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THE ET INTERVIEW: PROFESSOR JAMES TOBIN

Interviewed by Robert J. Shiller
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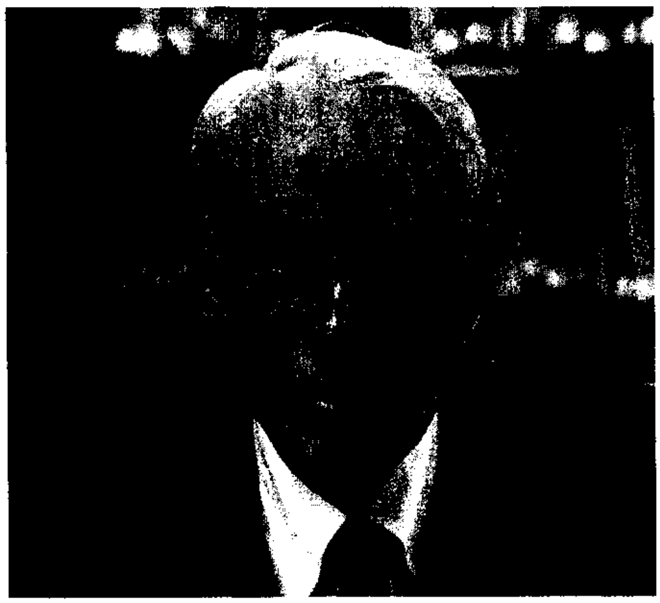

James Tobin, Sterling Professor Emeritus, Economics, Yale University. Photograph by Michael Marsland, Office of Public Affairs, Yale University.

Professor James Tobin is a figure of truly historic significance in the economics profession. He is one of the major developers of modern macroeconomic theory. He has contributed fundamental knowledge to the theory of investment, of consumption, of money and banking, and of economic growth. His theoretical work made possible the development of the capital asset pricing model that has been a central paradigm in modern finance. His work on limited-dependent variable models has started a field within econometrics.

Professor Tobin is the recipient of most of the highest honors awarded to economists. He won a Junior Fellowship at Harvard University in 1947–50. He

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won the John Bates Clark Medal in 1955. He was president of the Econometric Society in 1958. He was president of the American Economic Association in 1971. He won the Nobel Prize in economics in 1981. He is a member of the National Academy of Sciences and a fellow of the American Academy of Arts and Sciences. He has honorary doctorates from 21 universities and colleges. An anonymous donor to Yale University has created a James Tobin chair in economics.

He has been a guiding force not only in economic research but also in practical economic policy. He served in the Office of Price Administration and the Civilian Supply and War Production Board in 1940–41. He was a member of the President's Council of Economic Advisers under President John Kennedy, 1961–62. He has been widely consulted on national issues by congressional committees, government agencies, and foundations.

He was director of the Cowles Foundation for Economic Research at Yale University for 7 years. For 6 years, he was chairman of the Department of Economics at Yale University. He has been active and involved in university-wide issues at Yale University and is widely remembered throughout Yale for articulating new financial institutions for tuition support and for chairing a committee that revised procedures for appointments and promotions in the Faculty of Arts and Sciences. He retired from his professorship at Yale in 1988.

On May 15, 1998, in his 81st year, I spent an afternoon with him at the Cowles Foundation, where we both have offices, and discussed his long and productive career in economics. The conversation covered some of his life history and also his work in economics and how his life's events helped shape his research. We talked in addition of the philosophy and methodology of economic research and of how he perceives the directions and attitudes of the profession to have changed over the years. I tape-recorded this interview, and the edited transcript follows.

Let's start with your youth. You were a teenager in the depth of the Great Depression, and your mother was a social worker who worked with, among others, the unemployed. I am wondering if this personal experience was a factor in your developing your interest in macroeconomics and the problems of unemployment.

Yes, it wasn't just unemployment that I identified with my mother's work. I think it was poverty of all kinds, whatever the origins were, though a lot of it was unemployment at the time. It wasn't that my family themselves were in bad shape. My father did lose an investment in a building he had invested in on the site of his parents' home. It was in the middle of a town and should have been a good investment for an office building with a storefront. He lost all.

But in general, we were not in bad shape. I heard a lot about those problems my mother dealt with from day to day. My father was also an intellectual. He was a journalist, when I knew him the publicity man for the University of Illinois Athletic Association. Our house was just full of books and magazines, and he was a liberal on his own. So I was greatly influenced by him as well.

You have been described as one of the most moral of economists or most motivated by moral issues. Is there some philosophical or religious underpinning here?

I don't describe myself that way, so I have not consciously attempted to merit that description. There certainly is no religious basis. I guess I'm a "secular humanist." I've always attached a high social value to having greater equality in economic outcomes.

Yes, well a lot of people don't attach much value to that, it seems.

Especially these days, I'm afraid that's true.

Some people say that people living in the depression have a different outlook. You're not stressing that particularly.

Well, I do stress that because I think that a lot of contemporary economists who never had any experience with that catastrophe regard it as some kind of aberration so that they don't have to worry about accommodating it in their theories of macroeconomics. They just dismiss it as something that didn't happen or that they can't explain. But for people who did grow up in the depression, it was an obsession. After all, people were seriously concerned, at that time, whether capitalism and democracy could survive at all. It wasn't an unreasonable thing to worry about, given what was going on in Europe. And there were these rather cataclysmic diagnoses of the long stagnation and depression and claims that it proved that the whole idea of a market economy and market capitalism was a flawed way of organizing society. You know Marxism was a pretty strong ideology, even in American universities, in those days. So Keynes actually was a savior of capitalism and democracy because his diagnosis of what was wrong was really not anything terribly fundamental. It was something that was easily remedied, if you adopted his diagnosis.

You were very influenced by Keynes while still an undergraduate at Harvard?

That's right.

I note that nowadays, hardly any of our graduate students read Keynes; are they missing something?

Well, I would say they're missing a great deal, but that's an old man's nostalgia perhaps. Undergraduates are missing it as well.

Could you describe what it is about his methods that appealed to you?

What appealed to me when I was 19 years old was model building—the whole idea—I didn't know anything about economics before I went to college—I didn't really know anything about it until I was a sophomore, because freshmen weren't supposed to take economics in those days. So my introduction to economics, taking the elementary course and reading Keynes, were simultaneous in my sophomore year.

The same crazy graduate student who was my Ec A instructor was also my tutor. The system was that you met with a tutor once a week and did something extra, which wasn't graded, in your field of concentration. My tutor wanted us to read "this new book that people are saying is important," so that's what I did. I found it pretty exciting because this whole idea of setting up a macro model as a system of simultaneous equations appealed to my intellect.

I wouldn't think of looking at Keynes's *General Theory* for the inspiration for explicit simultaneous equation macroeconomic models; he didn't do that there. I thought there were other things about that book that set it apart.

Well, that set it apart if you looked at it from the right point of view. Other people were algebraizing models. Articles by Hicks and others used algebra and geometry quite explicitly to expound Keynes. Keynes's book was setting off a whole new scheme of economics—called then the *theory of output as a whole*, Joan Robinson's term for it. She made it appear quite distinct from the ordinary Marshallian partial equilibrium, which we got in our micro theory in our theory classes. That was what theory was in those days at Harvard.

Walras?

There wasn't much Walras at Harvard then, until Hicks finally came out with *Value and Capital*, but that was in 1940 or so. And so the trend in England and the United States was mainly classical in the original sense of the word. Keynes was fascinating because it looked like he had a fruitful new way of going about economics. To me, it looked like it was fun.

And then on the other hand this was also a revolt. I think revolts against old established wisdom are exciting to young people. It was exciting to me even though I had not been really taught the old economics enough to know what Keynes was revolting against.

Here we are 60 years later, and I guess you are still a Keynesian, though the dominant strand of macro theory dismisses Keynes out of hand because he assumed sticky money wages kept labor markets from absorbing the unemployed.

Bob Solow said Jim is not stubborn, he just doesn't change views lightly. The money wage issue is central. Keynes wrote a lot about it, and I wrote my undergraduate honors thesis on the subject and actually challenged some of Keynes's ideas. But I do think the profession has exaggerated the role of sticky wages in Keynes's own book and in later Keynesian doctrine. For one thing, he thought real wages would be flexible downwards if a lower real wage were produced by increases instead of reduction of money wages.

That sounds very behavioral!

It was behavioral. Keynes was a keen observer of the real world. It had the ring of truth to my ears back then. In my view, the classical market-clearing result has to occur everywhere continuously to guarantee full employment. Excess supplies and demands occur in many labor and product markets most of the time, resulting from sectoral as well as economy-wide shocks. In addition to that Keynes argued that even if money wages were flexible that wouldn't solve the problem. We would still have a problem of the adequacy of aggregate demand.

And you bought that; you buy that?

Yes, I "bought that." I "buy that." I have presented the models in which it would be quite reasonable. For one thing, the orthodox proposition depends on the "real balance effect" of a lower price level. That is quite dubious, because negative effects on debtors' spending could well offset positive effects on creditors. Secondly, expected disinflation and deflation have negative effects on demand. Thus the full employment equilibrium can easily be unstable.

But the Phillips curve, isn't that tied up in your mind with this kind of phenomenon?

Well, that might be an approximation to what happens to money wages in various circumstances. But I didn't go for the idea that the long run Phillip's curve is necessarily vertical at all rates of inflation. I had in my 1971 Presidential Address for the American Economics Association an explanation why you might have different unemployment rates in long run equilibrium, lower at higher inflation, just as long as inflation isn't too severe. For example, if you tried to reduce inflation—say from 4 to 3 to 2 to 1—you would be increasing the equilibrium unemployment rate at each step. The reason is that more and more labor markets are in excess supply situations requiring money wage cuts in order to adjust real wages, and these cuts take extra time.

Here is something that seems very unconventional from the standpoint of traditional economic theory.

Well, you see there is nothing about it that is inconsistent with each micro market having an equilibrium which is neutral with respect to prices. The question is what's going on in the short run and medium run disequilibria that always characterize most markets, not always the same ones. I think current economic theory is mistaken to pretend the economy is just one market and to think that all the rationality axioms, against money illusions, and so on, which apply in long run equilibria also apply every day. I think those are big mistakes.

Another interpretation of Keynes that is very popular now is that he failed to understand that his theory is assuming people irrationally fail to take account of the taxes that will be needed to pay back government debt.

That is, that he is assuming people fail to satisfy Ricardian equivalence, which is a basic consequence of elementary rational behavior. What would you say about that?

Well, I wrote a paper once which made several important arguments why Ricardian equivalence would not be a compelling reason for rejecting Keynesian theory. I understand the Ricardian theory, but it does depend on having immortal consumers or dynasties.

In 1952 I saw you were saying that there must be some tendencies in the direction Ricardo specified.

Yes, I said that. In the same article I noted some of the anti-Ricardian arguments of my later paper. I get credit for a lot of things like that, and then the ideas are pushed beyond where I intended.

Returning to your life story, you went on for a Ph.D., but then you were interrupted by World War II, before you wrote your dissertation. So I was thinking that must have been a very jarring interruption to your education, and I wonder how you managed that. Did you have trouble getting back to it? Or did the wartime experience change what you would have done?

Well, most of my wartime experience was completely divorced from economics and academics. I got to be navigator and second in command of a destroyer. I was on that same ship for $3\frac{1}{2}$ years. I was completely absorbed by that experience while it was going on, and I didn't think about economics at all.

Before we actually got into the war I got a job, thanks to one of my professors, Ed Mason, in one of the new economic mobilization agencies that were springing up in Washington. It was concerned with allocating materials away from civilian use to be sure that there was enough for the military programs. This was all before we were actually at war ourselves. Military programs for helping Britain and France were making some materials scarce—aluminum, steel, and so on. I was in an agency called Office of Price Administration and Civilian Supply.

After Pearl Harbor I enlisted in the Navy, and then I was on call to go to an officer training school. Meanwhile I held my Washington job for another three months. I enjoyed that a lot, working in the bureaucracy so to speak. There were friends whom I got to know there who wanted me to come back to Washington when I was demobilized in January 1946. So I had a choice whether I was going to go back to Harvard or whether I was going back to Washington.

You'd be an ABD then if you went to Washington.

Yes, I would have been.

You were thinking of that?

I was thinking of that or at least of postponing going back to Cambridge for a while. But meanwhile, I had ascertained from writing to the Harvard department that I could come back, and I got a letter from the chairman that he knew from what my professors had told him that I had an unusual opportunity to be an important economist, and it would be a shame if I didn't come back, and a waste of my career. So I did go back to Harvard right away.

But the alternative would have been a rather bureaucratic position?

Oh, yeah. Policy making, and all that. What I did was the right thing.

But did this somehow influence you to work as you did on consumption with constraints with probit, then "Tobit" model. Was this related to your experience trying to understand rationing?

No, probit and Tobit came later. The consumption function was a big, important, immediate issue of empirical economics and also an echo of the influence of Keynes's economics at the time. There was a common misinterpretation of the empirical evidence of the period between the two wars. The data enabled you to have a good estimate of a consumption function that fit almost perfectly just regressing aggregate consumption against disposable income. And then if you extrapolated that to after the war, it looked like you were going to have a hard time having enough consumption demand to keep the economy running at full employment.

That seems kind of naive, in retrospect. But there were serious econometricians who were making that mistake.

Oh, yes. Oh, yes. And then the question was, "What variable was missing from that simple consumption function, which fitted the interwar period so well?" It's all very well to say with hindsight it's obvious that that didn't make sense, but the question of what did make sense was still up for grabs.

Jim Duesenberry had a theory relating consumption and saving to relative income, and I was all for putting wealth into the consumption function along with income. Milton Friedman stressed permanent income and Modigliani lifetime income. These theories were not necessarily incompatible with each other. But they were important because this intellectual and theoretical and empirical puzzle was related to the key equation in Keynes's model and in the empirical econometric versions of it that were coming out, thanks to Tinbergen and Klein. Consumption was a key subject that anybody interested in macroeconomics would have been interested in. Lots of people were in 1947, when I wrote my dissertation.

As regards rationing, that was a separate subject. I wrote articles with Henk Houthakker when we were together at Richard Stone's Department of Applied Economics in Cambridge, England, 1949–50. I was very lucky. You asked how did I get back to economics. Well, I had first a year and one-half back at Harvard when I wrote my thesis and got my Ph.D. I was also teaching sections of the introductory course those three semesters. Then I was fortunate to get a Junior Fellowship, a 3-year fellowship, which really enabled me to get back into economics more generally.

How, then, did you arrive at Yale University?

The market for young economists was very strong in those early postwar years. I was courted by many departments in 1947–49, among them Yale. In 1949–50, when I was in England, all the job correspondence continued by mail. Yale made the best offer, associate professorship right away.

I understand that the Department of Economics at Yale had suffered a lot then and, with the death of Irving Fisher, did not seem to be on a good trajectory. The story is that it was greatly improved starting from around the time that you and Tjalling Koopmans arrived. I wonder why you came here under those circumstances and what you might have done to change things so much.

Yale had begun rebuilding by appointing Harvard Ph.D.'s whom I knew. Lloyd Reynolds and John Muller were a few years senior to me; Richard Ruggles was my good friend and classmate. They convinced me Yale was on the rise. Although New Haven didn't appeal, especially to Betty, we chose Yale and found we liked New Haven too. The commitment of the university to building economics was demonstrated in the next two or three years by appointments of William Fellner, Henry Wallich, and Robert Triffin. Paradoxically, the ridiculous attack of Bill Buckley, then in 1950 a senior in Yale College, in *God and Man at Yale*—he berated the economics department for being Keynesian and left wing—caused alumni to rally round the university and the department, with financial help to compete for young faculty and graduate students.

Fisher had died in 1947 at the age of 80. He had withdrawn from active roles in the department 20 or 25 years earlier. He had very few graduate students or junior faculty disciples. An exception was James Harvey Rogers, an excellent economist with Keynesian ideas of his own, who did have a few very promising students, notably Richard Bissell and Max Millikan. Rogers died in an airplane accident in 1939. Bissell and Millikan went into federal intelligence agencies at the beginning of World War II and never returned to Yale or economics.

Rebuilding had begun before I came in 1950 and before Koopmans came in 1955, as I noted. A major stroke of luck was the appointment of Art Okun in 1951. He was a Columbia graduate student struggling to finish his dissertation and was brought to Yale because we needed more section leaders in introductory economics.

There is a story that you brought the Cowles Commission from Chicago to Yale and renamed it the Cowles Foundation for Research in Economics. I am interested to know your role in this move and why it happened.

Alfred "Bob" Cowles, Yale College class of 1913, was the founder of the Cowles Commission. Originally it was in Colorado Springs, where Cowles was living for health reasons. He founded the commission, together with the Econometric Society, in 1932. He hoped that the application of mathematical and statistical methods would enable explanations to be found for the depression and for his own failures as a stock market investor and adviser in Chicago. During the war he moved back to Chicago and arranged for the commission, along with the society, to be associated with the University of Chicago. He turned the commission over to professional directors, and its record under the leadership of Marschak and Koopmans was phenomenal. In the early 1950s the commission was looking for a new director, but the first generation of its stellar young postwar staff had scattered. Ken Arrow could not be enticed to return to Chicago from Stanford. I had come to the attention of Marschak and Koopmans because I had served as a discussant of a Marschak paper at 1948 Christmas meetings. I had detected a mistake in the paper, in its economics more than its mathematics. They offered me the job in 1953. I was very flattered, because I had the greatest admiration for them and their commission. I visited Chicago. Betty and I were very content with Yale and New Haven. The neighborhood of the University of Chicago did not appeal to us. Although the Cowles appointment carried with it a professorship in the University of Chicago economics department, when I asked the chairman if the department would have been interested in me without the Cowles connection, he said, "No." I felt bad when I phoned Tjalling to turn down the offer. To my surprise he didn't seem disappointed. He immediately asked me if Yale might be interested in his spending his sabbatical in 1954–55 at Yale. I predicted that the Yale authorities would be enthusiastic. Tjalling came, the negotiations to move the commission to Yale started right away, and in 1955 the Cowles Foundation was established with me as its director. This was Koopmans's and Cowles's strategy right along. Relations, fiscal and intellectual, with the U of C had deteriorated. One incident was that Milton Friedman rejected Harry Markowitz's thesis, saying, "It's not economics." Bob Cowles was glad to have his offspring firmly established as part of his own alma mater.

The coming of Cowles really lifted the Yale department to the front ranks. An outstanding cadre of scholars came to Yale, and some scholars already at Yale jointed the foundation—Okun, for example. The foundation added macro and monetary research to Cowles's agenda.

The same Alfred Cowles who founded the Cowles Commission also sponsored the founding of the Econometric Society. What was the relation of you and the Cowles Foundation to the Society?

At Chicago the two institutions shared the same bureaucracy and offices. Here they became more separated. Richard and Nancy Ruggles and others managed the society. The finances were also clearly separated. The ES became self-sustaining. In 1976 the link was broken altogether, and the headquarters of ES were moved to Northwestern; Julie and Robert Gordon replaced the Ruggleses.

How did you first get interested in econometrics?

Well, when I was a graduate student, I tried to learn econometrics and statistics at a time when there wasn't much instruction in that at Harvard. One year we had a visitor named Hans Staehle from Switzerland who taught statistical demand analysis. That was the only econometrics we had. Those of us who were interested had to learn what was going on at the Cowles Commission and elsewhere on our own. We took mathematical statistics in the math department of Harvard.

The things I wanted to do, like the consumption function thesis that we talked about earlier, in 1947, did involve doing econometrics. And also, in 1949, I did a study of the statistical demand for food in the United States. I did it as well as I could using both cross-section data and time-series data.

So regression models?

Regression models.

Simultaneous equations?

No, not simultaneous equations but trying to use both kinds of information in order to avoid collinearity, as between for example the effects of income and prices.

A couple of years ago that essay was chosen by Magnus and Morgan as part of an experiment in which they asked modern econometricians to take an old article and see whether they would get the same or different answers if they used current methods of analysis.[1] I was actually quite pleased that my article was selected. One reason it was chosen was that it was self-contained. It had all the data in the article and explained exactly what was done, what calculations were made.

I went to the workshop where the results of the experiment were reported. That was rewarding because they didn't really come out with anything spectacularly different from what I had done at that time.

You were also one of the first to try to incorporate questionnaire survey data, about buying intentions or the like, in macroeconometric models.

I got interested in using survey data as collected by the Survey Research Center at the University of Michigan. But one thing that bothered me about that time was the fact that in many cases we had, in a sense, incomplete data. Let's say you were doing a survey of automobile purchases by households. Most households don't purchase an automobile in any particular year. To use all the zeros as if they were just other observations would not seem like the right thing to do. It was more likely that there was a decision "buy" or "not buy," and then if "buy" there was a decision how much to spend, depending on income, family size, and other variables, so that's why I developed the "Tobit analysis" rather than ordinary regression.

When I first came to Yale there wasn't much more statistics here than there had been at Harvard when I was a graduate student. In this very building, 30 Hillhouse Avenue, there was the one statistician, Chester Bliss, a biostatistician who had studied under R.A. Fisher. He was doing probit analysis—biological, pharmaceutical data, poisons, and so on.

So I got into what he was doing and made probit applications on economic survey data—e.g., buying or not buying a car. But then I thought the best thing to do would be to examine simultaneously for buyers how much they spent as well as whether they spent. So that was the origin of the so-called Tobit analysis.

Tobit?

Tobit—well that's related to "probit," so that's understandable. But it was also related to a reference to me in a novel by Herman Wouk, a friend of mine in the officers' training school in 1942, called *The Caine Mutiny*, where I appear for one or two sentences in the first chapter, and I'm named in a thinly disguised way as Tobit. I asked Arthur Goldberger why he used this label in his statistics text, whether it was the *The Caine Mutiny* or just the elision of Tobin and "probit." He wouldn't say. So I don't know.

You were living through the whole era of the development of macroeconometric models. Did you get involved in things like the Brookings model, the MPS model, the Wharton model, or any of the other large-scale macroeconometric models?

Not really, Bob. I think a lot of people were involved in the MPS model in the sense of being asked to come to conferences to discuss this or that equation or correlation, but I was not closely involved in it.

Why not?

I don't know. I guess I was busy doing other things.

Your general equilibrium monetary models look a little bit like they could have been incorporated into one of these . . . and the equations look like equations from the MPS model.

But we were also being critical of the MPS model. There is an article by Brainard and me called "Pitfalls in Financial Model Building" that is critical of these models. What these models did was to have several assets, as we did. But they failed to model explicitly the fact that there is a wealth constraint, so that the holdings of assets add up to the wealth of the households or the firms. They didn't estimate in such a way that there was proper consideration of the residual asset. It was left hanging there without any symmetrical attempt to estimate the system of asset holdings rather than just whichever one or two you wrote down. So that article was meant to be a criticism of the way in which asset demands and supplies were modeled in the usual macroeconometric models.

What do you think about other criticisms that were leveled? For example, Christopher Sims has argued that the prior restrictions that identified the equations are incredible. Were you involved with these disputes?

Well, not involved with that, but it seems to depend on the particular model or particular assumptions that are made.

I picture you as a person who is more willing than most to use vague but commonsense priors in modeling.

Yes, yes, exactly, I think, I want to be that person, maybe not vague, but commonsense.

What about the rational expectations critique?

I think that it's a great idea to have models in which the expectations are consistent with the model itself. It seemed like a canon for reasonable construction of a model. But I think that's something that should be expected to be true in equilibrium but not every day, so to speak. My theory of liquidity preference as behavior towards risk was built on a rational expectations model long before the terminology.

So you were one of the early rational expectationists?

I think most model builders are naturally rational expectations modelers, anyway for long run or equilibrium solutions. But now I think they attempt to convert all the dynamics of business cycles into that form. I think that's overreaching. And that's where I think modern macro is in trouble.

There is another criticism of much modern macroeconometrics as it is commonly practiced which I associate with Clive Granger, Ed Leamer, and others, about spurious regression. They claim that we mine the data too much and fail to appreciate how often spurious relations will appear significant, especially if not all our maintained statistical assumptions are valid. We run a regression and fill in variables and drop them until it comes out to be the sign we expect and publish only the final estimated form. Sometimes macroeconomists don't seem to be as enlightened about such problems in their research as they should be.

That's a good criticism. I recall hearing Tjalling Koopmans point it out, years ago. I think that the significance tests are lost or inapplicable when you do that. The traditional tests wouldn't apply if you mine the data that way. When I wrote my dissertation and when I wrote my article on demand estimation it took three days to do a regression with three independent variables.

I see.

Since you were not going to do many of those, you tried to be sure that your specification is what you really want to test.

That doesn't completely solve the problem either but "that's putting sand in the wheels again, a different story" as in the transaction tax . . .

That's true.

. . . research sanding the wheels.

I'm not saying it's a bad thing to have all this computing power, but the theory of significance tests was based on the view that you were only going to do one computation.

You have all these issues that we've talked about—do you feel optimism for macroeconometric modeling?

Well, I don't think there is any substitute for having more data and more decisive experiments that will choose among hypotheses. The main thing we need is ways of choosing among competing hypotheses. And that requires thinking about what data will do that, data we already have or data we might obtain that are geared to that task.

Like the negative income tax experiments. Do you advocate these experiments?

Experiments, yes, maybe but also perhaps some more ingenious ways of using the data that are ground out anyway for microeconomic purposes. I think the profession has got itself into a sort of illogical way of doing these things, which is to have a view of the world which has a lot of theoretical appeal to economists and to say, if we make some statistical calculation or econometric calculations and we can't reject that view of the world then we accept it. I think that's upside down.

There is a story that, when President John Kennedy asked you to serve on the Council of Economic Advisers, you said, "I'm just an ivory-tower economist," and then he said, "I'm an ivory-tower president."

Well, it's better than that. He said that—he asked me, and I said, "Well I don't think that I am the type for that; I'm an ivory-tower economist," and he said,

James Tobin at the Council of Economic Advisers, 1962.

"Well, that's the best kind, I'm going to be an ivory-tower president," and I said, "Well, that's the best kind."

It seems that you're less of an "ivory-tower" economist than most, the great majority, so that's why I thought it was a funny exchange.

I wasn't as much different from other academic economists in that respect in 1960 as now. The profession may have become more abstract.

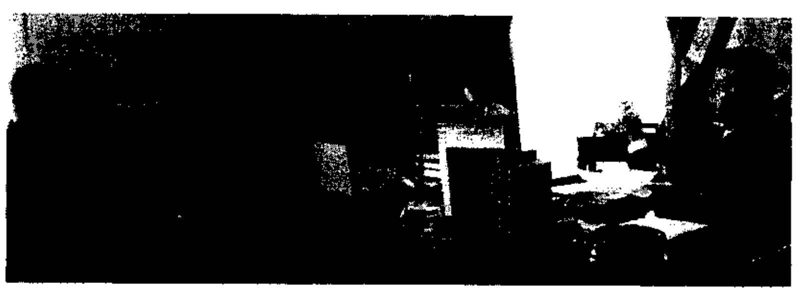
Is that right? I would have thought that genuine interest in economic policy was fairly rare then too. So you see a real change in the profession?

Yes, I see a real change in the profession. There was a lot of commonality between theory and policy in macroeconomics at that time. So for example, the strain of macroeconomics that Samuelson called the "neoclassical synthesis" was theory, but it was theory of policy, and I was one of those involved. He called me a "partner in crime" of this idea, if you will.

One could simply say that there are various combinations of monetary and fiscal policy that could lead to the same macroeconomic outcome, so policymakers are free to choose other criteria for deciding whether you want to have, let's say, easy monetary policy and tight fiscal policy or the reverse mixture. That was macroeconomics, and it was also very close to policy, so we didn't feel that there was a great divorce between the two.

Well, it strikes me that you must have spent a great deal of time on public policy issues—you seemed to be testifying and writing articles or op-ed pieces a lot, and, I think, that is something that a lot of younger people feel they don't have the time to do. It takes some commitment.

But I have four volumes of economic essays, and a 1996 book called *Money, Credit and Capital* which is really theoretical. I really wrote most of it many years ago. It was used in manuscript form in classes here and also at MIT. So I have my share of theory around, though not having been a general equilibrium theorist or a game theorist.



President with the Council of Economic Advisers, 1962. From right: John F. Kennedy, Walter W. Heller, James Tobin, and Kermit Gordon.

Well, I think you are unusual in your commitment to national issues or university issues or professional issues. You have a socially conscious desire to achieve things. Another thing that I thought to be different about you is that you're willing to advocate public policy on matters that most economists seem to stay away from because they are not willing to make the effort to understand or appreciate the real world issues that are involved. I'll give you a couple of examples from your work. One, you once advocated limiting tax deductibility for advertising, and another one is you have been advocating a transactions tax on foreign exchange markets to rein in speculation, to put "sand in the wheels" of the speculative machine. You are rather alone, and there aren't that many economists who would dare take positions that are so unconventional. The name that comes to mind, if I think of anyone who thinks about these things, too, is Larry Summers. It's a willingness that you show to think of practical policy measures that don't necessarily follow directly from canonical economic theory and that require some careful judgment about factors that are not stressed in the theory.

An important issue for me arose in the 1960 presidential campaign. Kennedy and the Democrats were accusing Eisenhower and the Republicans of letting the economy stagnate and not grow fast enough. There was a lot of discussion about growth, and there was complete confusion in the political discussion, as there still is. For example, the distinction between recovery from a business cycle recession and long run growth was something that was beyond the ability of most people, including economists, to keep straight. It still is today. *Growth* is still a slogan word that we hear all the time, used as the raison d'être of all kinds of policies. I guess something that is characteristic of me is that I see some purpose in wanting the economy to grow faster in some true, long run sense. It doesn't have to do with moving from unemployment to full employment but does have to do with increasing the rate of increase of productivity of potential output.

So, I worry about what kind of policy could you as an economist advocate with a good conscience that would increase that rate of growth. Actually, my Ely Lecture in 1963 before the American Economic Association was about this. It contained a theoretical argument why the market could produce a smaller rate of growth than was desirable for the long run.

An early endogenous growth contribution.

Well, it was not quite that, but it was saying that there is an externality there that could be exploited by policy. So then I wrote an article called "Growth through Taxation."

A provocative title.

A provocative title. It was published in *The New Republic* in 1960. It said that one way to get growth would be to have a tight fiscal policy and easy monetary policy. We were talking about that idea of the neoclassical synthesis a few minutes ago.

The idea was to increase tax revenue to increase public savings—national savings. I was doing some work for Kennedy when he was a presidential candidate, and he was wanting to do something about growth. But he didn't quite want to do that—to raise taxes to increase the rate of growth!.

But I also had some other ideas about what you could do—I must admit that I see what you mean by saying it's a similar focus in general, not in particular, to some ideas of Larry Summers. The question was, "What could you do to raise the rate of personal saving?" That was where the proposal for limiting tax deductibility for advertising came in. When I was appointed to the Council of Economic Advisers in 1961 I had to be confirmed, along with the other members, by the Senate. At my confirmation, one of the senators on the committee asked me about this proposal—which he regarded as a really terrible thing. And so, I was in a little trouble because of this idea, but they weren't that worried about who was on the council in those days. I still think it was a good idea.

Well, the transaction tax is another case. In fact, you and Summers both have endorsed that.

Well, Summers actually wrote an article, in *The New Republic* in 1987, during the Dukakis campaign, while he was working for Dukakis, entitled "A Few Good Taxes."

Yes, I remember that.

And one of them was the transaction tax—not just on foreign exchange but on stock market and other exchanges. Summers, of course, is no longer in favor of transaction taxes.

The Council of Economic Advisers, 1962. From left: James Tobin, Walter Heller (chairman), and Kermit Gordon.

Yes.

It's not a good thing for the deputy secretary of treasury to be for. So I wrote and asked him what happened. And he referred me to some journal article which seemed, in his view, to explain why these taxes didn't do any good. I don't know if that's true or not of the article; I didn't look it up.

In 1973, the Bretton Woods system of fixed exchange rates collapsed, and the United States abandoned the convertibility of dollars into gold. So then there was a big discussion in the world and in the economics profession about what would be the best international monetary system, and the usual candidates were market floating exchange rates and fixed exchange rates. I, however, thought the big problem, one which was going to be a problem whether we have one of those two regimes or the other, is the increasing volume of hot money that can move freely around the world very quickly from one currency to another, one national market to another. This could create excessive volatility in exchange rates. My main reason for advocating the transactions tax is not often understood. People assume it's aimed primarily at "speculators" and volatility. Its principal goal, however, is, more than that, autonomy of monetary policy in different countries. If there is really perfect mobility across currencies, no exchange controls, and no obstacles to moving funds, then interest rates would tend to be arbitraged into equality, and only a big country would be able to have a monetary policy of its own.

The world is not yet ready for a single currency like the Euro in Europe, so I thought it was going to be necessary to protect the international monetary system against excessive mobility of private funds. That was my diagnosis of the situation in 1973. Yes, and if that was a problem in 1973, as I thought it was about to be, it's surely much more of a problem now.

Well, it seems to be with recent events in Asia.

Yes, but this is not a proposal that attracted a lot of attention from my colleagues in the economics profession.

Well, I was wondering if there was some methodological difference that leads you to proposals like this.

(laughs) Methodological difference?

Well, in some sense you're less rigidly driven by preconceived models and more pragmatic in your response to observations.

Yes, this is a very pragmatic proposal. But there is not much interest in it in financial circles or in the circles of financial policy. Larry Summers is not for it and his boss is not, and no other minister of finance or central banker is for it, so it is not a live possibility.

But there are models which do confirm my intuition about it. And there are others which contest it. I rely on a very simple idea—maybe that's characteristic of me—which is the obvious fact that the transactions tax gives a preferential

James Tobin, chairman of the Department of Economics, 1975. Courtesy Office of Public Information, Yale University.

incentive to long run investments or long term round trips from one currency to another and back rather than short term. If you think of the annual rate equivalent of the tax, it's much greater for short term transactions than long term.

Another reason why many economists don't make policy recommendations is that they are more pessimistic about the ability of economists to influence policymakers. Milton Friedman in his book, *Capitalism and Freedom*, you remember, makes the case that all of the advice of economists is used by special interest groups cynically for their own purposes, and so we have to have very simple recommendations and this generally means staying away from any interference of markets. He says essentially that the message has to be very simple: just don't interfere.

Yes. Actually, I have made recommendations similar to those that Friedman has made in several cases, for example, the negative income tax.

But I have had trouble with this question whether you should refrain from recommending things to governments because they might have political consequences different from what you intend as an economist. My feeling has been that it's our business to recommend what we think is the best thing for policy, the best policy for the purpose, to the politicians. For example, should economists who thought that there were economic circumstances in which deficit spending would be desirable not have told the politicians that this might be true? I've read arguments that say, "Well, if you tell politicians then they will abuse this knowledge, and they will do deficit spending all the time, instead of doing it just when it's appropriate. And so we shouldn't tell them about it." My view is, it's not our business to conceal information like that from the politicians. We should tell them what we think is the best policy. We should tell them that we are not telling you to do deficit spending all the time, we are telling you to do it under certain circumstances, and so on. But that it is not our business to withhold what we regard as dangerous information.

So, let me ask you about some other things. You mentioned your "Liquidity Preference as Behavior towards Risk" article, 1958. This is very important. It laid the foundations for the whole fundamental theory of finance, the capital asset pricing model (CAPM). The separation theorem appeared there first. And yet you never seemed to join the finance research direction; you somehow started it and then left it to them. I wonder if you could say something about that.

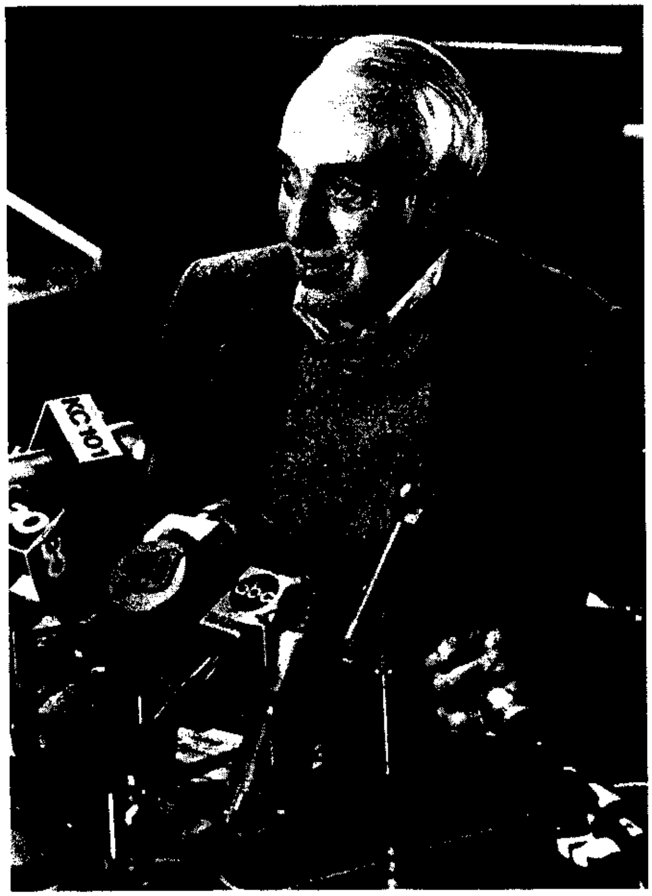
Well, I was being an economist all the time. My interest in the separation theorem was that I was looking for an explanation of the demand for money in particular, an explanation how the demand for money was related to interest rates and to risk. So that was my purpose; that was not a finance purpose. Finance is a subject which is telling individuals or firms how to behave in their own interest. That wasn't my purpose. My purpose was to develop a convincing and theoretically acceptable explanation of the interest elasticity of the demand for money.

I was concerned with liquidity preference in Keynes's terminology. Let's go back to what the debate was. Keynes said that there was an interest rate effect on the demand for money because there would be expectations of capital gain or loss which would vary with the current interest rate. But he assumed the individuals had a fixed idea what the future interest rate would be. So that seemed to leave a central part of the theory unexplained and to leave his theory vulnerable to what would later be described as the rational expectations critique.

If you said, as Keynes did, that during the depression the City, the investors in London, had the idea that the long term interest rates should be 3%, then when the interest rate goes below 3%, they will think there are going to be capital losses. Thus they don't want to hold those bonds at such a low interest rate. The question is why they continue, let's say, to think the interest rate all during the depression should be normally 3%, when in fact, it's always lower than that. That would appear to be a rational expectations failure, and that's what it seemed to be at that time. So I wanted to have an explanation for the demand for money that didn't depend on there being a different interest rate from the one which the model produced. That's perfectly good rational expectations methodology. Right?

If you could call it that over a decade before the rational expectations revolution in economics.

Yes, so that's what that article was all about. It wasn't about creating the CAPM model or the separation theorem. The separation theorem just came out naturally from the way I was modeling this thing, and I was very pleased by that, because otherwise, it seemed to me that I had a problem in that there are lots of portfolios people will hold. If there is just one riskless asset and lots of risky assets, how do



James Tobin on the occasion of the announcement of the Nobel Prize in 1981. Photograph by David Ottenstein. Courtesy Office of Public Information, Yale University.

I get a theory which is the same as if there is only one risky asset out there? The CAPM really amounts to the dual of what I was doing.

It's the dual of it?

Yes, I was taking the prices and inquiring what the quantities are to get a demand for money function, whereas CAPM takes quantities as given and inquires what the prices must be. So, yes, it is a fact that Lintner and Sharpe did the dual. It hadn't occurred to me to do that because that wasn't what I was looking for. I never was a part of the finance fraternity. In fact, in spite of writing a lot about finance theory, I was never asked by anybody on Wall Street for advice.

I guess we each have our niche. You have a niche on Capitol Hill, not on Wall Street.

I think my niche is academic. Capitol Hill is not a very good place to have a niche. Most of the time when you go to testify before a congressional committee the only member of the committee who is listening to you is the chairman. I'll never forget going down there for a hearing before the Senate, I think, it was the Senate Banking Committee, and the hearing was to be about macro policy. It was really a crucial time when there were very important issues of macro policy, in the early eighties. When I arrived, the whole committee was there, every single one of them. I was one of the witnesses, and Willie Fellner was another, along with other people of some moment in the economics profession. So I thought, why this is great. It turned out that it was the tail end of a hearing about a particular provision in the Savings and Loan Institution Act about exempting some associations in New England from some regulation that applied almost everywhere else. It was very important for certain associations, for certain firms, so all the senators were there; they all had their constituents' interests to protect. That hearing was lasting into the time that was supposed to be for our hearing before the same committee. Then everybody left, except for the chairman. Because he had invited us, he had to stay. They had been talking about a few dollars here and there in Rhode Island as opposed to New York. We were going to talk about hundreds of billions of national income, and they didn't want to listen to us. I wasn't as anxious to go to hearings after that.

Maybe you can tell me about the "Tobin's q" model, what this was and how this differs from the Hayashi version that followed it, that makes it a theory of adjustment costs. I understand that you think your theory was fundamentally different from Hayashi's.

Oh, I don't mind having it be a theory of adjustment costs. That was my idea too. I should say *our* idea, because "q" is a joint product of Bill Brainard and me. What I was objecting to with respect to Hayashi was the idea that we were proposing a "q" which is a shadow price of an optimal program solution. So his "q" was not a datum and not something you actually could measure as a market variable. It was the result of an optimization solution.

That's the Hayashi story?

That's the Hayashi story; that was not my story.

But why do you object to that?

I don't object to that. I just object to identifying our "q" with that "q." Our theory stressed a market variable comparable to an interest rate. It's a datum for individual agents, created by monetary policy interacting with the economy, a datum to which individuals and firms respond in their investments. It's analogous to the cost of capital. People don't regard the cost of capital as simply a shadow price. You've got to have cost of capital as a market phenomenon. Our "q" relates to the old Wicksellian idea, the difference between the market rate of interest and the "natural" rate of interest. In fact, it's easy to have a little model which shows that they're the same thing.

It sounds like you're taking Hayashi and putting him in a general equilibrium framework.

I'm not taking Hayashi. Hayashi's taking me.

Yes. But you're saying you're emphasizing something different; you're emphasizing a macro variable.

I'm emphasizing a market variable, a datum to an individual firm. My idea is that the Central Bank has something to do with "q's"—just as the Central Bank has something to do with interest rates.

I was going to ask you about the general equilibrium approach to monetary theory that you worked on, partly with Bill Brainard. These were ambitious models but different from today's general equilibrium models in that they did not involve utility maximization. You had a lot of complexity and had a lot of descriptive detail but had general equilibrium in a sense different from what we think now. Isn't that right?

Yes, I guess that's right, although we regarded the theory of portfolio selection, e.g., the liquidity preference article, as microfoundations. The main point was that the economy has a bunch of different assets, not just money and capital. Maybe the word *general* was not a good word to use. The point was to have a theory of monetary policy and money within a theory of the demand for lots of different assets all of which are in some way or another substitutable one for another, imperfectly substitutable, one for another. In the usual model that described monetary policy there were only two assets—there was money and there were goods, and that was what monetary theory was. Just those two assets.

So what happened to your general equilibrium approach to monetary theory? It seemed to be a movement for a while, right? Here at Yale a lot of people were doing this, and I haven't heard about such work lately.

Well, people would rather do the other thing because it's easier.

I think of your general equilibrium models also as embodying behavior patterns that would not be suggested by optimizing models, such as sluggishness of response. That sounds like some real human behavior that might be hard to square with optimal behavior unless one made some implausible assumptions about transactions costs.

Yes, we allowed possibilities like that, but more generally, we conceived of the equilibrium portfolio and then the speed in which one moved from actual to desired holdings of different assets.

And this having as a modeling parameter the speed of transition, that's not in fashion these days.

The whole thing is not in fashion. The whole idea of modern finance does not include imperfect substitution. I suppose in defense of ignoring it is the fact that we weren't actually able to solve the nonlinear equations with these adjustment mechanisms. Also, there is the big question of expectations and how you handle them at the same time you're handling these issues.

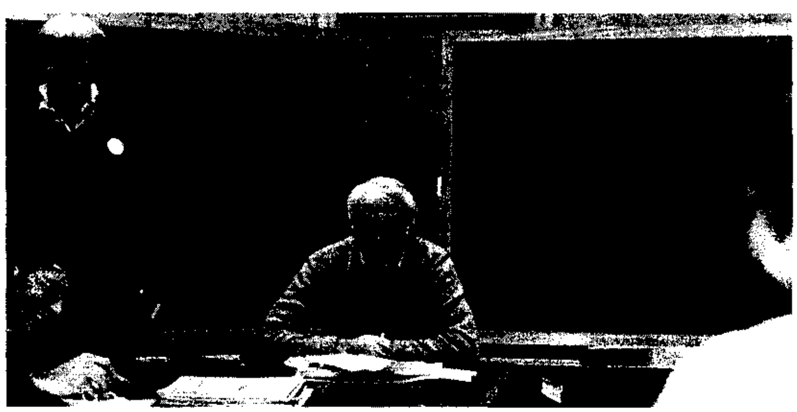
One thing that our model did was to be careful about modeling the relationship between the Central Bank and the banks, and other institutions, and equities, and so on. All of that is short-circuited now by the way people do macroeconomics and monetary economics. Maybe people are right; maybe it is just perfect substitution between assets, and what you need is just to know what expectations are at any given time.

Maybe there is another problem that the demand for money became very unstable since then, and people feel that they have no way of modeling that.

Well, we never regarded the demand for "money" by any arbitrary definition as something that you would expect to be stable.

Yes, people even have a lot of trouble defining what money is. I remember you were saying that if Milton Friedman has trouble even defining it theoretically, then how can it be so central to macroeconomics?

Obviously, there are all kinds of different moneys and near money, and they are imperfect substitutes for each other. It seems obvious, and yet that's not the way people wanted to look at it. Instead they wanted to play games as to what defi-



James Tobin in class at Yale University. Photograph by J.D. Levine. Courtesy Office of Public Information, Yale University.

nition you used, but each time you make another definition, you say, "There's a big gulf between whatever "*M*" you're talking about and everything else." So we were not surprised that the stability of the equations for the demand for $M1$ didn't last.

People often say that the instability of the demand for money had to do with new transactions technologies.

That could be, but it also could be because people didn't look at the substitutability between this kind of money and some other kind of money.

So, looking back over all the things you have written, I am struck by this spirited debate you had with Milton Friedman on the substance of monetarism. I just wonder how you view that debate now. It appeared at the time to me as an outsider in viewing it that it was a central debate in monetary economics.

Well, there were two debates. One was regarding monetary policy and fiscal policy. Friedman, by essentially saying that there were no interest rate effects on the demand for money and therefore no substitutes for his money, however he'd choose to define it, was essentially saying that there was no way for anything but changes in that money to have a macroeconomic consequence. So that would rule out fiscal policy. In other words, if you say the velocity of money is constant then there is nothing else you can do, right?

There is another debate which in some sense is more fundamental and which is going on beyond the first, and that is whether we are always at the natural rate of unemployment. If we are, neither monetary nor fiscal policy does anything real. That's the bigger debate.

In my opinion, I won the first debate. In the second debate, the antagonist had been changed from Friedman to Robert Lucas or somebody like that. I guess the profession does not agree with me, but people like you who stress "behaviorism" are clearly uncomfortable with Lucas and company.

I'm glad we managed to do this interview before you leave this summer for Wisconsin. You've been returning to your ancestral family home in Wisconsin every summer that I have known you. Most people don't do anything like that. It's unusual. You could travel to glamorous resorts in the tropics. Why do you keep returning to Wisconsin?

Well, I have had some nice trips as an economist, though maybe not glamorous, and have take Betty along with me. But our summer we have reserved for this family place which I have actually gone to all my life, from the time I was a small child, a baby indeed, to now. It's a gathering point not only for our family and children and grandchildren but for the extended families, my brother and cousins and their families, and for succeeding generations. So it's a very good thing. It happened that my wife, Betty, whom I met in Cambridge, Massachusetts, in 1946, far from Wisconsin, was from Wisconsin, too, and had grown up not too far away from the place that I had gone to.

You also have lived in the same house here in New Haven since around 1950?

'51. Not very enterprising. We were wise enough to buy a house that had five bedrooms, so it had enough room for all our children to have individual bedrooms. It's a good house.

I have a last question, which is how has your emeritus status been to you? And how should we look forward to or prepare for those years?

Well, I didn't do anything particular to prepare for those years. So I did not have any well-thought-out strategies as what I was going to do. I have enjoyed doing occasional teaching, keeping my hand in special undergraduate courses.

You still have some in prospect next year?

No, I think probably I will not do any more. I have a lot of things to do that are not very different from what I used to do. I guess one problem, which is not different when you are retired, is where does your program come from. You get all these invitations and requests and they want you to do this or that, and if you are not careful, your program is out of your own initiative—it's what other people want you to do. I've always had a problem with that—preserving my own autonomy, so to speak. Lots of times there are good reasons for doing what you have been asked to do.

I have not been doing any graduate teaching since I retired. I miss the contact I used to have with the graduate students. But I still have contact with a few of them, and for me this is very rewarding.

NOTE

1. J.R. Magnus & M.S. Morgan, Special Issue on the Experiment in Applied Econometrics (papers by J.R. Magnus and M.S. Morgan; H.M. Anderson and F. Vahid; H. van Driel, V. Nadall, and K. Zeelenberg; E.E. Leamer; P.M. Bearse, H. Bozdogan, and A.M. Schlottmann; H. Song, X. Liu, and P. Romilly; D. De Crombrugghe, F.C. Palm, and J.-P. Urbain; J. Tobin), *Journal of Applied Econometrics* (September–October 1997): 459–662.

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