STANLEY FISCHER

Stanley Fischer was born in Zambia in 1943 and took his BSc (Econ) and MSc (Econ) at the LSE during 1962–6, before obtaining his PhD at MIT in 1969. He taught at the University of Chicago until 1973, when he returned to MIT and where he became Professor of Economics in 1977. He has also held visiting positions at the Hebrew University, Jerusalem and at the Hoover Institution.


We interviewed Professor Fischer in his office at MIT on 19 February 1993.

What are the important business cycle ‘stylized facts’ which a good macroeconomic theory needs to explain?

Well the first fact is that there is a business cycle -- that there are periods of booms and recessions. The second is that inflation moves up and down over the cycle and sometimes moves quite independently of it. The third is that unemployment fluctuates -- and that unemployment is much higher in some recessions than in others.

Do you feel that the main differences between macroeconomists today are over empirical or theoretical matters?

Oh I think a very large part of it is theoretical. There is widespread agreement on certain facts, like the ones we have just been through. But then there are very different ways of interpreting the facts. Some people insist as a methodological precept on doing it ‘right’ and other people insist there’s a methodological precept of giving some sort of realistic -- in terms of what they or their grandmother can understand -- description of what’s going on. So I think of the differences as more methodological/theoretical than an argument about the basic facts. Of course there are also disagreements about facts -- like does deficit-financed fiscal policy have any impact? -- but they are somewhere lower down the food chain.

What do you consider to be the key papers/books which have had a major impact on the post-war development of macroeconomics?

Well after the General Theory I would point to the contributions of the Chicago School summarized in Studies in the Quantity Theory of Money [1956]. Patinkin’s Money, Interest and Prices [1956] was very important in drawing together everything that had gone before. The Phillips [1958] article was obviously very important and everything that came out as a result of that. Friedman and Schwartz’s [1963] book is extraordinarily important -- part of the Chicago tradition. Then Lucas’s [1973] article in the AER seems to me quite crucial in continuing the Phelps/Friedman Phillips curve analysis. After that I give an important role to the Kydland and Prescott [1982] real business cycle article and the Akerlof and Yellen [1985a] paper and everything that followed from that.

Why do you think there is more consensus amongst economists over microeconomic issues compared to macroeconomic issues?

In part because micro is less important and in part because, believe it or not, I think that the empirical standards are lower in microeconomics. Let me justify what I mean. When Rudi Dornbusch and I came to write our principles book I knew macro well but hadn’t done much micro for a long time. I thought it was going to be a breeze, the macro part we knew and the micro
part is all clear – there would be a thousand empirical equations out there to illustrate demand and supply curves. Well the empirical backing isn’t around very much – there are lots of stories and models but I don’t think micro focuses on a set of issues in the same way that macro does. Microeconomists are not called upon to explain real-world phenomena to anything like the extent that macroeconomists are. They don’t have a daily confrontation with policy makers, the newspapers and the capital markets. One of the very successful micro fields is finance, partly because they are really pushed to come up with something that will stand the test of a lot of very sceptical people.

*How important do you think it is for macroeconomic models to have choice-theoretic micro foundations?*

All else equal, I’d much rather have choice-theoretic foundations in any model. Furthermore I push my students very hard to do that. However I think it is more important than being methodologically right to have a model that you believe can fly in the real world. You know the House of the Lord has many rooms, so part of the profession can be busy doing one thing while other people can be doing other things. What I don’t think is smart is to go out and use, say, an overlapping generations model to model monetary policy because that’s the only way to get money in the model rigorously.

*In the introduction to the book you wrote with Olivier Blanchard (Lectures on Macroeconomics) you suggest that on the surface macroeconomics appears to be a field divided among schools, but that this represents public relations gimmicks and the strong incentives that exist in academia to differentiate products. What made you take this view?*

Oh I think a lot of what’s going on is people differentiating themselves. The new Keynesians are essentially the mid-1970s Keynesians. As between, say, Lucas and the Keynesians there is more to it than mere product differentiation.

*Where do you see yourself in the spectrum of opinion?*

Well at the business end I am a pretty straightforward Keynesian. I am very eclectic in how I teach and how I think of the value of the activities of different people in the profession. There are some very respected people who regard the real business cycle stuff as a waste of time, saying it doesn’t lead anywhere. Its basic premises are ridiculous and so on. Actually I think that they are developing a lot of material and a methodology which is very useful. So methodologically I am very eclectic, but at the policy end I am quite Keynesian.

*Does the Hicksian IS–LM model still have a useful role to play in introducing students to the work of Keynes?*

It is useful for two reasons. One as a historical device and two it’s still the basic model that people use – in fact I use it – in understanding the economy. We teach our incoming graduate students the IS–LM model and I don’t think that there is a better model for getting the intuition of the short-run adjustment of the economy right.

*How influential have Keynesian ideas actually been in the conduct of macroeconomic policy in the United States since 1960?*

You know they are really so simple and intuitive that even people who rail against them believe them. We’ve had Keynesian-style policies repeatedly. In 1975 there was a cash rebate to households, which was one of the Keynesian policies implemented to help us get out of the 1973–5 recession. The credit policies under Volcker in 1980–81 were very Keynesian and Bush knew the tax increases he was proposing would have an impact on slowing down the economy. The notion that fiscal policy is an inelastic demand and growth is believed right across the spectrum and has always been taken into account. Deep down most people believe that deficits matter, although it is politically convenient for some politicians to pretend that they don’t.

*How important do you think the discovery of the Phillips curve was for macroeconomic policy making in the United States?*

That’s tough. If you read what people were saying before that, you realize that they understood the fact that you tend to get more inflation as you get closer to full employment. It’s one of those many cases where economists managed to get a formal expression of something that was already widely understood. When I gave the Frank Paish lecture a few years ago I went back and read Paish. He already knew when he was writing, in the late forties, that there were problems with inflation as you got close to full employment – so did Keynes in the General Theory. I think that the way the profession works is that there is a small group at the top, who have fantastic intuition and after their initial training can operate without formal models. Then you have the great bulk of the profession who have to be taught to think within a formal model. The Phillips curve was very important in formalizing price adjustment, but the intuition was around.

*Do you think unemployment is a more important problem than inflation?*

You know it’s clear that politically things have changed completely. When I was a student at the LSE in the early sixties unemployment was ‘the’ problem and the notion that you could have a government re-elected in Britain with unemployment in excess of two million would have been considered
insane. So politically it's clear that the whole thing has shifted both in Europe and in the United States. Personally I still worry a great deal about unemployment. However there are two facts. One, we have much better social welfare systems than we used to, and the lot of the unemployed is not what it was in the thirties. Two, there is a much more sophisticated understanding of the problems of inflation than there was 25 years ago.

**What do you think of Lucas's idea [1978, AER] that macroeconomists should abandon the concept of involuntary unemployment?**

Well, that's an example of somebody differentiating their product. Patinkin has a great line in his chapter on involuntary unemployment in *Money, Interest and Prices* where he quotes the Talmudic ruling that in certain cases where a person's consent is required for a ruling you may coerce him to give his consent. In that sense people are always in a position they would like to be in. However I don't find it very useful to imagine that 25 per cent unemployment is a situation in which people are unemployed voluntarily. In one sense people don't offer to work for one cent per hour so they are voluntarily unemployed but I can't see the use of a definition like that.

**Does fiscal policy still have a useful role in stabilizing the economy?**

Yes, in extreme situations when you are in a deep recession, but not very often. The 1975 income tax rebate of 50 dollars per head across the board was very useful. But the political process in the States is so messy when getting a tax bill through that it is very hard to think of it being used for short-term purposes. One thing is clear, lousy fiscal policy decisions can make the task of monetary policy much harder. Germany's current cyclical problems derive from inappropriate fiscal policy.

**Did the monetarist counter-revolution serve a useful scientific purpose?**

I think so. There was a great overplaying of monetary policy. Friedman really put the issue on the front burner and into the public domain in a way that simply couldn't be overlooked. Nobody now doubts that monetary policy matters — that is due largely to Friedman. Bob Solow had a good line at the recent Clinton Economic Summit, when he told us that, when he was in the army, one of the enlisted men said that the trouble with their lieutenant was that he knew everything and realized nothing. Well, before Friedman, the profession knew money mattered — afterwards we realized that.

**Do you think that there is a natural rate of unemployment towards which the economy inevitably converges following a demand or supply shock?**

Yes, in the very longest of long runs.

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_In your view, has empirical research settled any significant issues in the macroeconomic debate relating to the role of money in the business cycle? I think so. I thought that the Friedman and Schwartz [1963] book made a very very strong case. The hyperinflation empirical research also tells you a lot about the role of money in the extreme cases._

_Given all the criticisms of the public choice school and the contributions from the political business cycle literature, can we trust politicians, when implementing policy, not to try and maximize their own welfare? No [laughter]. What you have to hope is that you have a constitutional system in which, when that's what politicians do, something reasonable comes out at the other end. I don't think ours is terrific because we lack the British party system. We don't have a mechanism which transforms the interests of the individual Congressman — which is to keep his constituents happy by giving them goodies — with the interests of the country. There is another thing that comes out of that literature: what advice should economists give when they know their views will be used for purposes they don't intend? There's a very nice game that weather forecasters play — they always forecast snow if there is any chance of it snowing because the loss function is asymmetric. Economists shouldn't play that game — they should say what they think will happen if XYZ happens. They should say what they think ought to be done on purely technical grounds._

_Macroeconomic controversy in the 1950s and 1960s revolved mainly around issues relating to aggregate demand. What caused the shift of emphasis to the supply side in the 1970s? It was the oil shock and the failure to understand it. I remember discussions at the time where I, and my monetarist friends, didn't understand at all what was going to happen to inflation as a result of the supply shock. People said that you just keep the quantity of money constant, it's 1 per cent of GNP, $MV = PT$, prices will go up by 1 per cent. That was the level of understanding if you came either from the quantity theory or an aggregate demand model._

**What do you regard as being the most serious criticisms that have been raised in the literature against new classical macroeconomics?**

Well let me try and define what I mean by new classical macroeconomics. When it's just the methodological precept of trying to explain everything from first principles then I don't think there are any serious criticisms you could make of that — except that it's too often used as an excuse for not giving policy advice. Beyond the methodological approach, which I generally agree with, the most serious criticism arises when new classical macroeconomics crosses over into talking about the real world. When you come up with a model which has
nothing of much to do with reality – like a real business cycle model in which monetary policy has no effect on anything or everything that happens in the economy is as a result of technology shocks – it’s at that point I part company. It’s not that starting from first principles is bad, it’s just that believing that you don’t have to do a reality check every now and again is very bad.

Over the years the rational expectations hypothesis has been presented in the literature in a number of different forms and versions. What do you regard as being the essence of the hypothesis?
The hypothesis, narrowly put, is simply that people form expectations using all available information and process it optimally. That seems to me a very nice methodological assumption.

Do you think the evidence supports the Ricardian equivalence theorem?
No, not at all [laughter].

What do you consider to be the most significant impact that the new classical approach has had on macroeconomics?
If we are talking about Lucas, one massive impact was to give people an excuse to throw away 30 years of empirical work in the econometric policy evaluation critique. After Lucas’s [1976] paper it was perfectly acceptable for people to say econometric models are useless. In fact the Lucas critique just tells you what may happen, not what does happen. That’s number one. Number two is a complete change in the methodological approach for doing macroeconomics. Whereas it used to be acceptable to do partial equilibrium stuff, now almost everybody uses general equilibrium analysis with a complete model where everything is explained somewhere in some way.

What do you think has been the main contribution that the new Keynesian literature has made to the development of macroeconomics?
Firstly, I associate the efficiency wage hypothesis with that and that’s a very important notion. All the work trying to explain why labour markets don’t work like the simple demand and supply model strikes me as being very important and really getting at something. I like the Akerlof and Yellen result that small costs of price change could have big effects – but the [1987] paper by Caplin and Spulber, which shows that stickiness at the micro level doesn’t translate into stickiness at the macro level, seems to have greatly reduced the importance of that approach. At the more general level, the most important thing it’s done is to make it respectable for very smart people like Greg Mankiw and David Romer to become Keynesians and have a research programme which can ask questions about the real world raised by the Keynesian revolution.

Is the crux of the difference between Keynesians and new classicists the speed at which markets clear?
Yes, but I hesitate a little because you can always say that markets clear, subject to the fact that people are constrained by contracts they have signed. But the forces of demand and supply don’t register in the market at every moment, so you can also say that markets don’t clear – although you don’t necessarily see excess demand or supply in a non-clearing market.

What contribution has real business cycle theory made to macroeconomics? Has it taken macroeconomics down a dead-end?
I think it was going down a dead-end for a long time, but everything comes around. At the first conference I came back to after working with the World Bank for three years, Bob King, who is a neoclassical economist from Rochester, gave a paper in which he said he couldn’t explain the role of money without sticky prices. So here we have a real business cycle model with sticky prices. If they had insisted that the original Kydland and Prescott model was it and money had nothing to do with anything then it would have been a dead end – but they’ve come back to the real world [laughter].

Can you elaborate on the view you expressed in your [1988] Economic Journal article, that the views of Friedman, Brunner and Meltzer are closer to those of Keynesians than those of equilibrium business cycle theorists?
If you take Friedman’s views of rational expectations you get some sense of what I mean. Friedman really doesn’t think that expectations are rational – he really thinks people adjust their views pretty slowly. If you read Friedman’s account of why there are long and variable lags in monetary policy, it’s a completely Keynesian account. You can read Friedman’s and Tobin’s statement of the transmission mechanism and you can’t tell who wrote which. The analogy I use is that when I was a kid I could tell cars apart coming down the road. Then the differences between say a Chev and a Ford were enormous. Now if I look at an old car I know it’s a 1950s model but I don’t have any idea whether it’s a Ford or a Chev because they look practically identical. In that sense the methods and models they used and the issues they looked at were very similar.

In view of the attacks on the desirability and effectiveness of activist fiscal and monetary policy by monetarists, new classicists and public choice theorists, should we adopt a policy regime based on rules?
On a monetary rule, definitely not because the monetary system keeps changing. There really is something to Goodhart’s Law/Murphy’s Law/the Lucas critique, that when you adopt a target you change behaviour. I don’t think we have seen a monetary rule successfully applied anywhere. Simons’s
monetary rule in the 1930s was to stabilize the price level – a rule about the objective, not the instrument, of policy. That seems to me to be a good way of proceeding. Trying to keep the money supply growing at a constant rate strikes me as a bad idea and one that has never worked.

Is Keynesian economics a lot healthier now than it was in the 1970s?
I think so. We have integrated exchange rates into the system. We now have a much better understanding of the Phillips curve and the role of expectations. Although Keynesian economics has a much better base than it had in the past you sit around knowing that some other damn thing is going to come up, which you have been overlooking [laughter].

How healthy is the current state of macroeconomics, given the level of controversy?
There is a very unhealthy element in the gap between what the practitioners do and what the profession talks about. If you want to take part in the policy debate, you will find that almost everything that is being done at the frontier is quite useless. The guys who are good at policies know the basic AD–AS model (dynamized) and a lot about the real world. But that’s not what we teach. What’s missing are attempts to get us back to real-world phenomena – focusing on empirical issues. When I started the NBER Macroeconomics Annual it was to try and bring theoretical and policy issues together. Over the eight years since it started in 1986, I have noticed that a lot of the papers which are supposedly front-line theory are more about the real world. So there has been a welcome change.

Do you see any signs of an emerging consensus in macroeconomics?
No. I don’t see any convergence, judging by what I hear coming out of Minnesota or Chicago.

Do you think that to be a good macroeconomist you need to know some fairly simple models well, like the IS–LM and AD–AS models? Is that sufficient to give you a good understanding of many real-world problems?
Marty Weitzman has this line that what you learn in the first course of economics is basically all you need to know. But then you have to go on learning for about eight years to understand just what it is you’ve learned in the first course and what the qualifications are. Unless you have a decent education you are going to make a lot of mistakes because the model is too simple. It’s only when you’ve thought through the growth and expectations issues that you are sophisticated enough to say when you can use these models.