Paul Davidson Interview of July 1997

by

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Paul Davidson Interview

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Colander: Is there anything in your childhood that led you in your rebellious ways?

Davidson: I had a fairly normal childhood. My parents were very strict Democrats. My father didn't like union workers because they did a lot of pot. My mother was a very strong supporter of unions. I thought I was very apolitical, basically. But, raised in a basically Democratic household, I suspect there was some influence on me.

Colander: How did you get into economics?

Davidson: My first contact was in college. My folks wanted me to be a doctor—the profession for any young Jewish boy. I didn't want to be a doctor, but I compromised and majored in biochemistry at Brooklyn College. Then I went on to graduate school in biochemistry at the University of Pennsylvania. However, as I approached my Ph.D. thesis in biochemistry I decided I didn't *like* biochemistry. In the meantime I had met Louise, and became more interested in her than in biochemistry. And she was still back in New York City.

So I quit biochemistry and went back to the city. And what do you do in the city? You have to go to business. So I thought of taking business and economics courses, as I'd never taken any such courses as an undergraduate. Louise and I took a Principles of economics course together. I decided I'd get an MBA at the business school of City College, and, while there, I took more economics courses and found out about econometrics. As a bio-mathematician I knew much more about statistics than most econometricians.

I had enjoyed teaching while I was at the University of Pennsylvania, so I thought, well, maybe I can be an academic teacher, not in biochemistry, but in economics. So that's what first steered me toward economics. But I don't think I really understood what an economist was.

Colander: Did you get your MBA?

Davidson: Eventually I did, but in the interim I spent two years as a biochemist in the Army, but, luckily, part of the time I was stationed at Camp Kilmer in New Jersey doing biochemical research. Being stationed at Camp Kilmer, I could go to City College at night. So I continued to take courses at City College at night for my MBA. By the time I got out of the Army I only needed a few more courses, so I got a job in an actuarial department at a life insurance company during the day and continued at night and finished up my MBA.

Colander: Were you married then?

Davidson: Yes, Louise and I got married just before I went into the Army. So we had been married about 2-1/2 years when I finished. She continued to work and support me.

Colander? Where did you work?

Louise: I was working at Macy's, part time, because I was still finishing up college.

Colander: So, then, you're coming out of the Army with the G.I. Bill. Where'd you apply?

Davidson: I applied to M.I.T., Harvard, Brown, Berkeley, and Pennsylvania. All of them offered me fellowships, but Penn offered me the most—$2500. I considered Berkeley, but we decided to go where we got more money,.

Colander: What would you have done if you had gone to Berkeley?

Davidson: Well, I would have been a good neo-classical Keynesian, I suspect. Hopefully I would have seen the light. But I'm not sure about that.

Colander: So Penn was really important in structuring your life?

Davidson: What a question! The answer is, clearly, going to Penn and coming under the influence of Sidney Weintraub at exactly the right time structured the rest of my career. You see, Sidney was a microeconomist. He was the first Jewish professor hired by the economics department and the second hired by the Wharton School. The first one at the Wharton School was Simon Kuznets. It was about 1950. His work in the 1940s was all micro. But somewhere in the late 1930s and 1940s, he got bitten with the Keynesian macro bug. In about 1954 he went over to England for a year and he wrote a book on income distribution. (He published some of it in the *EJ* and *AER*.) It was his attempt to bring aggregate supply back into Keynesian economics. When I was in my first year at Penn he was teaching from the manuscript since the book hadn't been published yet. He was overwhelming and bubbling with these ideas and I found him just mind-boggling.

Colander: Who else was at Penn?

Davidson: Nobody really. All the professors at Penn at that time—the older, full professors—were textbook writers who had been popular in the 1930s and during the War. Raymond Bye, for instance; he wrote a famous textbook in the 1930s, and on into the 1940s, which was one of the ones that Samuelson knocked off in his first edition. Bye was a "Socialist." He believed in 100 percent money. He taught me macroeconomics. He hadn't the slightest idea what an equation was. I remember once he wrote on the blackboard "y= c + i" and said, "Now, that's an equation." Then he said, "Wait a minute—I'm not sure," so he erased it and started off on a different topic. His macroeconomics was pretty bad.

We did have a whole bunch of textbook writers, some of whom had been called up before the McCarthy House UnAmerican Activities Committee and I suspect *were* card-carrying Communists.

I took Sidney for both microeconomic and Comparative Modern Economic Thinking. In this latter course, Sidney presented Joan Robinson's *Accumulation of Capital* chapter and verse along with Hicks's revision of demand theory. Sidney was a Cambridgephile. He loved Cambridge. So anything that Joan Robinson, Richard Kahn, or Roy Harrod was writing was what his students had to read and know.

The other thing that was, I think, great about my education was the preliminary examination. When you first became a graduate student they gave you a list of 100 great books in economics. There were 10 or 15 books in various subcategories such as income distribution, welfare economics, price theory. You had to read half of the books in each subcategory, list the books that you read, and give the list in to the committee. It then examined you in those books. It was an oral examination. They were allowed to ask you any question about any of these books, which were roughly 50, that you had listed.

Colander: Do you remember any of the books you chose?

Davidson: One was *The General Theory*.

Colander: What did you do your thesis on?

Davidson: I had contemplated doing something on econometric studies. After I met Sidney I lost interest in that. We discussed what I should do my thesis on and I said Richard Kahn or Harrod I wanted to do a thesis on and I wanted to do a thesis on whether Social Security payments would be sufficient to pay people when they retired. Sidney said that if I wanted to finish my thesis before I went on Social Security, I should not take that topic because it would take that long to do it. I couldn't think of any other thesis to take, so I said, "Well, what would you suggest?" He said, "Why don't you take the topic called 'Theory of Relative Shares'?" I had no idea what the theory of relative shares was. So I said, "Fine. What is it?" He said, "Good. You'll be able to handle it because you don't know anything about it." So that's what I took—a history of economics consideration of income distribution, which, of course, since Sidney had just finished writing his book, was what he wanted me to do.

Colander: When you left Penn, what did you do?

Davidson: I went to teach at Rutgers.

Colander: What was your research program there?

Davidson: The first article I ever published was on Ricardian rent sharing; it came right out of my thesis. The thesis was also published, as a book. So the first one or two articles I published were on income distribution or relative shares.

Colander: How was that approach to macro seen at that time?

Davidson: Sidney was the only economist who saw macro in this framework. His macro book was called *An Approach to the Theory of Income Distribution*. It was shocking, because nobody thought of income distribution as a macro topic. Everybody knew it had some macro implications, but nobody saw macroeconomics as directly determining the distribution of income

Joan Robinson, Richard Kahn, and Nicky Kaldor in the late 1950s had picked up this argument about profit shares from Kalecki. Kalecki had this profit shares as a function of investment shares, so the IS investment savings equality suddenly got translated into an income distribution curve. So there was this overlap.

Sidney's theory that not the same as Kalecki's theory. For about two or three years Sidney was trying to meld these two. He corresponded with Joan Robinson, about how these theories could be melded together. I always knew, and still do, that they don't meld, except in some superficial accounting way, and that Joan was absolutely wrong—she thought that Kalecki had discovered the General Theory before Keynes—she was absolutely wrong—that's a separate story. Sidney was much more ambivalent about the Keynes versus Kalecki debate..

After two years at Rutgers, my salary didn't improve dramatically and we now had two children. Considering my G.I. Bill income, when I took the job at Rutgers, my after-tax income actually went down by about 20 percent. So in the first two years at Rutgers we were living below what we did when I was a graduate student, and we now had a second child. So things were getting pretty tight at home, and I was not getting very high raises, so that's why I went and looked for a job. I was offered this job at Continental Oil Company—Louise actually applied for this job for me because I wasn't very interested. I ended up getting this job, which was for $15,000 a year compared to the $6,700 a year I made at Rutgers.

I worked for Continental for about 11-1/2 months, until 1960. High pay, good expense account, nice office, etc. But I didn't stay, for two reasons: first, the culture shock of living in the segregated South was very difficult. Second, I found out that I was losing my skills as an economist. Our job wasn't economics; it was public relations. So the pay was very nice, but professionally and culturally it was not. On the other hand, it did allow me to do a lot of research, and I did publish one article, in *The Southern Economic Journal*, on the effect of a general excise tax on macroeconomics. That was published after I left, but it was written while I was there at the oil company.

I also did a lot of research for the oil company on what the effect of Keynesian economics would be for our company, and for the industry as a whole. So when I came out I could do an article for the *American Economic Review* on problems of the domestic oil companies.

I also did a third article on income and employment multipliers in which I showed that the Keynesian income multiplier and the credit multiplier could be handled by Sidney's average aggregate demand curve. So after a year I went back to Penn. Penn hired me to teach Industrial Organization, not macroeconomics, because I had an MBA I was somebody who'd met a payroll and had worked for the oil company. So I started out teaching, in the graduate program, Industrial Organization.

Colander: At Penn, did you collaborate a lot with Sidney?

Davidson: No. Interestingly enough, Sidney and I only worked on two articles in our whole lives together, and both of them were done when I was at Rutgers the second time. I was working independently of Sidney. I did most of my work in micro.

The next important article I did was on the finance motive. Sidney had taught the finance motive in the course that I had taken with him. He didn't know what in the world to do with it. It always intrigued me that here was this fourth motive for holding money that no one seemed to know what to do with. So that's why I started working on the topic. I worked it out and Roy Harrod happened to be visiting Penn and I got to know him fairly well. I sent the manuscript to *AER* and it got terrible reviews . (The editor of the *AER* had switched to John Gurley.) So I showed it to Harrod. He took one look at it and said, "This is exactly what Keynes must have meant." He said he would get it published in the Oxford Economic Papers. So it was published in the Oxford Economic Papers. It was that article that made me a monetary macro economist. Up until then I had been just doing standard macro.

Colander: When did you turn into a Post Keynesian?

Davidson: Well, that was the great question. That didn't come about until the 1970s.

Colander: OK. Up through the 1960s you had been writing; you had a slightly different view, which was competing with the other Keynes views, but was not fundamentally different.

Davidson: That's right. The only difference about it, it had aggregate supply in it. And Eugene Smolensky and I wrote this manuscript called *Aggregate Supply and Demand Analysis*. It was dedicated, when it was a book, "To Sidney Weintraub, of course." Our argument was that "aggregate supply" had to play an equal role with "aggregate demand."

I remember that when we submitted the manuscript to a number of publishers, they all disliked the title. We sent it out, and everybody said, "Change it to *Macroeconomics* " or something like that. We insisted on the title, *Aggregate Supply and Demand Analysis*. When it didn't sell the editor and the publisher said, "We told you so!"

Colander: Who were the other Keynesians in aggregate supply focus? How about Lori Tarshis?

Davidson: We didn't really think of Lori as being part of the group but he clearly did have an aggregate supply focus. But basically we were the only ones. Kenneth Kuserka, who was at Rutgers at the time, edited a book in 1947. It was called *Post Keynesian Economics,* which is the first time, as far as I know, the term "Post Keynesian" comes up. It had nothing to do with Sidney or anything like that.

The interesting thing was to see an article, in that book, by Paul Samuelson, if I remember correctly, in which he's surveying what Keynesian economics meant since 1936. And he has a paragraph in which he says Sidney Weintraub was working on Keynesian economics with aggregate supply as a "lone wolf." And he gives Sidney this "lone wolf" connotation. And I think that's right. There were no other people working on aggregate supply. And Sidney tried to convince Roy Harrod.

And then Sidney writes a book. Sidney started writing these books with these tremendously long titles. In one he attempts to do the Harrod model in terms of an aggregate supply/aggregate demand analysis. But Harrod just wouldn't buy it. So in terms of aggregate supply Keynesianism there were very few of us. And then Smolensky drops off and becomes a poverty institute type. He goes to the University of Chicago, and that ends his game [laughter]. So then there was only Sidney and me.

Colander: So you kept on working. You were doing other stuff, too.

Davidson: Well, the next major thing that I did involved growth models. Harrod, and, of course, Sidney were both interested in growth models. In 1965 Tobin published an article called "Money and Economic Growth." I looked at it and I said, "This is a disaster." It was a neoclassical model where you had a substitutability between the demand for money and the demand for real capital. And I thought that was wrong basically. So I wrote an article, which I sent to *Econometrica*, because that's where Tobin's article was. It was on what's wrong with Tobin's article what the demand for money in a real Keynesian growth model really looks like. I had two prices: the spot price on old capital and a forward price on new capital. When the spot price rose above the forward price, demand for capital growth would rise. It was a strictly Marshallian market period versus short run price.

I sent it in to *Econometrica* and after sitting around for six or more months it gets rejected. It gets rejected because one of the referees says, "It's not rigorous enough. It sounds like it's a good idea, but it's not rigorous enough." So I called up Sidney and I said, "What do they want for rigor?" And Sidney said, "You don't have any equations in it." I had diagrams, but no equations. So I sat down and I took every paragraph where I had a diagram and wrote in an equation. In all, I added about 14 equations. I sent it in again. The exact same paper. I didn't change a word except to explain what each symbol meant. And it was accepted. So that gives us a sense of what they mean by "rigor." And I was pleased, because I thought that Tobin, a man whom I really admired, would respond, and that response would lead the aggregate-supply Keynesians and the real Keynesians to rejoin forces. I was shocked to find out that there was no response. Absolutely nothing.

I had submitted this in 1965. *Econometrica* didn't published it until the end of 1967 or early 1968. So it had a long publication date. I assumed, at the time, they sent it to Tobin and Tobin saw it. Tobin comes out in the middle of 1968 with his "q" ratio relationship, which was very similar to my analysis. I was always annoyed—I don't *know* that he saw any relationship between that analysis and mine, but I can't believe that he didn't. And that told me that I had to sit down and write a book. I couldn't just write these little nice pieces. So I then decided to write *Money and the Real World*, which came out in 1972.

Colander: We are getting ahead of our story; you were at Rutgers when you wrote *Money and the Real World*. How did you get from Penn to Rutgers?

Davidson: By 1966 I had been at Penn five years and I was still associate professor, reasonably good salary but still associate professor, and other people were getting promoted who had not published as much as I had published. I went over and told the provost I wanted to get promoted. I felt I had enough publications in major journals so that I should get promoted. And he said, well, I'd have to wait until X, Y, and Z, who had more seniority; were promoted. Yes, they hadn't published as much as I, but they were on the list and I'd have to wait 3 or 4 or 5 years before I could get promoted. At that point I started searching for a new job. And Rutgers happened to come around.

At the time Jan Kregel was a graduate student of mine at Rutgers, and he wanted to write on the reswitching controversy. I told him the thing was to go over to Cambridge, England, to do it, and I wrote to Joan Robinson and she invited him over. He was technically doing his dissertation under me but actually doing it under her. Jan told her I was writing this new book, and she wrote me back a letter inviting me to come over and let her look at this thing I was writing. So in 1970 I took a year off from Rutgers and went over to Cambridge, England.

Colander: When did the term "Post Keynesian" develop?

Davidson: Sidney in the early 1970s takes a leave from Penn and goes up to the University of Waterloo and decides he wants to start a journal. He wants me to come up, but Louise and I didn't want to go to Canada—it was too cold, among other things. Sidney thought he had financing for the journal but somehow it never got off the ground.

He came back in 1975 or 1976, and at that time I'm chairman at Rutgers, which means I had some secretarial resources. We agree to start a journal. We made up a list of 75 names of people that we thought would be supportive. We expected them to join the editorial board and also to send us some seed money. We expected to get about 25 people. In fact, we got about 65, 67 who agreed. Some of them wouldn't let us use their name but sent in money. A few of them didn't want either to give money. or to have their name used. One of the reasons why was we weren't sure of what the journal was going to be named. There were a number of suggestions, and one of them was "Post Keynesian." A few people objected from the American side because they—at this stage of the game I guess the name "Post Keynesian" was already being associated with Joan Robinson, although she called them "neo-Keynesian." And so some of them would give their money but they didn't want the name "Post Keynesian" because of the association with Joan.

So one of the other things we thought of was, "Let's just call it *Keynesian Economics*." But the problem with that was the acronym: J-O-K-E. So that knocked out *Keynesian Economics*; Joan had already used "neo-Keynesian;" so we took "Post Keynesian," although some people objected. About the same time Paul Samuelson starts calling *him*self "post Keynesian.," with a little "p" and a hyphen. So we made it with a capital "P" and no hyphen.

Colander: Tell me about Rutgers. That was known as a Post Keynesian school for a while, and then it wasn't any more.

Davidson: When I first came there the man who was the chairman was a man named Max Giedeon. He was a very conservative Chicago type. The second time, when I was recruited back to Rutgers, which was 1966, the chairman was Monroe Berkowitz; he was a Columbia Ph.D., an Arthur Burns type. Most of the other people there were either from Columbia or Harvard. So it didn't really have very much of a Post Keynesian flavor. I guess I was hired because they needed a normal Keynesian in those times,. and because I was known to be a nice personality—little did they know! And I had had these two articles published in the *AER* which made me somebody who had published in a mainstream journal. At that time, already, I had two in the *AER*, one in the *Review of Economics and Statistics*, and a couple of other major journals. So when I came there it was still a very orthodox department

Rutgers didn't become a center for Post Keynesian economics until the mid-seventies when the provost decided he wanted to differentiate Rutgers from other schools. There were six colleges, each with its own economics department. Each college reported to the dean of each of these colleges, and the dean reported to the provost. There was also an "area-wide chairman," so all the chairmen reported to this chairman, who reported to the dean "of the discipline" as opposed to the dean "of the college." Both the deans reported to the provost.

The provost came to me and asked how to differentiate the Rutgers economics department. *The Journal of Higher Education* had said the University of Massachusetts had become well-known because they had hired all these radicals that Harvard had not hired. He asked, "Couldn't we do something similar with this liberal deviant economics called Post Keynesian?" We decided one of the new departments would have heterodox economics—not only Post Keynesians but Marxists, radicals, and institutionalists—as its focus, and the other departments would be much more orthodox. And since students were allowed to register for courses in any of the colleges they were residentially associated with, any student could still get an orthodox education.

And so Livingston College became the resident heterodox college and Jan Kregel was the first Post Keynesian that I hired there. At no time were there more than three Post Keynesian faculty members at Rutgers in economics out of 81 faculty.

Colander: Who were they?

Davidson: Jan Kregel, Al Eichner, and myself, Nina Schapiro might have been considered Post Keynesian. She had come from the New School. And then there was Michelle Naples, who had come from the University of Massachusetts; Bruce Steinberger, who is now the chief economist at Merrill Lynch, had come from Michigan; and one or two others who had come from the off-beat programs. But only three of us were real Post Keynesians.

Colander: Let's go back to the starting of the *Journal of Post Keynesian Economics*. Louise, you played a big role here, right?

Louise: Well, in the beginning of the Journal, Al Eichner started holding these meetings at Columbia (even though he was at SUNY-Purchase he had been to Columbia and was able to use the facilities there). There must have been about four meetings at different times where people came from as far as Washington, Philadelphia ,and Wesleyan.

Colander: I attended a couple of those meetings.

Louise: Those meetings started systematic thinking about the Journal. Sidney became very enthusiastic that this was the time to start the Journal. So he's the one that pushed it.

Then the question was: how to do it, and that's when we asked people to contribute. The contribution was, I think, $50, although some people paid a little more.

Davidson: Galbraith said he would match whatever we raised.

Louise: But then it turned out that Sidney knew, a publisher, Mike Sharpe, who said, "Why don't you let us do it?" We also checked into the people at Rutgers. It seemed just very easy to do it with Mike Sharpe because he was going to take all the losses—and, of course, most of the profits (if there were any)—and that's what we did. Rutgers was not very generous. We had a computer out in the hall that other people used, and they gave me a desk in one of the satellite offices.

Davidson: Louise was the office manager. It wasn't much of an office; it was really a closet that had a window.

Louise: And I had to pay the postage out of the money that I had collected from the contributions.

Davidson: After we had agreed that we were going to do this, we sent out a little flyer to everybody that we knew that we knew was interested, and to people on a mailing list that we got from Mike. We told the people they could be charter subscribers. We got 400 people who sent in money.

Louise: We were trying to make it as inexpensive as possible because we wanted people to have it. That was the whole point.

Davidson: This must have been 1978. We had a meeting in New York at the AEA to celebrate the kickoff of the Journal, although it wasn't going to come out until September 1978.

Colander: Did you get lots of submissions at the beginning?

Davidson: We had an acceptance ratio of anywhere between 15 and 30 percent. We also commissioned articles. This was due to Galbraith. As I stated, Galbraith helped finance us, but he did it on one condition. This condition was a very interesting one. He said that his friend, Seymour Harris, used to run *The Review of Economics and Statistics*. And Seymour ran it on the basis that the articles weren't published just because they came over the transom—in other words, in the mail—but he continually organized symposiums where he would have invited groups of people who would focus on a particular question. So Galbraith extracted a promise from Sidney that we were going to have these symposiums, and do this relatively often.

And so one of the things has been that when submission flows get slow, I have gone out of my way to induce a symposium, so that some years we have many more symposiums than others.

Colander: How did you and Sidney split the work on the Journal?

Davidson: Initially, we just had both names listed and you could submit an article to either editor. We quickly agreed that both of us was free to make our own decision about acceptance, rejection, or revision. If we wanted to, we could ask advice from each other, but we didn't have to. I would say 85 to 90 percent of the time we made independent decisions.

Since all of the mechanics of getting the paper ready for publication were done at Rutgers, we always got a look at Sidney's manuscripts that he had accepted before he got a look at mine. And occasionally I would read something that he had accepted and I would be shocked that he had accepted it. Sometimes I would be even more shocked because he would have edited it without the author's approval. He would just send it in with the original typescript with his pen scrawlings and cross-outs all over the place. I would tell him he ought to get the author to at least approve of all these changes, but his response was always, "No, don't worry about it; they'll be happy to get the publication." And nobody ever complained to me. So I guess he was right.

Colander: Louise, how did you manage to deal with these two strong egos?

Louise: There was really no problem. They did have some serious arguments at the very beginning.

Colander: What were the arguments about?

Louise: Before they decided to each accept or reject on their own, they would argue about what papers to accept. Sidney would want to accept something from somebody that he knew, and Paul wouldn't think it met the standard.

Davidson: We also had agreed not to accept a lot of papers and have a long publication lag. So when we had a lot of acceptances, we would become much more careful about accepting further papers in order to make sure that anything we accepted would be published within 6 to 9 months of acceptance.

Colander: Let's switch tracks a bit. Louise, you were thinking of going on to study economics. You *could* have gone on, but now you are running a journal. Were you content with that??

Louise: Well, we came back from England in 1971 and I got very excited about political science, because we'd spent a lot of time with Galbraith, and I was very interested in the process. However, when we got back it was too late for me to register; then our son got sick, and I wanted to be home, so somehow I never got around to going back. So when the Journal came up in 1978, it seemed an interesting challenge; and I didn't have to work full time on it. I think it worked out just fine. Paul and I get along well; we can tolerate each other seven days a week.

Davidson: Well, I have to amend that a little bit. When we first started, Louise had the office next to mine for the journal. And when something would go wrong, I would storm into her office and shout at her. And she would say, "You wouldn't shout at me if I was a secretary. You're only shouting at me because I'm your wife." And I would say, "I would shout at you if you were the secretary!" And she would say, "Well, I never hear you shout at a secretary." So the question was whether I was picking on her because something would go wrong and I didn't like the way it was managed or was I picking on her because she was my wife. I suspect I would have picked on her regardless.

Colander: My suspicion is that had she been only a secretary, and not your wife, and you picked on her, she would have walked out.

Davidson: That's true.

Louise: He's never done that to anybody else. But that's O.K. After a while I just ignored him.

Colander: Louise, you followed all this. What are your views on Post Keynesian economics?

Louise: It's an interesting fight. I assume that if ever it became the mainstream (which is very unlikely), the fight would be over and so would the fun.

Colander: Was Sidney upset about never being accepted into the mainstream?

Louise: I think he liked the fight, too. You know, everybody wanted to get up there and get the Nobel Prize, but it's not a very likely scenario.

And you know, Paul is a person who doesn't like authority. He didn't tell you about that, when he was a child. And we have a son who also doesn't like authority. So I think it is fair to say that Paul is an inherent dissident.

Davidson: I think there's no question that Sidney was much more conciliatory to people like Solow, Samuelson, and Tobin than I am. I blame them for the failure of Keynesian economics to establish itself in the profession. And I think Sidney always made excuses for them.

Here's a telling story: The Royal Economic Society met at Cambridge University in the summer of 1983 to celebrate Keynes's 100th birthday. They invited me to attend, but they didn't invite Sidney. That really hurt. I don't know how it happened, but somehow Kaldor found out that Sidney hadn't been invited. So Kaldor wrote to him and said, "Look. I will give up my place on the program and you can present the paper on Keynes on the program instead." I thought this was wonderfully gracious of Kaldor. Sidney never accepted it, but that was because he was so sick; he died in January of that year, so it became moot whether he would have accepted it or not.

In June of that year we had the conference and there was a specific session where Samuelson was chair. Axel Leijonhufvud and somebody else gave papers. Solow was a discussant. Solow said something to the effect that one of the problems with Keynesian economics was that it never dealt with the problem of aggregate supply, and that reconstructed Keynesians ought to deal more with supply problems. That really rubbed me the wrong way. During the discussion I raised my hand and I said, "It is unfortunate that Sidney Weintraub can't be here, because he's somewhere else at the moment. But Sidney wrote this book about aggregate supply and aggregate demand, which was reviewed by somebody from M.I.T. who said that the problem with the book was that the whole thing was implicit theorizing about the aggregate supply function. Had Keynesian economics followed Sidney they would have had aggregate supply way back in the 190s and they would not have needed to wait until the 1980s. And it was somebody at M.I.T. who had written this."

Paul Samuelson immediately jumped up and said, "Don't blame me! It's *him*," pointing to Solow. Bob then got up and hedged, saying, "Well, . . . this and that." But he really had to admit that this was what axed down the aggregate supply approach to Keynesian economics. I don't think Sidney would have jumped on Bob like that.

Colander: Let's switch topics back to Rutgers and its connection to Post Keynesian economics. The Journal progressed and did fairly well in the late 1970s and early 1980s. Rutgers expanded and grew in reputation for a while, and then some problems arose. Can you talk about that?

Davidson: Well, we had all sorts of internal political problems. I had a five-year appointment as chairman. At the end of five years there was a fiscal problem and I had some fights with deans; I demanded more lines from each of the deans because the economics department was heavily overloaded with students. The student/faculty ratio was much higher than anywhere else in the college. I got some of the deans to agree with me, but not others. The provost supported me, but the deans, who made the allocations, refused so I resigned. I immediately got a call from Ken Galbraith who said, "Never resign. It lets the other bastards in." And he was right. So I was responsible for letting the neoclassicals in, who immediately started to attack the Post Keynesians. I attributed this to the fact that they didn't want Rutgers to be known as this weird place with Post Keynesians, although there were only at the time perhaps four or five people out of 81 faculty members who could be identified as Post Keynesians. I did a study and discovered that those five faculty members who were heterodox rather than Post Keynesians had published more in five years than all the 76 others together. After I left as chair an institutional witch hunt, a sort of McCarthy hunt, occurred in an attempt to weed out the Post Keynesians. Those who didn't have tenure, didn't get tenure no matter what their publications, no matter what their student evaluations.

Colander: What was the argument? That they hadn't published enough in the "right" journals?

Davidson: One case was Nina Shapiro's. She had 8 or 10 publications when she came up for associate professor. There was one young neoclassical professor who had one 4-page note in *The Review of Economic Studies*. He got promoted; she did not. So it was that kind of thing.

The witch hunt also affected me. I had always taught one half of the one-year introductory macroeconomics course in the graduate program and some orthodox economist taught the other half in that one-year course. When the new director of the graduate program came in, I was not permitted to teach that course. The argument was that I would not teach them orthodox economics, which was what the first year of the graduate program was about; if they wanted to take Post Keynesian economics, they should take it after the comprehensive examination, and I could teach it in their second or third year. So it was a systematic attempt to dilute Post Keynesian teaching from both the graduate and the undergraduate classes.

I was assigned to teach large sections of either the Principles of Economics or Money and Banking since they felt I could do no damage teaching those things. And for about two years that's what I actually did.

Al Eichner was the focal point that finally caused all this all erupt. As I said before, the dean wanted Livingston to be a Post Keynesian outpost. Jan Kregel, who was chairman of Livingston College. (I happened to be on leave at the time. I wasn't even involved.) Kregel and the dean got together and wrote a description for a full professor at Livingston that almost said that the applicant had to be Post Keynesian in order to get the job. They made the offer to Al Eichner. The other faculty members became very incensed about it. In order to get the appointment through, everybody in the whole economics program had to vote. Livingston had about 8 people, and they voted 8 to nothing for Eichner, but of the 81 other people who had a vote, about 52 of them voted against. The provost overrode the faculty and made the appointment. The dean of the faculty did it. I was not there so I did not recommend it or dis-recommend it I wasn't asked.

Colander: Was the vote against Al solely because he was a Post Keynesian, or based on Al's record? Al hadn't done a lot of writing.

Davidson: That's true. Al hadn't done a whole lot. He had written *The Megacorp*. He had written an article with Kregel in *The Journal of Economic Literature* . He also had a few other articles. He fit because the way the job description was written it was of course for a Post Keynesian economist.

The members of the faculty, for some reason or other, felt that I had gone to the provost and lobbied, for Eichner. I didn't really know Eichner that well. Jan knew him because they had written this article together for *The Journal of Economic Literature*. I knew him from the meetings at Columbia. But I was really overwhelmed with his brand of Post Keynesian economics, which was more Kaleckian , so I wasn't overly enthusiastic. On the other hand, I thought Eichner was reasonably good and would have supported him. I hadn't even spoken to the provost about it because I knew Jan was running the war. I fully suspected that because the provost and I had agreed that Livingston should be this heterodox college that he was going to approve Eichner. So I didn't have to lobby.

When Eichner came there was terrible animosity, and there was verbal abuse of all sorts. It became very unpleasant just to be there. When Kramer ended his term as chairman, a non-Post Keynesian, a typical neoclassical economist, took over as chairman at Livingston College. Kregel then went back and spent one term in Bologna, did one term in Italy, and then came back to Rutgers. There was a new area-wide chairman, and one year he assigned Jan Kregel to teach accounting. (Because the economics department didn't have a separate business school, we had accounting courses, finance courses, and marketing courses.) We had people who taught it, but instead of giving Kregel economics courses, he assigned him to teach accounting courses. And so when Kregel came back the second time, he was assigned to teach two accounting courses. Of course he didn't have any particular expertise in this. So he objected, and the guy said, "Well, somebody's got to teach 'em, and that's you." Jan ended up resigning, and that began the exodus.

So the situation was that young Post Keynesians didn't have a chance for tenure, and they had to leave. Established Post Keynesians, like Kregel, found it so unpleasant that they left. They started giving me what I call checkerboard teaching assignment: Monday at nine, Friday at three. So instead of concentrating courses so I could do research, I was teaching all over the book, eight o'clock in the morning, five o'clock in the afternoon kind of thing. It was done just to make things unpleasant. So that gave me the idea to look for another job as well.

I was the highest-paid professor at Rutgers, and in order to offset my influence, there was a new professor line opened up, what they called a "World Class Scholar" position. The pay was in the mid-$70,000s, which was excellent at that time. They recruited a very Solow-growth-model specialist, from Brown. I talked to him and I said, "Look. I have to vote on you. What do you think about the idea that you and I teach the basic graduate course together, because currently I'm not allowed to teach the basic graduate course?" And he said, "You're not allowed to teach it and I don't think you ought to be allowed to teach it." I was going to have a problem. I went to the chairman and I said, "Look. "For 74 grand we can do better than Sayda." They didn't want to give up on Sayda. So there was a big fight. I lost.

One other thing happened. One of the young people--I had hired as an assistant professor in the 1970s-- was fired because of the budget crisis. He looked for another job and Penn hired him. After three years at Penn, they were not going to renew his contract. Sidney called me up: "You know this guy; he's a nice kid. Why don't you take him back at Rutgers?" So I re-hired him. He came back. He was accused of sexually harassing his teaching assistant. I didn't know about it. Nobody knew about it. And then the graduate students came to us and said he had somehow sexually harassed this graduate student and had threatened her with the loss of her job if she told what had happened.

She complained to the chairman. The chairman had hushed it up and had gotten an agreement that he was not to teach any course that she was taking. That was the only punishment. And the students were incensed. So they came to me and Arthur ______. Arthur and I then went to the administration and complained that this was not sufficient punishment. That created additional animosity because it was looked upon as Post Keynesians picking on a neoclassical economist and trying to get rid of him by using sexual harassment as the excuse. And I think even that some of my colleagues thought that the graduate students who had complained were somehow in cahoots with us in making up this story.

In the end the administration decided that everybody, including the Post Keynesians, had to take sensitivity training.

We all went to sensitivity training and listen to all sorts of stories about sexual harassment. The only one who goofed off [was the guy who was accused of sexual harassment. He later became chairman of the department.

Actually, I wasn't seriously looking for another job, but it just so happened that at that time, somebody from Tennessee, sent me a letter. They had these special Positions of Excellence. Tennessee made me what I call a Godfather offer. It was such a great offer I couldn't refuse. And since I wasn't so anxious to go, I could negotiate not only for high salary but for lots of other things. Arthur was the only one left. I told Arthur he ought to leave, but he stayed on. After I left, or as I was leaving, many of the graduate students came to me and said, "Can't you stay?" but after they'd seen what was happening they were afraid they would have problems in their other courses. So it was not only that the professors were being intimidated but that a whole bunch of the graduate students were being intimidated.

Colander: What were your hopes for Tennessee?

Davidson: The people who recruited me were basically Clarence Ayres-type institutionalists and had a very strong non-orthodox flavor. I had gotten a macro Chair of Excellence. There was also a micro Chair of Excellence that was open, and there was evidence that I could help recruit someone in the micro position. So I saw this as a possible place to create a new center for Post Keynesian economics.

Two things happened. One was they had already made the offer of the micro chair to Kerry Smith, who was a former student of mine at Rutgers and was then at Vanderbilt, and he had turned them down. I thought I could persuade Kerry to come. It turns out that he was interested, but he had just moved to North Carolina and didn't want to make another move so quickly. So then they had to recruit somebody else, who used the job to upgrade his job search. He waited a year and got a better job. At that point, the funding for the line disappeared. In the interim, a lot of the institutionalist-sympathetic economists were retiring or leaving.

Colander: But you guys were in control. Why didn't you replace yourselves? It was an institutionalist haven?

Davidson: Well, that's a good question, why didn't they replace themselves. Part of the answer is that they thought they had to play honestly. For example, there was a micro line at an associate professor level. And I recommended somebody, I forget who it was, a very good mathematical economist with a Post Keynesian orientation. The neoclassical people had somebody else. The neoclassical people admitted that the Post Keynesian, the guy that I brought in, was much better than this neo-classical guy, who gave a terrible seminar, so even they wouldn't vote for the neoclassical. But they also wouldn't vote for my person. Then there were financial problems, many of those lines just disappeared. The number of replacements since I've been there were—I've been there 10 years—were what? three? Three young people in 10 years, two of them last year. So that explains why nobody got replaced. There was just complete shrinkage.

Colander: Where does that leave Post Keynesianism in the 1990s?

Davidson: Well, not in Tennessee—that's for sure. Not at Rutgers—that's for sure. There's a little bit at Denver; there is a possibility of something at the New School; and one or two other places. But I don't see any institutional base for Post Keynesianism. One of my fears is that Post Keynesian economics will die out because there is no place for it in terms of some institution where we can train graduate students. At Tennessee, for instance, the first few years I was there, I had a number of graduate students who did theses. My last one graduated a year and a half ago, and I have one student now.

Colander: Have you placed your students?

Davidson: I've always been able to place everyone, so far.

Colander: Any regrets?

Davidson: Oh, lots of regrets.

Colander: Let's hear them. What would you have done differently?

Davidson: I would have taken Galbraith's advice and not resigned. That was clearly a mistake, because I did have central administration support. That was the biggest mistake that I made. I'm not sure about other regrets, but that was clearly a mistake.

Colander: Would you say one lesson is that you've got to play the institutional games in order to arrive at your goals?

Davidson: I've been friendly with people like Paul Samuelson, Bob Solow, Jim Tobin, Franco Modigliani, Bob Clower—many of the big names in the economic profession who are accepted by the mainstream as well as the heretics. From time to time people have suggested to me that I did not use this old boy network properly. If I had been a different kind of person, had a different kind of personality, I might have been able to promote Post Keynesian economics through these people, or use these people to promote it.

Colander: Let me rephrase my question. As opposed to seeking compromise and integrating your view, you moved toward distinguishing your views. Any regrets along those lines?

Davidson: I think that's the correct way of saying it. I was brash. I was too willing to emphasize differences rather than similarities. No question but that I might have caught more friends with sugar than I did with the vinegar. So the question is: Would having emphasized similarities compromised my own views too much? Back in 1970-71 I had this long debate with Bob Clower along these lines. Both Bob and I agreed that mainstream economics was wrong—that Keynesian economics was wrong and from a policy standpoint you have to get it from A to B. The question was, How do you get it from A to B? My argument was: Well, you show them that the theory is wrong; to that you've got to show them what the right theory is and you make it to B. And his answer was: No, what you do is you take their theory and you make a marginal adjustment to their theory. You know where you want to be, so you know which equation to change. By changing their equations, and phrasing the argument in their terms, you make it so they won't object too much and you can get them to move in that direction. The constrained demand hat Bob used is an example.

My answer was No, that's not the way to do it, because ultimately they'll make another mistake. If you just use their theory with just one marginal change, they'll find another marginal change. That will throw them off again.

Let me give you an example. I was walking to a classroom with Nicky Kaldor in 1971, and Cambridge had just won the capital controversies. And he said to me, "How long do you think it will be before people in the United States stop teaching the marginal productivity theory?" And my response to him was, "Nicky, not in your lifetime and not in my lifetime, because there are a million other ways of justifying the marginal productivity theory."

Colander: From Chris Bliss, I did not learn the Cambridge line. I heard they were debating the wrong issue.

Davidson: Well, what did Cambridge win? Cambridge won the argument that if you had a simple monotonically decreasing function, you couldn't create a measure of capital.

Colander: No one ever debated that!

Davidson: That's right. All you had to do was introduce a more complicated constant and you could resurrect marginal productivity theory. And that's what I meant when I said to Nicky that he was never going to win.

Colander: You're making it a strong statement—about winning or losing. Debates are never won in terms of the set of debates; they're won in terms of what people move to—after they've argued and are totally exhausted and fed up with the topic. At that point you see who is willing to move a little bit this way or the other way. Even though you lose all the debates, you can still win the war.

Davidson: Well, I don't know. Let me give you two examples. Let me take the question of ergodicity. Sidney asked me in the early 1970s to look at rational expectations and why it was wrong. I had other things I was doing. For years I kept putting it off, and finally I decided I would take a look at it. And I had to trace that. Since I had had biometrics training, I thought of writing down the statistical theory behind it. I knew quickly enough when I started looking at the statistical economic literature that there was nothing there that could help me. And I knew quickly enough to go to engineering or biometrics because I immediately suspected what the problem was although I didn't know everything about it myself.

Sure enough, in the engineering library I found the term ergodicity; I wrote this thing about uncertainty being a non-ergodic system. I sent it to John Hicks. Hicks and I since the mid-1970s had had this discussion about uncertainty. He was really mixed up about Knight' s visions—the debate about uncertainty was not new with Keynes —as I say, Knight had it. The question was whether you could use probabilities or what it meant not to have probabilities. I got this lovely letter back from John Hicks saying, "You're right. I should have known better but I don't have access to the literature at the moment." So he did not have the access to the library that I had, so he said, "What you did was you found a word—ergodic" He said, "You don't beat somebody with nobody, but if you find the word, that will beat them. And that's the word."

The only person at that time that I found—there were only two times that I found the word ergodic. One was Malinvaud in his first edition of *Econometrics;* he dropped it in the second edition. The other one was Herman Rowan [?], who had written a book called *Analysis of Economic Time Series*. And that was the clue.

Ever since then—and I've written about it in *The Journal of Economic Perspectives* and elsewhere—suddenly Stock and Watson and all these people who are specialists in rational expectations suddenly point out that the system has to be ergodic as well as stationary. And so, if I had somehow compromised and said, "Well, it's like Knight's concept of complexity—and so forth," we would still be confused about what do you mean by uncertainty. There would not have been a nice precise specification of the problem. It seems to me you can't compromise when you come to something important. So that's one example.

Colander: I've followed the debate on the Post Keynesian network on ergodicity. There seems to be some debate about its meaning.

Davidson: A lot of people don't understand it. I agree with you. But Solow understands it. Solow, who, by the way, refused to be a referee on an earlier paper of mine, was a discussant when I first read that paper before it was published. I got very annoyed with him. He said he wasn't going to discuss this because it was too complicated. At the cocktail party later I asked him why he refused to discuss it. But a year later, in *The American Economic Review*, in 1984 or 1985, he said, "The problem with investment advice is that they assume stationary time series." Now 'stationary' is a necessary but not a sufficient condition for ergodicity. So here he was, one year later—when I had read the paper, he had refused to discuss it; but one year later he pointed out that the problem with orthodox economists was that they were assuming stationarity.

Colander: I remember in Arjo's book Solow made the comment that essentially he'd never really followed you and understood your views. Could you have led him along to address your views?

Davidson: I *did* lead him. After he discussed this later, he dismissed it with the following quip, which I think is a good quip—I got to use it some time later—he said something about "the problem with Davidson's paper is when I read it I felt like an American sailor who just came out of a Turkish harem and when asked what he did in the harem, he said, 'There were so many things to do, I didn't know what to do first, so I just left.'" Solow said, "That was the trouble with Davidson's paper. There were so many things to clear up I didn't know what to do so I'm just going to leave the paper."

So I went to him and I said, "Look, Bob," at the cocktail party afterward, "tell me what you think is wrong with the idea that economic series are non-ergodic. He said well, he thinks that's too strong a statement. You don't really believe that behavior patterns carry over.