AN INTERVIEW WITH PAUL DAVIDSON

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Paul Davidson, Holly Chair of Excellence at the University of Tennessee, has been a leader in heterodox economics for over 40 years. I first met Paul at the initial Post Keynesian seminars at Rutgers and Columbia back in the 1970s. I have followed his work, with admiration, since that time. His contributions to economics are multitudinous—almost 20 books and hundreds of articles. (A bibliography is presented at the end of this interview.) His focus on true uncertainty (we live in a non-ergodic world) as the appropriate framework for Keynes and all economic thinking, has provided many of us with a sense that there was much more to Keynes than could be found in the textbooks. His insistence that Keynes must be seen as a Marshallian is now generally accepted, and his proposals for international monetary reform are playing significant roles in current policy debates.

Paul's knowledge of Keynes' work is legendary; in discussions he will often cite chapter and page number where arguments are to be found. His work on aggregate supply and demand, now mostly forgotten by the profession, is still far closer to correct than that presented in the current texts.

But going through his writings alone is insufficient to convey a full sense of his contribution to heterodox economics. Through discussion and organizing activities he has encouraged an entire generation of young economists to question the models they are being taught, and to approach economics with a critical eye. He is a regular on the PK Net, and he spends an enormous amount of time patiently, and sometimes not so patiently, explaining the logic of his position to newcomers.

His wife, Louise, has been a constant companion and co-organizer throughout his career. In her role as managing editor, and in his role as editor, of the Journal of Post Keynesian Economics, they have guided the Post Keynesian movement, and kept it in the public eye.

The interview was conducted with Paul and Louise at the University of Tennessee in his office in 1997. I specifically asked that Louise be at the interview because of the central role she has played in guiding Paul, and thereby in guiding the Post Keynesian movement.

REFERENCES


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

Davidson: I had a fairly normal childhood. My parents were very strict Democrats. My father didn't like union workers because unions created a lot of problems for him as a small business entrepreneur in the construction industry. 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 biometrician I knew much more about the use (and abuse) of statistics than most econometricians did in those days.

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: (To Louise) 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—$2,500. 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 1930. 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 1945 he went over to England for a year and he wrote a book on income distribution. (He published some of it in the /Ed 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 those 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 Byer, 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 "Te = 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 Un-American Activities Committee and I suspect were card-carrying Communists.
I took Sidney for both microeconomics 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 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 150 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 on 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 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?
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 macro.

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 ARE and it got terrible reviews. (The editor of the ARE had switched to John Gurley.) So I showed it to Harrod. He took a 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 was that it had aggregate supply in it. 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 Lorie Tarshis?

Davidson: We didn't really think of Lorie as being part of the group but he clearly did have an aggregate supply focus. But basically we were the only ones. Kenneth Kurzhal, 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" come up. It had nothing to do with Sidney or anything like that.
between that analysis and mine, but I can’t believe that he didn’t. I would have thought that the editor of Econometrics would have sent him a copy of my paper by late 1966 (when the paper was accepted for publication), and hence Tobin must have been aware of my spot vs. forward price analysis before he published his q-ratio analysis. Yet there was no indication—nor had there been any indication till this day—that Tobin was aware of my spot vs. forward price analysis in Econometrics. And the absence of any recognition by Tobin told me that I had to sit down and write a book. I couldn’t just write these little nice journal article pieces. I had to put the whole thing together. 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 Chairman of the Economics Department that 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 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 at Rutgers, and he wanted to write on the rewitching 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 agreed 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 67 who agreed. Some of them wouldn’t let us use their name but sent in money. A few of them didn’t want to give money or to have their name used. One of the reasons why 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 felt—at this stage of the game I guess—that 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 the Journal of 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 himself “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 Gold oversea. He was a very conservative Chicago type. The second time, when I was recruited back to Rutgers, which was 1966, the chairman was Maurice Blochowitz, 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 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 fairly 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 undergraduate colleges at Rutgers, 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 six undergraduate chairmen also reported to this area-wide chairman of the discipline. The area-wide chairman then also reported to the provost.

When I was area-wide chairman, the provost came to me and asked how to differentiate the Rutgers economics department. The *Journal of Higher Education* had recently had an article on how the economics department of 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 six undergraduate departments would be the base for all types of heterodox economics—not only Post Keynesians but Marxists, radicals, and institutionalists—as its focus, and the other five departments would be much more orthodox. And since students were allowed to register for courses in any of the colleges, any
student who wanted to could still get an orthodox education—or exposure to heterodox ideas.

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 or four Post Keynesian faculty members at Rutgers in economics out of 81 faculty members as a total in the six undergraduate colleges at Rutgers in New Brunswick.

Colander: Who were they?

Davidson: Jan Kregel, Al Eichner, and myself. Nina Shapiro also 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 Steinberg, who is now the chief economist at Merrill Lynch and who, had come from Michigan; and one or two others who had come from some program with exposure to heterodox notions. These few people—never representing more than 6 per cent of the economics faculty—represented non-mainstream or heterodox economics on the New Brunswick campus of Rutgers University. But only three of us were clearly real Post Keynesians.

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

Louise: Well, in the beginning, 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, $60, 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?". 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 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 1977. 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, had 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 Harris 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 were free to make our own decisions 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 get 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 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 my 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 suspension is that she been only a secretary, and you picked on her, she would have washed 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 1963 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 in the American Economic Review 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 func-
tion. Had Keynesian economics followed Sidney then they would have had aggregate supply way back in the 1930s and they would not have needed to wait until the 1980s. And it was somebody at MIT who had written this attack on a Keynesian analysis that included aggregate supply."

Paul Samuelson immediately jumped up and said, "Don’t blame me! It’s him," pointing to Solow. Bob Solow 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 as I did.

Colander: Let’s switch topics back to Ruggins and its connection to Post Keynesian economics. The Journal progressed and did fairly well in the late 1970s and early 1980s. Ruggins 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 vicious attack by the orthodox members of the department to the fact that they didn’t want Ruggins 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 Keynesian or at least heterodox. They believed that it was better to be a third-rate imitation of MIT, then to obtain stature (they believed notoriety) as a center for Post Keynesian analysis.

I did a study and discovered that those five heterodox members of the Rutgers faculty had published more in five years than all the 76 other economics department faculty members together. After I left as chair an institutional witch hunt was instituted, a sort of McCarthy hunt, by my successor in an attempt to weed out the Post Keynesians. These heterodox economists 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. He had published in the "right" journal, Nina had not.

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 my course. The argument was that I would not teach them orthodox economics, which was true, but the first year of the graduate program was about: if students 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.

At Eichner was the focal point that finally caused all this to erupt. As I said before, the provost and the Dean of Livingston College wanted it to be known as a Post Keynesian center. At the time of the job search for the position Eichner ultimately occupied, Jan Kregel was chairman of Livingston College. (I happened to be on leave at the time so I wasn’t even involved in developing the criteria for that position.) 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 mainstream faculty members became very incensed about this procedure. 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 73 other people who had a vote, about 52 of them voted against. Nevertheless, the Provost overrode the faculty and made the appointment of Eichner. The Dean of Livingston College strongly supported Eichner’s appointment. I was not there so I did not recommend it or dis-recommend it. I wasn’t even 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 really wasn’t overly enthusiastic. On the other hand, I thought Eichner was reasonably good and would have supported him. I hadn’t even spo-
I didn’t know about the harassment charge or the alleged solution. I believe most economics department faculty members did not know about it. But graduate student friends of the teaching assistant knew about it and they were incensed by the University’s solution to the sexual harassment charge. And so the graduate students came to me and to Eichner and told us that Professor Y had somehow sexually harassed this graduate student and had threatened her with the loss of her job if she told what had happened. Al Eichner and I then went to the administration and complained that this was not sufficient punishment. That created additional animosity within the department 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. All the faculty went to sensitivity training sessions and listen to all sorts of stories about sexual harassment. The only one who goofed off and often did not show up at these sessions was the professor who was accused of sexual harassment.

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 Chairs 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. When I left, Al Eichner was the only remaining heterodox economist at Rutgers. I told Al that 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, many who came to Rutgers because they could get some exposure to heterodox ideas, were being intimidated as well.
University and then turned Tennessee down. At that point, the funding for this chair of excellence had disappeared. In the interim, a lot of the institutional-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 and give the other side the right to express their own views. The result was we could never get a close to unanimous vote to present to the Dean.

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 to give a seminar, was much better than this neo-classical guy, who gave a terrible seminar. So even the neoclassical people wouldn't vote for the neoclassical job candidate. But they also wouldn't vote for the better Post Keynesian microtheorist. Accordingly, no one was hired and the line was used to hire TAs.

Then the University began to have financial problems as a recession occurred and the state cut back funding (in nominal dollars) to the University. Many of the lines of people just disappeared as the Dean recaptured these lines in order to reduce the College's expenditures. While approximately 10 members retired or left since I have been at Tennessee, the number of replacements were what? Three? Three young people in 10 years, two of them in 1986. 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.
wipe the slate clean and develop the "proper" theory. That was Keynes's revolutionary message.

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—rewriting debate with MIT Cambridge Mass. And Kaldor 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 as long as the basic neoclassical theory remains as the microfoundations of macroeconomic 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 the neoclassical economists had to do was introduce a more "well-behaved" production function that ruled out reworking by assumption and then they 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 afterward they're 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 some examples. Let me take the question of ergodicity. Sidney asked me in the early 1970s to look at rational expectations and explain 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. 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. Then I found two items. The first was a single sentence reference in Malinvaud's first edition of Econometrics; but he dropped it in the second edition. The other one was from Herman Wold, who had written a book called Analysis of Economic Time Series. And that book gave me the clue as to what I had to investigate. And I knew quickly enough to go to an engineering and/or a biometrics library because I immediately suspected what the problem was although I didn't know everything about it myself.

Sure enough, in the engineering library of Princeton University (I lived in Princeton, New Jersey at the time) I found the answer in terms of the theory of ergodic processes. I then wrote this article "Rational Expectations: A Fallacious Foundation for Studying Crucial Decision-Making Processes" (published in 1982) where I related uncertainty with a non-ergodic system. I sent the paper to John Hicks, who I had become professionally friendly with since meeting on the microfoundations of macroeconomics in 1974. Since then Hicks and I had continual discussions whenever I visited England, either in London, Oxford, or Perch House (Hicks's home). Many of these discussions involved uncertainty and economics. Hicks was really mixed up about Knight's vision—the debate about uncertainty was not new with Keynes. As I say, Knight had used the term as an integral part of his theory of profits. The question was whether you could use probabilities or what it meant not to have probabilities to use to forecast the future.

After I sent him my rational expectations paper, I got this lovely letter (dated 12 February 1983) back from John Hicks saying, "I just have been reading your RE [rational expectations] paper, ... I do like it very much. I have never been through the RE literature; you know that I don’t have proper access to journals; but I had had just enough to be put off by the smell of it. You have now rationalized my suspicions, and have shown me I missed a chance, of labeling my own point of view as non-ergodic. One needs a name like that to ram a point home".

Ever since 1982 I've been writing about uncertainty and nonergodicity—even in The Journal of Economic Perspectives (1991). Smart orthodox economists such as Stock and Watson and others who are specialists in rational expectations suddenly began pointing out that the system has to be ergodic as well as stationary. And so, if I had somehow compromised and said, "Well, Keynes's concept was 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 in terms of ergodic vs. nonergodic processes. 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, by the way, after reading an earlier Brookings (1974) paper of mine on the energy crisis where I attacked mainstream analysis of all prices and the Hotelling theory, refused to be a discussant on that paper because it was unfair to mainstream analysis. Solow was a discussant of an early (1983) paper of mine on ergodicity and the failure of American Keynesianism. Solow's discussion started with the statement that he wasn't going to discuss this because it would require too much time to straighten it out. He then went on to give his version of Keynesianism. At the cocktail party later I asked him why he refused to discuss the ergodic axiom and its role in mainstream economics. He didn't give a very coherent reply. But a little over year later, in the May 1985 American Economic Review he wrote "Much of what we observe cannot be treated as the realization of
a stationary stochastic process without straining credulity. There is enough for us to do without pretending to a degree of completeness and precision which we cannot deliver. My impression is that the best and the brightest in the profession proceed as if economics is the physics of society.

Now "stationary" is a necessary but not a sufficient condition for ergodicity, while non-stationarity is a sufficient condition for non-ergodicity. So here was Solow, one year later, arguing that non-ergodicity was an essential element in economic analysis—something that he refused to discuss a year earlier. But one year later he pointed out that the problem with orthodox economists was that they were assuming stationarity.

Colander: I remember in Arje's [Klamer] 1983, p. 1372 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 try to lead him. He had started his discussion of my paper with the following quip, which I think is a very clever quip—I got to use it some time later myself. Solow 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 at the cocktail party afterwards I went to him and I said, "Look Bob, tell me what you think is wrong with the idea that economic time series are non-ergodic. He said well, he thinks that's too strong a statement. You don't really believe that behavior patterns do not carry over time." Yet by 1985 Solow is chiding economists for assuming economic time series are stationary!

Colander: What's the future of Post Keynesian economics?

Davidson: Well, it depends whether you ask me on Monday, Wednesday, and Friday or Tuesday, Thursday and Saturday.


Davidson: My pessimistic assessment is that it has no future; that the control of the economics profession has become so tight that, unless a cataclysmic economic crisis occurs in the real world, nobody would turn to an alternative. If there's a great depression, then clearly there's a possibility of an alternative. But, as I see it, the profession has become more and more controlled by a group, I hate to say. I don't think there's a conspiracy. It's not because they conspired to control it, but because they believe they have vision and everybody else is, sort of, wrong. I give as a good example the last issue of the Royal Economic Society's newsletter. I don't know if you saw it or not. New editors have taken over the Economic Journal. They argued that the Economic Journal has not had the stature of the American Economic Review, and some of the other journals, which feature more technical articles and ignore controversial ones. They suggest that the lesser stature of the RJE is due to its publishing less technical articles than the AER. What the new editors are going to do is to restrict some issues of the Economic Journal each year for strictly technical articles. In other issues they will still permit controversy. This says that basically the same journal is going to have to have A and Series B issues, where Series A is going to be solely technical articles—and they hope that these technical oriented issues will be more professionally praised. If all journals go that way, and I think they will, then that is the end of Post Keynesianism. All heterodox sorts are always beating up on each other. That's true today and back there in the 19th century as well. Heterodox economists were never considered more than a bunch of cranks, tinkering at the edges.

Colander: Well, that's your Monday view. What's your Wednesday view?

Davidson: My Wednesday view is there's still hope. Every once in a while some person from the establishment sends a signal to me that indicates that they really do think there's something in Post Keynesian economics and asks can we somehow amalgamate the two, integrate the two. Hopefully there will over the years be some ultimate movement in that direction. I don't think Post Keynesians will ever take over the economics profession as the Neoclassicals have. But I hope there's some way where it becomes an influence, turning the Neoclassical system into a much more compatible Keynesian system.

Colander: Now what would that Neoclassical system be?

Davidson: Well, the argument which I've made is basically the question of what is the axiomatic foundation of the logic. I should say, for example, when I started this ergotic-nonergotic axiomatic distinction at Cambridge, which interestingly enough has a long history even before I started it, nobody in the establishment worried about this. Since I've raised that issue I've noticed a lot more people have now raised that issue, and so, as Keynes' General Theory, they will see the orthodox neoclassical system as a special case of a more general theory. And the question is which axioms are applicable to the particular system that they're looking at at the moment. Then Post Keynesianism has a niche.

Colander: Any different Friday view?

Davidson: No, on Friday I rest.

Colander: Any final words of wisdom for young researchers?

Davidson: Well, I would hope that they wouldn't follow the invisible hand completely, and that they would follow their intellectual minds, basically. I suspect that that will lead them into things that are not necessarily orthodox. I would argue that's the way to go.
I was lucky, my initial publications were really still pretty much orthodox and I managed to make a number of AEJ's and JEF's and journals of that sort, so that even though I'm Post Keynesian, I'm sort of semi-establishment in some sense. People know about me. A young person coming out has got a very difficult problem, it seems to me, and if they follow the invisible hand they may be successful in the sense of income and establishment, but not successful in terms of intellectual development.

Colander: Thank you very much.

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