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MD DIALOGUE

CONVERSATIONS WITH JAMES TOBIN AND ROBERT SHILLER ON THE "YALE TRADITION" IN MACROECONOMICS

Conducted by David Colander
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Every graduate school has its own distinctive history that makes it unique in some way, but every graduate school is also part of the broader economics profession and reflects the currents in the profession. The following dialogue focuses on the question: Is it useful to distinguish a "Yale school of macroeconomics" from other schools of economics? The idea for this dialogue came from William Barnett in a discussion with Bob Shiller. Bill suggested to Bob some names of individuals who might conduct the "dialogue" and I was selected from that list. I happily agreed because, from my knowledge of the writings of the Yale faculty, I felt that there was a uniformity of ideas with which I was sympathetic, and which might deserve to be called a "Yale School"—a view shared with Bob Shiller.

Exploring the issue further, I found that there was far less agreement on whether the macroeconomics work that currently goes on at Yale can be classified meaningfully as "the Yale school." The objections to specifying a separate Yale school were the following: (1) The term, Yale school, had been used in the 1960's to describe Jim Tobin's position in a debate with monetarists. Some felt it would be confusing to use the Yale school classification to describe a broader set of works that are not connected to that earlier, more narrow, use. (2) Calling the work in macroeconomics currently done at Yale a "school" distinguishes it too much. The work that goes on in Yale is similar to the work that goes on in any top graduate economics program. It is not so clear how the work at Yale differs from, for example, MIT or Princeton. It would need to be more distinct to warrant calling it a "school." (3) There is a diversity of approaches that are used at Yale, and it is not clear that they actually fit together. For example, Chris Sims's work follows from a time-series statistics tradition with influences from real-business-cycle and calibration work; Shiller's work follows from a Keynesian tradition. Fitting them together requires a bit of a stretch. (4) The degree of continuity in the Yale school over time is not as great as I had first imagined. There was little linkage at Yale from Irving Fisher to Jim Tobin; thus the historical continuity needed for specifying a Yale school does not exist. These objections are elaborated in the dialogues below.

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After discussing these issues with a number of Yale faculty, I decided that there probably wasn't a Yale school of economics, but that there was a Yale tradition. We also decided to have a conversation with only two individuals—Jim Tobin and Bob Shiller—because they are major figures in maintaining what I believe is a Yale tradition. The conversations were held separately, although I asked many of the same questions to both, and focused much of the conversation on the issue of whether it is useful to distinguish a Yale school. Thus, the conversations discuss the work of other individuals at Yale more than a dialogue with another focus would have, and do not cover Tobin's or Shiller's current work as much as conversations with an alternative focus would have. The results are, I believe, interesting. They provide some useful insight into both the Yale tradition and current thinking and debates in macro.

Keywords: Yale School, Macroeconomics, Neo-Keynesian, Tobin, Shiller

A Conversation with James Tobin

Fall 1997

Colander: You went to Harvard as an undergraduate.

Tobin: That's right; I graduated in 1939. I didn't leave Harvard graduate school until two years after; it was 1941. I got the MA in one year because I had taken so many graduate courses when I was still an undergraduate.

Colander: At that point you were still working for your Ph.D., right?

Tobin: Yes. I was still taking more courses, more seminars, and so on. In the spring of 1941, I had taken a course with Ed Mason on the economics of defense. I was also teaching myself econometrics. The Harvard economics department didn't have much in the way of modern statistics then. They had statistics courses, which I took, but they didn't have a course in econometrics as we now think of econometrics, and the teachers of economic statistics were not very enthusiastic about using statistics. Mainly, they were telling us the pitfalls of using statistics, so, aside from a seminar by a visitor, Hans Staehle from Switzerland, on demand analysis, we didn't have much going on at Harvard in this area. I took some mathematical statistics in the math department, and I took some advanced mathematical theory with Edwin B. Wilson, who was in the Public Health School but was, among other things, a first-rate mathematical economist.

In Mason's course, I had used the regression analysis that I'd been learning to estimate the demand for steel in the United States. Ed was involved in questions of mobilizing the economy for defense, so he suggested that I go to Washington and work in one of the new agencies which was supposed to be cutting down civilian uses of some of the potentially scarce metals like steel, aluminum and nickel. They weren't prohibiting the civilian uses of these things; the point was to cut them down and then to allocate them to the civilian uses that were still to be allowed. This

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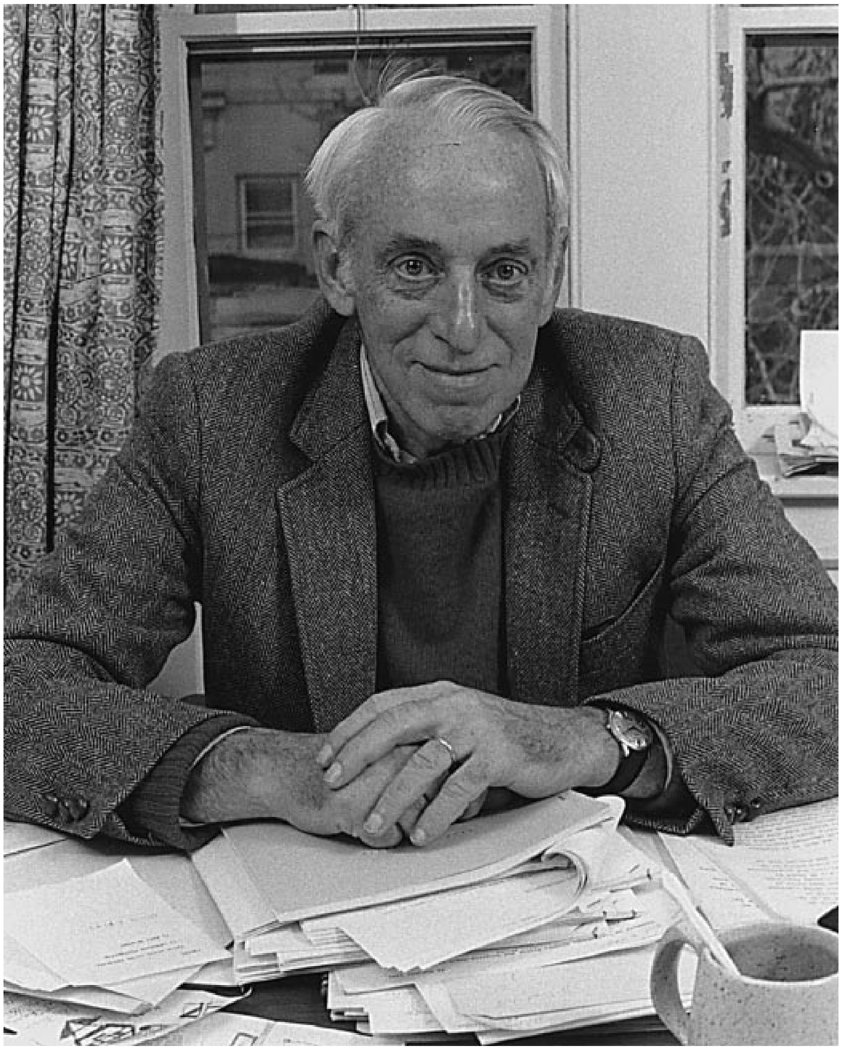

FIGURE 1. James Tobin.

was one job of an agency called the Office of Price Administration and Civilian Supply, and I went to work in the civilian supply part in the summer of 1941.

I moved to a different agency, called the War Production Board, after the war started but, meanwhile, after Pearl Harbor I decided I would not want to spend the war doing this, so I enlisted in the Navy and then I was actually called to duty, duty being to go to school to learn to be an officer in 90 days, in April 1942. And then I was gone from economics until January 1946.

Colander: Then you went back to Harvard.

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Tobin: Yes, I went back to Harvard. I got out of the Navy in the middle of December 1945, close to Christmas, and I went home. I had been on the same destroyer all that time; after I got my commission. I went home, and I had friends who were still in the government, and they were offering me jobs to come back to the government. I had been very successful as a government economist, and so those were attractive jobs. They paid a lot of money—what *looked* like a lot of money—so I didn't know what to do. Meanwhile I made inquiries about going back to Harvard. I wrote to Seymour Harris, whom I'd known extremely well at Harvard. He had taken an interest in me even though I had never been in one of his classes. I asked him if I could get a tutorship in one of the colleges—that would give me a room—and I asked the chairman of the department if I could get a job as a teaching assistant. I told these professors that I was considering whether to come back and finish the degree or to go back to Washington.

The chairman of the department was Harold Hitchings Burbank. He was a very conservative economist. He liked to run the department and chronically did. He replied by letter that he had been told by people who had examined me and people who had had me as a student that I had an unusually good chance of being a distinguished economist and therefore it would be a great mistake if I left academia. He and I hadn't been particular friends—we hadn't been enemies, but we hadn't been particular friends—but I was very much influenced by that letter, and I went back.

Colander: You got your Ph.D. in 1947 and you went to Yale in 1950. . . .

Tobin: Yes. Meanwhile, I had a Junior Fellowship at Harvard. It was actually my first job although it was not meant to be. It was meant to be a substitute for a doctor's degree, not a postdoctoral position. But they waived that requirement of the Junior Fellowship for veterans like me, because they could understand that the first thing I wanted when I got back was to get a degree. So the Junior Fellowship I got worked out as postdoctoral. I spent two years at Harvard, and the other year I went to England to Richard Stone's Institute, the Department of Applied Economics at Cambridge. *Then* I went to Yale.

Colander: Were you thinking of going any other place?

Tobin: I was away from the United States in the year before I was going to take a job, so a lot of the things had to be done by mail. I had been invited all over the country in the previous year—out to California, to Stanford, and all over the place. So I had a lot of opportunities. The best thing that had happened to me when I went back to graduate school in 1946 was that I met my wife. I met her in the spring of that year and we got married in the fall, and if I'd gone to Washington I wouldn't have. She said she'd go anyplace except New Haven, but we went to New Haven and she loved it.

Colander: I want to discuss a bit about the Yale school.

Tobin: I should say, in regard to Bob Shiller's contacts with Barnett, that I think there's a little bit of confusion between what is described as "the Yale school" and other informal institutions such as the eleven-o'clock coffee group, which meets most every day. This eleven-o'clock coffee group is not the Yale school—it includes people who might be identified as part of the old macroeconomic Yale school but it includes other people, too: Chris Sims, who's an advocate of real business cycles; Martin Shubik, a very interesting guy, but I don't think of him as a member of any particular school except his own; John Geanakoplos, who is sympathetic to my macro views but who wants to reconcile them to Arrow/Debreu; Herb Scarf, a math theorist; and T.N. Srinivasan, a tough neoclassical economist. I think that the people listed by Barnett and Shiller are part of this coffee group, but I would not classify them as being a Yale school. They are just a congenial and interesting subgroup of the Yale department.

Colander: How would *you* summarize the Yale school?

Tobin: I think that what people meant by the Yale school goes back to the 1950's. It is identified with my, and Art Okun's, macroeconomic and monetary views and teachings. (The importance of the late Art Okun, and the loss to all economists of his premature death, can't be exaggerated.) In our work we attempted to provide a reasonably systematic view of what Keynesian economics was, and what applications were possible. In monetary theory, the Yale school provided an alternative to monetarism. It involved the possible roles of monetary and fiscal policy. I was very much involved in that controversy with monetarists.

Around 1970, some Yale graduate students produced a T-shirt. On the back, it read "Yale School," an obvious counter to the much touted "Chicago School." On the front it read "Q is all that matters." The latter was intended to be a parody of monetarism's "M is all that matters," which I had criticized, arguing that the most you could say is "M matters." I think this usage supports my view of the meaning of the term, Yale school. We of the Yale school were not the only macro-and monetary economists at Yale at the time. There were Willy Fellner, Henry Wallich, Robert Triffin, and Richard Ruggles, each with his own ideas and interests. We were all on good terms and learned from one another.

In retrospect, that controversy doesn't look as important as the one between Keynesian economics and *New* Classical macroeconomics—about whether or not the actual economy is best described as a continuous full-employment solution.

Colander: What would be the Yale school's position on that?

Tobin: Well, the Yale school position on that is that sometimes the economy is characterized as being at, or close to, or maybe above, full employment. In that case, the opportunity-cost logic of neoclassical economics applies. At other times, however, the economy is better described not as a perfectly competitive market-clearing situation, but as a situation with general excess supply (particularly in the labor markets). In this case it is possible for monetary and fiscal policy to increase aggregate demand and increase output. That doesn't mean that opportunity cost calculations are ruled out, but it does change the nature of the calculation. The macro-calculation concerns how you expand the economy. Since there are several different ways of getting to full employment from a situation of excess supply, one should apply welfare analysis to choose the appropriate path. For example, you might want to recover by monetary expansion rather than fiscal expansion, if you were trying to do something to improve long-term growth while you restore full employment.

I always regarded myself as a partner in crime, as Samuelson described me, in developing the so-called Neoclassical Synthesis. I don't think Neoclassical Synthesis is a good name for it, since it was a Neoclassical/neo-Keynesian synthesis, but I guess it was a modification not of Keynes, but of some of the Keynesians whose view was that the economy was always at an under full-employment equilibrium, and that thus the Neoclassical rules never apply. That was not the view of American Keynesians, myself included, in the early postwar years. It was, however, the view of many of Keynes's followers, especially in the UK. They had no use for monetary policy at all in the early 1950's. I differed from that group in that I taught that monetary policy was a possible tool of macroeconomic policy and that to neglect it was a mistake.

Colander: One of the arguments against the neo-Keynesian portion of the neoclassical synthesis is that its interpretation of Keynesian economics is incompatible with the Walrasian model, but the neoclassical synthesis nonetheless forces it into that framework.

Tobin: Well, it doesn't do *that*—it just recognizes that that might be a good start for a model of a full-employment economy over a long period of time. In that sense, neo-Keynesian economics regards the business cycle as a departure from a Walrasian market economy, and explicitly says that that's what it is—situations of excess supply and excess demand at existing prices and wages. So I don't think it's guilty of doing that. It's just not throwing away the insights of Neoclassical economics.

Colander: Most of the formalizations of neo-Keynesian economics assume pure competition in the goods market.

Tobin: Well, I don't have to assume that. It might make some difference for some things, but let's think about the question of whether, as Keynes assumed, the labor market is always on the marginal productivity curve, which is the Neoclassical view. That implies that, given capital stock and technology, the lower the marginal productivity of labor, the greater the employment of labor, and therefore the lower the unemployment rate. So if we're going to have an increase in employment—a reduction of *un*employment—we're going to go down the marginal productivity curve and have a lower real wage. That's a kind of pure-competition result, applied to an environment where you wouldn't expect market clearing to be a part.

Why did Keynes do that? I think he did that because he wanted to make the point that his quarrel with Classical economics was valid even though he accepted a large part of Classical doctrine, this particular thing being a an important example.

Then, there was the empirical finding of several economists such as John Dunlop that, even in the late 1930's before the war, actual real wages did not move counter to the business cycle—but, instead, increased during cyclical recoveries.

I think it's a mistake to think that this real-wage observation requires some big correction of Keynes and that Keynes had made a serious error in this respect. It has seemed to me, always, that this actually just strengthened Keynes's case; it did not diminish its logic. Say you ask "What is the consequence of adopting Keynesian monetary or fiscal policies to eliminate unemployment?" If you say, "This can actually be done without a reduction in the real wage," so much the better; that just makes the case for Keynesian policies that much stronger. So it's not a devastating criticism of Keynes that he used this "Classical" view of the demand function for labor in relation to the real wage. On the contrary, his case is strengthened. If it is true that, in the short run, the behavior of prices is not competitive, and that maybe there is not increasing marginal cost in the short run for the firm as he assumed, it actually strengthens Keynes's case.

Colander: When New Classicals really tried to force a microfoundation to the analysis, and assumed perfect competition in both goods and labor market in a Walrasian framework, they showed you can't really have a problem.

Tobin: Well, of course, but that's exactly what the Yale school believes. That's my view. The Walrasian solution doesn't apply in situations in which there's excess supply at existing prices, where prices are not moving rapidly enough, or correctly enough, to clear the market by price at every moment of time—whether it's monopolistic competition or pure competition. I think it's absolutely absurd, and contrary to a lot of empirical observation, to say that the observations we get during short-run business cycles are the result of price fluctuations and wage fluctuations which are always clearing the market. But that doesn't undermine the Walrasian model as something that is a useful way of looking at economies.

Colander: So you'd accept that the Walrasian model can still be used as the model in the long run.

Tobin: Yes. You can amend the Walrasian model to have a world of monopolistic competition in the long run also. That also could be a situation in equilibrium, where the firms in monopolistic competition with each other have no incentive to change the prices, or the wages, that they're offering to employees, so there is a long-run monopolistic-competition counterpart of market clearing.

Colander: John Geanakoplos has worked on microfoundations of an aggregate economy with multiple paths. He and others have argued that it might be useful to analyze the economy in a long-run model with multiple paths.

Tobin: I'm not persuaded that that's a useful way to go about macroeconomics in the sense that I'm interested in it, which has much to do with stabilization policy—macro policy in the short run. A lot of people have tried to save a combination of Walrasian economics and allegedly Keynesian outcomes by multiple equilibrium paths, but it seems to me not to be Keynesian in its consequences because, in every such equilibrium, there's no involuntary unemployment; there's no excess supply. There's market clearing in every equilibrium, so I don't think that that's a fruitful way of making a combination of Neoclassical economics with market clearing and Keynesian results. It could be true that some of the results with multiple equilibria—that some of the equilibria are better than others for labor or for whomever. But none of them is a situation of involuntary unemployment, which is a situation in which there's not market clearing in any of the equilibria—not market clearing at the existing prices.

John Geanakoplos and other moving-equilibrium theorists also worry in a rather deep sense about phenomena like missing markets. If the modern theory of

Walrasian economy is Arrow-Debreu equilibrium, then it's pretty obvious that there are all kinds of missing markets. Some markets are prohibited from existing—you can't sell yourself into slavery and that sort of thing. So people like John Geanakoplos like to look at all problems—including Keynesian problems—as examples of these fundamental defects or departures from pure Walrasian outcomes. I say, more power to them. That's not the Tobin Yale school, though; the Tobin Yale school is more pragmatic than that, and, in a sense, much less fundamentally theoretical.

Colander: What's your view of the Clower-Leijonhufvud approach?

Tobin: I don't have anything against that; I just never found myself really instructed by it.

Colander: What's your view of the New Keynesian approach?

Tobin: I'm not sure what *that* means. If it means people like Greg Mankiw, I don't regard them as Keynesians. I don't think they have involuntary unemployment or absence of market clearing. It is a misnomer to call Mankiw any form of Keynesian.

Colander: How about real-business-cycle theorists?

Tobin: Well, that's just the enemy.

Colander: [Hearty laughter.]

Tobin: That's what we've been fighting about all these years, and that's just a repetition of the conflict between Keynes himself and the economists he regarded as Classicals—not the best word to use for them. The New Classicals and the real-business-cycle believers are much more extreme than the people that Keynes was arguing with in his day, but it's the same argument over again. Actually, Pigou was a much more reasonable, plausible economist than Lucas and some of the other New Classicals.

Colander: What do you think of the recent Hahn/Solow book on Keynesians in a rational-expectations framework.

Tobin: I thought that book was the multiple equilibrium thing over again. It doesn't seem Keynesian to me, but maybe I'm missing something.

Colander: I was introduced to the Tobin Yale school when I studied your debate with Brunner and Meltzer. How would you respond to Brunner's comment that "the most serious and pervasive flaw is that the Yale monetary theory offers no rationale for money."

Tobin: I think that's ridiculous.

Colander: What is the rationale for money within this Walrasian system?

Tobin: First, what Walrasian system are you referring to? I don't understand *that*. I have a multiasset description of the financial sector; Brunner and Meltzer also have one, and I never did understand how at the same time they have multiasset substitutable assets and yet, in the end, they come to a monetarist result which seems to be inconsistent with the assumed substitutability among assets, including substitutability of some assets for money proper. I never understood how, then, they could combine having a similar multiasset framework with having a monetarist conclusion that depends on there not being substitutability of other assets for

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money. I never did understand that, so I don't understand how the Brunner/Meltzer framework has an explanation for the usage of money whereas my framework doesn't have it.

Saying that money is all-powerful is not an explanation of why it's there. The explanation of why it's there goes back to Jevons and the advantages of the society agreeing upon a common means of payment. Once that agreement has been reached, the advantage of accepting money as means of payment for goods and services is that you know you'll be able to use it again in another transaction, avoiding, as Jevons said, the double coincidence of wants. I have written a paper on money—just "Money." That's the title of it. It's in the Palgrave on money and finance, and I take up this issue that you've just referred to.

Colander: What if they mean—if one has money, one needs transactions in the model, and the model of each of the markets has to have a transactions cost?

Tobin: You're seriously saying that about *me*?

Colander: No. I'm saying that about the assumption of perfect competition, which underlies the Walrasian model.

Tobin: This isn't perfect competition. We're talking about the supply and demand for different assets. I have a paper, a well-read paper, on transaction costs for money, for moving money to interest-bearing assets, other assets than money assets, so I don't think I'm vulnerable to that particular criticism. I think the deeper kind of problem is why anybody holds money, because it doesn't have any value in the end, and so you say, "Well, nobody will have money on Judgment Day or an hour before Judgment Day, and if anybody held money an hour before Judgment Day they wouldn't want to hold it *two* hours before Judgment Day, so why do they want to have it *now*? So, in some sense, if you want to get involved in this kind of philosophical argument, you could say that it must be that whatever date is guessed at for the end of the world, there's always some probability that it's going to last longer than that. So there's some reason to hold money because you're going to want to make transactions, or your heirs are going to make transactions beyond that. I can't get excited about that.

Colander: Of the people in the 11:00-a.m. coffee group, who would fall within the Yale school in the sense that you are talking about?

Tobin: I certainly don't want to appropriate for *myself* the adjective "Yale." There are lots of people in macroeconomics around Yale who wouldn't have been regarded as sympathizing in all respects or even some fundamental respect, with me and Art Okun, like William Fellner who was a good friend, but who had a quite different approach to macro. I learned a lot from him and we agreed on a lot of things, but we certainly didn't agree about Keynesian economics.

But who, now, at Yale would be regarded as sympathetic to my views in macroeconomics? That would be Ray Fair; Bill Brainard, who was a partner in developing a lot of what I did in the models we were just discussing; Bill Nordhaus, who was a student of mine, and a collaborator of mine not so much in macro but in other things like the MEW (the measure of economic welfare); and Bob Shiller, who's, again, not a student of mine or a collaborator, but who thinks about macroeconomic things in similar ways to me and Bill Brainard (see Figure 2). That's probably it.

Colander: How about the Tobin School outside of Yale? Who would you include as its members?

Tobin: There's Gary Smith, who was at Yale before and who's now at Pomona. He did a lot of work with us when he was at Yale, so he certainly is one. I don't know; it's hard to answer that question without thinking about it a while. There are many people who tend to agree with me about the general thrust of macroeconomics, which included several people at MIT like Franco Modigliani, but I wouldn't say he learned anything from me—he had it all himself; Bob Solow and Paul Samuelson and younger people such as Stanley Fischer at MIT and Alan Binder at Princeton.

More directly involved with the Yale school is Janet Yellen who is currently chair of the Council of Economic Advisers and was my T.A. and collaborator, and George Akerlof, although I didn't have him as a student except maybe as an undergraduate. I'd also include Don Hester, my first R.A., now at Wisconsin; Don Nichols and Steve Durlauf, also at the University of Wisconsin; Jim Pierce, now at Berkley; and Ralph Bryant at Brookings.

Colander: How would you explain a lot of the younger students choosing to work within a New Classical framework?

Tobin: Well, I think there was a counterrevolution against the Keynesian economics of the 1960's, and it occurred both within the profession itself and in the general opinion in the country, probably the result of the Vietnam War and the inflation that came as a result of that war and the price shocks in the 1970's. I think that Keynesian economics was erroneously blamed for the inflations of the Vietnam War period and especially erroneously blamed for the inflations of the 1970's and the early 1980's. I read the histories that some people write—what went on in the world—that attribute everything that happened in those years to bad monetary policy, without any recognition of the external shocks involved.

Within the profession itself, I guess there is a strong current for equilibrium solutions. There always has been. The rational-expectations New Classical real business theory also offered young economists of a mathematical bent a new outlet and challenge for their talents. I think some of the appeal of Keynesian economics in the 1930's, 1950's, was *that*, also, but, by 20 years later, those challenges had been exhausted, so if you were a young economist looking for something exciting to do, rational expectations was the thing. So, the idea that you should have microfoundations of everything you do, everything you say is going on in the economy, including short-run behavior, has a surface plausibility. Actually, I think there's not much possible content in trying to describe the behavior of every individual in the society as a solution of a dynamic programming problem so that you explain the whole of what is obviously to me, disequilbrium behavior—I think we don't have very much knowledge of how to model it, so that idea of microfoundations, I think, meant that it seems plausible. Then what it means is that you can't do any *macro*economics. The result of that is we have this schism between abstract academic theory and practical macroeconomics, which is done by the

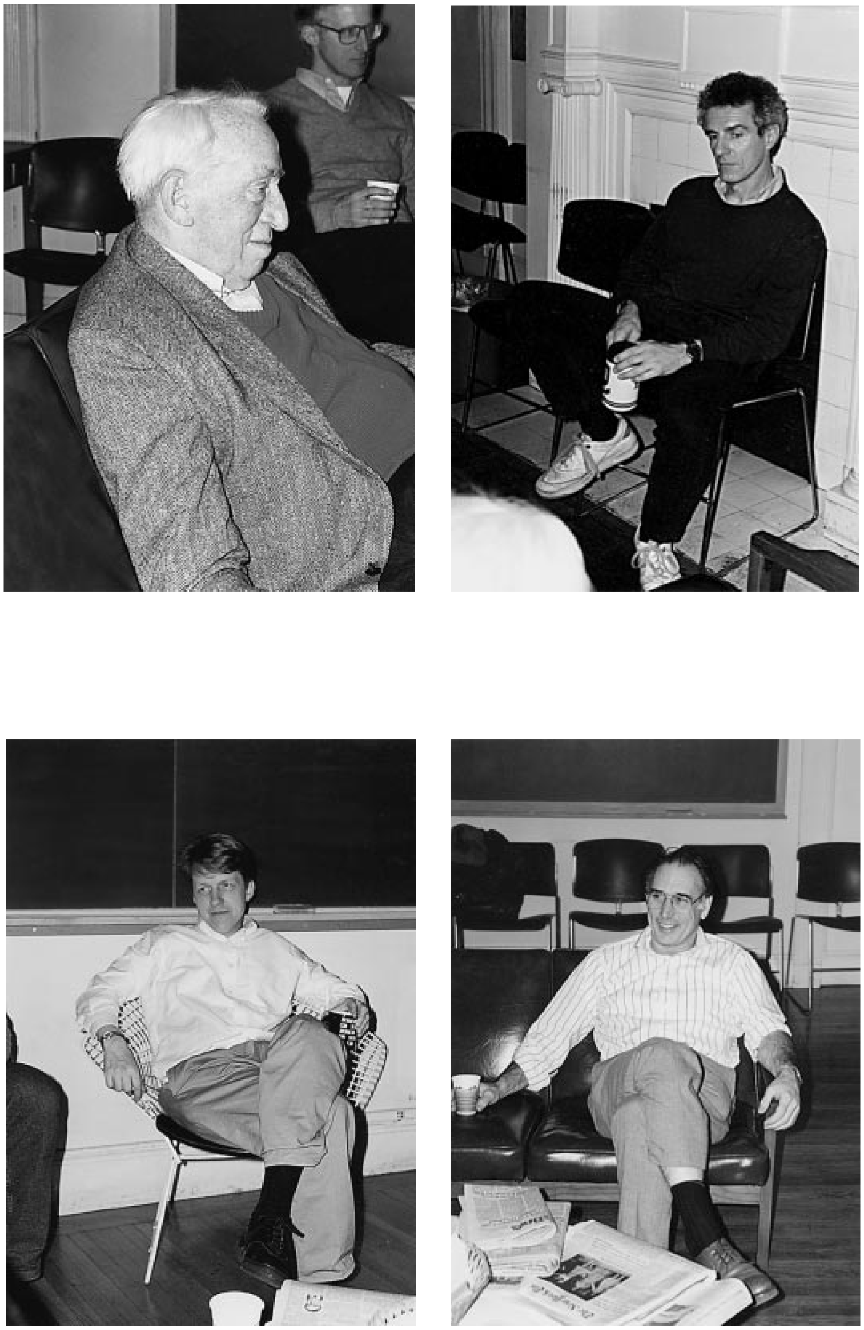

Figure 2. Participants in Yale Cowles coffee hour, clockwise from top left, James Tobin and Bill Nordhaus, Ray Fair, Bill Brainard, and Robert Shiller.

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people who actually have to make decisions about these things—the Congressional Budget Office—the executive government, and central banks.

Colander: I would view the Tobin School as the foundation of practical macroeconomics.

Tobin: I would like to think of it that way, and as a field that progresses and uses methods spawned in the professional journals. It has to go on because there are practical problems that have to be addressed and solved. But meanwhile, graduate students are not going to be able to publish what they do unless they use the currently approved methodologies. That is creating a schism between academic professional training and the kinds of economics that are useful in policymaking. There's a lot of economics being written on practical problems these days and there's a lot of it that's very good stuff. It's not so much about macro policy that we're talking about, but a lot of important economic problems like Social Security, health care, and government budgets. I'm not saying the profession is going completely to the dogs; I'm just saying that it would be nice if there were a little more acceptance of different ways of looking at things in the macro area.

Colander: Now, again, we've said the Yale school didn't have a history, but if we're thinking about Yale over a longer period, Irving Fisher (Figure 3) clearly comes to mind. Would you see any connections there—Irving Fisher and you. Is there continuity in the "Tobin Yale school," or did it begin in 1950?

Tobin: There isn't much continuity, as a matter of fact. I have the greatest admiration for Irving Fisher's work. He made outstanding contributions to many subjects that have been of interest to me: transactions' demand for money, theory of interest and investment, multiperiod consumption, debt burdens, deflation, and depression. But Fisher was a quantity theorist, a monetarist, and had no use for Keynes or fiscal policy. His debt-deflation theory of depression was leading toward Keynesian macro. His views on monetary policy, gold, and reflation in the 1930's were unorthodox and correct. Fisher was an example for all economists, but there is not a continuity between his work and what is generally known as the Yale school.

Irving Fisher died in 1947 when he was 80 years old. By that time he had been formally retired for 15 years, or more, and before he formally retired he had not been teaching much at all and he had not had graduate students. Irving Fisher did not have a "school" created by his own teaching and scholarship because he didn't have any graduate students at all. He essentially withdrew from active participation in the department around 1925. He did all of his work in his house with his own research assistants. The only real Fisher disciple was James Harvey Rogers. He was a sort of Keynesian before Keynes in the 1920's and 1930's. He was not a slavish disciple of Fisher but he was a clear follower. He was a very good economist. He died in an airplane accident when he was, I think, around 50 years old. He was long gone when I got there so there was no continuity.

There were some young people who taught macroeconomics at Yale in the 1930's: for example, Richard Bissell, later of CIA fame and notoriety, and Max Millikin. They were young people and they were the Keynesian vanguard at Yale,

FIGURE 3. Irving Fisher. ca. 1945.
Source: Irving Fisher Papers, Manuscripts, and Archives, Yale University.

but they went to the war and OSS and CIA and they were outside Yale and mostly out of economics after that. So there wasn't continuity from the prewar Yale to the postwar 1950's Yale and the prewar Yale, with Fisher's and Rogers's exception, was very conservative and not particularly good.

Colander: That's about all the time we have, and this is a good place to stop. Thank you very much.

A Conversation with Robert Shiller

May 1998

Shiller: You talked to Tobin already?
Colander: Yes, we talked last fall.
Shiller: I understand he is opposed to using the term "Yale school" broadly.
Colander: Yes, he felt the "Yale school" has a much narrower connotation.

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Figure 4. Robert Shiller.

Shiller: He may be right. Maybe we shouldn't use the term "school" to describe any department of economics. Yale's department of economics has changed in many ways over the years. Yale's department of economics, like almost all departments, is a grouping of people of diverse interests and approaches, and at any given time reflects the profession at large at the time much more than any one school of thought.

And yet, there are subtle differences in traditions, philosophies or in methods that do distinguish departments somewhat, and people actually care a lot about these differences. Subtle differences in approach to economics end up influencing students' decisions where to go to graduate school, and where to take a job thereafter. While these differences are hard to describe, they are actually more important than the rankings of departments that are given so much attention.

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I think there is a tradition at Yale, call it the Yale school if you will, that precedes Tobin and goes beyond him. I see the Yale school in economics starting with Irving Fisher, carrying forward with the Cowles Foundation, and continuing with Tobin and beyond. Also, the tradition seems possibly to fit into a humanistic Yale tradition that extends beyond the department of economics.

Colander: For me, the Yale school is specific. I understood the term to describe Jim's position in reference to his debate with Brunner and Meltzer.

Shiller: There was a time when Jim was viewed as the counterpoint to Milton Friedman. Jim wrote a very critical review of the Friedman and Schwartz *A Monetary History of the United States* in the *American Economic Review* in 1965. When Friedman published his "theoretical framework" in the *Journal of Political Economy* in 1970, it was Jim who wrote a strong and widely noted rebuttal in 1972. So, from all this debate, it must have seemed to many, if there was a Chicago school, there must also be a Yale school. The public sense of a school of thought seems to be built around one or a few intellectual giants who take a strong public stand.

There are other historical reasons why some might easily arrive at a conclusion that there is at Yale an alternative to the Chicago school. The Cowles Foundation at Yale was taken from the University of Chicago in 1955 by Tjalling Koopmans after a dispute with the department of economics at Chicago, a department that did not at that time support the kind of quantitative research that Cowles represented. Koopmans was publicly critical of Friedman's methods too.

I am not sure how people view these debates today, so many years later. At times the Yale school must be thought of as politically much more liberal than the conservative Chicago school. Certainly, one hears from Tobin, Bill Brainard, and Bill Nordhaus more new ideas about new government initiatives than about ways of reducing the size of the government. Tobin's recent campaign for a tax on currency transactions, to put "sand in the wheels" of currency speculation, must seem antithetical to Chicago conservatism. But, overall, I would call the Yale school mostly apolitical, and certainly not associated with political parties.

To me, there is something else about this Yale tradition that should be noted. Tobin stands for an approach that is respectful both of solid economic theory and of difficulty in adapting it to the complexity of the real world. Tobin is a realist who knows the importance of studying institutions and history, and who has a deep motivation to see that economic policy really works as it is intended. One doesn't have to use the words Yale school to capture what he had in mind, but I've always thought that there is such a strength at Yale. I suppose you could use the term with caveats.

Colander: I agree. When I initially accepted Barnett's request, I felt that there was a broad methodological framework that would tie together the work of a number of people at Yale. I was thinking that there might be something called the New Yale school that I could juxtapose to the New Chicago school because I think Chicago is not the Chicago of old. Within that broad methodological framework, I saw three complementary lines of research.

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One was highly theoretical and abstract. Martin Shubik's and John Geanakoplos' work fit in here. I think their theoretical work starts from a fundamentally different premise than does "new Chicago research." It requires what one might call a sociological face; for example, Shubik's work requires thinking about money in a different way than the profession has. I saw John Geanakoplos' work as possibly fitting into this broad theoretical framework.

On the empirical front, I saw Chris Sims's is work fitting in this framework; it was trying to draw what one can out of the data without assuming, or not assuming, that markets clear. Finally, on the practical side, I saw Ray Fair's work as being another dimension to this; his econometric work is a nice middle ground. I also saw your work as nestled between the others, and a natural evolution of Tobin's approach.

Shiller: Well, there are some common elements here.

Colander: Yes. But it is clear that there was opposition to such a use of the term, so I will limit my discussions to you and Jim.

Shiller: I know. Some in our department were opposed to participating in these conversations, thinking that it would be more misleading than helpful if we tried to characterize the Yale school. They said that there is no more agreement here on tradition or method than there is in the profession as a whole, and that we should not misrepresent to people what they might find if they come here as students or faculty, but I am uncomfortable with their conclusion. If we followed their advice to the letter, and never discussed how one department differs from another in basic philosophy, then prospective students might have no advice for choosing among departments except those silly quantitative rankings based on popularity contests, published page counts, or the like. It is the intellectual traditions that really matter, and we have to try to characterize these traditions.

Interest in practical economic policy is an essential part of what I would say characterizes the Yale school, though its great interest in economic theory sets it apart from public policy schools per se. As departments of economics go, this department used to send a lot of people as advisors to Washington. It's a pretty good sign that economists are connected to the real world if they're being invited for advisory posts. Lately, this seems to happen much less often for Yale people. We do have a good representation of our economists in foreign countries, President Zedillo of Mexico was one of our economics Ph.D.'s, for example.

Colander: You have people like Truman Bewley. He changed his research program upon coming to Yale.

Shiller: Yes he was a great case of someone who changed fundamentally when he became a professor here, abandoning work in the most abstract of mathematical economics for empirical work on how individual wage setters make decisions. His forthcoming book, representing the results of over 100 personal interviews with wage setters, resulted in a very deep understanding of a central issue for macroeconomics: why wages are sticky through time. I'm very impressed at what he's currently doing. So I think that either there's a subtle Yale influence on people or else Yale attracts people with certain kinds of emerging interests.

Colander: The question is: Does this influence warrant its classification as a separate school?

Shiller: As I said earlier, I think there is something distinctive about Yale that represents a long tradition. I see the Yale school going back to Irving Fisher. He was, in fact, the first person to receive a Ph.D. in economics from Yale, in 1891. He spent his entire career at Yale, until he died in New Haven in 1947. This spring we had a conference sponsored by the Cowles Foundation at Yale on the occasion of the 50th anniversary of Fisher's death. There will be a conference volume about his work. Most of our macroeconomics and theory faculty participated. If this isn't evidence of a departmental tradition I don't know what is.

Fisher is the man who gave the most convincing clarification of the theoretical role that the rate of interest plays in economic decisions, and his theoretical advances in capital theory influence much thinking even today. He is also the man who invented the term "money illusion" and who wrote an entire book on the subject, indicating an early awareness in him of the importance of behavioral economics. He is also the prime exponent of inflation-indexed bonds, which the U.S. Treasury just created last year. This indicates an awareness in him of the importance of institutional change. His work shows more vitality today than that of any other American economist from the first half of this century, and I think that it is elements of his approach that are responsible for this.

Such a tradition might continue to attract people to this department. Even if differences across departments of economics are in many ways subtle, a tradition represents a focal point where people of similar interests can converge, along lines that theorists have specified as a factor that can break multiplicity of equilibrium. Rather than choose a department randomly, students and professors can choose a department that involves some symbol that represents their approach or philosophy, thinking that others of like mind will tend to be attracted there. A departmental tradition can survive interruptions, I believe.

Its fairly rare that a department of economics will focus strongly on a particular approach. You had the University of Chicago, which shows a strong focus under Frank Knight, Milton Friedman, and others on advocacy of laissez-faire economics, and that period in Chicago history was a wonderful success, when judged from the legacy it left for us all. But even that Chicago tradition has changed now. It's quite different from what it was, much more technical and less practical-policy oriented. Now the Chicago approach is almost inseparable from a variety of other schools' approaches, and some argue that the old Chicago tradition is dead. But I would not agree. The symbols of Chicago's past will continue to influence the future of that department.

Colander: I think the key to answering the question "Is there a new Yale school?" is to discuss how Yale is distinct. Let me ask you the following: How does Yale differ from MIT, Princeton, and Harvard?

Shiller: That is so hard to answer briefly. There are so many different people and so many different dimensions, but each has a slightly different intellectual history which may tend to attract different people. Take MIT, for example. When you think of MIT, you may think of Samuelson, Solow, Modigliani, and Diamond. Because of them and others there, it has a wonderful intellectual tradition that attracts people who are motivated by any number of things in their work. Their legacy becomes a kind of a public focal point that helps to define their department, I think. At Yale I would still say that Irving Fisher and Jim Tobin would be a similar tradition.

Colander: So how would you contrast Samuelson and Tobin, for example?

Shiller: They are both great economists whom I admire. Both of them have very broad scope, beyond narrow economic models, and a commitment to social philosophy.

Colander: So you would characterize the Yale school as having a stronger focus on social philosophy.

Shiller: Maybe that's partly right. Its hard to be general about this. What image do we have of Tobin? To me, he comes through as a very moral person and who has genuine sympathy for others. That means he sees what other people are suffering and he wants to correct that. You get that sense more from him than from very many economists.

Colander: Could we distinguish what you have in mind about Yale by saying that there is a different motivation for theoretical work here? I'm thinking of your theoretical work. In my view, it starts from a different perspective—one that did not initially assume that markets work, but instead assumes that markets work because institutions make them work. It then tries to understand that interplay between institutions and theory. That seemed to me the epitome of what I felt characterized a broader Yale school. It included Jim Tobin's work, your work, and a number of others here.

Shiller: Yes, I think there is some difference in motivation here, though, as I have said, this motivation does not apply to everyone here. For me, it is central to my work. For the past decade, my work has been focused on improving economic institutions (as in my book *Macro Markets*, which proposed fundamental new financial markets), and on incorporating lessons from other social sciences, such as psychology and sociology, into economics.

Others here have shown a real interest in practical institutions and policy. Christopher Sims has been developing macroeconomic models for evaluating monetary policy. Giancarlo Corsetti has written a book (with Willem Buiter and Paolo Pesenti, both formerly here at Yale) about European monetary cooperation. Ariel Pakes has been studying index number theory, in connection with a differentiated products model, to try to understand how account should be made, in computing the consumer price index, of the changing quality and ever-increasing list of choices for consumers.

T.N. Srinivasan has been studying policy toward customs unions and regionalism in world trade. Bill Nordhaus and Robert Mendelsohn are deeply involved in applying economic theory to understand the economic dilemmas that will come due to global climate change. Of course, most departments have some people involved in practical policy, but I think Yale is one of the departments with a particular strength here.

In my view a department gaining a sense of identity is an accident of history. Things that happened long ago still tend to influence. I view Irving Fisher as part of the identity of this department even though he died in 1947. Another identifying aspect of the Yale department was having the Cowles Foundation here, which was a central early econometric mark.

Colander: I see the Cowles Foundation in some ways as playing a role in the abstract direction that macroeconomics went. My reading of that period is that some of the Cowles Foundation work that was done here really tried to provide a full scientific basis for the sets of models that were there. Thus, they were, in my view, claiming more for the macroeconometric models than what could be claimed. That's why I think Ray Fair's latest piece, talking about macro models in a different way, is important. They're workable models; they are not models that provide the grand scientific foundations for things. What macroeconometric models are trying to do is to understand things enough to handle policy. That seems to me fundamentally different from the approach the Cowles Foundation started out thinking about macro models. In some ways the picture I paint is not so pretty for the old Keynesians because the old Keynesians in some ways were trying to have it all—both direct policy relevance and scientific basis.

Shiller: Yeah, I thought it was incredible hubris for Keynes to call his book the "general theory," suggesting associations with Einstein. Ray Fair has based his macroeconometric modeling work on a very practical, unglamorous, and commonsense approach, involving a careful testing of the predictive power of his models. He has explicit models that work as forecasters, out of sample, better than any simple model, as my work with Ray has demonstrated. I find it remarkable that there isn't more interest among academic economists in developing explicit macroeconomic forecasting models with a sensible account of real-world factors such as taxes and monetary policy and carefully testing the ability of the models themselves, without human intervention, to provide information about the future. There hasn't been much academic interest in getting into the practical minutiae of forecasting well in real time.

Colander: I think there were a number of different elements of Keynes' work; some were practical and some were theoretical. I actually think that Keynes' implicit theory is more general if it is interpreted as a multiple equilibria model. He never developed that but it's at the heart of my understanding of Keynes' theoretical contribution. That aspect of Keynes was quickly lost as researchers tried to fit Keynesian economics into a unique equilibrium Walrasian model. These two traditions, it seemed to me, were unmeshable. The Walrasian model left out of it all sorts of sociological and institutional issues that were central to Keynes' world view.

Colander: Let's talk a little bit about you. How did you get into economics?

Shiller: When I was in college I was interested in just about everything. I thought choosing a field was an impossible decision to make but I had to make some decision. I can't say exactly what tipped me toward economics. I thought I wanted to help the world, and it seemed that economics was a good way to do that. I have very cosmopolitan interests and I could have ended up at any department at the university. I choose economics and went to MIT, where I worked under Franco Modigliani. I admired his approach to economics, and his commitment to moral and social issues. I remember that he and I were occupied with concern about the Vietnam War then.

Since then, I have done a lot of work in what may be called a behavioral economics mold, in both macroeconomics and finance. For a decade now, Richard Thaler and I have been organizing a series of seminars in behavioral finance, sponsored by the Russell Sage Foundation Roundtable on Behavioral Economics and by the National Bureau of Economic Research. George Akerlof and I have been organizing a series of seminars on behavioral macroeconomics for some years now. I have had a lot of support from my Yale colleagues for these endeavors, but these are more profession-wide seminars.

Colander: How would you characterize or differentiate the MIT-Modigliani approach from the Yale-Tobin approach?

Shiller: That's hard to do; I admire both these departments and people. In many ways, MIT represents to me the same tradition. To me, a lot of what I admire about the Yale school is carried forward also in another institution, the Brookings Panel on Economic Activity, with its publication, the Brookings Papers on Economic Activity. These were founded by Arthur Okun, a Yale economics professor very much in the Yale school tradition (see Figure 5). The Brookings Panel, organized now by Yale's William Brainard and Brookings' George Perry, draws people doing this kind of practical, theoretically sound, policy-oriented research from many departments around the country.

Colander: If you can't separate Yale and MIT, doesn't that suggest that what we were talking about as the Yale school is really a broader school that goes beyond Yale and instead represents an "older Keynesian" tradition.

Shiller: It would often be Keynesian in a certain sense. It's an approach that is less methods bound than comes from Keynes. It is an approach than recognizes the richness and complexity of the real world. It is an approach that is responsive to reality and to inductive research, and sees sensible and effective policy formulation as the ultimate objective. It's an approach that involves being alert to, and open to, basic facts.

Colander: How would you respond to the argument that economics is trying to get underneath surface facts and observations, and get to the core motivating driving force of the economy?

Shiller: In my view, that argument reflects a false view of reality. There's kind of a group-think that develops in the profession that makes many economists think that there's a simple theme to human behavior, a single key that explains it all, such as expected utility maximization with a simple utility functional form that many economists use for no good reason. Human behavior is so much more complex, so we have to take our cue a lot from facts and do inductive work.

Colander: What percentage of the profession shares your approach?

Figure 5. Meeting of the Brookings Panel on Economic Activity in January 1978. Joe Pechman (left) was the Director of Economic Studies at Brookings. Art Okun (center) and George Perry (right) were co-founders of the Panel.

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Shiller: I don't know. I think in any profession, most of the people will be spinning their wheels, unfortunately. That is the nature of research, but I think that it also happens more than it should because people specialize too narrowly and define their research problems too narrowly. I wish a higher fraction of the economics profession were interested in history, psychology, sociology, institutions, and economic policy. In this, I don't mean necessarily day-to-day concern with politics. I'm not saying I want economists to be more like the kind of television-news sound-bite economists who are always ready to discuss what was said on the floor of the House yesterday.

Colander: Your view strikes me as having similarities with the Santa Fe complexity view. Would you agree?

Shiller: I have attended a couple of their conferences in New Mexico and found them very worthwhile. My view of the complexity of human nature (and, increasingly, the view of an enlightened segment of the economics profession, I think) reflects modern work in evolutionary biology. That work emphasizes that the human species is a product of natural selection, both genetic and cultural. That means that any little habit or pattern that was advantageous was reinforced, and any little habit or pattern that was not, was repressed. The outcome is a set of human motivations that is extremely complex. Lacking knowledge of evolutionary history, there is no underlying sense to it. We just have to accept these motivations in all their complexity. You have to understand this whole constellation of motives, desires, and behavior patterns that served us well as primitive hunter-gatherers, as isolated farmers, or as Victorian merchants, but which may not serve us well now. Trying to understand events that happened in very simple terms with very simple models is a fundamental method of our research, but we should not make the error of elevating these simple models too far.

Colander: How many people here at Yale share that point of view?

Shiller: I'm not sure.

Colander: How about Martin Shubik?

Shiller: He might agree; he also stresses incorporating the complexity of institutional facts into model building. He has shown how proper account of some of these facts about the circumstances we find ourselves in allows us to use game theory to provide insights not only into economic behavior, but also into such diverse fields as political science and social psychology.

Colander: What about Chris Sims? His empirical work seems to be challenging the way we pull information from data.

Shiller: Yes indeed. He had an article in 1980 called "Macroeconomics and Reality." I was very sympathetic to that article because he was pointing out some weaknesses in the profession's then-standard approach to macroeconometric modeling. This article is at least as important as Lucas is "Econometric Policy Evaluation: A Critique," which points out different weaknesses. The profession tends to develop a structure for modeling the economy, often not very well supported by any evidence, and focuses too much on the approach. Sims, in that article, was questioning these assumptions that are really without basis or fact and he was going back toward an econometric approach that was not driven by this unanalyzed structure.

Colander: But that article was written before he came to Yale, wasn't it?

Shiller: Yes, this was written before he came to Yale. His macroeconomic theory course to our first-year macroeconomics students today is very tightly focused on the mathematics of intertemporal optimization under rational expectations. I disagree somewhat with him about this focus. Students should certainly learn some of this material, but it excludes other things. Economic agents more often satisfice, to quote Herbert Simon, than optimize, as a matter of simple fact, and the costs of calculation prohibit their behaving as represented in these optimizing models. Probably, however, our core macro sequence for first-year Ph.D. students at Yale works very well with both Sims and me in it, presenting different views.

John Rust has been worrying about the apparent unrealism of our economic models in their assumptions about agents' ability to compute. He has some results showing that massive parallelism, social memory, and decentralization can make it more plausible that people really can do these calculations, in effect. It is good that he is trying to confront these issues, though I am afraid that an aspect of unrealism will remain in many of these models.

Colander: Should there be a different methodology for macro than for micro?

Shiller: The terms macro and micro represent schools of thought as much as different subject matter. The terms suggest that macro is an aggregation of micro but in fact the differences between these schools of thought are perhaps as much in terms of method as of subject matter. The difference is a bit analogous to calculus and geometry in math. Geometry naturally seems to lend itself toward axiomatization, but calculus is rarely presented to students as an axiomatized system. I think that macro can be, in this sense, more like calculus. We start from some intuitive feeling; we build little models but they're not complete models; they don't work from first principles and so there's often been more willingness to introduce real-world complexity in human behavior in macro than in micro.

Colander: What's your view of the IS/LM model?

Shiller: Well I'm still teaching it although that's hardly the only thing I teach. The IS/LM model is not a complete model; it takes things as given. If that's all you know, you're far too limited.

Colander: Much of the work on the foundations of IS/LM has been done within the Walrasian general equilibrium model. What's your view on that work?

Shiller: It goes back to my fundamental thing: You say complexity; I say you can't reduce all human behavior to simple rules. We talk about IS/LM as the Keynesian model. But Keynes talks about so many different things, like envy or social comparisons, that go beyond the IS/LM story. I think of economic habits—patterns of behavior that are in our minds for no good reason. An example is money illusion. Money illusion is an important phenomenon. People have a preference for nominal quantities. This preference should be fundamental to macroeconomic theory.

Colander: Lets go back to the foundations people. Was it a natural step if you look at the evolution of macro? After IS/LM became standard, it started getting modified by Keynesians such as Modigliani and Tobin. It was a natural step from their works to providing simple micro foundations for the model's conclusions.

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Shiller: Yes, it was a natural step because Keynes' book was a muddle. It was impossible to comprehend because there were all these loose ends trailing off.

Colander: But what I'm asking is whether the Neo-Keynesian work was a first step that started us down the micro foundations path. Was it the beginning of the assumption that you could provide simple explanations for complex things?

Shiller: Well, first of all, it's desirable to have simple explanations for complex phenomena—a physics model for economics. It would be nice to have it, but I think the physics methodology doesn't work as well in economics. We are not going to discover universal laws like $F = MA$ of the same importance in economics. I think the hope of finding such things has harmed some people's research in economics.

Colander: How do you see IS/LM fitting in with Walrasian general equilibrium theory? Are the two compatible?

Shiller: The Walrasian model is very abstract and would apply to civilizations on another planet or wherever. The IS/LM Keynesian model was designed for twentieth-century institutions and human behavior patterns. The IS/LM model is not really satisfying as a theoretical model; it is just an aid to thinking about some tentative conclusions from an intuitive theory.

Colander: What are your views of monetarism?

Shiller: Milton Friedman was the prime advocate of monetarism. I hope it is not seen as inconsistent with my philosophy that I am an admirer of him too. His Monetary History, written with Anna Schwartz, was a very interesting book, although the lack of stability of the money multiplier in recent years has blunted what they considered a major message of that book.

How can I be both an admirer of Friedman and of much in the Chicago school tradition, and yet also question the optimizing-model-based approach to macroeconomics that some would say is quintessentially Chicago? The answer is that what is really attractive about people's work is often not the dogmas that they choose to stress about them, and would have others adopt. One may be inspired by some of their work even while rejecting these dogmas. Milton Friedman is sometimes viewed as advocating exclusive reliance on certain kinds of rational-optimizing models, but I take a different perspective on his relation to this work.

Friedman wrote a book on methodology—his *Essays in Positive Economics*. The first essay in that volume has been used by many people to justify building elaborate economic models from counterfactual assumptions. He gives a story in that essay about how one would model the behavior of a skilled billiard player. The best way to do this might be to describe his plays as if he were solving an optimization problem in theoretical mechanics. It wouldn't be a criticism of this modeling method to point out that the billiard player cannot understand the mathematics of the theoretical mechanics. Thus his famous assertion that you cannot judge a model by the realism of its assumptions. Friedman is basically right about this, and this example does justify in a way the value of building optimizing models from counterfactual assumptions. Still, many who cite this example misapply this insight, as Koopmans forcefully argued.

I think we can't blame Friedman for all the misapplications of his methodology. In looking at his own method of research, you often see a lot of real strengths and good sense. His *Monetary History* with Anna Schwartz, for example, was one of the early examples of searching for natural experiments (they called them "quasi-controlled experiments") to sort out cause and effect, and not to rely exclusively on some enshrined method of model building as the only approach.

Colander: So you see some overlap between monetarism and Tobinesque macro?

Shiller: Well that's why we are having a problem with characterizing a Yale school. There's going to be overlap. The Chicago school isn't just at Chicago and what I might call the Yale school isn't just at Yale.

Colander: What's your view of the Clower-Leijonhufvud approach?

Shiller: I remember their works: That was an interesting literature years ago. I used to lecture about that but I haven't done that for a while. The ideas are mentioned in my course but only very briefly. I tell students about the difference between notional and effective demand, and about sticky prices. Their work evolved into a literature on disequilibrium, some of which was very important.

Colander: What's your view of New Keynesians?

Shiller: Acknowledging that wages and prices are sticky through time is extremely important for macroeconomics. There is also the related phenomenon of wage compression across types of people, which, Giuseppe Moscarini has shown, appears to account for the fact that less skilled workers tend to bear more of the brunt of macroeconomic fluctuations.

Colander: What would you say the relationship of the Yale school with Keynesianism is?

Shiller: Keynesianism is such an ill-defined thing now, and many people would say it's dead, not dead on the policy side, but on the theoretical side. However, as I said: Trying to build a model that captures a lot of observed phenomena rather than starting from first principles is important. Thus, I think there's a close relationship in terms of motivation and philosophy.

Colander: What is your view of the real-business-cycle approach?

Shiller: Real-business-cycle theory has been a prominent movement in macroeconomic theory ever since Kydland and Prescott's famous "time to build" piece in 1982. It has expanded through the profession and differentiated, so that it is not always recognizable as a distinct movement anymore.

The real-business-cycle modelers find basic facts about the macroeconomy, simple characterizations of the economy, such as which quantities and relative prices are changing over the business cycle and how these correlate with each other. They then try to relate these prices and quantity movements to a calibrated optimizing model of individual behavior. It is an attractive exercise to try to make sense of the basic facts this way.

We have a number of people at Yale who are pursuing what I would lump loosely under the heading real-business-cycle theory. Chris Sims, with Eric Leeper, Tao Zha, and others, has been doing time-series analysis of linearized dynamic stochastic general equilibrium models. Stefan Krieger builds business-cycle models with heterogeneous debt-constrained firms exposed to idiosyncratic production risk. George Hall models plant managers' capital utilization in terms of a dynamic programming model, and allows us to explain the behavior of production and inventories over the business cycle.

In the future, I would like to see more work combining the basic insights provided by this work with other information. The real-business-cycle theorists often limit themselves. There ought to be more recognition of the limited ability of people to calculate that I referred to above, their tendency to use rules of thumb, and the influence institutions have on their behavior. These theorists will say sometimes that, while they acknowledge these problems, they cannot see a good way to take account of these problems in their models. But it is very important to try.

Colander: I think we're running out of time, so we should probably end here. Thank you very much.

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