Interview of Stanley Fischer, by Olivier Blanchard, Russell Sage Foundation, May 2004.

O: When and why did you decide to go into economics?

I was a schoolboy in what was then Southern Rhodesia, later Rhodesia, later yet Zimbabwe. The educational system was British, which meant you had to specialize during the last two years of high school. I originally specialized in physics, math, and chemistry, thinking I would become an engineer or maybe a scientist or a mathematician. At some point I had a conversation with the son of friends of my parents. He had studied at the LSE, and told me I should become an economist. He gave me a few lessons, which were interesting – I think we used Samuelson's introductory book. Also, amazingly enough, I took an economics course during my last year at high school.

O: An economics course at school. This sounds very unusual?

Well, this was the British sixth form, where students have to specialize. The teacher was extremely good. We studied Hicks' *Social Framework*, and I was introduced to Keynes. In the vacation between school and college I read the *General Theory* and was hooked by Keynes' use of language, although I'm not sure I understood the book. I had decided to study in England and ended up at the LSE.

O: Why LSE? Why not the US?

We didn't think of the US then. For us England was the center of the universe. My teachers told me the choice was Cambridge or the LSE. I ended up at the LSE partly because the person who had introduced me to the subject had gone there and partly because they were willing to give me a very early decision (the academic year in the southern hemisphere ends in December instead of June). Although the LSE had the reputation for being left wing, that was not true of the economics. We took very conventional courses. Richard Lipsey taught the first principles course, and he was very good. Frank Paish taught an applied economics course. I recall his showing his slides early in 1963 and saying: "You see it goes up and it goes down and then it goes up again. And that's why we're going to have a balance of payments crisis in 1964. " The crisis took place on the appointed date, and I was very impressed.

O: This was an exciting time at LSE. Did you pick up a sense of that excitement?

I had a great time at the LSE, this was my first experience of the big world, and I took advantage of London and of the continent. But I was quite unsophisticated about academic life and intellectual life. That was not something you picked up in my high school—good as it was as a teaching institution. I thought the main aim of studying was to get through the exams.

In retrospect I realize there was intellectual excitement there at the time. The LSE was then in the midst of the controversy about the Radcliffe committee report. Richard Sayers, who was at the LSE, was the main intellectual force behind the report. It suggested that monetary policy worked, and was later seen as the beginning of the revival of monetary policy in the UK. But it was full of qualifications. It featured a three-gear view of monetary policy: if you changed gear drastically enough, you could have an impact. Still, it marked the end of the period in which it was believed in the UK that monetary policy didn't work, and the beginning of a new era in which monetary policy has increasingly been seen as a powerful driver of the economy.

Karl Popper was the dominant force in the philosophy department, and everyone became a Popperian in methodology. Phillips of the Phillips Curve was there, though I didn't take a course from him, but I did see his machine of the economy. And there was a lot of work being done on the Phillips curve.

Other memories include a lecture by Bob Solow. He must have been about 40 at the time. But he looked much younger, and was very funny, even in talking about production functions. I remember him saying: "When I say K, I mean Kuznets. Capital is that thing that Kuznets measures. " I also remember on one occasion being incredibly excited when someone explained to me what an econometric model was. That you could use data to estimate parameters, and then if you put the whole thing together, you had a set of equations that described the economy. That was really exciting, it meant you could control the economy. And it was obvious that was immensely important.

At the LSE then you wrote exams only at the end of the first and third years (it was a three year degree), and you didn't get course grades. So I didn't really know whether I was a good student or an average student. I had won an economic history prize and some other prize at the end of the first year. But I had no real idea of where I stood in the class. So I planned to work in a bank when I graduated.

O: (laughing) It eventually happened, but it took a long time…

Touche. ... I wrote the first degree, and it turned out that I did very well. I had gone to Israel to work for the summer, where I got a telegram indicating that I had gotten a scholarship and should come back to do graduate work. So I didn't know I was going to be an academic until after the first degree

O: So passion was not there yet.

No it was not there. I really liked the subject, but research was not central to what we did. That came later, with the graduate studies at MIT. Even though I did a master's degree at LSE, up to that point my view of economics was always that it was what these great professors did and do. Your job as a student was to study what they said.

O: Was MIT the logical choice for you when you wanted to go on or was Chicago in the picture?

To me, MIT was the logical choice. Everybody said "Why do you want to go to MIT?" And I'd say "Samuelson and Solow." Even though Harry Johnson, who was at LSE while I was there, recommended Chicago, and even though we read Friedman's restatement of the quantity theory, I thought MIT was the best. And I got in there and went there.

O: How was MIT?

Because I hadn't thought of going to graduate school until after completing my first degree, I had done a one-year master's degree at the LSE. So I was better prepared when I went to MIT than most of the students. But that wore off pretty quickly. The faculty member I was most close to, almost by geographic accident, was Miguel Sidrauski who was a Chicago graduate, an Argentine, who started as an Assistant Professor at MIT the same year I arrived. We happened to live in the same apartment building in Cambridge, and became very friendly.

Miguel was a terrific mentor. The relationship of the young assistant professor with the student is a very nice one, because you are so close in age. I worked a lot with Miguel in my first summer as a research assistant for him and Duncan Foley. Tragically Miguel died of cancer at the end of his second year at MIT. I also worked for Don Patinkin, who was visiting MIT and was one of my heroes. So I really got into things.

My MIT experience was truly formative. The professors were great and the courses were great. The department emphasized good teaching, and most of the professors were available if you wanted to talk to them. And we had enough term papers to do to be drawn into research. Samuelson used to say interesting things in class and throw out interesting problems. Sidrauski was an excellent teacher, who made you understand the economics that was represented in his phase diagrams, and Bob Solow did that too. Frank Fisher taught econometrics, and had a big influence on the students. And there were many others who influenced us.

No less important, I was with a really remarkable group of fellow students. The class above me in particular included a whole host of people whose names you know. Bob Hall was there, as was Bill Nordhaus, Avinash Dixit, Bob Gordon, Ray Fair, Mike Rothschild, Joe Stiglitz and others who later made their mark. Avinash Dixit could do the Times crossword puzzle in about 10 seconds. Bob Merton arrived a year after me, and we shared an office for a year.

O: Your thesis was on macro. Why?

I focused on macro as a graduate student, as I had at the LSE. I think I liked macro because I was interested in big questions but that may be an ex -post rationalization. Maybe it was because I had read Keynes' *General Theory* and was intrigued. I had this image of the world as we knew it having nearly collapsed in the 1930s, and that these guys had saved it.

My thesis was actually on lifetime portfolio choice. We were very much into the microfoundations of macro at the time, and that topic was about the microfoundations of portfolio choice and saving, in the presence of life insurance. In part I chose that topic because Paul Samuelson was working on lifetime portfolio choice at the time, as was Bob Merton.

O . Then, you moved to Chicago.

I went to Chicago as a post-doc, financed partly through Al Harberger's Latin America workshop. It was the best university that made me an offer. My first year as post-doc I went to the Money Workshop, Harberger's Latin American workshop, the trade workshop, Milton Friedman's money course, and no doubt much else. That was also the year I met Rudi Dornbusch and Jacob Frenkel, and other outstanding students, including Mike Mussa. Chicago enabled me to combine MIT's analytics with the policy relevance that Milton Friedman typified.

O: MIT was more in theory mode?

That was the impression I had at the time. I remember a discussion at MIT with the faculty during the student disturbances at Harvard in 1968 when I said that "we know a lot of economics, but we don't know much about the economy." And Chicago *then* was the perfect antidote for this. Plus Chicago too had an extraordinary group of students. I taught micro with Harberger and later taught macro.

But as I reflect on the question, I realize I must have been thinking of what I did at MIT, rather than what was happening there in the late 1960s. After all, that was when Franco Modigliani and his students were working on the FRB-MIT-

Penn macromodel, and Bob Solow was the devotee *par excellence* of using small tractable analytic models to get to the essence of a problem.

O: How central was Milton Friedman in all of this?

To the macro? Absolutely central. Macro was the money workshop. It was his workshop. In those days I regretted that they did not have people from another tradition except for a few of us, Bob Gordon in particular. Later, I decided that if Chicago wasn't Chicago, who would be? It was all right for them to pursue a particular line. But, as an assistant professor, although I benefited in the long run, it was at times difficult.

O: But did it change the way you did macro? The way you thought about macro? Your choice of topics?

It did have a long-run impact. I started working then on monetary rules with Phil Cooper, an MIT fellow-student, who became an Assistant Professor at the Chicago Business School. I also talked a lot to Rudi Dornbusch, and served on his thesis committee, and wrote a few papers with Jacob Frenkel.

O: You went back to MIT in 1973?

During the time I was at Chicago I had taken a six month sabbatical at the Hebrew University. And we had very seriously thought about living in Israel. When I went back to MIT in the fall of '73, I thought that it might be an interesting two year interlude on the way to living in Israel and teaching at the Hebrew University.

The first course I co-taught when I got back to MIT was monetary economics with Paul Samuelson. That was intimidating. He would insist on taking the chalk and explaining things better than me. Then, I sort of eased into the role with which you are familiar. You came in 1973, right?

O: Right. 73.

… And gradually I became a decent teacher. I taught the introductory macro, got a lot of students over the course of time. Coming from Chicago where the money workshop was so central, I built up the money workshop at MIT. Franco Modigliani was the star attraction. I loved advising on theses. Then in 75, I persuaded my colleagues to bring Rudi Dornbusch to MIT. He had taught at Rochester and then went back to Chicago. He was very analytic. Not very interested in the real world. Very pure. He wrote his ``overshooting'' article within a few months of coming to MIT. Our collaboration grew, and that also made a huge difference. So I had terrific elder colleagues, terrific students, and a great contemporary colleague in Rudi. Probably an ideal setup.

O: Did Israel recede as an option?

We always maintained a close contact, and took several sabbaticals there. But after we made up our mind to live in the States, around 1975, we didn't look back. At least not more than once every two weeks or so… But we never really came close to changing our minds because MIT was such a wonderful place to be. And because we liked Boston. And our kids were growing up

O: When did you shift towards more applied topics?

I should have mentioned that one of the things that got me interested in economics, peculiarly, was that Dag Hammarskjold was an economist. When I was in high school, Dag Hammarskjold was this great man. Then he was killed in the then-Belgian Congo, right next door. I knew he had done good in the world and my parents had brought me up to believe I should do good in the world. I realized that economics would help you do good. So I always wanted to use what I had learned. That factor was probably there and moved me over the course of time.

My first really intense applied work was when I visited the Bank of Israel and spent a month there in 1979. They gave me a lot of applied questions, since they were suffering from high inflation. My real opportunity came in 1983 when George Shultz asked me join an advisory group he was creating on the Israeli economy. I had in the meantime become somewhat of an American expert on the Israeli economy.

That was when I got into the policy game. It was a very fortunate introduction. It's extremely unusual to have the Secretary of State take some young guy he doesn't know and appoint him as an adviser, and then let him have an active role. Herb Stein and I were appointed as George Shultz's advisers on the Israeli economy. On the occasions Herb and I traveled to Israel, we essentially had George Shultz's authority behind us. And we could say, "The Secretary of State believes this." As a professor, that didn't especially impress me. But when you say "the Secretary of State believes'' to a government that depends on the United States, they are not listening only to the economics.

O: Was the shift to more applied topics in the air in the 1980s ? Rudi Dornbusch moved in a similar way at the same time.

Well, there were all those high inflations around, and we'd studied and taught about them. There were countless conferences on what to do about inflation and that seemed to be the general policy problem of most of the countries we were working with. Also, foreign travel was exciting. I went to Japan in 1981, together with Ben Friedman and Jeff Sachs, on a trip organized by Ezra Vogel of Harvard. I had never been to the East. There was this incredibly exciting economy, Japan, which was doing the most amazing things, growing by leaps and bounds. It was exciting. So I can't quite explain my transition, except that these opportunities came along and they were interesting. I guess it was a combination of being interested in the real world, wanting to be useful, being able to travel, and being given interesting problems.

O: That's the right transition to the next stage: the World Bank.

The World Bank was another opportunity to be in the policy world, and so I was very happy to take the offer.

O: What did you know about development at the time?

I had studied development and taken development economics as one of my fields in the MIT generals. And on the macro side, I did know the economies of developing countries. Also, at that time, the main issues were stabilization and the debt crisis, and I knew a lot about them.

At the World Bank, I got into structural adjustment and associated issues. I visited China, visited India for the first time. I spent ten days in China and met

Zhou Ziang, then the premier, about six months before Tiananmen Square. I was impressed by how much he knew about western economics. He told me that my views differed from those of Milton Friedman and from those of Lawrence Klein. I couldn't imagine that the Prime Minister was studying all these matters, but he was. Visiting India was also a wonderful experience.

I was gripped by the problem of development. And that problem hasn't left me. I grew up in a very small town in Northern Rhodesia for the first 13 years of my life, living among Africans. So the development issue was with me all the time. I also began to understand a lot both about the way organizations work, because the World Bank is an unusually complex organization, and also about the problems confronting the world. And so I left with a much better idea of what mattered and what needed to be done.

O: You stayed at the World Bank for two years?

It was originally a two-year term and it ended up being two and a half years. I did think about staying, and giving up my MIT tenure. But because I was not ready to give up MIT tenure, and for family reasons, we decided to go back to MIT. It was hard readjusting. I remember going to theory seminars and saying to myself, what difference does it make whether this guy is right or wrong, why should anyone care about that theorem and so forth. But I did somewhat readjust. I was obviously more interested in the policy side of things. I continued my involvement with Israel. I began writing a column for an Italian newspaper. I tried my hand at writing occasional columns for American newspapers. I was interested in both international problems and American problems but I became tagged as an international expert more than a macro expert.

When I got back everyone thought I would revolutionize development at MIT, but I didn't. The younger generation, which came not so long after, including Abhijit Banerjee, probably did.

O: Do you think we have today the macro tools we need to understand the world?

The quantity theory goes a very long way in dealing with inflation. And the intertemporal budget constraint, and the equation for debt dynamics, take you further along… I'd say the political economy is much harder. There's a bunch of guys who try to get policies done and the question is how they get them done. This became clear to me even before the World Bank, when, in 1985, I was involved in the implementation of the Israeli stabilization. That stabilization was the work of Michael Bruno and colleagues. Discussions about how to do it were exciting, and I learned a tremendous amount. But I learned even more watching the administrative and political battles that had to be fought in making the program work.

O: How much does it matter at those critical moments to have people with clear minds?

Oh! It matters entirely. The Bruno team understood what it was doing in a way politicians wouldn't. These guys knew what to fight about and what not to fight about. Some of the fights were very vigorous; politicians always think "Well okay we've done it now so let's open up for business again in the same old way." If you hadn't had Bruno appointed central bank governor soon after the stabilization, it probably would not have worked. It certainly would not have worked as well.

So, yes, it matters to have people who understand the environment in which they are working.

O: I think we are coming to the IMF. Being chairman of the MIT department was not enough?

Being chairman of MIT was only partly inspiring. You know Alfred Kahn's description of the role of the Dean at an academic institution: the dean is to the faculty as the fire hydrant is to the dog. It wasn't quite like that, but it had certain elements in common. I was quite involved in various academic/quasi-academic initiatives on Middle East peace during 1990-93. I did a lot of work on the economics of Middle East peace, which looked very prescient until recently. But unfortunately I fear it has not had much influence in practice.

And then I joined the IMF. I took to that job like a duck to water. I was ready for it. Having been in the World Bank I had a good idea of what these institutions were about. Having studied and taught macro and about macro stabilization, I was probably better suited for the IMF than for the World Bank. Having lots of my students all over the Fund, and in many of the member country governments, helped. Having Larry Summers as Undersecretary of Treasury certainly helped, since we were friends. It was just a terrific place to work.

I had an enormously gifted boss in Michel Camdessus, who however did not take to me initially. It took a while till we hit it off. I think he thought of me as a critic of the IMF when I arrived. I may well have criticized the IMF before I arrived. But within about a year or two our relationship was a very good one.

Within a few months of my arriving, we had the Mexico crisis, which was a good learning experience for everyone, including the US treasury.

O: Was this because the mechanisms of the crisis were different?

Well, it was a different sort of crisis, and the first economic crisis for the Clinton administration. The U.S. tried to handle the crisis on its own, and then realized that it couldn't. It is much harder for one country to impose conditions on another than for an international organization to have one of its members sign up to those conditions. The US also realized that the IMF had a team and a way of dealing with countries in crisis.

As you know, the Mexican crisis was subsequently called the first financial crisis of the 21st century. It was set off by massive capital flows. I learned a lot in that crisis, including from Michel Camdessus. He was so much calmer throughout the entire crisis than I was. I thought western civilization as we knew it coming to an end. But he had seen this particular play before.

O: Was there a sense that you were all flying without a parachute, that this crisis was more complicated than you understood?

Yes. Events would happen. You would suddenly discover about tessobonos. You knew they existed. But the difference between knowing something and realizing what it implies can be quite intense. There are 47 factors in the background. Always. The key to managing a crisis is to figure out which of them really matters.

In every crisis something takes prominence that you hadn't thought was necessarily going to be critical.   You could understand events in game theory terms.   I understand fully why they issued tessobonos: because that was a signal that a devaluation would be very costly for Mexico, therefore implying that a devaluation was less likely.   That was quite right…   The problem was that making the devaluation less likely didn't prevent it – and it *was* very costly.

Fortunately the Mexican crisis was over relatively quickly. Mexico was back in the market within six months. There was another jittery period in November 1995, but that was soon overcome. Nonetheless, for a few months the Mexican crisis seemed chaotic. And there were days I felt the crisis would just get worse and worse.   For instance, there was a day in January 1995 when Asian markets tanked as a result of contagion from the Mexican crisis. And I thought this is one of the most insane things that had ever happened because I thought Asia was in great shape.

O: Did you have the sense that you understood at least the basics? For example, in the Mexican or the Russian or the Asian crisis?

I thought I did; possibly I didn't initially understand just how severely the financial sector had been hit. Initially, in Thailand, the Asian crisis looked like an old-fashioned balance of payments crisis. We knew the banks were in deep trouble. We knew the government was going to have to bail them out in some way. But in the Asian crisis, I don't think I initially appreciated just how costly it would be to bail out the banks. Financial system weaknesses were central in almost all the crises of the 1990s, except Brazil.

In dealing with the crises, I couldn't get quite as excited about moral hazard as most of the critics. The issue was prominent in the case of Mexico. But it was important to help Mexico come back to economic health quickly. And everyone who had invested in Mexico, except for the tessobono holders, had taken a terrific hit. Given that, and given the contagion in so many countries, I couldn't see why people thought investors believed they would always be bailed out in future crises. So I couldn't figure out why so many people argued that investors hadn't learned their lesson about the dangers of investing in emerging markets from this crisis. I still believe that today.

O: What else did you take away from the Mexican crisis?

The importance of having a coherent government and having very good technocrats – Mexico had them, but many countries don't. President Zedillo's steadiness, and the steadiness of the team, with lots of American support of course helping them stay steady, was very very important. I took that for granted at the time, because the team I'd worked with in Israel, which was my previous crisis experience, was also very steady. But that is not a common feature.

Following the Mexico crisis, we got into the whole architecture discussion and what to do to prevent future crises. We focused on statistics and transparency and more macro and on the financial sector. But I suspect the main lesson I took away was: Don't run balance of payment deficits of 8 percent of GDP financed by short term borrowing. Seemed to me a good lesson to have learned (*laughing*).

I don't think I internalized fully how dangerous the fixed exchange rate system was. I knew that was the proximate cause of the problem. But that such a system was a standing invitation to a crisis, I don't think I internalized fully at that stage.

O: Why?

All the Asians were doing well with more or less pegged rates, and I thought they could adjust in time. What I hadn't figured out was the political economy of a firm peg. If you really peg the exchange rate, then the longer the peg has been in place, the more the entire economic policy and the entire structure of the financial system depend on the continuance of the peg. The longer you stay with it, the more costly it is to depart from this system. Argentina suffered from this problem, in spades. I knew how dangerous the political economy of strong pegs was by the time of the Argentine crisis, but there wasn't much you could do about it at that stage. One reason it took me a while to figure that out is because I was still struck by the Israeli experience, where they pegged the rate, but adjusted it a few times, then began a crawl, and eventually floated; Poland did something similar. But Israel had enough control over capital flows to manage that, whereas the Mexicans didn't.

O: What about the later crises?

The Asian crisis, followed by the Russian crisis, the LTCM crisis in the United States, and then the Brazilian crisis, certainly kept us busy, and at times frightened. From about October 1997 till March 1999, when it became clear that Brazil was stabilizing, there was an almost physical pressure hanging over us. I once read an account of some aspect of the Korean crisis, and wondered why I couldn't fully remember that event – and then realized that was because we were dealing simultaneously with the Indonesian crisis, which was at its height at the same time.

The Russian crisis was particularly dramatic. We saw the storm clouds gathering, especially from May 1998, but there was nothing that in the end could be done to stop the crisis from happening. The last hope was the IMF loan plus the attempted debt restructuring in July 1998. When that failed, the collapse was guaranteed. It was just a matter of how it would happen, and what the consequences would be for Russia and the rest of the world.

Global capital markets were hit very hard after the Russian devaluation and debt default in August 1998, and in September it seemed that the emerging market debt crisis was spreading to industrialized country financial markets. Fortunately the Fed acted resolutely by cutting interest rates in October 1998. It was followed a bit later by the ECB, and a few months later the global crisis seemed to be over.

Of course, we did much more than deal with crises at the IMF. I was there during the transition process of the former Soviet-bloc economies, and I think we helped those countries a lot. And then there were other economies that were not in the headlines, where I thought we made a positive difference, for instance Jordan.

So my time at the IMF was really an absolutely fabulous experience. I was fortunate enough to be at the center of a financial storm in a period of immense historical interest, with highly talented colleagues. It was a great privilege to be in that position at that time.

O: On to your most recent move, you joining Citigroup

I decided to leave the Fund at an age when I could still have another career. I debated long and hard, very hard, among going to an academic or research institution, or becoming an independent – to work at home, act as an economic adviser, take part in conferences and sit on boards – or joining a financial institution. I decided I would like to join a financial institution, to try my hand at something at which I might be good, and for which my previous experience would be a benefit. I'd never been in a private sector and it interested me. That's how I decided. And among the alternatives, Citigroup looked the most attractive. Bob Rubin, Jeff Shafer, and other colleagues I knew, were there.

Working in the private sector is very interesting. Our team is as good as you are going to find anywhere. The challenges are intellectually as tough as you are going to find anywhere. The organizational problems in an organization of 280,000 people are more severe than you are going to find almost anywhere. And

I am able to continue to do economics because among other assignments, I am the head of the country risk committee. I think about finance, because a lot of the guys are doing finance, I think about the macro because we are talking about what is going to happen in the US and other economies all the time. There are fascinating human interactions all around the place. So it is a very exciting, intellectually very exciting, life.

O: Is having been an academic an enormous plus or minus?

I used to say in the IMF, when someone would try to dismiss an argument by saying that it was academic, "You mean the conclusions follow from the assumptions and the arguments are logical?"

At the IMF my background was probably an ideal fit. Here the finance side is more important, and fortunately I do have a lot of finance from various stages of my MIT career. It is immensely useful to have the intellectual framework of a macroeconomist. It probably doesn't matter that much for the guys who are trading. Some of them are very good at trading, even though they probably don't have a macroeconomic framework in mind. What they have is experience, and smarts. They probably do have some framework but it is not the one I have. We can have good conversations, and we probably educate each other. But I don't think like they do and they don't think like I do, and they are better at what they do, and I am probably better at what I do.

O: Are there weaknesses of being an academic in a place like that? Do we think too much. Do we see both sides too much? Are we a bit slow?

The caricature would be that we dither. A possible weakness of being an academic is that you don't understand you have to make decisions even when you don't know everything. But I don't think most academics would be subject to this problem when put in a situation where decisions have to be made. If they didn't like making decisions, that isn't where they would end up. It was the same at the IMF. At the end of meeting you had to make a decision. So you did.

O: How often did you find you had to make a decision based on much less information than you'd have wanted?

In a crisis, much of the time. That's almost the definition of a crisis. In a crisis you find yourself saying, we have to choose a or b… or c. You would always look for the d or the e. But things are collapsing now. And if they collapse, many people's lives and well-being will be even more badly damaged. The situation has to be saved in some way, you object to moral hazard and you object to bailouts, but in the end you ask what's the best among several difficult choices. You would like a few more days or weeks to think about it but you don't have that time.

It's the same here in the private sector. You cannot sit something out when you are not sure of what is happening, for you are never sure; you have to take bets. One of the many strengths of Bob Rubin is that he is totally calm about that. Very rational, committed to probabilistic thinking. That means he does not commit the fallacy of: If I lost money yesterday I made a mistake yesterday. A fallacy many of us are inclined to commit. Of course, if you lose money every day for a year you probably made a mistake.

O: Back to academic economics. How do you see the state of macro today?

I am very happy the field seems to come back to dealing with a world I can recognize, or at least is trying to deal with a world that I can recognize. There was a long period during which arguments were more about methodology than about the way the world worked – the rational expectations revolution or the equilibrium revolution – seemed set to conquer the world and you didn't know what to do about it. That tension is reflected in our book *Lectures in Macroeconomics,* because we allowed in those somewhat (*laughing*) inconsistent chapters in the end of the book. I was very happy to have them in the book. There just *is* a useful macroeconomics.

I see two evolutions today. One is that there is a bunch of researchers who do a lot of really relevant and very good work, which I think is much better today than it has ever been. There is also much progress in our use of data. For many years, we said "with the data we now have available we should be able to understand much more." Well, finally this is happening and we are learning a lot from these large databases. The second, which makes me very happy, is the beginning of the end of the great split between freshwater vs. saltwater economics. While the split is still evident, convergence is also clearly under way. And I think that is very healthy for the profession. I pick up the QJE frequently, the AER likewise, even the JPE, and find myself even if not reading the articles then at least interested in the conclusions and saying "Gee, this is really interesting. Gee, this really makes a difference." So I think we are way better off. And I am glad that all this happened. Many people get the credit, Larry Summers prominently among them, and many others at the National Bureau.

O. : The next question is related. Tell us about macro seen from the various seats that you've had. Are there big holes, big issues that we are not working on?

When I had to deal with a problem, I was more impressed by the fact that there was typically a model that was useful than by the fact that nobody had ever worked on something. I don't know what the big holes are. Are we working on the right issues? The right issues in macro are the ones about what determines long-term growth, what determines productivity growth, what determines why some economies have these structural problems. We are certainly working on it very intensely, and I think with considerable insight. The hard part, as I said before, is the political economy, and how to make things happen.

Let me take the risk of saying something that I cringe at when I hear it: Which is that I think we more or less know the policies that would work. Saying that always makes me nervous, but now I've said it.

O: Including Africa? Forgetting political economy issues, we know what should be done?

You're right. I am less convinced of that for Africa than I am for Asia. Then I have to ask myself why. It comes down to human capital. We have underestimated how much work needs to be done on two things: human capital creation, and the closely correlated issue of the creation of institutions supportive of economic activity and growth. So those issues are there. But to say nobody is working on them is certainly not right. Everybody is working on them. So I don't expect some blinding revelation to emerge that will say that, if only people would work on x, y and z, we could all do things much, much better.

O: You just focused on the long run, not on the short run at all. Are you buying the Lucas argument that it is less important? Or do you think we actually understand it well enough?

I think we are in somewhat better shape on the short-run. There are two aspects of the Lucas argument. One is: In the long run growth is all that matters. That's something that has been understood since the days of Adam Smith. That is why people worked on growth theory for so long. The other proposition is that recessions don't really matter because the loss of consumer surplus is five decimal places down. That strikes me as a problem with the calculation or in the welfare function.   So that is not what leads me to focus on growth. It's that, growth problems aside, we know a lot of what to do on the macro end. You may say "well, you need to tell me how to get fiscal budgets balanced at times of prosperity so I can use fiscal policy actively in recessions." Well that's political economy again. And there it's harder.

O: One last question here. You can answer it if you want. Advice to young researchers.

It is critical to get yourself the right set of tools when you are young.

O: You used to say this when I was a student. You haven't changed your mind.

I haven't changed my mind on this topic in the least. If you look at our profession, there are people I know who are extremely gifted, but who spend all their lives apologizing for not being able to do econometrics or algebra. Some, like the late Charlie Kindleberger, manage to surmount that. Some don't. And you need to get yourself the self-confidence of knowing you can handle these techniques, and can operate at a fully professional level.

Once you've got that, then you need to find a problem that is both interesting and important – potentially important. I've been surprised over the years by how often friends who worked on things I didn't think mattered, later turned out to have hit a jackpot. One of my MIT graduate student friends wrote a thesis on gas pipelines in Canada. It seemed to me as totally lacking in interest, even though he was interested. But his ship came home in 1973. And so, you need to believe your topic has the potential to be important at some time. And you should certainly find it interesting.

Once you've done that once, it becomes easier and easier to find interesting problems as you do more research.