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In 1981, Lucas and Sargent published a selection of empirical contributions to new classical economics. All the papers were written by economists who are directly associated with new classical economists except John Taylor. His work is generally critical of new classical economics. For example, a well known article that he coauthored with Phelps (1977) states the importance of contracts and rejects the equilibrium assumption of new classical economics. Nevertheless, his inclusion by Lucas and Sargent certainly indicates an affinity between his work and that of new classical economists. We will want to explore this tension.

Like Blinder, Taylor was an undergraduate at Princeton. He earned his Ph.D. at Stanford in 1973 and taught at Columbia and Princeton. He served on the Council of Economic Advisers in 1976–77 and is now at Harvard University. We talked in Princeton in November 1982, while he was then professor of Economics and Public Affairs.

BACKGROUND

Why did you get interested in economics?

I majored in economics as an undergraduate and got very interested in the chance to apply scientific ideas to problems that seemed very real. It seemed to give an understanding of which factors determine economic conditions in the world. I was very fascinated by the whole thing.

I wrote a thesis at Princeton, where I was an undergraduate, on monetary and fiscal policy for stabilization purposes. It was very quantitatively oriented. I developed numerical models which allowed me to do simulations.

Was any faculty member in particular stimulating?

No, I think that it was more the faculty as a whole. Phil Howrey, who is now at the University of Michigan, helped me quite a bit. He was interested in time-series analysis at the time. Burt Malkiel was stimulating; I took a class with him on corporate finance.

Then you went to Stanford.

Yes. One of the reasons I was attracted to Stanford was their heavy emphasis on quantitative and econometric techniques. My thesis was almost as much on econometric theory as it was on things that are useful for macroeconomics. It was on ways to learn about the economy by estimating econometric models at the same time that you try to use these models for policy purposes. That is what you call a joint problem of estimation and control.

Why were you so interested in the quantitative aspects of economics? Was it because you were good at it?

No, I wouldn't say so. I was interested in the problems. It seemed to me that quantitative economics is an area that needed a lot of work and had a lot of open questions. It is on the forefront of the connection between theory and the real world. Economics is a quantitative science: it deals with quantities.

There is also a sense of excitement, because you seem to make a lot of progress in understanding substantive issues using mathematical or quantitative techniques.

Progress?

Economists have been studying economics for hundreds of years, and in some sense the basic principles of economics have not changed very much. What has really changed is the methodology. You can now address the issues more rigorously, more systematically, and in more detail. You can say what you mean and you can document what you say. People can check whether what you are saying is true much more easily. That all is very useful.

If the same principles still apply, what is then the substance of the progress? Do we, with the improvement in techniques, understand better how the world operates? What did we learn?

The underlying assumptions about economic behavior have not changed. There are criticisms of this approach, but in general economists still rely on the maximization postulates. The progress is in the application of techniques. The development of computer technology has been very helpful. There are also much more data.

It seems that Phelps had an important role in your career. For example, you wrote an article with him and acknowledge him in many of your other papers.

Yes, he was a colleague of mine for seven years at Columbia. We interacted a lot. Without question he influenced me. I guess it was his interest in macroeconomic questions. He is an imaginative thinker, and he thinks about substantive issues such as inflation and unemployment.

ABOUT ECONOMICS AND ECONOMISTS

How would you describe yourself as an economist? The conclusions you draw in many of your papers would suggest that you belong to the neo-Keynesian camp, or at least the younger generation in this camp.

I find it very difficult to use such terminology, quite frankly. I like to focus on the improvement of models, and in particular how macroeconomists model expectations. There is a big difference between modern macroeconomics and macroeconomics before the 1970s. For example, there is a much better understanding of how we can model expectations, both quantitatively and theoretically.

The term "new classical" usually refers to a type of modeling which not only includes better treatment of expectations but also a description of the way in which markets behave. It is the latter part of new classical economics that I do not think is acccurate or particularly helpful. But I have used the rational expectations part for as long as I have been doing work in macroeconomics, at least since I left graduate school. I think that the expectations approach is very helpful. It is not always right; sometimes it is not realistic at all. But it is a big improvement.

There is much more to the progress in macroeconomics than just stating that expectations are rational. When you study business cycles, you consider behavior over a long period of time. You are asking questions about how the economy would respond to different types of policy systems over 10 or 15 years. The questions concern strategy. There is more concern in macroeconomics with the details of how markets work, for example, how flexible prices are. I am spending a lot of my research

efforts trying to design improvements over the assumption that markets always clear, and that prices are perfectly flexible.

In your article with Phelps you suggest that your conclusion that stabilization policy is effective implies a victorious restoration of an old doctrine. That is neo-Keynesian, isn't it?

I think you use the word "Keynesian" in a way that is not appropriate. The paper that I wrote with Phelps attempted to show that monetary policy, even if anticipated, would have an effect on the economy. That is true for most Keynesian models. It is also true of monetarist models, and for that matter, it was true in Irving Fisher's view of the world before Keynes. Of course, you have to go back and try to interpret what early economists actually said. Because they were never quite as explicit as economists tend to be now, this is not easy. It seems to me that the idea that even anticipated changes in money affect the economy is not exclusively a Keynesian idea, though Keynes helped us understand why that is the case. Phelps and I wrote the paper in response to the Sargent-Wallace paper to restore this idea.

In which sense do you differ from people like Blinder and Tobin, who do not resist the label of neo-Keynesian?

I think that you have to read our papers carefully to get a feeling for that. I have great respect for both Blinder and Tobin. We don't see eye to eye on everything. I might put more emphasis on expectations; I also have different views about government policy. But I don't think you want to divide people up along what their view is on government policy. You also want to consider their view as to the way the economy works and the useful strategy of doing economics. I have no major disagreements on the scientific approaches of either Blinder or Tobin or, for that matter, with those of Lucas and Sargent.

How did you get into rational expectations? You were quick in picking it up, unlike many others.

I thought that it was an important modification of conventional ways of doing macroeconomics. The importance of expectations seems very clear after the work of Phelps and Milton Friedman. In the late '60s there was increasing interest in models of expectations and the effects of expectations on the economy. So it was a very important development, one that needed a lot more research and that would be useful in helping us to explain behavior.

What do you think are the major characteristics of the work of Lucas and Sargent, apart from rational expectations?

They have done a lot of things. It is really impossible to describe all the work that they have done in a short interview like this. Part of Lucas's work is on the way the Phillips curve works; it is based on informational problems. I think that that is a major contribution: it showed a way to model a puzzling feature of economic behavior very logically, very rigorously, with an emphasis on uncertainty and on the problems that individuals have because they don't have perfect information. Lucas has also done a lot of important work on investment theory and, more recently, on financial economics.

Do you know Lucas and other new classical economists?

Yes, I do. I have discussed my work with Lucas. I talk a lot with Sargent and Barro; we are working in the same field. It would be very unusual if we would not discuss our work.

You disagree with them, though. What is the disagreement about?

I do not think that you can accurately model macroeconomic behavior assuming that prices are perfectly flexible. That is an element of disagreement which is still there and which will be solved somehow, maybe empirically.

Do new classical economists really assume perfectly flexible prices? I thought they allow for the role of contracts.

If you take their models literally, they assume that prices are perfectly flexible. My understanding is that they feel that if contracts are designed optimally, they have characteristics that would duplicate a world of perfectly flexible prices. Then the assumption of perfectly flexible prices seems warranted.

I don't think that contracts can be designed to mimic a world of perfectly flexible prices. In the real world we see contracts that are not designed that way, for various reasons. Because these contracts exist, I think it is impossible to model accurately economic fluctuations without taking them into account.

Don't you risk the criticism that your modeling of contracts becomes ad hoc if you do not make them consistent with the postulates of optimal behavior?

My general view is that there is a lot more optimality about existing relationships than an economist can see at first glance. So it is quite likely that many characteristics of contracts are optimal. We may at a certain point be able to see that apparently suboptimal contracts are not in fact suboptimal.

The bottom line in Lucas and Sargent's thinking is that we should explain why agents choose to do what they do in accordance with the postulate of optimizing behavior. You don't do that.

My reaction to that kind of criticism is that at some point we always will have to impose what appears to be arbitrary restrictions. Lucas and Sargent restrain the information that is available to people, but that is arbitrary. If the information barrier is so important you would expect that people find a way to get around it. I think that such a restriction is just as ad hoc as the assumption that prices adjust slowly.

Also, models that use the assumption of perfectly flexible prices and market clearing are assuming, implicitly, that there is an auctioneer. And there are no auctioneers in many markets. This is another example of an artifical imposition or restriction in that style of modeling.

I would say that as a general guiding principle for research that I would like to minimize the extent to which we rely on such arbitrariness. It seems to me that economic progress can be described as moving further and further away from such devices. I think that is a good way to proceed. It is misleading to say that one style of macroeconomic modeling has a lot more of that than others. To some extent it is a matter of taste. I certainly don't like everyone's style. But that is natural.

The difference between your style and that of new classical economists seems to be responsible for different conclusions.

That is right. The different assumptions generate different conclusions. Strictly speaking, the models with which I work show that government instruments can have stabilizing effects on the economy. Going from that to designing stabilization policy is another matter. One view is that the political system is too complex to use these instruments effectively.

Is rules versus discretionary policy an issue?

That depends on the group with which you are talking. I think that it is very important to try to emphasize macro policy as a rule, as a sys-

tem, rather than as discretionary changes in the instruments. That is a very useful way of thinking about macroeconomic policy. The more that is done, the better. It encourages a longer view.

There are, however, limits to this. You can never plan for all contingencies. There are unique events that require discretion. But it seems to me that a lot of events are recurrent, for which you can make contingency plans.

How do we account for 10.4 percent unemployment?

The attempt to reduce inflation, which was too high in the opinion of a lot of people, required a contractionary policy. According to the model with which I work, such a contraction in money growth will cause a recession. That gives us the major part of the explanations for the high unemployment. There are also some demographic factors; the population is changing. But a large fraction of the high unemployment is explained by the contractionary policies.