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BOOK REVIEW

INSIDE THE ECONOMIST'S MIND

Paul A. Samuelson and William A. Barnett, eds.
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This book collects a set of 16 interviews with many of the major figures who shaped economic thought and policy during the second half of the 20th century. Those interviewed are all major figures—half are Nobel laureates—and the interviewers scarcely less so. The interviews were conducted between 1997 and 2005, and originally appeared as features in the journal *Macroeconomic Dynamics.*

William Barnett, editor of the journal and coeditor with Paul Samuelson of the book, explained that the goal of the interviews was to provide these economists a forum for communicating their ideas directly, without the constraints of peer review or the formalism of other academic venues. Such an enterprise gives us an opportunity to learn answers to some of the questions we're all curious about but never see addressed in the published research of the scholars themselves, such as, how did Milton Friedman account for the breakdown of the money-income correlation ("the result of eliminating the prohibition on paying interest on demand deposits" [p. 115]), and how did he explain the nonneutrality of money:

> I think the most important thing is the tendency for expectations to be backward looking and to be adjusted slowly so that it takes time before any expectation is altered by the impact of an event. (p. 137)

Or, does Robert Lucas believe that price stickiness is an important phenomenon ("yes" [p. 63]), and how important does he consider technology shocks to be in accounting for business cycles:

> The answer must depend on which cycles we are talking about. If we are discussing the U.S. Depression in the 1930s or the depression in Indonesia today or Mexico five years ago, I would say that technology shocks are a minor part of the picture. On the other hand, if we are talking about the fluctuations in the postwar United States the

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> relative importance of technology and other real shocks is *much* larger, something like 80% of the story. (p. 63)

And, just what does Thomas Sargent think about calibration:

> Calibration is less optimistic about what your theory can accomplish because you'd only use it if you didn't fully trust your entire model, meaning that you think your model is partly misspecified or incompletely specified, or if you trusted someone else's model and data set more than your own. (p. 313)

The interviews also offer a fascinating glimpse into some of the personal details of these scholars' lives that makes these intellectual giants more human, as we learn about how Lucas quit smoking (p. 65), Robert Shiller's reasons for keeping a diary (p. 242), Robert Aumann's religious beliefs (p. 347), and the secret code Franco Modigliani relied on for communication during his dangerous return to Italy to defend his dissertation (p. 87).

If one were to take these interviews as a guide for how to follow in these luminaries' footsteps, the book presents almost as many paths as it does interviews. Paul Volcker seemed to view his career as the result of a series of fortuitous circumstances:

> I wrote my thesis on the Federal Reserve, again sort of by accident. . . . I had to write a senior thesis, and in my procrastinating way. . . . I sat around, floundering around, not knowing quite what to write about, and I didn't do anything for that whole semester. I don't know where I got this idea of the Federal Reserve, but it seemed simpler and more straightforward to some degree than other things and, as I say, I was intrigued by money and banking. (p. 166)

Paul Samuelson, by contrast, seems from the start to have been destined for greatness:

> Our subject had myriads of challenging open problems—problems that mathematical techniques could throw light on, and also close out. I once described this as being like fishing in a virgin Canadian lake. You threw in your hook and out came theorem after theorem. (p. 154)

It is interesting that several of the great scholars interviewed here credit in part a mundane willingness to develop new datasets. Robert Shiller attributed his findings on stock markets to his development of a time series on prices and dividends back to 1871 (p. 238), and on real estate to his development of appropriate price indexes (p. 246). Milton Friedman described the way data helped shape his development of the permanent income hypothesis:

> I started out with a hypothesis that is similar to that which underlies the distinction between real and nominal interest rates. How do people adjust their expectations? How do they decide what fraction of their income to spend? I developed the hypothesis along these lines. I put it in a form in which it could be tested and I derived its implications. . . . As you know, the original pressure for the analysis was the apparent inconsistency between two bodies of data: long time-series data and cross-sectional budget data on consumption and income. . . . [t]he permanent income hypothesis seemed to me a much more elegant way to rationalize that difference. And it had, as

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> special cases, almost all of the alternative hypotheses, so it was a consolidation of a lot of empirical evidence as well as theoretical analysis. (p. 127)

Friedman summarized his general approach to data this way:

> I believe that you have a more secure basis if, instead of relying on extremely sophisticated analysis of a small body of data, you rely on cruder analysis of a much broader and wider body of data, which will include widely different circumstances. The natural experiments that come up over a wide range provide a source of evidence that is stronger and more reliable than any single very limited body of data. (p. 133)

The most common path whereby these scholars made their contributions, however, seems to be in taking the themes and ideas that were being kicked around at the time and be willing to carry them forward in almost a playful, curious way. I particularly liked Lucas's account of how he came to develop his remarkably elegant theory of asset pricing:

> I was interviewing Pentti Kouri, then a job-seeking new MIT Ph.D., in my office in Chicago. Kouri didn't want to waste our half hour talking about Chicago winters, so he asked me: "How would you price assets in the following economy?" and then went on to describe the model that is treated in my paper. I went to the blackboard and began writing Bellman equations and clearing markets, and the fact that you didn't need to know the value function to get a very tractable functional equation for prices fell right out in a few minutes. Kouri was not interested in collaborating, so I wrote up these results and others myself. (p. 65)

Perhaps the book's greatest value, as noted in an introduction to the volume by E. Roy Weintraub, is in chronicling for posterity some of the details of how the economic history of the 20th century came to be what it is. Samuelson debunked claims of an oral monetary tradition at Chicago (p. 150). Milton Friedman described the importance of political factors in creating the great inflation of the 1970s:

> From the moment Burns got into the Fed, I think politics played a great role in what happened. So far as Nixon was concerned, there is no doubt, as I know from personal experience. I had a session with Nixon sometime in 1970—I think it was 1970, might have been 1971—in which he wanted me to urge Arthur to increase the money supply more rapidly [*laughter*] and I said to the President, "Do you really want to do that? The only effect of that will be to leave you with a larger inflation if you do get reelected." And he said, "Well, we'll worry about that after we get reelected." (p. 116)

For me, the most fascinating of all was Paul Volcker's description of how he came to be a "practical monetarist":

> It always seemed to me that there is a kind of commonsense view that inflation is too much money chasing too few goods. You could oversimplify it and say that inflation is just a monetary phenomenon. There are decades, hundreds of years, of economic thinking relating the money supply to inflation, and people to some extent have that in their bones. So I think we could explain what we had to do to stop inflation better that way than simply by saying that we've got to raise interest rates. It was also true that we had no other good benchmark for how much to raise interest rates in the midst of a volatile inflationary situation.

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> At least as important was the idea to discipline ourselves. People in the Federal Reserve don't like to raise interest rates. So the danger is you're always too little too late. I think that would apply to the current situation [April 2000]. So, when inflation really had the upper hand, it was, I think, very important to put something out there so you could discipline yourself. For that kind of a commitment, you've got to know what's at stake, and it does make some broad sense if you have that much inflation. (pp. 178–179)

The practical realities forced Volcker's hand in the fall of 1979:

> What really propelled me to make the change was when we raised the discount rate for the second time, when I was first down there. The vote was 4–3. I thought it was a reasonably strong move and we'd get a favorable reaction in the market, but we didn't. The response was, "Well, gee, the Federal Reserve is behind the curve anyway, the vote was 4–3, and that's the last increase of the discount rate we'll see." So the market reacted very badly, which surprised me. I guess I was a little naïve. I remember very clearly, I didn't bend over backwards to try to twist the arms of the three people who voted the other way. I knew I had four votes. If we had to increase the discount rate again, we'd have another 4–3 vote. But that's not the way the market read it. Then I realized that we had this credibility problem worse than I thought. That got me off and really thinking operationally about the other approach. But when it was sprung on them, everybody was very much in favor, even those who were voting against the increases in interest rates. (pp. 178–179)

Of course, if you can start being a "practical monetarist" that easily, you can stop even easier:

> Then in October [1982], or whenever it was, the money supply (by some measures) was increasing again rather rapidly. We had a tough explanation to make, but I thought we had come to the point that we were getting boxed in by money supply data that was, in any event, strongly distorted by regulatory changes and bank behavior. We came to the conclusion that it was not very reliable to put so much weight on the money supply any more, so we backed off that approach. (p. 183)

Although these interviews make interesting reading, the ultimate craft of these scholars is not what they say over coffee but, rather, what they put together in written scholarship, where they have had the opportunity to ponder and choose carefully which words to include (and exclude). If all you knew about these scholars was what you found in the pages of this book, you would have no idea how eloquently Lucas can express himself in writing, how elegantly Samuelson crafted mathematical models, or how thoroughly Shiller documented his claims. This book could only serve as a supplement, rather than a replacement, for understanding the methods and contributions of these scholars.

But it's very interesting material indeed, for anyone curious to learn more about how we got to where we are today.

INTERVIEWS

1. Wassily Leontief interviewed by Duncan Foley.
2. David Cass interviewed by Stephen Spear and Randall Wright.

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3. Robert E. Lucas Jr. interviewed by Bennett McCallum.
4. János Kornai interviewed by Olivier Blanchard.
5. Franco Modigliani interviewed by William Barnett and Robert Solow.
6. Milton Friedman interviewed by John Taylor.
7. Paul Samuelson interviewed by William Barnett.
8. Paul Volcker interviewed by Perry Mehrling.
9. Martin Feldstein interviewed by James Poterba.
10. Christopher Sims interviewed by Lars Hansen.
11. Robert Shiller interviewed by John Campbell.
12. Stanley Fischer interviewed by Olivier Blanchard.
13. Jacques Dréze interviewed by Pierre Dehez and Omar Licandro.
14. Thomas Sargent interviewed by George Evans and Seppo Honkapohja.
15. Robert Aumann interviewed by Sergiu Hart.
16. James Tobin and Robert Shiller interviewed by David Colander.

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