General Discussion:
The Evolution of Economic Understanding and Postwar Stabilization Policy

Chair: Stanley Fischer

Mr. Fischer: I leave David and Christina Romer to answer what Samuelson really meant. David, however, you are going to divide it? Why don't you come back fairly quickly on what Tom Sargent said and then we'll turn to the audience?

Mr. Romer: Let me just provide three responses. First of all, thank you very much for the kind words and insights. Second, on Samuelson and Solow, Tom said we give them a dramatic role. If you read our paper, we don't give them a dramatic role. We mention them in the introduction. We don't claim to have studied that part of the intellectual history and the link between what Samuelson and Solow did and subsequent policy carefully enough to have a view on whether their role was dramatic or not.

Third, and more importantly, Tom gave a long list of ideas that don't get a big role in our paper. It is an interesting list. It has a lot of things that surely played some role, and so I want to say thanks for the suggestions, and that there is surely something there. But I don't want to go too far. We left those things out of our story because we thought, in looking at the big picture of policy and, indeed, at lots of the medium-sized twists and turns, that those things are not central.

To give one example: We don't think, and we are in company with a lot of people, that what happened in the 1960s and 1970s was that people fell into a Kydland and Prescott equilibrium where you know exactly what is going on, but because you can't precommit, the optimal policy for you to follow is one that involves high inflation. So, that is one idea that we would say to a first approximation just was not important. Some of the others clearly did have some role, but we feel that they are not the essence of the matter.

Ms. Romer: As the historian on the team, I wanted to take a second and talk a little bit about research methodology and about the narrative approach. Here I'll actually invoke Stan Fischer, who had a much more important role than he will ever know in shaping our views. In our introductory graduate macro class, he said, "How do we know money matters?" It wasn't a VAR or some other complicated regression. It was Friedman and Schwartz.

I think that this view has definitely affected our research strategy. Often narrative evidence is, in some ways, the most powerful. If you think about the question that we are asking—what were the beliefs of policymakers—there are not a lot of data one can use. We tried to get somewhere with the Fed forecasts. But this is inherently a question of what people were thinking. To answer that, you need to read the narrative sources.

I also want to take exception to the idea that the narrative approach is literary. I prefer to think of it as science in a different form. It is not getting a few fun anecdotes that make for better reading. It is, in fact, taking serious sources, reading them systematically, carefully, confronting them with a hypothesis. If it is done well, it is certainly a legitimate technique. In terms of how subjective it is, all of us who have done empirical work know that there are a multitude of decisions and judgment calls that one makes any time one does research. That is true of both narrative research and empirical work.

The last thing I'd say concerns verifiability. How is any work ever tested or verified? It's by someone else trying to replicate it. It is true of empirical work: another researcher gets the data and tries it again. It is true of narrative work: someone else looks at the same sources, or at new sources, and sees whether they reach a similar conclusion.

Mr. Fischer: Thanks very much, Christina. We'll take a couple of questions at a time and then come back to the contributors.

Mr. Blinder: It is very flattering for those of us in academia to imagine that policymakers were sort of trundling along after academic fashion. I agree with Tom that while, at a 10,000-foot level there is mostly truth in what David and Christina write about, there are at least two things that to me seem like very large omissions. (By the way, these overlap Tom's excellent remarks quite a bit.)

First of all, when you read the paper you get the impression that the level of confidence in the natural rate theory, and in the NAIRU itself, was higher than ever existed. It is true that views shifted. That is what I mean by taking the 10,000-foot view. But, if my colleague Mark Watson were here, he would insist that the standard error of the estimate of the NAIRU is about 3 percentage points. You can rationalize pretty much any policy with a 3 percentage point error band on the NAIRU.

Related to that, the idea that erroneous beliefs let the inflation cat out of the bag in the 1960s and 1970s does not take into account the Vietnam War. We know from narrative history that Lyndon Johnson was being told time and time again, by people who would call themselves and be called Keynesians, that he should cut government spending or raise taxes to prevent inflation. But he didn't do it for political reasons. So, it wasn't economic beliefs that led policy astray. And ditto, there is almost nothing in the paper about the fact that there was an election in 1972. I'll just leave it at that. Everyone in this room knows about the election of 1972.

And, finally, there is the Volcker disinflation. Beliefs do not change on a dime, but Fed chairman can. Paul Volcker became chairman of the Federal Reserve in August 1979. He was a very different person from Miller or Burns, who preceded him. He had a mandate to fight inflation and, in fact, he didn't need much of a mandate because he wanted to do so. So, while economists beliefs didn't turn on a dime, the Fed's policy did because Volcker became chairman of the Fed.

My second point overlaps, amplifies, and makes concrete a couple points that Tom made. There is very little in the paper about what I think is another very important aspect of the evolution and the interaction of beliefs and policy: understanding supply shocks—what they are, how they affect the economy, and how policyworkers might or might not react to them. If you remember the 1970s, this realization was a very big deal in terms of reorienting the thinking of both academics and policymakers. And, finally, once again just amplifying and making concrete something that Tom said, I point you to Orphanides' very excellent paper on the episode in the 1970s. He shared how long it took to learn that the productivity trend had been deflected downward, and how that might easily have led policymakers and others astray. Many people have pointed to the 1990s as the obverse case, where the productivity trend accelerated, and it took a similarly long time for people to catch to the fact that productivity was higher.

Mr. Fischer: Thanks. Allen Sinai, please.

Mr. Sinai: The paper is really quite excellent. I want to hone in on some of the raw material you used in your work—the Greenbook forecasts and the association of Greenbook forecasts with the models and, therefore, the policy actions. Now, I'm an outsider. My perception is that that is one input into the policymakers' actions and votes and that we do see in recent years twice-a-year forecasts that are consensus forecasts of the Federal Reserve, which are the forecasts of each of the members who have taken information from more than just what goes on internally. That is just an example of a question: Why should we conclude that that source of evidence really tells about changing beliefs or models that policymakers in the monetary sphere have used to make policy? I think I have doubt about that.

I have an example of this doubt, which relates to what Alan Blinder said. That has to do with the natural rate, in your comments, the stress on the natural rate as the model framework for looking at what the central bank has been doing. As an outsider, it is my perception that the natural rate, as a practical tool for making policy, was essentially rejected in the mid-1990s. Why do I say that? If the natural rate forecast had been followed at that time, which was somewhere in the 6 percent area, we obviously would not have seen the kind of monetary policy actions in the second half of the 1990s that we saw because that natural rate, if believed and if that were the framework, would have led to rises in interest rates rather than the actual actions we saw.

Outside of this, a very quick nitty-gritty comment. In paragraph one in the first page—just a minor thing, "inflation has been firmly under control for more than twenty years now." I don't think inflation was firmly under control nor that we can say that it was firmly under control in 1980, 1982, 1984, and 1985. You might say that we were headed on a path toward getting inflation firmly under control, but I really don't think your statement is right.

As the Chairman stressed in his comments, presumably meaning that we've been through a bubble in the equity market, what could we have done, or not done, about it in monetary policy to deal with it? Is it possible that the view of a policymaker and the way it is expressed could, in itself, contribute to the formation of a bubble in asset prices—namely, the goal of maximizing the sustainable rate of economic growth which was not stressed I think by Romer and Romer and probably should be stressed because, it has been such an important part of the last five to ten years of policymaking?

Mr. Fischer: I apologize Allen, but we must move on with our discussion. Thanks. Allan Meltzer, please.

Mr. Meltzer: I certainly agree with one point that is implicit and sometimes explicit in the Romer paper, and that is that very often the Fed staff certainly and perhaps the policy board reflects very much the mainstream views of the economic profession. There is real coincidence. In writing about the 1920s and 1930s, I believe that if you would change the composition by just drawing at random, you would have found the same belief in the gold standard and the real bills doctrine and so on very rampant. So, in general, I take that point.

I want to make a couple of comments that are smaller than some of those that have been made but that might help to improve your paper. First, one of the things that is very striking to me in reading the narrative history that you wrote is that the words "real interest rate" do not appear until very late in the 1970s. A big part of the problem in the 1960s and the 1970s was that Martin and other people, including people in the academic profession, believed that nominal interest rates of 6 or 7 percent were really very high rates. And even though they had learned to think about expectations, they had not learned to think about real interest rates and they don't use the words "real interest rates" until very, very late in the period. So, that was a big thing. That was the same problem they had in the 1920s. They just didn't believe that interest rates of 6 or 8 percent were politically acceptable.

Second, I think it is very difficult to explain the 1950s and 1960s without recognizing that Chairman Martin was there during both periods. If there is anything that I know from having met with him and his consultants at various meetings and reading what he had to say in the minutes, it is that he certainly did not have a macroeconomic model. He would have been the first to deny that he had anything like a macroeconomic model. He didn't like macroeconomic models. I am not even sure whether he liked economists. So, you need to explain how this same person—Martin—could have had a low-inflation strategy, and very decidedly so, in the 1950s and then given it up in the 1960s. How did he happen to help finance the Vietnam War?

I would say that for most of the 1950s, the dominant personality in terms of thinking about how the Fed reacted was Winfield Riefler. He did not have an economic model. He had not had an economic model in the 1920s, except for a very short-term relationship between member bank borrowing, or free reserves, and what the Fed was doing. What Winfield Riefler had in his head, which he says many times in those minutes is, "You have to look at the relationship between money growth and the rate of growth of output." And that got lost. Martin didn't really believe much in monetarism. In fact, he didn't believe in it at all. But Winfield Riefler did and, to a considerable extent, so did Thomas, who took his place after Riefler retired in the late 1950s. That is an important part, and you need to explain something about how Martin changed.

The third thing I would add, which I think is missing in your account, is the role of politics. You have Eisenhower at one end and

Reagan at the other. That makes a considerable difference to what is going to happen both to fiscal policy and to the kinds of pressures that the Fed is going to be under. Then, you have people in between, like Johnson and Nixon. Nixon never tired of telling Arthur Burns, "Arthur, you warned me about the 1960s but cost it me the election. You are not going to do that again, are you Arthur?" Or, you have Lyndon Johnson who tells Walter Heller, "Call up Robertson and say, 'If you are going to be reappointed, will you follow your president or will you work against your president?'" And Robertson, according to Heller, says, "Of course, I'll follow my president." So, Johnson says to Heller, "Okay, tell him we are going to reappoint him." Politics makes a big difference. And politics doesn't play a role in your story but it certainly played a role in the pressures that were on the Fed in the 1960s and the 1970s—from Lyndon Johnson, Richard Nixon, and probably the Carter Administration. And finally, of course, what got lost in the late 1960s and the 1970s was the belief that money growth had something to do with inflation.

Mr. Fischer: Thanks very much, Allan. Just to clarify, Allan is also writing a history of the Fed, so he has taken a close look at the record. We will turn now to the Romers for a quick response. We then have four more questions. I would ask the next questioners to be brief.

Ms. Romer: I will be brief too. I want to make one thing clear: We are very much not about where policymakers' beliefs came from. One of the ways that we limited our paper is to only look at what policymakers believed not why they believed it. The role of academics and the role of learning are at some level outside our story. It is not that we don't think these issues are important, it is just that they are beyond the scope of this study.

Likewise, on the role of politics, the way we envisioned our question is—how far can we get in explaining the changes in stabilization policy with only the change in policymakers' beliefs. Again, I agree that an element of politics is certainly important. What I think surprised us is how far we could get in explaining the evolution of policy with only views.

On Chairman Martin, one thing to say is he may have not said that he had a macro model, but he had a framework. He was making decisions, he had views about how the economy worked, and what inflation did to the economy. You can't make policy without some view about how the economy works. On this idea about how quickly the framework changes, and how Martin changes, it is not necessarily that a particular person's view changes. Rather, what may change is the belief carrying weight within the FOMC. Our view of Martin is that at some point—and again this is speculating and something we are working on—loses faith in his own framework, the framework that had inflation being very costly.

In response to Allen Sinai's comment on the Greenbooks, again we're looking for data. We were trying to get some indicator of policymakers' beliefs other than narrative evidence. When the Fed staff members make their forecast, does that reflect the Board? Does it influence the Board? I guess my naive view is that if the staff were coming in with a wacky model that wasn't being supported by the members of the FOMC, they wouldn't be there for long. So, I would still stand by this notion that there is some relationship between the model inherent in the staff forecast and the beliefs of actual policymakers. And, whether the modern Federal Reserve rejected the natural rate hypothesis in the 1990s, I think the much more plausible view of what happened is that they kept the framework and they greatly lowered the estimate of the natural rate. So, I don't think you have to say they threw away the whole model.

Mr. Romer: Two very brief things. Alan cited the standard error for estimates of the natural rate. That was a paper published in roughly 1997. It was a stunning result. Reading especially the *Economic Reports* of the 1960s, you expect from their tone to see the second and third decimal places on their estimates of the natural rate. They really think they understand what is going on, and they are willing to discount evidence that goes against it. They are willing to work very hard to move the economy to what they think is the natural rate.

Regarding politics, Tom Mayer had a line that I found very persuasive. He said, "If the political story were really central, what you would see in reading the records of the Fed, is that the Fed is straining at the leash all the time." You occasionally see a Fed that is in conflict with the White House. You don't see a Fed that for two decades is trying to do something that it wants to do but feels grossly constrained by outside pressures.

Mr. Fischer: Thanks.

Mr. Cotis: I think this paper is fascinating, but it's empirical part may be less convincing than it could be. I think one big omission is supply-side shocks and more specifically oil shocks. This omission has an impact on some of your empirical findings, and it might lead you to overstate a little bit your case increased knowledge of the economy leading to better performance of policy makers over time.

Let's look at two or three illustrations—accuracy of the Fed forecast, for instance. If we look at chart 1 in your paper, we see that errors are massively concentrated during the oil shock period. Like everyone else, Fed forecasters were taken by surprise and their forecasts were too inert. After that, we moved to a much steadier inflation regime, and the accuracy of the forecast got much better—basically because the task was much easier too.

The second illustration deals with the natural rates of unemployment implicit in the Fed forecasts. Had you controlled for variations in the terms of trade in your calculations, you would have ended up with a much smoother series and a lot less hiccups to explain away through political considerations.

The third illustration is distance to the Taylor rule in terms of interest rate setting. The period where the mismatch is the biggest by far is the oil-shock period. I'm not sure that a very simple version of the Taylor rule provides us with the best gauge to assess the accuracy of monetary policy. In these very special circumstances, we need to disentangle between underlying and actual inflation. So, maybe you are overstating a little bit your case by really not taking into account the changing in the nature of shocks hitting the economy over the period.

Mr. Fischer: Thanks very much. The final three questions or comments will be from Bill Poole, Philippa Malmgren, and Larry Summers.

Mr. Poole: I think the paper should give more emphasis to the debates on monetarism. I would comment on two aspects of it. Certainly, the debate over the role of money was continuous during this period. It had an important bearing on Fed policy and on market behavior. As the 1970s went on, you saw more and more market responses—interest rate responses—to the weekly announcements of money growth clearly was important in the policy turn in 1979. So, it had a real bearing on what happened in monetary policy.

Secondly, there was a big debate over the relative roles of monetary and fiscal policy. In the 1960s particularly, monetary policy within the Federal Reserve was thought to be almost a sideshow, and the critical issue the Fed saw at that time was the need for fiscal response to the Vietnam War. That was part of the reason why the Fed's response was so delayed. Here again, this was an important academic debate—the relative role of money and monetary and fiscal policy. The Fed was on the wrong side of that—certainly in the mid- to late 1960s.

Ms. Malmgren: I thought the central idea of your paper—that the objectives of policy have not changed over the years: high growth, low inflation, stability—are all there. But the relative mix of them has changed at times. There were three pieces that could be added that would help explain the change in the mix.

One is (and I'll pose it in the form of a question): Has the quality of the data that the policymakers are working with changed sufficiently over time to help explain why certain decisions were made? In other words, the absence of certain information, or low quality of it, perhaps is an important factor.

That feeds into a second point, which is the speed at which policy influenced the economy. Has that changed over time? Is that an important feature of that relative mix?

And third, something you hinted at but did didn't go into, is whether the personal and recent experiences of policymakers influence that relative mix as well. In other words, it is that old idea that somehow central bankers can sometimes be dominated by looking in the rearview mirror at the last accident that occurred. I am wondering whether there might be a role for that to play in the analysis that you undertake.

Mr. Summers: I thought the paper was terrific. The first comment I was going to make was on Allan Meltzer's comment about nominal and real interest rates, which feels like a major issue to me. Part of what was terrific about the paper was that it stayed away from the explanation of specific events in terms of political factors. An important argument for that approach is that if you looked at the broad history of England and, I suspect, much of Europe through much of this period—low inflation, expansion, of getting it back under control in the 1980s—it would be parallel and that speaks to the importance of what the paper is trying to analyze, which is the broad Zeitgeist in which policy was operating.

That said, I thought Tom Sargent had it more right than the authors did on the 1970s. It seemed to me that what the authors attribute to misleading estimates of the natural rate and to belief that disinflation works painfully could also very well be understood, as everybody had a built-in expectation that inflation was just going to continue, and it was so tough to fight that it wasn't worth the bother. Then, there was a growing understanding in which the academic literature probably played a small part of the importance of credibility—doing things to gain credibility. The heavy focus on the notion of independent central banking that became a part of discussions of this kind in the 1980s, but was not nearly as much a part of discussion in the 1950s, supports that interpretation. So, I would tell the story somewhat differently for the latter period of policy error.

Mr. Fischer: Thanks, Larry. Christina and David. Do you have any final comments?

Ms. Romer: I want to address this issue about the real interest rate. While I certainly agree with Allan Meltzer that it is not mentioned, I think that is too simplistic. In particular, when you read the Federal

Reserve records for the 1950s, policymakers were not stupid. And, although they don't say the term, "the real interest rate," there is a lot of discussion that the nominal rate is high because expectations of inflation are high. They certainly understand that there is something else out there that matters. There is a view that somehow the 1960s and 1970s were all just an accident: monetary policymakers were looking at the wrong indicator, they were looking at free reserves, or the nominal rate, and they just missed what was going on. But what we are talking about are gross changes. These are not subtle little changes over the last fifty years. The 1960s and the 1970s had a very different policy—there was an extreme expansionary bias to policy. This gross change is coming precisely from policymakers' model of the economy.

Regarding the point about the changing objectives, what we want to say is that objectives didn't change somehow for intrinsic reasons. Policymakers didn't wake up one day and say they care more about inflation than before. I believe that their objectives changed because their model changed. If you have a model where inflation is unbelievably costly, you care more about it. It is not somehow distinct from the model.

Mr. Fischer: David, any last word?

Mr. Romer: I don't have a lot to add to that. On this question of whether these things come from tactical errors or supply shocks: Looking at what they thought was going to happen to the economy will get you a long way from these issues about tactics and so on. If you look either explicitly at the forecasts or you look at what they were saying, they were thinking most of the time they were going to operate the economy at what, in retrospect, looks like a pretty high level. Depending on the era, either they thought inflation was going to nicely go away by itself or, in some periods, that inflation would persist. They were just willing to live with that. I can't pronounce complicated German words, but I think Larry is right to say it is the Zeitgeist of the era that is the driving force and not the narrower things.

Mr. Fischer: One last comment that Bill Poole has left. Before the session, Bill was saying, “We also need to remember that we had a pegged exchange rate in the 1950s and into the 1960s.” That had an influence, probably, on creating more coherence in William McChesney Martin’s thinking than various comments have implied.