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[1] *In what places have you taught or done research?*

I started in 1982 at the University of Chicago Graduate School of Business, and I taught there until 1983. Then, I visited Carnegie-Mellon University in 1984, and, after that, I went to the Federal Reserve Bank of Minneapolis, where I worked for seven years. My main activity at Minneapolis was research. Since 1992, I've been a professor of economics at Northwestern University.

[2] *What happened to the monetary business cycle (Lucas, 1975)?*

Business cycles are the persistent and simultaneous movement up and down across a wide range of sectors, of output, employment and investment. After discussing this in his famous paper on business cycles, Lucas (1975) concludes that business fluctuations must reflect the effects of some kind of shock that hits all the sectors of the economy. He asks this question: could it be that this shock is a technology shock? And he came up with a very short answer, 'no'. He said that it does make sense to think of technology shocks, but only at the firm level. These shocks drive some firms up and others down, but are unlikely to have any appreciable impact on the economy as a whole. He conjectured that, to understand business cycles, we need to find a shock that hits all sectors, and Lucas hypothesized that it was unlikely that that shock would be a technology shock. He concluded that you have to look for an obvious

aggregate shock, and he suggested that monetary shock is a good candidate.

Now, we move forward a few years, and Kydland and Prescott(1980, 1982) came along. They displayed a model in which there's one shock moving the whole economy up and down, and it's a technology shock. As I said before, this was specifically considered and ruled out by Lucas in 1975. The irony was that Lucas applauded when Kydland and Prescott wrote their paper. No one really asked the question: what happened to Lucas's critique of technology shock driven business cycle models? Many jumped onto the new band wagon, and, for several years, the real business cycle model was very prestigious.

So, what was going on here? Two things. First, Kydland and Prescott (along with Long and Plosser) made an important substantive contribution when they drew attention to the idea that aggregate fluctuations might have something to do with things other than fiscal and monetary shocks. However, I believe that this aspect of their contribution was relatively less important, because it was not worked out very clearly. The literal notion that there is an aggregate shock to technology seems unappealing. And, Lucas had pretty effectively criticized the notion that aggregate fluctuations reflect the effects of technology shocks operating at the firm level. Kydland and Prescott did not sketch out a way around these problems (later, Boyan Jovanovic and Andre Shleifer made important contributions to this.)

Second, and here is where the really profound contribution lies. Kydland and Prescott produced a great methodological advance in macroeconomics. They (and Long and Plosser) showed that equilibrium models that previously had been the exclusive province of theorists less interested in data, could be taken to the data. And this is what I think

Lucas was applauding, the methodological advance. He was not specifically applauding the death of the monetary business cycle.

Now that the initial methodological innovation of Kydland and Prescott has matured, researchers are taking a critical look at the idea in real business cycle models, that an aggregate technology shock drives the cycle. I don't know where this will all lead. There are researchers who are working to overcome Lucas' original objections to technology-shock driven business cycles. Perhaps they will prevail. Important work here is being done by Bill Dupor, Mike Horvath, and Boyan Jovanovic, among others. There are others working on monetary business cycles. Maybe they are onto something and that will turn out to be an important source of business cycle shocks. Perhaps the source of shocks is somewhere else. Another possibility is that there are many, many different sources, some more important at some times and others more important at other times. Whatever the answer turns out to be, however, I believe the quantitative methodology of Kydland and Prescott has a central role to play in helping us to find it.

Let me turn specifically to monetary shocks. There is empirical evidence that seems to support the notion that monetary policy is important in the business cycle. First, many are convinced by Friedman and Schwartz's evidence which suggests bad monetary policy played an important role in the Great Depression. Also, with the exception of the 1990 recession, essentially every business cycle in the postwar United States is preceded by a very sharp run up in short term interest rates. Some say this reflects that post war recessions are the consequence of tightening by the monetary authorities. Along these lines, there is a colorful quote ascribed to Rudiger Dornbusch, that US business cycle

expansions do not die of old age. They are murdered by the Fed.

There is another reason people think of the business cycle properties of interest rates as reflecting the effects of monetary policy. This is the perception that it is hard to find a non-monetary explanation for these properties. For example, the standard real business cycle model cannot account for these properties. In standard real business cycle models, a high interest rate is a symptom that there has been a good realization of the technology shock, so that good times are ahead. But, in fact, in the US the high interest rates are a signal that bad times are coming, and this fact is true in Europe as well. So, there's some evidence that appears to support the notion of monetary business cycle.

Still, there are at least two reasons for skepticism about the notion that money is important in business cycles. First, money tends not to have a very big effect in economic models. Of course, this may just reflect that existing models are not very good. Second, the literature on measuring monetary policy shocks tends to find that these are a relatively unimportant source of business cycle fluctuations. Third, there is some preliminary evidence that the systematic part of monetary policy may not be very important in business cycles. For example, Christopher Sims, in a paper presented to a recent conference at the Boston Federal Reserve Bank, argued that Friedman and Schwartz may have been wrong, and that the course of the Great Depression may not have been much influenced by monetary policy. Finally, there is evidence that the business cycle properties of interest rates may not be so difficult to understand with a real business cycle model after all. Jonas Fisher and I discuss this in a recent paper (see Christiano and Fisher, 'Stock Market and Investment Good Prices: Implications for Business Cycle

Analysis').

So, what happened to the notion that money is important in the business cycle? It is certainly still on the table, under consideration. People are working hard to incorporate money into business cycle models, and to see whether it can account for business fluctuations. The jury is still out about a complete assessment.

[3] *Do you agree with the research framework of Kydland and Prescott(1982)?*

Yes, absolutely. I think that Kydland and Prescott's (1982) approach was a great advance, a great advance for macroeconomists. But, not for the substance of the narrow proposition that an aggregate technology shock is driving the cycle, which is what they argued, but rather because of the big advance in terms of thinking about explicit, quantitative, dynamic models.

[4] *Is real business cycle research still alive? What is its aim? [see, for example, Boldrin, Christiano and Fisher(1995), Chari, Christiano and Kehoe(1994, 1995), Christiano and Eichenbaum(1992a, 1995)]*

I think it is alive in at least two senses. First, 'real business research' stands for a methodology for doing macroeconomics: be explicit about the environment and be quantitative. This is very much alive. Second, even the idea of 'real' shocks is still very much alive. Researchers are studying how technology shocks at the firm or industry level might (contrary to Lucas' supposition) aggregate up and account for aggregate fluctuations.

There are other developments too, in the direction of incorporating frictions. For example, people are

incorporating frictions that account for the demand for money. The real business cycle model is, basically, an Arrow-Debreu model [see, for example, Arrow and Debreu(1954)] without frictions, and a lot of research is pushing on in various dimensions, and trying to incorporate frictions. This work uses the real business cycle model as the basic foundation for the construction of other models. I think almost all this research can be thought about in that way. One can even go, for example, to the two volumes edited by Mankiw and Romer on new keynesian economics (Mankiw and Romer, 1991), and interpret many of those papers as built on the real business cycle model. Most of those papers are taking the real business cycle perspective, in the sense that they are explicitly putting in people's objectives and constraints, and incorporating a variety of frictions on top, and, then, seeing how things go from there. One relatively minor sense in which Mankiw and Romer's (1991) volumes don't fit with the real business cycle approach, is that there is no dynamics, generally speaking. The real business cycle framework starts, in part, by taking dynamics seriously. I think the real business cycle idea that one ought to model things explicitly has been adopted by essentially everyone. I say essentially, because there are some exceptions.

[5] *What factors result more revelant in the real business cycle?*

In the real world?

[5. Cont.] *Yes, in the empirical evidence.*

I think one of the substantial things that made real business cycle models possible, despite, for example, the initial objections of Lucas in 1975, was the perception that

the seventies had been heavily influenced by supply shocks, like oil shocks. I'm not sure of how important those oil shocks really were, though. For example, in 1973 there was a lot of debate about what the right kind of monetary policy was at that time, and, in particular, about how to handle the high inflation. I think it's not clear at this moment to what extent monetary policy contributed to the big recession in 1973-74, and to what extent it was the oil shock. But, anyway, there was a wide consensus until 1980 that it was the oil shock. I think everyone agreed that the oil shocks were crucial in the big recession of 1974, and that agreement helped increase the credibility to the real business cycle model. Of course, in 1980 there was a big recession, and everyone agrees that was essentially monetary. So, that encouraged an increased emphasis on monetary issues.

But, historical events are not the only thing that drive the evolution of thinking in macroeconomics. There is an internal dynamic too. There was considerable tension between macroeconomists and other economists for a long time. The rest of our colleagues in economics had absorbed the lessons in, for example, Paul Samuelson's 'Foundations': economics is a science that begins with budget constraints, objectives, and so on. But, initially, macroeconomics was not affected by these developments. The notion was that macroeconomics is too complex and too subtle for those tools to be applied effectively, and, that the best tool is just informal reasoning. But then the work of Lucas, Prescott, Sargent and Wallace in the 1970s convinced many people that maybe this view is wrong. Lucas, Prescott, Sargent and Wallace, who initially appeared to be naive and unrealistic, eventually convinced people that perhaps it is possible for macroeconomists to use the same general framework that other economists had found so useful. An important contribution in

this respect was Lucas and Prescott's (1971) paper 'Investment under Uncertainty', where they articulated the notion of a rational expectations equilibrium in a stochastic economy. This represented a huge conceptual leap forward for thinking about how large numbers of people interact. The new way of thinking caught on fairly quickly, especially with graduate students. It was exciting and it helped put an end to the embarrassment that macroeconomists suffered at the hands of other economists who laughed at their primitive ways of doing economics. I think these sociological considerations and intellectual developments fueled the efforts of the seventies and eighties, more than substantive developments about oil shocks. This was an intellectual revolution.

[6] *What are the main implications of your models for economic policy?*

First of all, most the models that I'm working with have the implication that the private economy left to itself does not yield efficient outcomes. I'd like to talk a little bit about that basic idea, because I think it touches on a fundamental question in macroeconomics: does the business cycle represent an efficient response to exogenous forces, or does it call for some remedial government policy?

In 1970, when the new macroeconomics was being constructed, the model economies being studied all had the property that this is the best of all possible worlds, and the real business cycle model is the most unqualified articulation of that view. In such a model, a recession is a time when it is not productive to work. For example, when there is a drop in the temperature and a farmer's orange crop freezes, then there is no point in going out to harvest the oranges. Although this is regrettable situation, there is obviously nothing government fiscal or monetary can do to

reverse the bad effects of the weather. In principle, of course, one could implement a tax cut to encourage farmers who go out and work anyway, and this would indeed keep employment up. But, this would be an inefficient and counterproductive way to deal with the problem. So, the new macroeconomics constructed starting in the 1970s generally had the implication that fiscal and monetary policy designed to fight recessions was counterproductive. That view represented a radical shift from the previous consensus in macroeconomics, which held that monetary and fiscal policy had an important role to play in stabilizing the business cycle.

Bill Poole's work is the classic example of that approach. Poole wrote a very famous paper, in which he asked: should the Fed target the interest rate or should the Fed target money supply? (Poole, 1970). And the answer to that question was: whatever works best to reduce the variance of output.

But, the new models of the 1970s said: when output is low, it's because there's bad weather, or something technological like that. And, though unfortunate, this is not something that can be ameliorated through fiscal or monetary policy. The new macroeconomists made fun of the previous macroeconomists who focused on minimizing output variance, suggesting that researchers who believed in the desirability of countercyclical fiscal and monetary policy were just not thinking very carefully. I recall, as a graduate student, doing homework exercises with models in which it was feasible to devise a tax policy that smooths the fluctuations in employment, but only at the cost of a major reduction in welfare. The subtext of these homeworks was that researchers like Bill Poole, though well meaning, were misguided. So, there was a radical shift in views about policy there. In my

own case, I followed that shift and worked on real business cycle models, where activist monetary and fiscal policy was generally counter-productive. But, events of the 1980s were to prove that what was lasting and new about the Lucas-Prescott-Sargent-Wallace revolution was not this shift in perspective on policy. It was a revolution in methodology.

In 1980 there came another shift in views on the proper role of policy. Costas Azariadis(1981b) and Cass and Shell(1983) discovered models that actually rationalized some of the type of ideas that people like Bill Poole and others had been pushing [see, for example, Poole(1970, 1976)], and that had been at the core of macroeconomics before the 1970s. They showed that a recession could occur for reasons other than bad technology. It could occur for reasons that were correctable by the appropriate choice of fiscal or monetary policy. Now, initially, Cass and Shell(1983) and Azariadis(1981b) were very much in the background, just like Lucas, Prescott, Sargent and Wallace had been in the early 1970s. In their early days, Lucas, Prescott, Sargent and Wallace had been viewed as little more than hackers working in their garages on irrelevant things. Cass and Shell and Azariadis were the hackers of the late 1970s and early 1980s. Initially, their models were considered strange and irrelevant. Significantly, Costas Azariadis and others working on these new models were squarely in the real business cycle tradition, in the sense of formulating everything very carefully at the level of preferences and technology. This new paradigm grew, and a major contributor is Michael Woodford [see, for example, Woodford(1986, 1990, 1991)]. But there are other important papers, for example, there are influential pieces by Bryant (1983, 'A Simple Rational Expectations Keynes-Type Model,' Quarterly Journal of Economics, Vo. XCVIII, no. 3) and Cooper and John(1988).

These new models revealed very clearly that the revolution launched by Lucas, Prescott, Sargent and Wallace was fundamentally not about substantive policy issues like whether we should or should not do something to stabilize the business cycle. It was a revolution primarily in how one thinks and goes about doing macroeconomics. It was a revolution that said, 'in talking and thinking about macroeconomics, you must be precise about the assumptions you are making about agents' preferences, technologies and other aspects of the environment.' This revolution constituted an important and lasting scientific advance.

The people working in the real business cycle model tradition are now moving in a variety of directions. On the one hand, you have versions of the real business cycle model without frictions, which imply that recessions can be really bad, and that's one group of people who now argue for policy. I've written a paper with Sharon Harrison emphasizing the resemblance of these theories (that is, the Azariadis and Cass-Shell ideas) to the old views of Bill Poole and so on. On the other hand, there's another branch going off from real business cycle models, which is introducing frictions into models, and those frictions are also rationalizing a role for government policy.

An early paper on this is actually not written by a person we normally associate with real business cycle analysis, it was written by Stanley Fischer(1977). In that paper there was a role for active monetary policy. Fischer's argument was: let's suppose that it's somehow socially a good thing that people get together and form labor contracts. Let's not ask why that is. But now there is a problem, the problem is that the economy may not respond appropriately to shocks, for example, bad technology shocks. With a fixed nominal wage it may be that real wages does not fall enough

to prevent unemployment when there is a bad shock. Then, there's a role for active monetary policy: to generate a rise in the price level to bring the real wage down. So, what Stanley Fischer argued was something very much along this other branch, where you put frictions into real business cycle models, and, then, you analyze policy with those frictions.

I've worked on this area as well, and what you find generally, from the point of view of monetary policy, is that you end up rationalizing an old view, something called the 'real bill doctrine', which is the following: sometimes, when it's really desirable for the economy to be very active, it may be efficient to supply the funds or the liquidity that is needed for that activism, and that creates a role for monetary policy.

So, the answer to what the implications of my models are for economic policy ... Generally speaking the implications are: some form of activist policy, even a pro-cyclical type of policy. But these developments have come out in the last few years, so, we don't know where they're going. The early contributions, by Cass and Shell(1983) and Azariadis(1981b), worked in models that initially seemed strange, and that's why at first mainstream macroeconomists had a hard time making sense out of those contributions. It's only recently when Benhabib, Farmer, Jordi Gali, Woodford and others, have written down sunspots model that look very similar to normal models and people started to take all this stuff seriously [see, for example, Farmer(1993), Benhabib and Farmer(1994), Farmer and Guo(1994), and Gali(1994a, 1994b)]. So, I think the implications for economic policy are that some form of policy design is desirable.

[7] *Is there still some advantage in studying the IS-LM*

model?

Absolutely. I think that the IS-LM model is a very useful language for talking about macroeconomics with undergraduates. All the ideas of economics that have been discussed in the past couple of decades can be articulated with this framework, with minimal setup costs for the student.

[8] *However, many graduate students say that their professors criticize the IS-LM model ...*

The IS-LM model is a language. Its strength is its extreme flexibility, it can accommodate different elements, you can use it to express almost any idea. That's an advantage for undergraduate teaching, it may also be an advantage when thinking casually. But, it is probably not appropriate for research.

The big development since the seventies, in my view, is not so much substantive. The basic ideas of the pre-rational expectations revolution are still there: the economy needs some kind of institutional design to improve the performance of the business cycle. The economy, left to itself, is not going to necessarily end up in a good place. The basic ideas are still here.

But there is a very important difference. It is that now we are able to talk about those ideas with a level of precision that no one could have dreamt of before. And that level of precision is important for two reasons. First of all, we can make clear, when we discuss with each other, what are we talking about. The second thing is that it allows us to quantify things.

For example, suppose someone wants to argue that business cycles are triggered by infectious waves of optimism in investment. In the past, a researcher might have explained this idea with a sudden shift to the right in the IS curve. This is now not considered appropriate. Now, the research must explain in detail how an infectious wave of optimism might take hold among people. The answer may involve externalities in production. If so, then the researcher must provide evidence that the externalities are sufficiently large empirically to justify the notion that infectious waves of optimism can occur. The new macroeconomic framework introduced by Lucas, Prescott, Sargent and Wallace made it possible to impose high standards on the degree of precision and clarity expected of researchers. As a result, it is easier than before to weed out bad ideas, and to build confidence and understand more deeply, the good ones.

So, the lack of precision and clarity in the IS-LM model renders it inappropriate for economic research and the teaching of future economic researchers. Still, at the undergraduate level it can be very useful because of its simplicity and the absence of technicalities.

[9] *What is the relationship, if any, between your real business cycle models and the growth models?*

Perhaps the distinction between these models is best described as one of emphasis. The basic framework, optimizing agents, clearing markets, etc., is the same. Business cycle models elaborate more on some things, while growth models elaborate more on other things. For example, in typical business cycle models, the source of growth is simply an exogenous shift up in the location of the production function, without any discussion of why that happens. In

typical growth models this would be unacceptable, since an important objective of growth theory is to understand the reasons for that shift up. For example, the shift up could be due to an increase in human capital, and that increase in human capital could reflect the type of investment activity like going to school. Also, some business cycle models focus very carefully on details of the monetary transmission mechanism, and they tend to abstract from the kind of facts more related to growth. In growth models, money has received little attention, presumably because people think that money has little impact on long-run growth. Finally, in business cycle models there are typically shocks, while the standard growth model usually abstracts from shocks. To summarize, the basic framework of growth models and real business cycle models is the same, however, the emphasis is different.

[10] Sala-i-Martin(1994), in a book about economic growth, criticizes the macroeconomists that are not interested in growth, because he says that other areas of macroeconomics don't have the relevance of the research on growth. What do you think of that?

This is a view or an argument that Lucas made some years ago. He assumed that the average person had a particular simple utility function. He then calculated how much better off that person would be if the business cycle were eliminated. He found that that person would not be much better off at all. Moreover, he found that the improvement in the well being of the average person would be far greater if the economy's growth rate were increased a little bit instead. These findings are sometimes interpreted to mean that economic research which discovers how to reduce business

cycle fluctuations can at best generate only modest improvements for society, while economic research that makes it possible to increase the growth rate of the economy would generate massive improvements. In brief, macroeconomics is not important, and growth theory is. Sala-i-Martin is repeating this argument that Lucas made.

In response to this, let me first say that I certainly agree with the idea that growth theory is very important. A one per cent increase in growth over a not very long period of time, say one generation, produces a huge difference in living standards. So, growth is, clearly, of enormous importance. Moreover, the basic subject of growth is absolutely fascinating from an intellectual point of view.

Consider the difference between countries like India and countries like the United States. There's an enormous difference in standards of living, and one wonders why on earth that is. Imagine now that an economist figures out the answer. Imagine the economist goes to India and says: 'here is the trick'. And, then, in two generations, the typical Indian is as wealthy as the Americans are. That economist would have made a great contribution indeed.

So, an interpretation of Lucas' argument is that there would be a big gain to society if we transferred some economists out of macroeconomics and into growth. I'm skeptical, and I imagine that the marginal productivity of economists across these two fields is closer to equality. I suspect that the Lucas argument overstates the gains from adding economists to the study of growth, and understates the loss from taking them out of macroeconomics.

Let me explain why Lucas might have understated the potential gains from studying macroeconomics. The first reason has to do with his emphasis on the 'average person'. To see that focusing on the average person could be very

misleading, consider the following example. Suppose there are 10 people living on 10 desert islands that are so isolated that all 10 people must remain alone. Each person has one coconut to survive the following week, and suppose that this is just barely enough to sustain life. Now imagine that a flash occurs in the sky and five people lose their coconuts, while the other five end up with two coconuts each. Evidently, the flash in the sky has had no impact on the average person. However, thinking about the welfare impact of the flash by focusing on the average person is clearly very misleading. The fact that five people lose their coconuts means those five will starve. In fact, the social cost of the flash is evidently very high.

This example illustrates a feature of business cycles. When the economy goes down, the fall is not experienced equally by all. Job losses are concentrated on a subset of the population, and the cost to them might be very high, even if the cost to the 'average' person is quite small. The jury is still out on whether taking account of the differences among people will overturn the Lucas conclusion that the social cost of business cycles is very small.

But, there is reason to question Lucas' conclusion that the costs of business cycles are small, even if you accept his strategy of focusing on the average person. In particular, there is good reason to think that the particular utility function Lucas used might have biased down his estimate. In one of the most famous papers on empirical economics ever written, Mehra and Prescott showed that with Lucas' utility function, it is impossible to understand why it is that, given the very high return on stock and the low return on government debt, people are still willing to hold the latter. One interpretation of this result is that people's aversion to the risks associated with stock is much

greater than what is captured by Lucas' utility function. Now, suppose you find a utility function that better captures people's aversion to risk, and you redo Lucas' calculations. Presumably, you will find that the erratic ups and downs of the business cycle are more painful to the average person than what Lucas found. You might even find that the cost of business cycles to the average person are much higher than what Lucas estimated them to be. Such an estimate would be more credible than Lucas' if it is consistent with simple observations about people's aversions to risk, such as those suggested by the stock market observations I just described.

Lucas' argument is enormously famous. And, this is justifiably so. It is very simple and the implications are immense. The gripping nature of the argument focuses attention on an issue that really matters. It is arguments like this that make economics so much fun.

But, I don't buy the argument. Clearly, growth theory is important. But, so is macroeconomics. The Great Depression was a horrendous social disaster, and one thing macroeconomists do is to think about why that happened and to devise institutions that will reduced the likelihood of it happening again. If we prevented just one Great Depression, that would surely pay the salaries of the entire economics profession for many decades! The great financial crisis we are now witnessing in Asia is also a social disaster. One job of macroeconomists is to understand why it is happening and to think devise structures that will reduce the likelihood of it happening again. These are activities of the highest importance.

[11] *Do you agree more with the endogenous growth models or with the classical ones?*

Your question draws attention to the fact that business cycle models, what I assume you mean by classical models, and endogenous growth models, are two separate sets of models, one for the study of business cycle models, and the other for the study of growth. One issue is whether macroeconomists shouldn't be incorporating elements of endogenous growth models into macroeconomic models. It's clear that there could be important payoffs from this. Suppose, for example, that the accumulation of human capital is strongly influenced by job experience, as many people believe. Then, if you have a severe recession, that throws a lot of people out of the jobs, and they are not accumulating human capital. In fact, their human capital stock may be depreciating during that period, and that can have long-run consequences. So, a case can be made that it is very important to incorporate endogenous growth factors into business cycle models. These type of considerations suggest to me that the current practice of abstracting from endogenous growth elements of business cycle models may be a mistake.

[12] *Is current macroeconomic research too mathematical?*

I think the crucial thing about macroeconomics, the great step forward, was the great thing that Lucas, Prescott, Sargent and Wallace did: to bring precise reasoning into macroeconomic research. Because the phenomena we are talking about in economic research are so subtle and so complex, the need for precision is very important, and mathematics is the perfect language for this type of precision. It comes with a very large cost, however, which is that it's difficult to learn. It's like a European or an American having to learn Chinese before being able to engage in some activity. It is a significant barrier to entry into macroeconomic research.

Actually, macroeconomics was a relative late-comer on this dimension, and mathematics is represents a barrier to entry into essentially all areas of economic research. The problem is that the language of mathematics is very difficult to learn. Indeed, it takes years of practice until it actually begins to feel like a language, and not just a bunch of weird symbols on the page. But my own view is that, for researchers, mastering the language of mathematics is crucial. I say let's keep the IS-LM model, so that we can still communicate with other people who are not doing research in macroeconomics, but who would like to have access to the basic ideas. I think the IS-LM model is a marvelous and flexible tool for doing that, but for researchers a different, more precise language is needed.

[12. Cont.] *And what happens with the people on the street?*

Let me address why it is that macroeconomists may seem divorced from regular people on the street. An important reason flows from the fact that some of the initial macroeconomic models that were developed in the 1970s were quite abstract. As a result, a very superficial description of these models could make them look just plain silly. And, in those days, the battles among macroeconomists were heated. Macroeconomists on both sides of the debate had powerful incentives to make the models and arguments of the other side look silly. An abstract model, even if it carries the most profound insights, can always be described in a way that makes it seem foolish.

But I think the models that we are now talking about are less abstract, and less subject to misinterpretation. Moreover, there is now less disagreement and fighting among macroeconomists, so there is less time spent making the other

person's model look ridiculous. As macroeconomists articulate better what the nature of their models is, using a flexible language that people can learn quickly, like IS-LM, people will see that macroeconomists are really working on very reasonable models.

Some might think that macroeconomics might seem divorced from the person on the street because it is very mathematical. I think this is wrong, and that this notion that macroeconomics is really divorced from reality derives from an impression of the early models. I don't think it's a concern about mathematics 'per se'. Quite the contrary, I think mathematics allows us, because of the subtlety and sophistication of mathematical language, to talk about really realistic models, and, hopefully, people will become aware of that as we move forward through time.

[13] *Is there a convergence today in macroeconomics?*

Yes. I think the essence of the real business cycle revolution has been absorbed by everyone, and the essence of the rational expectations has been absorbed by everyone. The standards in macroeconomics, at least in the United States, have been changed, I think, forever. There's an emphasis on rigor, on precision, and there's an increasing recognition that this framework is flexible enough to articulate a great variety of views, not just the views emerging from macroeconomic models of the 1970s. These tended to take the position that the business cycle reflected the economy's efficient response to exogenous shocks. Now, we even have variants of the real business cycle model which can articulate views about business cycle stabilization policy that were at the center of macroeconomics before 1970. (This argument is

developed in detail in a paper of mine which Sharon Harrison.) So, there's convergence, in the sense of using a similar set of tools.

[13. Cont.] *And in the methodology?*

In the methodology, I think, there's a general agreement.

[13. Cont.] *And also in the implications?*

I think that now there's more interest in the importance of government policy, and in the potentially beneficial role that it has to play. Although people continue to differ, still there is more agreement compared with the seventies, when a significant number of macroeconomists believed that any intervention would probably be counter-productive, I think that now the center of gravity has changed. Almost every model I see today rationalizes some constructive role for government intervention.

Monetary policy is an important area where there is intense debate over what kind of government policies would work best. The modern line of research starts with Kydland and Prescott(1977), then, Barro and Gordon(1983a, 1983b). One question this literature asks is, what type of person would make a good central banker? Is it one who is very concerned about unemployment, as well as inflation, or would it be better to have someone who is a little hard-hearted? Rogoff was the first one to say that the latter might actually be best [see, for example, Rogoff(1985, 1987)]. And, then, there's a question about whether and how to constrain the central bankers' actions. People speak a lot about New Zealand, about the law that requires that the central banker target inflation, and that provides sanctions in case the targets are missed.

And now, of course, Europe is an important topic. What

should be the rules for bringing in the various countries of the European Union? And, what should the new European central bank do? Should it target expected inflation? Should it target money growth? Should it not have explicit targets? So, that's a clear example where there is active discussion about the institutions in which monetary policy is conducted. And, there is a consensus that this is important stuff. If all this was irrelevant, as it would be under the real business cycle model, nobody would be wasting their time talking about it. But there's a perception that it does matter, and that the way you set up your government institutions matters a lot. You want to do it in the right way, not in the wrong way.

In the United States there is also intense discussion about monetary policy. In the United States, as in other countries, there was a take off in inflation in the sixties, with further increases in the seventies. There's a general perception that it took the costly recession at the end of the seventies to get rid of the inflation. So, there's a perception that, at least in the sixties and seventies, monetary policy was not very well executed in the United States. People ask what were the monetary institutions that allowed these bad things to happen, and, more important: could our monetary institutions today allow something like that to happen again? There is controversy over the answer. Some say yes, and some say no. The latter say: 'We have learned our lesson and this will guarantee that we will remain vigilant over the dangers of inflation. No change in institutions is required. Vigilance on the part of policy makers is enough to prevent high inflation from happening again.'

The people who say, 'yes, it can happen again', point out that history is filled with 'lessons' about the bad effects of high inflation. But, that doesn't seem to stop high inflation from occurring from time to time. In the 1960s and

1970s people understood very well that the European hyperinflations of the post-World War I period couldn't have occurred without the fuel of rapid money growth. The high inflation of the 1960s and 1970s occurred anyway, despite this understanding.

I think the record is clear. The high inflation of the 1960s and 1970s was not a consequence of ignorance on the part of policy makers about the inflationary effects of high money growth. Nor did it reflect ignorance about the dangers of inflation. After all, this was the hey day of monetarism. Even the chairman of the US Federal Reserve at the time, Arthur Burns, was a card-carrying monetarist. The bible of monetarism, Friedman and Schwartz's majestic 'Monetary History' was published in 1963.

If ignorance or stupidity is not the reason that modern central bankers sometimes allow inflation to occur, then what is it? Some argue that the problem lies with our monetary institutions, not with the people running them. They argue that it is in the nature of modern monetary institutions that, given the right set of circumstances, it is possible for a central banker to have no choice but supply the extra liquidity that fuels high inflation. To Arthur Burns it was clear as a bell that the high US inflation could have been stopped in the 1970s. As he himself emphasized often, all he had to do was to stop money growth. Despite this, he felt compelled to keep money growth going. Why? Because he felt that stopping the inflation by stopping money growth, though feasible, would impose unacceptable social costs.

So, the problem of high inflation may reflect not the evil or ignorant intentions of monetary policy makers. It may instead reflect something about our monetary institutions which sometimes puts policy makers in a position where they feel they have no choice but to supply high money growth. The

macroeconomists who argue this say that the mechanism works like this. Something happens in the economy (say an oil shock) to raise the price level. When people see the rise in prices they think this signals a takeoff in inflation. The reasons for this might be vague in their minds, or they may think that the rise in prices will be made persistent because the central bank will be compelled to accommodate it. Either way, they then sign wage and price contracts which incorporate the higher inflation expectations. The central bank is then put in a position where it has two choices: either accommodate the higher inflation expectations and suffer high inflation, or don't accommodate and suffer a recession. A rational central banker, like Arthur Burns for example, when confronted with this alternative, might reasonably choose the high inflation road.

But why should people necessarily infer from a rise in prices that inflation has taken off? Is it that they are stupid? No. In the 19th century prices went up and down all the time. When prices went up, people didn't automatically infer that inflation had taken off. We know that, because long term interest rates responded very little to price rises then. Although people might not have understood all the technical details of the gold standard, they did understand that inflation could not simply 'take off'. They understood that the nature of their monetary institutions made that simply impossible.

Likewise, it is argued that if our current monetary institutions were modified so that people could not imagine that a rise in prices could trigger a take off in inflation, then the periodic price changes that occur because of exogenous shocks, like oil shocks, would not raise inflation expectations. Such shocks would not motivate people to build higher inflation expectations into wage and price contracts.

And then, there would be no high unemployment when inflation ultimately fails to materialize under this system.

People who argue that no change in institutions is necessary to insulate the economy against inflation like to point to the good inflation performance of the 1980s and 1990s. However, the big price shocks in this period went in the opposite direction than the two oil price shocks in the 1970s. In 1986 we had an oil shock which drove oil prices down by as much as they rose from 1976 to 1979. If those oil prices had gone the other way, and people would have responded by raising inflation expectations, I think there is a strong chance that we would have had a repeat of the experience in the 1970s. The high unemployment that would have resulted from the oil shock would have given rise to calls for the Fed to ignore rising prices and concentrate on real output instead. This in turn would have further encouraged the rise in inflationary expectations. This in turn would have increased the amount of monetary ease required to keep unemployment from going too high. This vicious circle, also known as the wage-price spiral, is the ugly scenario by which we might have gotten a repeat of the 1970s. As it happened we were lucky. The price shock went the other way.

I've gone off a bit on a tangent. My main point is that government policy is back to center stage in macroeconomics, and there is active debate and discussion over it. The nature of the discussion is not between some people who think policy matters and other people who think it doesn't. Instead, it is among people who all think it matters, but differ in what should be done. This represents a big shift relative to the early days of the new classical revolution.

[14] *Is it possible today to differentiate between micro and macroeconomics? The microfoundations of macroeconomics ...*

To me, the word 'Macroeconomics' refers to a set of questions. For example, why do we have business cycle fluctuations? What should be done about them? This is what differentiates macroeconomics from microeconomics.

Some time ago, another important distinction between macroeconomics and microeconomics lay in the conceptual framework used. The conceptual framework used by the former was dominated by the IS-LM model, while budget constraints, profit and utility maximization, and equilibrium dominated the latter. Now, the distinction in conceptual frameworks has largely disappeared, in favor of the microeconomic tradition.

But the conceptual difference has not disappeared in all quarters. There are some macroeconomists who continue to work with the IS-LM model. This includes a subset of researchers working on central banking. They tend to use reduced form models of the economy. I should have said something about this earlier, when I was talking about IS-LM model. Almost all macroeconomic researchers have abandoned the IS-LM paradigm, but there are some who still use it in formal research. Professor Lars Svensson is a prominent example of this. He does by a very self-conscious choice, because he is a highly regarded theorist. So, he is not using IS-LM because he doesn't know what he's doing. He's using it because he knows what he's doing, and he thinks that's the right way to do it.

[15] *What still remains of the new classical macroeconomics revolution?*

My answer to that would be the same as: is real business cycle research still alive? The answer is yes, but it's greatly evolved.

[16] *What do you think about the new keynesian macroeconomy?*

The new keynesian macroeconomists are providing a lot of inspiration. I see people like Chari, Kehoe and McGrattan, and others in Minnesota, who are working on models of fixed prices in general equilibrium, and I'm doing it myself, with Eichenbaum. We're writing models with fixed prices, staggered pricing, fixed wages, and all these are ideas associated with the new keynesian macroeconomics. So, I think the new keynesian macroeconomics is very interesting.

[17] *How do you consider yourself? A new keynesian or a new classical economist?*

I think labels like this are dangerous because they are potentially divisive. There is a natural tendency for people to lash out at other people, at people who they view as members of a different group. Inside each of us there is a devil and an angel. The devil is egotistical and overly competitive with the other 'team'. The angel in us works to structure the environment so that, when the devil comes out, he can't do too much harm. One way to do this is to de-emphasize labels.

[18] *Where will macroeconomic research be going in ten years?*

I think it's going to be very heavily oriented towards institutions. It's going to resemble a lot of discussions you see in Europe now, where they literally are constructing institutions. I think that economists have been very unsophisticated in the last fifteen years, and I see a greater move towards interesting discussion about how the institutions of our society should be organized.

[18. Cont.] *Now, there are a lot of economists in Europe studying these topics. The independence of central banks ...*

Yes, I think that's the whole thing. Ten years is too far, but I'd say five years.

[19] *Do you agree with the economic policy that your government has been implementing in recent years?*

I'm nervous about US monetary policy. Our most recent 'war', was the high inflation of the sixties and the seventies. The perception is that the Fed lost that war, in the sense that they should not have permitted the inflation to take off. The question is how do we stop this from happening again. The line that the United States, or the Fed, is taking, is that the reason why the war was lost was that prices were allowed to rise in the 1960s and early 1970s, and this triggered a wage-price spiral: People started to expect high inflation, which the central bank had to accommodate in order to avoid triggering a recession; this brought out more inflation, higher inflation expectations, more accommodation, etc. Now the Fed, whenever there's the slightest sign of a rise in prices, adopts a very restrictive policy, because it's afraid that if it lets inflation go just a little bit too far, a wage-price spiral might start again.

Now, my concern is that the Fed has adopted a dangerous policy. It's true that any particular time you fight against a small rise in prices, things might work out fine. But there's a danger of triggering a recession. For example, in 1994 US economic growth was very strong, and the Fed became nervous that this could trigger a rise in inflation. So, the Fed raised interest rates a lot, and they were actually very aware they might have overdone it. It looks like they didn't

overdo it, it looks like they didn't create a recession. But I think they came close.

But, maybe there is a different lesson to be learnt from the 1970s. Perhaps the problem had to do more with the nature of monetary institutions. Perhaps if those institutions could be changed so that, in effect, there is more commitment in monetary policy, then perhaps there is no reason to worry that some rise in prices might trigger an increase in inflation expectations. For example, a price rise due to, say, a harvest failure during the gold standard era didn't trigger wage-price spirals.

If we found a way to modify our monetary institutions so that an up-tick in prices didn't trigger a rise in inflation expectations, there might not be a need to implement a risky monetary tightening whenever the economy showed signs of strength. So, I'm nervous about the policy of the Fed. Possibly, the right lesson from the experience of the sixties and seventies is that we need to change our institutions a little bit.

[20] *How would you evaluate the US economy in comparison with the Japanese economy and the European economies?*

One hypothesis about Japan's good economic performance (at least, prior to 1990), and also Germany's, reflects these country's natural response to the devastation of World War II. Japan suffered greatly after the war, with people being close to subsistence. In any economy which has been damaged severely, we expect to see very strong growth for a while. Now, in the case of Japan, it took some time for this to actually happen, and one interpretation of this, is that the Japanese were initially too desperate to muster the resources necessary to start investing. But when you get up to the

1960s, Japan begins to grow rapidly. So, one interpretation of Japan's exceptional growth is that it reflects the natural response of an economy to the very severe destruction that was suffered in the war. Under this interpretation, the really exceptional growth rate was a temporary phenomenon. (See Christiano, 1989, and Hayashi, 1989, Federal Reserve Bank of Minneapolis Quarterly Review, Spring.)

My hunch is that a pessimistic view about the US economy is wrong. For example, in terms of productivity per worker I think the US still leads the world. US economic performance was a problem in the eighties in terms of levels, but in terms of growth rates the eighties were very strong. If we take employment, for example, and you go from 1970 to 1985, or so, I think you're talking about a 30 per cent increase in the employed labor force in the United States, and a zero increase in Europe.

Anyway, I think the world economy is poised for strong growth. We have a lot of new developments in computer technology, information technology, biotechnology, and other things, and my guess is that these will result in a very splendid couple of decades ahead. There was a little slow down in the seventies, but I see things taking off, and, eventually, the Eastern Europeans will get their act together. I see a tremendous explosion of creativity and activity in the next decades.

[21] *What theories do you consider best explain the high unemployment rate in Europe?*

I suppose most economists would be inclined to relate the high unemployment in Europe to the excellent unemployment insurance system. The problem with that story is that the excellent unemployment system was instituted in the fifties

and sixties, and the unemployment rate didn't rise until the middle seventies or something like that. So, if you relate high unemployment in Europe to the unemployment system, you have to come up with a reason why it took so long for the cause to have its effect.

One explanation was recently advanced by Ljungqvist and Sargent [see, for example, Ljungqvist and Sargent(1995a, 1995b, 1996)]. They argue that a central feature of the unemployment system put into place in the 50s and 60s is that if you lose your job you will be supported in relation to the wage you were earning in your last job. But back in those days, argue Ljungqvist and Sargent, the type of job losses that you saw were of this type: you lose your job at the ESSO station, then you go to work at the TEXACO station. Job losses were not associated with a big fall in your human capital and, hence, wages. Then, the unemployment system was not such a big deal, because if you get 80 per cent of your last wage under the unemployment system, and 100 per cent of your last wage at the TEXACO station, you will go to the TEXACO station.

But Ljungqvist and Sargent say that now the situation is different. According to them, the type of job losses that have occurred in recent years involve a larger destruction of human capital. Now, if you lose your job at the ESSO station, you're likely not to be able to find a job anywhere, except selling hamburgers at a much lower wage, maybe 50 percent of your old wage. For a person like this, the fact that the unemployment system supports you at a high fraction of your old wage makes it relatively attractive. Ljungqvist and Sargent argue that the number of people like this is greater now than before, and this is why unemployment in Europe has gone up so much. The validity of the Ljungqvist and Sargent explanation is still very much under dispute. A fundamental question is whether the nature of job changes has evolved in the way that their story

requires.

My guess is that there's something else going on too, which has to do with the changing nature of the signal you send when you become unemployed. In the fifties and sixties that signal was not very good. The unemployment rate was very low in Europe, about half of what it was in the US. Suppose that there are some fractions of 'losers' in the population, and they spend a large amount of time in the unemployment pool. Suppose that they dominated the unemployed in the 50s and 60s. Entering the pool of unemployed in those days would have sent a signal to potential future employers that you were probably a loser type. Not surprisingly, people would resort to great efforts to avoid being in the pool of unemployed.

But now suppose that something happens and pushes a lot of people into the unemployment pool for reasons beyond their control. At a time like this, being in that pool no longer sends out the signal it once did. Before, being in that pool indicated there was something wrong with you. Now, regular people are in there too. So, now the signaling reason for avoiding unemployment is lower. I imagine this is another factor behind the high unemployment in Europe.

Finally, to some extent the high unemployment in Europe must be a measurement problem. For example, in Spain, the unemployment rate is extremely high, over 20 percent. It's hard to believe that so many people are really looking for work and not finding it. I imagine that in Spain there are probably a lot of people that are actually working, while registered as unemployed. As I mentioned above, it would have been a humiliation in the fifties and sixties to be registered unemployed, because there were so few and these were dominated by lower quality workers. Nowadays, if you register for unemployment benefits, the company you are in is not so bad.

[22] *What do you expect of the integration of the European economies?*

I don't know. I looked at this recent experience where the British and the Italians withdrew from the EMU. That reflected a fundamental disagreement about policy, and these countries, in the end, are sovereign countries. They perceive themselves as sovereign entities, and they feel that if they don't agree with the policy of the other countries they're going to withdraw. Then, if the European Union gets together it's going to be take a miracle to make it work, because they are sovereign states, with a long tradition of sovereignty. It's hard to imagine them just shedding this overnight.

But, on the other hand, it might happen. In the United States something like that did happen, although at a less extreme level. In the United States we also have an integrated community, of all the states, but I don't think we had this long tradition of sovereignty, and hostility. So, you may say: well, the United States was able to do it, maybe Europe can do it. But it's a different setting. And you have also different languages and so on, in Europe.

I think European Union is an excellent thing. I hope it works, because all these barriers have to be an impediment to progress. It's a miracle that the European Union has been able to go as far as it has. So, if one miracle is possible, maybe two or three more are possible too.

[23] *Finally, what do you consider is your main contribution to economics?*

I've tried to contribute in, at least, two areas. One of these areas is something we haven't mentioned at all, which is empirical: econometric tools for time series analysis, and,

particularly, promoting the use of something called 'new statistical techniques' [see, for example, Christiano(1990), Christiano and Eichenbaum(1990), and Christiano and Ljungqvist(1988)]. And the other area I hope to have contributed to is good thinking about structuring policy institutions. I've worked on fiscal and tax policy [see, for example, Aiyagari, Christiano and Eichenbaum(1992), and Chari, Christiano and Kehoe(1991, 1994, 1995)], but, more concretely, I hope to have contributed to the analysis of monetary policy institutions [see, for example, Chari, Christiano and Eichenbaum(1995, 1996), Chari, Christiano and Kehoe(1991, 1995), Christiano(1991), Christiano and Eichenbaum(1992b, 1992c, 1995), and Christiano, Eichenbaum and Evans(1996a, 1996b)].

[24] *Related to the previous question: what is the role of statistical tools in macroeconomic research?*

I enjoy talking about this topic, which is very controversial. What is the role of statistical techniques, and, in particular, of econometrics in macroeconomics?

The story of the current wave of debates on this question begins in the early 1970s, although Marty Eichenbaum tells me that the basic issues have been debated for a long time, well before World War II. For example in the thirties, there were debates between Keynes and Tinbergen, and other people, but I'm not familiar with those debates, although I'm told that the same issues were at stake as now.

What happened in the late seventies is that you had, on the one hand, macroeconomists, in the tradition of Lucas, Prescott, Sargent and Wallace, who were at that time forging important conceptual advances in macroeconomics. Meanwhile, you still had a highly developed system for econometrics,

associated with the Cowles Commission. A crucial part of econometrics is identification, the theory of how one can exploit the restrictions provided by economic theory to work backwards from a data set to a set of parameter values for a model. But, this aspect of the Cowles Commission econometrics was constructed for pre Lucas-Prescott-Sargent-Wallace macroeconomics. Econometrics was well suited to the old macroeconomics, but seemed less useful for the new ways. As a result, there developed a kind of split. Macroeconomists, who are potentially important users of econometric tools, came to view econometricians as distant and irrelevant.

Then, when we get up to the late 1970s and early 1980s, Sargent said, rightly, this is intolerable, we're all living in the same world, with the same data, why are we separated? So, what Sargent did, in collaboration with Lars Hansen [see, for example, Hansen and Sargent(1991)], was to create a new econometric framework that was suitable for estimating and testing the new macroeconomic models. This involved primarily developing a new set of tools for identification. The hard-core statistical stuff having to do with sampling theory and consistency results continued to work fine with little or no adaptation. Hansen and Sargent achieved a new synthesis of macroeconomics and econometrics. Those were exciting times. It seemed like Hansen and Sargent were producing a brilliant new paper each week. I can remember awaiting with excitement the birth of each new contribution. This was a time of great advances and important, lasting contributions. But, here was a downside.

A problem was that a great deal of specialized knowledge was required to implement the new synthesis. Not only was a knowledge of maximum likelihood theory and sampling theory needed. In addition, a high degree of sophistication in techniques of numerical analysis was required. This had three

consequences. First, too often the requirements posed a significant barrier to entry into empirical macroeconomics, Good people interested in real empirical puzzles, but who leaned more towards theory and less towards econometrics, were discouraged from entering. Second, among people applying the new synthesis, there was often an excessive concern with econometric technique, and not enough with the nature of the model and data being analyzed. It used to be said that data was a form of 'food' for complex econometric estimation programs and that researchers hardly cared whether the 'consumption' data they were analyzing was nondurable goods consumption, consumption of services, durable goods consumption, or the sum of all three.

With good researchers being discouraged from entering and practioners often obsessed with details fundamentally removed from important substantive macroeconomic questions, the situation was bad. The new synthesis had moved to an extreme, counterproductive position.

It was time for someone to step in and say 'no'. The synthesis had such a strong hold in the early 1980s that it took a courageous person to do this. The person who did this was Edward Prescott. He said, let's spend less time on the technical details, and more on understanding how the data are constructed. Let's spend more time on the theories we're trying to match to the data. Let's spend more time being thoughtful about the questions we are studying.

Prescott carried a special authority, because he had made profound contributions to macroeconomics. It was not possible to attack him for not being serious. Moreover, one couldn't attack him for trying to make a virtue out of not having done his econometric homework. With Cooley, he had made important contributions to the econometrics of estimating models with random coefficients [see, for example, Cooley and

Prescott(1973, 1978)]. Another factor which gave great weight to Prescott's opinions was that, with Mehra, he wrote one of the most influential empirical papers, on the equity premium. This paper, which is one of a handful of really important empirical papers, completely avoids the econometric tools of the new synthesis. At the same time, the new synthesis, for all its tools and sophistication, did not produce research with empirical findings of the same order of importance. Despite these factors, Prescott still came under severe attack when he said 'no'. But, he had the courage and conviction to stick it out. And macroeconomics benefitted from this.

Prescott reminded us that empirical macroeconomics is about real questions about the real world. He said, 'your first priority is to think hard about the data, the economics, and the basic questions that motivate your analysis.' This was good advice because, all too often, concern over technical econometric details crowded out time for thinking about the really important questions.

Still, in many ways the forces unleashed by Prescott pushed the pendulum too far in the other direction, away from the synthesis constructed by Hansen and Sargent. Many macroeconomists abandoned econometrics altogether. There is now a reawakening of interest in the econometric framework constructed by Hansen and Sargent. Hopefully, this time around it will be applied with a better sense of balance.

[25] *How is it possible today to be a good economist? Because you need to know econometrics, mathematics ...*

We always say that good economists are born early, or make their biggest contributions early. I think, in the case of macroeconomics, that maybe it's going to be different. Apparently, in literature, and other fields, you get great

contributions by people in their fifties and sixties. Maybe now macroeconomics is going to be like that, because the knowledge required today to do macroeconomics, as you say, is vast.