to say?

Friedman: I don't want to say anything else. I've already said too much.

Taylor: Thank you. I have enjoyed this interview greatly.

NOTE

1. On editing the transcript of our conversations, Milton Friedman added the following explanation of his reference to "thermostatic control":

> The temperature in a room without a thermostat but with a heating system will be positively correlated with the amount of fuel fed into the heating system and may vary widely. With a thermostat set at a fixed temperature, there will be zero correlation between the intake of fuel and the room temperature, a negative correlation between the intake of fuel and external temperature. Also, the room temperature will vary little.
>
> By analogy, without a successful monetary policy to stabilize the economy (thermostat), there will tend to be a positive correlation between the quantity of money (the fuel) and GDP (the temperature), as there is in Figure 6.1 before 1992, and both may vary widely. With a successful monetary policy, there will be a zero correlation between the quantity of money and GDP, as there is in Figure 6.1 after 1992.

Money may still vary widely, but GDP will vary little, as in Figure 6.1 after 1992.

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7

An Interview with Paul A. Samuelson

Interviewed by William A. Barnett

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It is customary for the interviewer to begin with an introduction describing the circumstances of the interview and providing an overview of the nature and importance of the work of the interviewee. However, in this case, as Editor of this journal, I feel it would be presumptuous of me to provide my own overview and evaluation of the work of this great man, Paul Samuelson. The scope of his contributions has been so vast (averaging almost one technical paper per month for over 50 years) that it could be particularly difficult to identify those areas of modern economic theory to which he has *not* made seminal contributions.[1] In addition to his over 550 published papers, his books are legendary. He once said: "Let those who will—write the nation's laws—if I can write its textbooks."

Instead of attempting to provide my own overview, I am limiting this introduction to the following direct (slightly edited) quotation of a few paragraphs from the Web site, *The History of Economic Thought*, which is maintained online by the New School University in New York[2]:

> Perhaps more than anyone else, Paul A. Samuelson has personified mainstream economics in the second half of the twentieth century. The writer of the most successful principles textbook ever (1948), Paul Samuelson has been not unjustly considered *the* incarnation of the economics "establishment"—and as a result, has been both lauded and vilified for virtually everything right and wrong about it.

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Figure 7.1 Paul A. Samuelson.

Samuelson's most famous piece of work, *Foundations of Economic Analysis* (1947), is one of the grandest tomes that helped revive Neoclassical economics and launched the era of the mathematization of economics. Samuelson was one of the progenitors of the Paretian revival in microeconomics and the Neo-Keynesian Synthesis in macroeconomics during the post-war period.

The *wunderkind* of the Harvard generation of 1930s, where he studied under Schumpeter and Leontief, Samuelson had a prodigious grasp of economic theory, which has since become legendary. An unconfirmed anecdote has it that at the end of Samuelson's dissertation defense, Schumpeter turned to Leontief and asked, "Well, Wassily, have we passed?" Paul Samuelson moved on to M.I.T. where he built one of the century's most powerful economics departments around himself. He was soon joined by R.M. Solow, who was to become Samuelson's sometime co-writer and partner-in-crime.

Samuelson's specific contributions to economics have been far too many to be listed here—being among the most prolific writers in economics. Samuelson's signature method of economic theory, illustrated in his *Foundations* (1947), seems to follow two rules which can also be said to characterize much of Neoclassical economics since then: With every economic problem, (1) reduce the number of variables and keep only a minimum set of simple economic relations; and (2) if possible, rewrite it as a constrained optimization problem.

In microeconomics, he is responsible for the theory of revealed preference (1938, 1947). This and his related efforts on the question of utility measurement and integrability (1937, 1950) opened the way for future developments by Debreu, Georgescu-Roegen, and Uzawa. He also introduced the use of comparative statics and dynamics through his "correspondence principle" (1947), which was applied fruitfully in his contributions to the dynamic stability of general equilibrium (1941, 1944). He also developed what are now called "Bergson–Samuelson social welfare functions" (1947, 1950, 1956); and, no less famously, Samuelson is responsible for the harnessing of "public goods" into Neoclassical theory (1954, 1955, 1958).

Samuelson was also instrumental in establishing the modern theory of production. His *Foundations* (1947) are responsible for the envelope theorem and the full characterization of the cost function. He made important contributions to the theory of technical progress (1972). His work on the theory of capital is well known, if contentious. He demonstrated one of the first remarkable "Non-Substitution" theorems (1951) and, in his famous paper with Solow (1953), initiated the analysis of dynamic Leontief systems. This work was reiterated in his famous 1958 volume on linear programming with Robert Dorfman and Robert Solow, wherein we also find a clear introduction to the "turnpike" conjecture of linear von Neumann systems. Samuelson was also Joan Robinson's main adversary in the Cambridge Capital Controversy—introducing the "surrogate" production function (1962), and then subsequently (and graciously) relenting (1966).

In international trade theory, he is responsible for the Stolper–Samuelson Theorem and, independently of Lerner, the Factor Price Equalization theorem (1948, 1949, 1953), as well as (finally) resolving the age-old "transfer problem" relating terms of trade and capital flows, as well as the Marxian transformation problem (1971), and other issues in Classical economics (1957, 1978).

In macroeconomics, Samuelson's multiplier–accelerator macrodynamic model (1939) is justly famous, as is the Solow–Samuelson presentation of the Phillips Curve (1960) to the world. He is also famous for popularizing, along with Allais, the "overlapping generations" model which has since found many applications in macroeconomics and monetary theory. In many ways, his work on speculative prices (1965) effectively anticipates the efficient markets hypothesis in finance theory. His work on diversification (1967) and the "lifetime portfolio" (1969) is also well known.

Paul Samuelson's many contributions to Neoclassical economic theory were recognized with a Nobel Memorial prize in 1970.

Barnett: As an overture to this interview, can you give us a telescopic summary of 1929 to 2003 trends in macroeconomics?

Samuelson: Yes, but with the understanding that my sweeping simplifications do need, and can be given, documentation.

As the 1920s came to an end, the term macroeconomics had no need to be invented. In America, as in Europe, money and banking books preached levels and trends in price levels in terms of the Fisher–Marshall $MV = PQ$. Additionally, particularly in America, business-cycles courses eclectically nominated causes for fluctuations that were as diverse as "sunspots," "psychological confidence," "over- and underinvestment" pathologies, and so forth. In college on the Chicago Midway and before 1935 at Harvard, I was drilled in the Wesley Mitchell statistical descriptions and in Gottfried Haberler's pre-*General Theory* review of the troops.

Read the puerile Harvard book on *The Economics of the Recovery Program*, written by such stars as Schumpeter, Leontief, and Chamberlin, and you will agree with a reviewer's headline: Harvard's first team strikes out.

Keynes's 1936 *General Theory*—paralleled by such precursors as Kahn, Kalecki, and J.M. Clark—gradually filled in the vacuum. Also, pillars of the $MV = PQ$ paradigm, such as all of Fisher, Wicksell, and Pigou, died better macroeconomists than they had earlier been—this for varied reasons of economic history.

Wicksell was nonplussed in the early 1920s when postwar unemployment arose from his nominated policy of returning after 1920 back to pre-1914 currency parities. His long tolerance for Say's Law and neutrality of money (even during the 1865–1900 deflation) eroded away in his last years. For Fisher, his personal financial losses in the 1929–34 Depression modified his beliefs that V and Q/V were quasi constants in the $MV = PQ$ tautology. Debt deflation all around him belied that. Pigou, after a hostile 1936 review of *The General Theory* (occasioned much by Keynes's flippancies about Marshall and "the classics"), handsomely acknowledged wisdoms in *The General Theory*'s approaches in his 1950 *Keynes's General Theory: A Retrospective View*.

I belabor this ancient history because what those gods were modifying was much that Milton Friedman was renominating about money around 1950 in encyclopedia articles and empirical history. It is paradoxical that a keen intellect jumped on that old bandwagon just when technical changes in money and money substitutes—liquid markets connected by wire and telephonic liquid "safe money market funds," which paid interest rates on fixed-price liquid balances that varied between 15% per annum and 1%, depending on price level trends—were realistically replacing the scalar M by a vector of (M_0, M_1, M_2, . . . , M_{17}, a myriad of bonds with tight bid-asked prices, . . .). We all pity warm-hearted scholars who get stuck on the wrong paths of socialistic hope. That same kind of regrettable choice characterizes anyone who bets doggedly on ESP, or creationism, or. . . . The pity of it increases for one who adopts a simple theory of positivism that exonerates a nominated theory, even if its premises are unrealistic, so long only as it seems to describe with approximate accuracy some facts. Particularly vulnerable is a scholar who tries to *test* competing theories by submitting them to *simplistic* linear regressions with no sophisticated calculations of Granger causality, cointegration, collinearities and ill-conditioning, or a dozen other safeguard econometric methodologies. To give one specific example, when Christopher Sims introduces both *M and an interest rate* in a multiple regression testing whether M drives P, Q/V, or Q in some systematic manner congenial to making a constant rate of growth of money supply, M_1, an optimal guide for

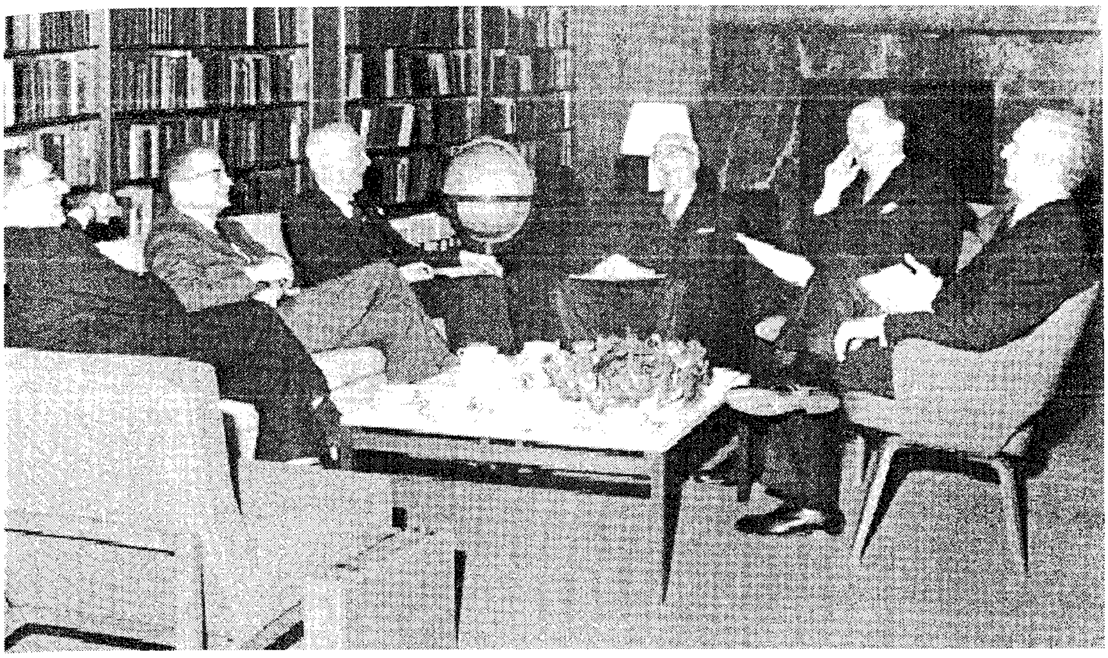

Figure 7.2 New York, February 19, 1961. Seated left to right, participating guests who appeared on the first of The Great Challenge symposia of 1961: Professor Henry A. Kissinger, Director of the Harvard International Seminar; Dr. Paul A. Samuelson, Professor of Economics at MIT and President of the American Economic Association; Professor Arnold J. Toynbee, world historian; Admiral Lewis L. Strauss, former Chairman of the Atomic Energy Commission and former Secretary of Commerce; Adlai E. Stevenson, U.S. Ambassador to the United Nations; and Howard K. Smith, CBS news correspondent in Washington, moderator of the program. The topic: "The World Strategy of the United States as a Great Power."

policy, then in varied samples the interest rate alone works better without *M* than *M* works alone or without the interest rate.

The proof of the pudding is in the eating. There was a widespread myth of the 1970s, a myth along Tom Kuhn's (1962) *Structure of Scientific Revolutions* lines. The Keynesianism, which worked so well in Camelot and brought forth a long epoch of price-level stability with good *Q* growth and nearly full employment, gave way to a new and quite different macro view after 1966. A new paradigm, monistic monetarism, so the tale narrates, gave a better fit. And therefore King Keynes lost self-esteem and public esteem. The King is dead. Long live King Milton!

Contemplate the true facts. Examine 10 prominent best forecasting models 1950 to 1980: Wharton, Townsend–Greenspan, Michigan Model, St. Louis Reserve Bank, Citibank Economic Department under Walter Wriston's choice of Lief Olson, et cetera. When a specialist in the Federal Reserve system graded models in terms of their accuracy for *out-of-sample* future performance for a whole vector of target macro variables, never did post-1950 monetarism score well! For a few quarters in the early 1970s, Shirley Almon distributed lags, involving $[M_i(-1), M_i(-2), \ldots, M_i(-n)]$, wandered into some temporary alignment with reality. But then, outfits like that at Citibank, even when they added on Ptolemaic epicycle to epicycle, generated monetarism forecasts that diverged systematically from reality. Data mining by dropping the M_i's that worked worst still did not attain statistical significance. Overnight, Citibank wiped out its economist section as superfluous. Meantime, inside the Fed, the ancient Federal Reserve Board–MIT–Penn model of Modigliani, Ando, et al. kept being tweaked at the Bank of Italy and at home. For it, *M* did matter as for almost everyone. But *never did M alone matter systemically, as post-1950 Friedman monetarism professed.*

It was the 1970s supply shocks (OPEC oil, worldwide crop failures, . . .) that worsened forecasts and generated stagflation incurable by either fiscal or central bank policies. That's what undermined Camelot cockiness—not better monetarism that gave better policy forecasts. No Tom Kuhn case study here at all.

Barnett: Let's get back to your own post-1936 macro hits and misses, beliefs, and evolutions.

Samuelson: As in some other answers to this interview's questions, after a struggle with myself and with my 1932–36 macro education, I opportunistically began to use *The General Theory*'s main paradigms: the fact that millions of people without jobs envied those like themselves who had jobs, while those in jobs felt sorry for those without them, while all the time being fearful of losing the job they did have. These I took to be established facts and to serve as effective evidence that prices were not being *un*sticky, in the way that an auction market needs them to be, *if full employment clearing were to be assured.* Pragmatically and opportunistically, I accepted this as tolerable "micro foundations" for the new 1936 paradigm.

A later writer, such as Leijonhufvud, I knew to have it wrong, when he later argued the merits of Keynes's subtle intuitions and downplayed the various (identical!) mathematical versions of *The General Theory*. The so-called 1937 Hicks or later Hicks–Hansen IS–LM diagram will do as an example for the debate. Hansen never pretended that *it* was something original. Actually, one could more legitimately call it the Harrod–Keynes system. In any case, it was isomorphic with an early Reddaway set of equations and similar sets independently exposited by Meade and by Lange. Early on, as a second-year Harvard graduate student, I had translated Keynes's own words into the system that Leijonhufvud chose to belittle as unrepresentative of Keynes's central message.

Just as Darwinism is not a religion in the sense that Marxism usually is, my Keynesianism has always been an evolving development, away from the Neanderthal Model T Keynesianism of liquidity traps and inadequate inclusion of stocks of wealth and stocks of invested goods, and, as needed, included independent variables in the mathematical functions determinative of equilibria and their trends.

By 1939, Tobin's Harvard Honors thesis had properly added Wealth to the Consumption Function. Modigliani's brilliant 1944 piece improved on 1936 Keynes. Increasingly, we American Keynesians in the Hansen School—Tobin, Metzler, Samuelson, Modigliani, Solow, . . . —became impatient with the foot-dragging English—such as Kahn and Robinson—whose lack of wisdoms became manifest in the 1959 Radcliffe Committee Report. The 1931 Kahn that I admired was not the later Kahn, who would assert that the $MV = PQ$ definition contained bogus variables. Indeed, had Friedman explicitly played up, instead of playing down, the key fact that a rash Reagan fiscal deficit could raise V systematically by its inducing higher interest rates, Friedman's would have been less of an eccentric macro model.

I would guess that most MIT Ph.D.'s since 1980 might deem themselves *not* to be "Keynesians." But they, and modern economists everywhere, do use models like those of Samuelson, Modigliani, Solow, and Tobin. Professor Martin Feldstein, my Harvard neighbor, complained at the 350th Anniversary of Harvard that Keynesians had tried to poison his sophomore mind *against saving*. Tobin and I on the same panel took this amiss, since both of us since 1955 had been favoring a "neoclassical synthesis," in which full employment with an austere fiscal budget would *add to capital formation* in preparation for a coming demographic turnaround. I find in Feldstein's macro columns much the same paradigms that my kind of Keynesians use today.

On the other hand, within any "school," schisms do tend to arise. Tobins and Modiglianis never approved of Robert Eisner or Sidney Weintraub as "neo-Keynesians," who denied that lowering of real interest rates might augment capital formation at the expense of current consumption. Nor do I regard as optimal Lerner's Functional Finance that would sanction any sized fiscal deficit so long as it did not generate inflation.

In 1990, I thought it unlikely ever again to encounter in the real world liquidity traps, or that Paradox of Thrift, which so realistically did apply in the Great Depression and which also did help shape our pay-as-you-go nonactuarial funding of our New Deal social security system. In economics what goes around may well come around. During the past 13 years, Japan has tasted a liquidity trap. When 2003 U.S. Fed rates are down to 1%, that's a lot closer to 0% than it is to a more "normal" real

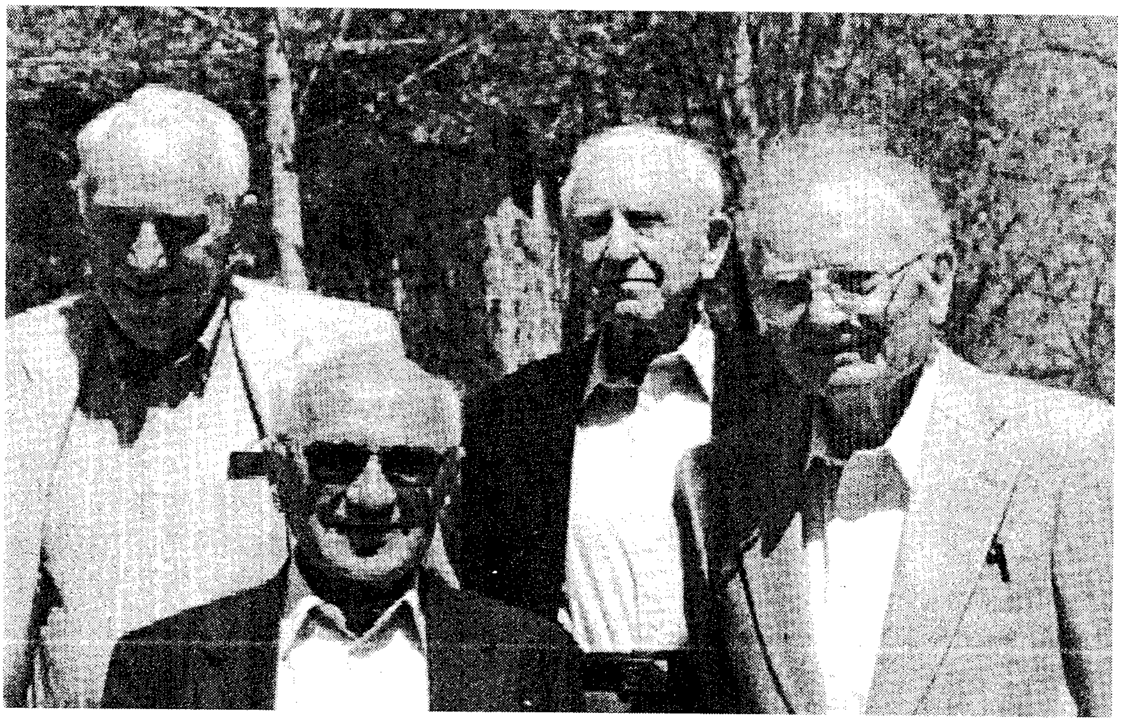

Figure 7.3 From left to right at back: James Tobin and Franco Modigliani. From left to right in front: Milton Friedman and Paul A. Samuelson. All four are Nobel Laureates in Economics.

interest rate of 4% or 5%. Both in micro- and macroeconomics, master economists know they must face up to *nonstationary time series* and the difficulties these confront us with.

If time permits, I'll discuss later my qualified view about "rational expectations" and about "the New Classicism of Say's Law" and neutrality of money in effectuating systemic real-variable changes.

Barnett: What is your take on Friedman's controversial view that his 1950 monetarism was an outgrowth of a forgotten subtle "oral tradition" at Chicago?

Samuelson: Briefly, I was there, knew all the players well, and kept class notes. And beyond Fisher–Marshall $MV = PQ$, there was little else in Cook County macro.

A related and somewhat contradictory allegation by David Laidler proclaimed that Ralph Hawtrey—through Harvard channels of Allyn Young, Lauchlin Currie, and John H. Williams—had an important (long-neglected) influence on Chicago's macro paradigms of that same 1930–36 period. Again, my informed view is in the negative. A majority of the Big Ten courses did cite Hawtrey, but in no depth.

Before comparing views with me on Friedman's disputed topic (and after having done so), Don Patinkin denied that in his Chicago period of the 1940s any trace of such a specified oral tradition could be found in his class notes (on Mints, Knight, Viner), or could be found in his distinct memory. My Chicago years predated Friedman's autumn 1932 arrival and postdated his departure for Columbia and the government's survey of incomes and expenditures. I took all the macroeconomic courses on offer by Chicago teachers: Mints, Simons, Director, and Douglas. Also in that period, I attended lectures and discussions on the Great Depression, involving Knight, Viner, Yntema, Mints, and Gideonse. Nothing beyond the sophisticated account by Dennis Robertson, in his famous *Cambridge Handbook on Money*, of the Fisher–Marshall–Pigou $MV = PQ$ paradigm can be found in my class notes and memories.

More importantly, as a star upper-class undergraduate, I talked a lot with the hotshot graduate students—Stigler, Wallis, Bronfenbenner, Hart—and rubbed elbows with Friedman and Homer Jones. Since no whisper reached my ears, and no cogent publications have ever been cited, I believe that this nominated myth should not be elevated to the rank of plausible history of ideas. Taylor Ostrander, then unknown to me, did graduate work on the Midway in my time and has kept copious notes. I have asked him and Warren Samuels to comb this important database to confirm or deny these strong contentions of mine.

Having killed off one 1930s Chicago myth, I do need to report on another too-little-noticed genuine macro oral tradition from the mid-1930s Chicago. It is not at all confirmatory of the Friedman hypothesis, and is indeed 180 degrees opposed to that in its eclectic doubts about simplistic monetarism. Nor can I cogently connect it with a Young–Hawtrey influence.

You did not have to be a *wunderkind* to notice in the early 1930s that traditional orthodox notions about Say's Law and neutral money were sterile in casting light on contemporary U.S. and global slumps. Intelligently creative scholars such as Simons and Viner had by the mid-1930s learned something from current economic history about inadequacies of the simple $MV = PQ$ paradigm and its "M alone drives PQ" nonsequitur.

Keynes, of course, in shedding the skin of the author of the *Treatise*, accomplished a virtual revolution by his liquidity preference paradigm, which realistically recognized the *systematic* variabilities in V. Pigou, when recanting in 1950 from his earlier bitter 1936 review of *The General Theory*, in effect abandoned what was to become 1950-like monistic Friedmanisms.

Henry Simons, to his credit, already in my pre-1935 undergraduate days, sensed the "liquidity trap" phenomenon. I was impressed by his reasonable dictum: When open-market operations add to the money supply and at the same time *subtract equivalently* from outstanding quasi-zero-yielding

Treasury bills that are *strong money substitutes*, little increase can be expected as far as spending and employment are concerned. Note that this was some years before the 1938 period, when Treasury bills came to have only a derisory yield (sometimes negative).

Experts, but too few policymakers, were impressed by some famous Viner and Hardy researches for the 1935 Chicago Federal Reserve Bank. These authors interpreted experience of borrowers who could not find lenders as a sign that during (what we subsequently came to call) "liquidity trap times" money is *tight* rather than loose: Safe Treasury bills are cheap as dirt just because effective tightness of credit chokes off business activity and thereby lowers the market-clearing short interest rate down toward the zero level. Hoarding of money, which entailed slowing down of depression V, is then not a psychological aberration; rather, it is a cool and sensible adjustment to a world where potential plenty is aborted by failures in both investment and consumer spending out of expectable incomes (multiplier and accelerator, rigidity of prices and wages, et cetera).

Go back now to read Friedman's article for the 1950 *International Encyclopedia of the Social Sciences*, where as an extremist he plays down (outside of hyperinflation) the effects of i (the interest rate) and fiscal deficits on V, to confirm that this Simons–Viner–Hardy Chicago oral tradition is not at all the one he has for a long time claimed to be the early Chicago tradition. (In his defense, I ought to mention that Friedman had left Chicago for Columbia by the time of the Viner–Hardy publications.) The commendable 1932 Chicago proclamation in favor of expanded deficit fiscal spending was itself a recognition of the limited potency of $\partial(PQ)/\partial M$. In terms of latter-day logic, a consistent Friedman groupie ought to have refused to sign that 1932 Chicago proclamation. Meantime, in London, Hayek's 1931 *Prices and Production* had converted the usually sensible Lionel Robbins into the eccentric belief that anything that expanded MV or PQ would only make the Depression worse!

Barnett: You first surfaced as a comer at the University of Chicago. What is your final take on your Midway days?

Samuelson: I was reborn when at age 16 on January 2, 1932, 8:30 a.m., I walked into a Midway lecture hall to be told about Malthusian population. At the zenith of Hutchins's New Chicago Plan, I got a great education in width: physical, biological, and social sciences topped off by humanities.

January 2, 1932, was an auspicious time to begin economic study for two unrelated reasons. The Great Depression was then at its nadir—which attracted good minds into economics and which presented exciting puzzles needing new solutions. The Chicago Midway was a leading center (maybe *the* leading center) for neoclassical economics, and I

Figure 7.4 From left to right, at the University of Chicago Centennial, 1991: Rose Director Friedman, Milton Friedman, Paul A. Samuelson, and George Stigler.

found exciting Frank Knight, Henry Simons, Jacob Viner, and Paul Douglas. My very first teacher, Aaron Director (now around 100), I liked as an iconoclastic teacher. He was the only man alive who could (later) speak of "my radical brother-in-law Milton Friedman." Long without Chicago tenure, his bibliography was epsilon. But without any database, he was a primary creator both of the second Chicago School—of Friedman, Stigler, Becker after Knight, Viner, Douglas, Schultz, Nef, and Simons—and present-day antitrust inactivism.

What incredible luck, while still adolescent, to stumble onto the subject that was of perfect interest to me and for which I had special aptitudes! What work I have done has been for me more like play. And always I have been overpaid to do it.

Director's published works are nearly nil, but his was later a major influence on (or against?) antitrust policy, and his stubborn iconoclasm had a significant role in creating the Second Chicago School of Friedman, Stigler, Coase, and Becker. (See the Stigler autobiography.) Since I entered college before graduating from high school, I missed the 1931 autumn quarter during which the Social Science Survey 1 curriculum surveyed economics popularly. As a makeshift, I was put into an old-fashioned, beginners' course that was being phased out. Slichter's *Modern Economic Society* was Director's assigned text, even though he did not speak well of it. (The following quarter, Lloyd Mints carried on with Richard Ely's best-selling *Outline of Economics*, with micro theory largely by Allyn Young.) Director's best gift to me was his unorthodox assignment of Gustav Cassel's *Theory of Social Economy* chapter on "the arithmetic of pricing," as stolen by Cassel from Walras. Few knew in those Model T days about the mathematics of general equilibrium in economics.

But it was Henry Simons, Frank Knight, and Jacob Viner who most influenced my mind. I may have taken more different economics courses at Chicago than anyone before 1935. Certainly, I was overprepared when entering the Harvard Graduate School in 1935. I also carried the baggage of excessive admiration of Frank Knight until time eroded that away.

The best that Knight told us in those days was that in rare depression times, inexplicably Say's Law and market clearing somehow didn't obtain temporarily. Most of the time, normalcy would serendipitously return and maybe then we could live happily ever after. Maybe. Meantime the only present choice was between communism and fascism. And for himself, Knight would not choose the latter. Later, understandably, he recovered from that failure of nerve and reneged on his circulated text. Somewhere in my files will be found a copy of his doomsday text.

This explains the second reason why 1932 was a great time for an eager teenager to enter economic study. Our subject had myriads of challenging open problems—problems that mathematical techniques could throw light on, and also close out. I once described this as being like fishing in a virgin Canadian lake. You threw in your hook and out came theorem after theorem. Viner is a useful example. He was a great economist, and perhaps the most learned one on the 1931 globe. He was also a subtle theorist. With suitable training at McGill and Harvard, Viner could have been a leading mathematical economist. However, Stephen Leacock and Frank Taussig taught him no mathematics at all. This made him fearful of acne-age students like me and our generations, who seemed to provide him with painful competition. (To do Viner justice, let me state that the 1930s graphics of trade theory by Lerner, Leontief, me, and Meade was in its essence already in a 1931 LSE Viner lecture, that the young Lerner would probably have attended.)

I carried a stout staff in the fight to lift the level of mathematical techniques during the second third of the twentieth century. But an evolving science does not wait for any one indispensable genius to arrive.

Others in plenty would have come along, trained by Hotellings, Evanses, and Frisches to accomplish that overdue task.

Although I've had an acquaintanceship with scores of leading world mathematicians and physicists, I've been surprised at how little help I've been able to garner from presenting orally some unsolved puzzles to them. I should not have been surprised. It is not that a Birkhoff, or Quine, or Ulam, or Levinson, or Kac, or Gleason was incapable of clearing up my open questions. Rather, it is the case that a busy mathematician has no motivation to waste his (or her) time getting intuitively briefed on someone else's models in the idiosyncratic field of mathematical economics. Fortunately, access to the good Harvard and MIT libraries enabled one to ferret out needed book expositions. And it was my good luck that Harvard's E.B. Wilson, only protegé of thermodynamicist Willard Gibbs, provided essential hints that helped in the development of revealed preference and the anticipation of the inequalities techniques in post-1945 economics programming.

Barnett: For some months in 1936 at Harvard, legend reports, you resisted conversion to Keynes's *General Theory*. Any truth in that?

Samuelson: After 1936 February, when copies of *The General Theory* arrived in Cambridge, I did struggle with my own initial criticisms of the book; and I suspect my begrudging acceptance of the Keynesian revolution in paradigm was importantly the result of Henry Simon's remark about short-term bonds as a substitute for *M*, when the interest rates are low. I was influenced by that, plus my earlier recognition that prices and price levels are sticky, and therefore neutral money and Say's Law lose realism. I knew 100 people without jobs in 1931–34 and 100 with jobs. The groups would never voluntarily change places: the latter felt very lucky. The former, about equal in ability, felt unlucky. That's not what happens when auction markets equate supply and demand.

Timing is everything. My Society of Fellows 1937–40 prewar leisure enabled the publication in 1948 of *Foundations of Economic Analysis.* Groups of youngsters all over the world joined to master its fundamentals. Not until 1983 did I prepare an enlarged edition with terse exposition of post-1947 developments. Why did this better book sell so poorly in comparison with its predecessor? It was because practitioners everywhere had become so much more sophisticated by the end of the century. Schumpeter would say: Monopoly profits are bound to erode away, as knowledge spreads, which is a good thing.

Barnett: So why did you leave Chicago for Harvard?

Samuelson: Given my volition, I would never have left Chicago, but a new Social Science Research Council Fellowship, awarded to the eight most promising economics graduates, bribed me to go to a different university. The effective choice was between Harvard and Columbia. Without exception, my Chicago mentors advised Columbia. By miscalculation, I opted for Harvard, not even knowing that it was about to move out of lean seasons, thanks primarily to the European immigrants Schumpeter, Leontief, Haberler, and also later Alvin Hansen.

Three years later, at Harvard, I did thank providence for my hegira *away from* the Midway—where I would have missed out on three great twentieth-century revolutions in economics: the mathematics revolution, the imperfect competition revolution, and the Keynesian effective-demand revolution. I deplore adversary procedures in the healthy evolution of a scientific discipline. Remaining at dogmatically conservative Chicago or accepting its lucrative 1947 professorship would have made me more radical than I wanted to be. For my temperament, serenity would be much more fruitful than the stimulus of polemical debate. I speak only for myself.

Barnett: Franco Modigliani, in his interview in *Macroeconomic Dynamics* [see Chapter 5], stated that he was discouraged from pursuing an offer early in his career from Harvard University by its Economics Department chair, whom Modigliani characterized as anti-Semitic and xenophobic. When you acquired your Ph.D. from Harvard as an A+ student, having produced one of the most extraordinary dissertations of all times, you were offered a position by MIT, but not by Harvard. Do you believe that the prejudices of the Harvard department chair at that time had a role in Harvard's enormous mistake in that regard? If not, why did they fail to hire you immediately upon receipt of your Ph.D.?

Samuelson: Anti-Semitism was omnipresent in pre-World War II academic life, here and abroad. So, of course, my WASP wife and I knew that would be a relevant factor in my career at Harvard. But by 1940, times were changing. Perhaps I had too much of William Tell's hauteur in my personality to ingratiate myself with the circles who gave limited weight to merit in according tenure. When MIT made a good offer, we thought this could test whether there was great enthusiasm for my staying at Harvard. When Harvard's revealed preference consisted of no majority insistence that I stay, we moved three miles down the Charles River. (My Mark Perlman *Festschrift* piece provides a memoir of an earlier "politically incorrect" age.)

In retrospect, that was the luckiest decision I ever made. In less than a decade, postwar MIT developed into a powerhouse in frontier economics. The Ivy League snared future Rhodes scholars. Our magnet attracted most of the NSF Fellows in economics.

Barnett: Tell us about Harvard in the 1930s.

Samuelson: Hitler (and Lenin) did much for American science. Leontief, Schumpeter, and Haberler brought Harvard to life after a lean period. Alvin Hansen was for me an important influence. Outside of economics, both in the physical sciences and the medical–biological sciences, the U.S. dominates. Actually, toward the end of World War II, when victory was no longer in doubt, I was lent by the Radiation Laboratory to help the Vanevar Bush Secretariat draft *Science, the Endless Frontier*. Biochemist John Edsall (Harvard), Robert Morison (physiologist at the Rockefeller Foundation), and I did a lot of the drafting—of course under the instruction of I.I. Rabi, Edwin Land, Olivier Buckley (head of Bell Lab), and other members of Bush's appointed committee. Against some resistance, what emerged was beyond my fondest hopes: an NSF (inclusive of the social sciences), a vastly expanded NIH, rather than a nominated plan to give every U.S. county its population quota of dollar subsidies for research.

Barnett: As you have mentioned, Hitler was responsible for an extraordinary migration of many of Europe's greatest economists to the United States, including Koopmans, Leontief, Schumpeter, Marschak, Haberler, and Kuznets, along with most of the Austrian School of Economics. They in turn helped to attract to this country other major European economists, such as Hurwicz, Debreu, Theil, Bhagwati, Coase, and Fischer. But it is widely believed in much of the world that the United States no longer has the clear political advantage for scholars over Europe that existed at that time, and in fact there is now an increase in the number of American students deciding to study in Canada. Is America in danger of losing its intellectual comparative advantages for economists to other countries?

Samuelson: I do not discern any trend toward foreign out-competition of U.S. science. Sole reason: our predominant real GDP, and the brain drain *to us* it has induced.

Barnett: Your research from the beginning has shown exceptional influence from the physical sciences, and you mention the work of physical scientists extensively throughout your research, as you did in your famous *Foundations*. How did you become so heavily influenced by physical scientists? Did you study their work at some point in your education?

Samuelson: I would be rash to ignore analytical sciences outside of the social sciences. But I would be stupid, if out of "physics envy" or snake oil salesmanship, I would inject into economic theory analytical mathematics that fit only gases and liquids. In my writings, I have criticized wrong analogies to physics by Irving Fisher (whom I admire as a superlative American theorist). Even the genius of von Neumann has not escaped my critical auditings. I have given only qualified approval to

Marshall's hope for a more *biological* and less *physical* approach to future economics. But that has not aborted my writings in demographical genetics, not all unqualifiably admiring of R.A. Fisher's genetical writings. Maybe someday, future Philip Morowskis or Roy Weintraubs will better fine-tune their nuances.

Barnett: Throughout your career, you have tended to have your "finger in every pie" within the field of economics. But at the present time, it is difficult to think of any economists who are "generalists" in such a total sense. To be influential in any area of economics requires a degree of specialization that virtually rules out broad influence throughout the field. Is that because of the dramatic expansion of the field and its growth in both breadth and depth, or is it because we don't yet have another young Samuelson on the scene?

Samuelson: If only because of the explosion of total numbers of academic and nonacademic economists, no young Samuelson today could hope to be the kind of generalist that I used to be. Remember I got a young start. I was a fast and voracious reader who turned the pages of *all* the newly current exchange journals at Harvard's *Quarterly Journal of Economics* office. The micro tools that worked in general theory also worked in trade theory. With some help from me, post-Keynesian macroeconomics lent itself to complete general equilibrium techniques. Post-Fisher pure finance theory was poised to explode. Since probability was a passion with me, the banal statistics taught at Harvard naturally spurred me on to Fisher, Neyman–Pearson, and Wald–Savage further developments.

Having a facile pen helped. Before MIT Chairman Ralph Freeman drafted me to author an elementary text, I wrote for *New Republic* and other publications. Hansen brought me into Washington New Deal circles.

Barnett: The economics profession widely was in error about the consequences of the Second World War. It is well known that a large percentage of the economics profession, including you in an article in the *New Republic*, expected an economic collapse at the end of the war. There were a few exceptions, such as Alvin Hansen and Sumner Slichter. Why did so many economists expect the economy to perform badly at the end of the war? In retrospect, it is difficult to understand why that would have been believed, especially in the United States.

Samuelson: Often I've stated how I hate to be wrong. That has aborted many a tempting error, but not all of them. But I hate much more to *stay* wrong. Early on, I've learned to check back on earlier proclamations. One can learn much from one's own errors and precious little from one's triumphs. By September of 1945, it was becoming obvious that oversaving was not going to cause a deep and lasting postwar recession. So then and there, I cut my losses on that bad earlier estimate. Although Hansen was wise enough to expect a postwar restocking boom, it was his and Keynes's teachings about declining investment opportunities that predisposed my activist contemporaries to fear a post-peace depression. Aside from Hansen and Slichter, Willy Fellner and W.W. Woytinski taped things right: Accumulated saving from the way we financed the war and rationed resources, plus lust for long-delayed comforts and luxuries, were the gasoline that shifted resources from war to full-employment peacetime uses. I knew that argument but did not know what weight to give to it. (Scores of older economists were optimists about 1946 full employment. But if their only support for this view was a dogmatic belief in Say's Law, they [Knight is an example] carried little weight with me.)

Mention should be made of another mid-1940s Samuelson error. I judged that the market-clearing real interest rate level would be 3% or less. That big mistake of course correlated with the earlier unemployment error. I was too stubbornly slow in cutting my losses on that hunch.

Barnett: You were an important adviser to President John Kennedy. To this day, politicians of both major political parties tend to point to Kennedy's economic policy for support of their agendas. To what degree were those policies influenced by you, and who else played a role in those economic policies?

Samuelson: With great reluctance, I let Senator John F. Kennedy recruit me to his think tank. From nomination date to inaugural day I became his chief economic advisor. Our styles and chemistries clicked. I've never regretted staying out of Washington for two reasons: (1) Research is my true love. (2) The CEA team of Heller, Tobin, and Gordon was the greatest ever. (I did help pick them.) Only when they needed my extra heavy lifting from Cambridge did I weigh in.

Barnett: How did you become a mathematical economist? Legends proliferate that you began in physics, or mathematics, and then levitated down to economics.

Samuelson: The truth is that, although I did have aptitude for school math, it was only early in my economic studies that I realized how useful more, and still more, math would be for the puzzles my generation would have to face.

Beulah Shoesmith, spinster, was a famous mathematics teacher at Hyde Park High School near the University of Chicago. A number of scientists came from her workshop. Two of the eight recipients of the 1996 Medal of Science had been her pupils, as were Roy Radner and my brother Bob Summers. I took the many courses offered: advanced algebra, solid geometry, and (boring, surveyor-like) trigonometry. However, in the old-fashioned curriculum, neither calculus nor analytic geometry was considered to be a precollege subject—a terrible mistake. So, after my freshman college year, I hurried to make up for lost time.

Aside from mathematics coursework, I was to a considerable degree self-taught. (When I thought determinants were boring, graduate student George Stigler showed me the big ones Henry Schultz assigned. That wised me up.) Before I knew about Lagrange multipliers, I had worked out the Stackelberg improvements on the Cournot–Nash solution to duopoly. In working out a theory of the circulation of the elite, I discovered matrix multiplication before I knew about matrices—Markov, Frobenius, or Minkowski. I took or audited, at Chicago or Harvard, useful courses from Barnard, Graves, George Birkhoff, Hassler Whitney, Marshall Stone, and especially Edwin Bidwell Wilson. E.B. had been the only protegé at Yale of Willard Gibbs. Since I was Wilson's main protegé, that makes me kind of a grandson to Gibbs.

Fortunately, I was enough ahead of my contemporaries in economics that I had all the time in the world to spend in the library stacks on mathematics. Never did I reach a limit to usefulness of more elaborate mathematics. My economic problems dictated where my math preoccupations should go—not vice versa. Of course, it was Edgeworth, Walras, Pareto, Gibbs, E.B. Wilson, Griffith Evans, Frank Ramsey, Bowley, R.D.G. Allen, Hicks, Frisch, Lotka, Leontief, and von Neumann who were my masters. I'm afraid that I was a captious pupil, often stubbornly critical of my betters. (Example: von Neumann's foundations for cardinal utility in stochastic Laplacian choice begged the issue of the Ramsey–Marschak–Savage–Debreu independence axiom by burying that in his zeroth axiom. Worse, he stubbornly ignored all of his critics.)

Figure 7.5 Paul Samuelson with Bill Clinton in the White House.

At Harvard [1935–40], economists learned little statistics, except in E.B. Wilson's small seminar. Outside Schultz's specialized graduate course, the Chicago economics curriculum had been little

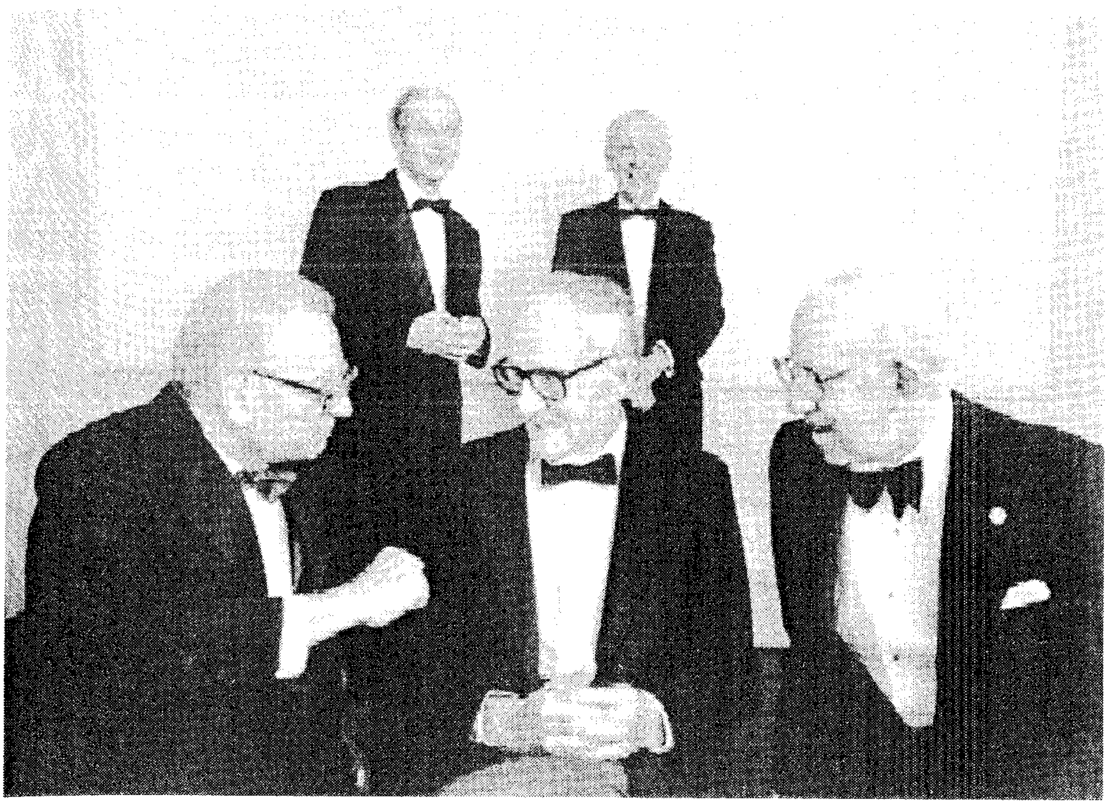

Figure 7.6 Paul Samuelson (front left) with Jerome Friedman (Nobel Prize in Physics), Theodore Schultz (Nobel Prize in Economics), James Watson (Nobel Prize in Biology), and George Stigler (Nobel Prize in Economics) at the University of Chicago Centennial, 1991.

better. In the early 1930s, I had to read, on my own, Thurstone's little potboiler to learn about the rudiments of statistics. Only at Columbia was Hotelling teaching 1920–30 R.A. Fisher. Of course, all this changed rapidly once Wald, Feller, Tukey, and Savage entered the scene.

Barnett: How can we relate your Stolper–Samuelson work, and your later Heckscher–Ohlin–Samuelson research to the present revolts against globalization? Can this trend among some of the world's youth be viewed as opposition by the political left to the implications of your work on trade?

Samuelson: Trade is confirmed to be a substitute for massive immigration from poor to rich countries. U.S. labor has lost its old monopoly on American advanced know-how and capital. U.S. total real GDP has net gained [1950–2003] from foreign export-led growth in Pacific Asia and the EU. However, free trade can also systematically affect U.S. wages/GDP share and overall inequality. My little Nobel Lecture ["International Trade for a Rich Country," lecture before the Swedish–American Chamber of Commerce, New York City, May 10, 1972: Stockholm: Federation of Swedish Industries pamphlet, 1972] pointed out that a rich place *can lose net* when a poor one newly gains *comparative* advantage in activities in which previously the rich county had enjoyed comparative advantage. Free trade need not help *every*body *every*where.

Barnett: Do you have views and reactions to the "rational expectations" approach and real-business-cycle theory? In the dialogue between James Tobin and Robert Shiller in *Macroeconomic Dynamics* [moderated by David Colander; see Chapter 16], Tobin stated that real-business-cycle theory is "the enemy." In contrast, as is seen in much of the published research appearing in this journal, the use of rational expectations theory (sometimes weakened to include learning) and stochastic dynamic general equilibrium theory is common within the profession among macroeconomists of many political views.

Samuelson: Yes, but a lot of different things are loosely related to the words "rational expectations." One extreme meaning relates to "the New Classical doctrine," which alleges in effect that Say's Law does obtain even in the short run. I do happen to believe that the U.S. economy 1980–2003 behaves nearer to Say's Law's quasi full-employment than did the 1929–60 U.S. economy, or than do say the modern French and German economies. But this belief of mine do not necessarily require a new Lucas–Sargent methodology. Sufficient for it is two things:

(1) The new 1950–2003 freer global trade has effectively intensified competition with U.S. labor from newly trainable, low-wage Pacific Rim labor—competition strong enough effectively to emasculate the powers of American trade unions (except in public service and some untradeable goods industries). Nowadays every short-term victory by a union only speeds up the day that its industry moves abroad.

(2) There has been a 1980–2003 swing to the right among voters, whose swing away from "altruism" is somewhat proportional to the *time elapsed* since the Great Depression and since the U.S. government's effective organization for World War II's "good" war. As a result, trade unions no longer benefit from government's help.

A "cowed" labor force runs scared under the newly evolved form of ruthless corporate governance. In contrast to Japan, when a U.S. CEO fires redundant workers quickly, Wall Street bids up the price of the firm's shares.

Another weak form of "rational expectations" I agree with. "Fool me once. Shame on you. Fool me twice. Shame on me." Economic historian Earl Hamilton used to agree with the view that, when New World gold raised 1500–1900 price levels, nominal wages tended systematically to lag behind. Kessel and Alchian had a point in suspecting that people would at least in part learn to anticipate what has long been going on. I concur to a considerable but limited degree.

Some rational expectationists overshoot, in my judgment, when they exaggerate the "neutrality of money" and the "impotence of government to alter *real* variables." Friedman's overly simple monetarism à la 1950, was criticized from his left for its gross empirical errors. What must have cut him more personally would come from any Lucas follower who accused Friedman of *fallaciously* predicting that mismanagement of M in $MV = PQ$ was capable of deep real damage rather than of mere nominal price-level gyration.

Modern statistical methodology, I think, benefits much from Lucas, Sargent, Hansen, Brock, Prescott, Sims, Granger, Engle, and Stock–Watson explorations and innovations. But still much more needs to be analyzed. Strangely, theory-free vectoral autoregressions do almost as well. Also, variables that pass Granger causality tests can seem to perform as badly in future samples as those that fail Granger tests. And, still the nonstationariness of economic history confounds actual behavior and necessarily weakens our confidence in inferences from past samples.

This does not lead me to *nihilism*; but hopefully, only to *realism*, and, à la Oliver Twist, to urge for *more* research.

At many a Federal Reserve meeting with academic consultants, there used to be about one rational expectationist. So unuseful seemed their contributions and judgments that the next meeting entailed a new rational expectationist. And each year's mail would bring to my desk a few dozen yellow-jacket manuscripts from the National Bureau, purporting to test some version of rational expectationism. Many were nominated for testing; few passed with flying colors the proposed tests. I continue to live in both hope and doubt.

In some quarters, it is a popular belief that macroeconomics is less scientific than micro and less to be admired. That is not my view. I think macroeconomics is very challenging, and at this stage of the game it calls for wiser judgments. A lively science thrives on challenges, and that is why I transfer a good deal of my time and energy from micro to macro research. Probably as a syndicated columnist, I have published at monthly intervals a couple of thousand different journalistic articles. Maybe more. My aim is not to be interesting but rather, as best as I can, not to be wrong. When my conjecture is still a conjecture, I try to mark it as such. My notion of a fruitful economic science would be that it can help us explain and understand the course of actual economic history. A scholar who seriously addresses commentary on contemporary monthly and yearly events is, in this view, practicing the study of history—history in its most contemporary time phasing.

NOTES

1. Perhaps those rare exceptions might include game-theoretic and topological models and maybe the recent literatures on complex unstable nonlinear dynamics, sunspots, and incomplete markets. But I would not be surprised, if he were to correct those speculations as misperceptions, if I were to ask.
2. The current URL of that Web site is http://cepa.newschool.edu/het/home.htm