Federal Reserve Board Oral History Project

Interview with

David J. Stockton

Former Director, Division of Research and Statistics

Date: April 22, 2013

Location: Washington, D.C.

Interviewers: Jaime Marquez, David H. Small, and Lawrence Slifman

Federal Reserve Board Oral History Project

In connection with the centennial anniversary of the Federal Reserve in 2013, the Board undertook an oral history project to collect personal recollections of a range of former Governors and senior staff members, including their background and education before working at the Board; important economic, monetary policy, and regulatory developments during their careers; and impressions of the institution's culture.

Following the interview, each participant was given the opportunity to edit and revise the transcript. In some cases, the Board staff also removed confidential FOMC and Board material in accordance with records retention and disposition schedules covering FOMC and Board records that were approved by the National Archives and Records Administration.

Note that the views of the participants and interviewers are their own and are not in any way approved or endorsed by the Board of Governors of the Federal Reserve System. Because the conversations are based on personal recollections, they may include misstatements and errors.

Contents

Background 1

Early Years at the Board (1980s) 6

Working at the Board in the 1990s 13

Terrorist Attack on the United States on September 11, 2001 (9/11) 24

Communications Policy under Greenspan 25

Financial Crisis in the 2000s 25

Functions of the R&S Division 28

Financial Crisis in the 2000s Continued 29

Inflation Target 33

Composition of the Board 33

Financial Crisis in the 2000s Continued 34

Relationship between R&S Staff and Board Members 39

Working with Fed Chairmen and Other Board Members 43

Forecasting 47

Transparency 49

The Structure of the Board 50

The Structure of the FOMC 53

Bank Supervision and Regulation 56

Congressional Oversight 57

Life after the Fed 59

The Structure of the R&S Staff 61

Former R&S "Think Tank" Group 62

MR. MARQUEZ. Today is April 22, 2013. This interview is part of the Oral History Project of the Board of Governors of the Federal Reserve System. Today we are interviewing David Stockton, who began working at the Board in 1981 and was the director of the Division of Research and Statistics (R&S) from 2000 until 2011. I am Jaime Marquez, senior economist in the Division of International Finance. I'm joined by David Small from the FOMC (Federal Open Market Committee) Secretariat in the Division of Monetary Affairs [and Lawrence Slifman, a retired senior adviser in R&S]. This interview is taking place in the Federal Reserve Board, Washington, D.C.

Background

MR. MARQUEZ. Tell us about your background, your undergraduate and graduate work.

MR. STOCKTON. I was an undergraduate at the University of Connecticut, and I got my bachelor's and master's there in four years. It was a wonderful experience with great professors, and great opportunities.

MR. MARQUEZ. In economics?

MR. STOCKTON. In economics. I also minored in mathematics. I went from there to Yale University. I had received a Danforth Fellowship, which I don't think exists any longer. The Danforth family funded private fellowships for people who intended to become university professors, which was exactly what I was intending on doing when I went to Yale. At the start, my interests were more in public finance and environmental economics. But the times were really interesting on the macroeconomic front in that period of high inflation.

MR. MARQUEZ. What year was this?

MR. STOCKTON. I started there in 1976. The department was quite strong in macroeconomics, with James "Jim" Tobin, William "Bill" Brainard, William Nordhaus, and Tjalling Koopmans.

MR. MARQUEZ. Larry Slifman, who is part of the interviewing team, has arrived.

MR. STOCKTON. I switched from public finance to macroeconomics. I did my thesis under Jim Tobin and Bill Brainard. And, I was the teaching assistant for the graduate course in macroeconomics at Yale. I had a great experience there.

MR. MARQUEZ. When did you graduate?

MR. STOCKTON. I got my degree in 1983. I came to the Board in 1981 with my dissertation mostly written but not complete.

MR. MARQUEZ. What was it about economics that fired you up?

MR. STOCKTON. When I was in high school, I thought that I'd be interested in studying either physics or chemistry, but I had interests in politics and social issues, and it just seemed like a great blend. I took an economics class my senior year in high school. I got jazzed thinking about social issues in a more analytical type framework than you would typically get in sociology or political science.

MR. MARQUEZ. Hence, your interest, your original interests, in public finance or environmental issues.

MR. STOCKTON. Yes, certainly the public finance part of that. I worked for the Massachusetts Audubon Society for a few years, with a fellowship. I was very interested in environmental economics. Working for the Massachusetts Audubon Society, I saw the difficulties of being an economist. For business interests, they thought you were just a tree hugging, anti-growth kind of person. By contrast, the environmentalists would look at an estuary

and say, "That's absolutely priceless." But an economist says, "No. That's not true. That's not really priceless." Nobody on either side really liked or paid much attention to economists in that area, so it made it difficult.

MR. MARQUEZ. Tobin and Brainard were your advisers.

MR. STOCKTON. Yes.

MR. MARQUEZ. What are your recollections from interacting with them?

MR. STOCKTON. Tobin was an incredibly thoughtful, humane person. He was a terrific adviser. He cared a lot about teaching not just his own research. That was wonderful. Brainard was incredible intellectually. He had lots of energy. He was quite demanding as a thesis adviser, but I really got a lot out of working with him.

MR. MARQUEZ. So you were interacting mostly with him and then Tobin was overseeing?

MR. STOCKTON. I interacted mostly with Tobin and, to a lesser degree but quite a bit, with Brainard. Those two were close collaborators as well.

MR. SLIFMAN. What was the subject matter of your dissertation?

MR. STOCKTON. The subject matter of my dissertation was the effect of inflation on relative prices. One of the big issues at the time concerned better understanding the costs of inflation. There was a line of research mostly done in the Rochester–Chicago school that looked at the effects of unanticipated money shocks on relative prices. Such shocks were hypothesized to create confusions between nominal and relative prices. My thesis was taking more of a Keynesian approach and argued that costs of adjustment to relative price changes could lead inflation to create some distortions of relative prices through that mechanism. There was some skepticism on the part of Tobin because he thought that the costs to inflation were small, and I

was trying to argue in my research that the costs of inflation might not be that small. That was an interesting experience. A second element in my work was to demonstrate that causality could flow in the opposite direction: Large changes in relative prices could cause movements in aggregate prices.

MR. SMALL. In that period, you can frame some of the debates as: Tobin versus Friedman, IS versus LM, stable versus unstable Phillips curve. Do you remember how that was presented in the coursework, on the lecture board, in debate and seminars? Yale versus Chicago is another way of saying that.

MR. STOCKTON. When I was at Yale, that sort of debate was transitioning between the Tobin–Friedman debate to the Tobin–Lucas debate—rational expectations and underlying complete market-clearing-type models that people such as Robert E. Lucas, Thomas J. Sargent, and Neil Wallace were proposing. In those models, there was virtually no role for the government, and the government could only do bad things to the economy. Tobin pushed back on that notion quite hard. One could argue that Tobin was not receptive to some of the changes taking place in the field that probably were useful advances in macroeconomics, such as thinking about rational expectations. But he proved prescient in resisting the fad of flexible-price market-clearing macro models.

During my third year, when I was just getting started on my thesis, John Taylor was a visiting professor. I think he was at Columbia at the time. He spent a year at Yale. I spent quite a bit of time with John discussing some of the same issues, and I liked some of the approaches that he was taking.

MR. SLIFMAN. That was the overlapping contracts?

MR. STOCKTON. Yes, it was the overlapping contracts. And he was working with Ray Fair on the Fair–Taylor model. It was a massive computational effort at the time. John was taking macroeconomics in a direction to incorporate rational expectations while trying to understand why sticky prices mattered. Both of them were good experiences. Tobin never lost sight of the big issues of macroeconomics, like unemployment, which featured heavily in his own mind and was conditioned by the experience of the Great Depression. But still you could look out there at the world in the late 1970s and see that persistent high inflation created serious costs.

MR. MARQUEZ. In 1980, right?

MR. SMALL. Did you have any recollections from him about the early Kennedy years? Fairly or unfairly, he's often tagged as too strongly believing in this stable Phillips curve, but that belief turned out to be a big mistake.

MR. STOCKTON. In the early 1970s, the Federal Reserve Board published a volume entitled "The Econometrics of Price Determination."[1]

MR. SLIFMAN. From the conference in October 1970.

MR. STOCKTON. If you read Tobin's overview in that volume, he covered a lot of the issues that were going to unfold over the next decade. It was not just Tobin's overview, but all the papers in that volume gave a window into the looming debates about inflation. It was a remarkable collection of papers foreshadowing future developments. Tobin thought there was a flatness to the Phillips curve at low rates of inflation that was brought about by downward nominal rigidities, an issue that appears to have come into play during the Great Recession. He

[1] Board of Governors of the Federal Reserve System and Social Science Research Council (1972), *The Econometrics of Price Determination,* proceedings of a conference held in Washington, D.C., Oct. 30–31, 1970 (Washington: Board of Governors).

was certainly aware that inflation expectations mattered in the Phillips curve, but the models that he was thinking about didn't capture the upward drift in inflation and inflation expectations of the magnitude that occurred in the 1970s, and then the subsequent disinflation. There was a view—and it wasn't just Tobin's view but one often characterized as the "Brookings view"—that the disinflation was being pursued in the early 1980s would require a decade of high unemployment to bring inflation down by some modest degree because people thought that the Phillips curve was very flat or inflation expectations would not adjust very quickly.

MR. SMALL. One piece of lore that's used to show how off the mark the old school was is the claim that the Tobin–Brainard model didn't distinguish between nominal and real interest rates. Do you remember anything like that?

MR. STOCKTON. No, I don't, but I'd be shocked because we certainly discussed that distinction in graduate courses.

MR. SMALL. I'm glad I characterized it as lore.

MR. STOCKTON. It probably was lore.

Early Years at the Board (1980s)

MR. SLIFMAN. Tell us about your career progression at the Board, and then we'll go back to talking about when you were a young grunt in the trenches.

MR. STOCKTON. In 1981, I started as an economist in the Wages, Prices, and Productivity Section, with responsibilities for inflation analysis and forecasting. I did that for four or five years before I went on to coordinate the staff economic projection. In November 1987, I was promoted to the official staff and was made chief of the Economic Activity section, shortly after Mike Prell took over as the division director. Then there was a series of promotions.

MR. SLIFMAN. When did you take over as director of the division?

MR. STOCKTON. In June 2000.

MR. SLIFMAN. When you started at the Board in 1981, inflation was probably 12, 13 percent, depending on how you measure it. Talk about the effort in helping to put together the forecast on the inflation. At that time, what was the prevailing analytical framework being used by the staff to think about inflation?

MR. STOCKTON. The basic framework was a Robert Gordon-type Phillips curve standing on three legs: inflation expectations, output gaps, and relative price shocks. We used a variety of different specifications of that model. Jim Glassman, Sandy Struckmeyer, and I were looking at alternative specifications, for example to see whether a Barro-type specification of money growth and unanticipated money growth was helpful in explaining inflation. We were also looking at market-clearing rational expectations models, and also at what we would call the St. Louis monetarist models of inflation. We published a bunch of papers comparing the forecast performance of those different types of inflation models.

MR. SLIFMAN. Mostly, the staff was using what could charitably be called adaptive expectations; some might call it naïve, backward-looking expectations.

MR. STOCKTON. The work that we were doing suggested that none of these other models performed all that well. But Phillips curve models, more often than not, got you in the right ballpark, whereas there were spectacular failures with these other specifications. The paper that I wrote with Sandy Struckmeyer in the mid–1980s, involved running different types of

specification tests of these nonnested models and showed that the Phillips curve, by those tests, was a misspecified model but not in ways that were fatal to its performance, at least at that time.[2]

MR. MARQUEZ. At least not misleading for monetary policy.

MR. STOCKTON. Yes.

MR. SLIFMAN. If you look at transcripts or even minutes of FOMC meetings at that time, there's a lot of talk about energy prices or food prices, or this or that, airline fares or whatever. To what extent do you think that kind of disaggregated analysis of prices was useful for thinking about overall inflation? Weren't these just relative price movements?

MR. STOCKTON. That discussion largely predates my appearance at the Board. By the time I arrived, Jim Glassman and I had conversations with you, Larry, already disputing the usefulness of that kind of non-macroeconomic approach. The analytical framework had already moved to something that I felt comfortable with because it was so much more a macro-type approach. Not that the effect of significant relative price shifts on inflation weren't considered; in fact, energy price shocks figured into this type of framework. As noted earlier, part of my thesis was not just looking at how inflation affected relative prices but how relative price changes also affected inflation. So we moved to a more macroeconomic approach to thinking about inflation. That wasn't something I brought to the Board. That was already gaining traction with the staff, although there were several people in the section that were still thinking about the world in that micro framework.

[2] David J. Stockton and Charles S. Struckmeyer (1987), "Tests of the Specification and Predictive Accuracy of Nonnested Models of Inflation, " Economic Activity Section Working Paper Series 71 (Washington: Board of Governors of the Federal Reserve System, March)

MR. SMALL. That was the period of the great disinflation under Paul Volcker. So you had some persistent inflation and massive unemployment that your Phillips curve had to struggle with.

MR. STOCKTON. This particular period, I would characterize as a triumph for that version of the Phillips curve. The models that we were using suggested that there would be a lot more disinflation with that high output gap than most people, including most of the policymakers at the Board, were thinking was going to be the case. Most people thought this was going to be a much more costly disinflation. It was costly, but inflation did come down faster than most people were expecting. While we didn't get the cost of disinflation exactly right, the Board staff generally outperformed other forecasters during this period, as documented by Romer and Romer.[3] Most of the achievement of that advantage, relative to either private sector forecasts or other efficient forecasts, was driven by the forecast performance of the Board staff's inflation forecast in the early to mid–1980s. Whether that was luck or prescience could be open to some question. There was skepticism on the part of the division director at the time about our forecast and the disinflation we were forecasting. And there was some discomfort with the forecast, but ultimately he supported our view.

MR. SLIFMAN. You mentioned that when you were doing your horserace kinds of things, you looked at monetarist models as well, with inflation. To what extent did monetarist considerations play a role in the work that was going at the Board, as opposed to say, St. Louis?

MR. STOCKTON. It was pretty minimal because most folks at the Board did not view the monetarist framework and the accompanying reduced-form model as a reliable descriptor of the inflation process.

[3] Christina D. Romer and David H. Romer (2000), "Federal Reserve Information and the Behavior of Interest Rates," *American Economic Review*, vol.90 (June), pp. 429–57.

In 1983, you, Joyce Zickler, and I did a briefing at Volcker's request on what it would take to get to price stability. It was an amazing request because it came at a time when inflation was still pretty high. And the idea that we would get down to something that looked like price stability seemed a little quixotic. In that briefing, we took head on the monetarist view versus the Phillips curve view. At that point, the Fed had taken its foot off the break and it was beginning to put its foot on the accelerator. Money growth was speeding up rapidly, and a lot of people, certainly the St. Louis type monetarists, were expecting that money growth would show through into high inflation quickly.

MR. SLIFMAN. "Quickly" is the important word.

MR. STOCKTON. Right, we didn't dispute that inflation was ultimately a monetary phenomenon. But, we were arguing that, with unemployment as high as it was, we just didn't think that an acceleration of inflation was imminent. It wasn't just the St. Louis folks who were predicting a reacceleration of inflation. Bob Litterman had a VAR that also relied heavily on money growth that was predicting a significant step up in inflation. That particular briefing was interesting from an historical perspective because it lays out what the staff's thinking was at the time, what the counterarguments were, and the Fed's thinking early on about the possibility of not just living with 5 percent inflation, but what would be required to take inflation down considerably lower.

MR. MARQUEZ. Was this an FOMC briefing?

MR. STOCKTON. Yes.

MR. SLIFMAN. So inflation comes down, and it hangs at around 3 to 5 percent for several years. What was the staff view on why further progress toward price stability seemed to have stalled for a while?

MR. STOCKTON. At that point, unemployment had come back down considerably. And the view was that the natural rate of unemployment was relatively high, certainly higher than it is today. I don't remember there being a big mystery about inflation at the time. Inflation expectations had leveled out as well. The Fed wasn't pursuing an especially tight monetary policy to bring inflation down further.

MR. SMALL. Do you remember issues of productivity growth—whether changes in productivity growth over the years were throwing off the forecast, throwing off NAIRU (non-accelerating inflation rate of unemployment) estimates?

MR. STOCKTON. Only in the following respect: There was this issue about the productivity slowdown in the 1970s, and what role that may have played in boosting the NAIRU. The hypothesis was that there were some built-in institutional structural features of the labor market that might have caused the reservation wage not to adjust down when productivity growth slowed. For example, union contracts had not just cost-of-living increases built in, but productivity-related real-wage increases built into contracts. Those were no longer sustainable when productivity slowed down, but were adjusted in those contracts only slowly. That imparted upward pressure on labor costs and inflation when productivity slowed down. So there was discussion on the staff about that connection between productivity and inflation.

Through most of the 1980s, productivity wasn't that big an issue. Steve Braun had a nice little model that we employed for a long time that pooled labor market information using unemployment rates and Okun's law and using hours input and productivity to forecast current quarter GNP growth[4]. You can only do that if productivity is stable enough to make that second

[4] Steven N. Braun (1987), "Estimating Current-Quarter GNP by Pooling Preliminary Labor-Market Data," Economic Activity Section Working Paper Series 75 (Washington: Board of Governors of the Federal Reserve System, June).

piece work—multiply productivity times hours to get output. That worked well for a while, so that I don't recall productivity as a big issue at that time in the 1980s.

MR. SMALL. As a young staffer, did you brief or interact with Paul Volcker?

MR. STOCKTON. Volcker, relative to the subsequent two Chairmen, was not one who dealt closely with a wide range of staff, so I had very few interactions with him as a junior economist. The FOMC briefing that I just mentioned was a Volcker request. It became an opportunity to interact both with him and with the FOMC on a policy issue of great consequence. That happened early in my career. I'd only given one regular Board briefing before I did this FOMC briefing, so that was an unusual opportunity.

MR. SLIFMAN. In passing, you mentioned the NAIRU being higher than expected. The natural rate had drifted up and perhaps the staff didn't fully appreciate the extent to which it drifted up. Is that correct?

MR. STOCKTON. That was probably true in the late seventies and early 1980s. But by the middle of that decade, the question flipped: Were our estimates of the NAIRU too high and was it drifting lower? If so, maybe there was more of a mystery to inflation stabilizing than we thought, because we were rationalizing the stability of inflation as a reflection of the unemployment rate being somewhere in the vicinity of the NAIRU. Another paper that Sandy Struckmeyer and I wrote in the mid–1980s, using a more general equilibrium type framework, estimated a NAIRU of 5.5 percent. I remember Jim Kichline, our division director, being quite uncomfortable with that result. He did not ask us to change anything. He just wanted us to be sure that we were confident that we hadn't made any obvious errors because it sounded like a very low estimate to him. I'm not sure whether our result was right for the right reasons, but it was interesting.

MR. MARQUEZ. One question that I have relates to your dissertation. The literature on inflation and relative prices focused on the time when inflation was rising and the question of whether that was distorting the relative prices. Then, beginning in the mid–1980s, inflation declines. Were you curious about whether there was going to be the opposite effect taking place, that the declining inflation was going to reduce the volatility of relative prices?

MR. STOCKTON. Yes. One of my early Board briefings looked at what was happening to relative price variance as inflation came down; relative price variance was falling in that period. The other pressing issue was whether the lower level of inflation would show through in either a higher level of productivity or a higher growth of productivity. That was a controversial issue, both through the 1980s and into the early 1990s.

Working at the Board in the 1990s

MR. SLIFMAN. In moving from the 1980s into the 1990s, inflation came down quite a bit. There had been further analytical work in the academic literature. There had been further work on thinking about inflation and the determinants of prices and so on. Did the staff view change as we moved from the 1980s into the 1990s, and if so, how did it change?

MR. STOCKTON. The principal change was a more explicit incorporation of inflation expectations into the models that we were using, for example, following along the lines of some of the work John Roberts. He joined the staff in the late 1980s and was a student of John Taylor's. Through his work, he, along with some others, brought to the Board more explicit consideration of models with inflation expectations, particularly proxied by survey measures of inflation expectations. There was a greater appreciation that the parameters of an adaptive-expectations Phillips curve model would change if inflation expectations were stabilized. You needed to account for the fact that there was something to the Lucas critique. The world was

changing. With inflation expectations nailed down, changes in the output gap or in relative price shocks would cause only temporary fluctuations in inflation, in contrast to the more persistent changes in inflation that were predicted by a naïve Phillips curve that assumed inflation expectations depended on lagged inflation. We were on the cusp of a long period of stable inflation and stable inflation expectations.

There were other issues people were thinking about as actual inflation moved lower. One concern was whether there were so-called goal line effects—that disinflation would be harder to achieve between let's say 5 and 3, than between 7 and 5; and if so, what did that reflect? Was that some reflection of downward nominal wage rigidities or some other kind of factor? The key issues then shifted from disinflation to how best to forecast inflation when inflation itself has become low and stable and what might disturb that stability.

MR. SMALL. What about the econometric model and people being able to solve a rational expectations model? How important were those developments? Was that important or did that come later?

MR. STOCKTON. There was a lot of work on rational expectations, almost from the time that I got to the Board. But it certainly intensified in the late 1980s and early 1990s. Jeff Fuhrer and George Moore were trying to build rational models that could empirically explain the degree of inflation persistence observed in the data. But there were other major enterprises under way. Gary Anderson and lots of other people were thinking about this. This was beyond my technical capabilities, but they were working to code and solve large-scale rational expectations models.

MR. SLIFMAN. There was a young guy in Dick Porter's section, who was also working on rational expectations models early on, like in the 1980s or 1990s. I've forgotten who it was.

MR. STOCKTON. The great leap forward from the staff's forecasting perspective occurred when we moved from the MPS model to FRB/US. The FRB/US model allowed the staff to tell stories in which forward-looking expectations explicitly mattered.[5] It took a lot of work by a large number of people to make that effort come to fruition. That was a sea change in the way the staff told stories about policy and the economy in the 1990s. The economic models that we use are important intellectual organizing devices, and the new model changed the way the staff communicated to the FOMC about policy and macroeconomics.

MR. MARQUEZ. In your work, in your day-to-day grinding, to what extent were open economy considerations relevant for inflation, apart from the price of oil?

MR. STOCKTON. You were probably involved in some of this work, Jaime. There was a lot of discussion in the 1980s and 1990s, about the role of import prices and inflation. Research was generating a lot of different estimates, even here among the Board staff, as to how large that effect was going to be. I guess that debate continues on to this day. A lot of energy was devoted to that topic.

MR. MARQUEZ. The debate today is the extent to which globalization matters so much more than domestic considerations in the behavior of the inflation.

MR. STOCKTON. Yes, that's true, but that was also an issue in the 2000s. Those issues were discussed a lot by the staff in both R&S and International Finance.

MR. SLIFMAN. Inflation moves down from roughly around 3.5 percent at the first part of the 1990s, and then falls to about 2.5 percent in the rest of the decade, as we edge closer and

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[5] MPS is an abbreviation of MIT, University of Pennsylvania, and Social Science Research Council. The 1985 version of the MPS model is presented in Flint Brayton and Eileen Mauskopf (1985), "The Federal Reserve Board Quarterly MPS Quarterly Econometric Model of the U.S. Economy," *Economic Modelling*, vol. 2 (July), pp. 170-292. A discussion of the original version of the FRB/US model is in Flint Brayton and Peter A. Tinsley (1996), "A Guide to FRB/US: a Macroeconomic Model of the United States," Finance and Economics Discussion Series (Washington: Board of Governors of the Federal Reserve System, October).

closer to what most people would think of as being price stability. What was the staff view about what led to that further step down in inflation over the course of the 1990s and then kind of stays down there? What were the factors that the staff thought were involved?

MR. STOCKTON. I don't necessarily know if I can characterize the staff's view on that, but I can give you my view. This downward movement of inflation and the achievement of price stability over that period owes importantly to Alan Greenspan. Volcker obviously inherited an enormously difficult economic situation—very high inflation—and had to pursue policies that were incredibly painful for the country to bring inflation down. He shouldered the responsibility for that.

Greenspan inherited an economy with inflation running in the 4 to 5 percent range. At that point, people were relieved that we weren't in that very high inflation environment any more, but there wasn't a great deal of political consensus that inflation had to go lower. Greenspan can take some credit for actions taken in the early 1990s, coming out of that recession, that facilitated the subsequent further disinflation. Under his leadership, the FOMC responded in a measured way so as to promote a recovery, but one that was not so strong that disinflation stalled or even reversed. The FOMC then moved quickly, in 1994, when policy needed to be tightened up in response to an economy with diminishing slack in resource utilization. Those actions prevented an upward drift of inflation and solidified Fed credibility and inflation expectations. Then, in the second half of the 1990s, the good luck of favorable productivity shocks cemented low inflation.

MR. SLIFMAN. Just to follow up on the step-up in productivity growth in the second half of the 1990s. How did the staff respond to that? How do you think it affected the analysis

in the forecast that the staff was putting together? Were they slow to pick up on that, were they reluctant to pick up on it?

MR. STOCKTON. Greenspan was well ahead of the staff on the productivity acceleration, and so we were always a step or two behind him in recognizing exactly what was going on there.

MR. SLIFMAN. As an analytical matter, how did the staff incorporate it into our models? The staff says that Greenspan is right, there's been a step-up in productivity. How then do you incorporate this step-up in productivity into a model of inflation? How was the staff doing that?

MR. STOCKTON. It wasn't just the model of inflation. If you go back and take a look at some of the work that John Williams was doing in the Macroeconomic and Quantitative Studies section at the time, using FRB/US, he demonstrated that productivity shocks had effects on both aggregate demand and aggregate supply—[the] productivity growth story wasn't just an aggregate supply story. There was an aggregate demand piece of the story too that had implications for the course of monetary policy and for output and inflation. Again, FRB/US was a useful device for simulating those kinds of effects and trying to understand what combination of output, inflation, interest rates, equity prices, you would expect to see from a productivity shock. That work was presented to the FOMC at various points in time.

MR. SMALL. Going back to the first half of the 1990s, some terms that are used to describe that period are "soft landing" and "preemptive tightening." Do you remember that? How strange or experimental was that? How much was it a Greenspanian initiative?

MR. STOCKTON. It wasn't so much that it was novel or experimental because that particular aspect of Greenspan's thinking was similar to the staff's thinking about how to

achieve a soft landing. There were certain tactical issues about the pace of tightening in 1994 that were probably driven by his views of financial market dynamics. I doubt that we were, at the beginning of that year, expecting policy to move as promptly as it did.

MR. SMALL. Was that policy move more forecast-based than had been the case in the past? Was there more pressure on the staff to have that forecast right because it was forecast-based?

MR. STOCKTON. I don't think so. Policy actions had always been forward looking to some extent, so it always relies on somebody's view about the future. But Greenspan's risk management approach probably incorporated more than just the first moment of the forecast. He was thinking about where the risks lay and the consequences of moving slowly or moving quickly to tighten policy in response to an economy that was improving, but was not yet so strong. He wasn't compelled by the staff forecast to move as quickly as he did.

MR. SMALL. What was it like to work with Greenspan? The picture drawn of him is someone incredibly involved in the data—the railroad shipments or arcane statistical releases. Would you go in and talk to him and he would have different numbers and different datasheets than you had ever dreamed of looking at?

MR. STOCKTON. That characterization is probably accurate. He was somebody who looked at enormous amounts of data, and sometimes in idiosyncratic ways that allowed him to think through economic developments in unconventional ways. So he constantly had a series of large, complex data organizing or accounting projects for which the staff was providing support. Some of those we found useful, and I suspect still remain in use here at the Board; some of them were probably less useful to the staff, even if they were useful to him.

MR. SMALL. One of his great successes was seeing the call on productivity. What was your perspective of the Greenspan–Larry Slifman–Carol Corrado work effort and how that evolved?

MR. STOCKTON. Since he was consistently correct in his calls through that period, I'd say we were always a step or two behind him. Early on, that did seem like a pretty adventuresome forecast of his because there wasn't a lot to go on. He was looking at improvement in orders for capital goods that suggested to him that profit margins had yet to be squeezed. That suggested to him that the economy wasn't getting close to its capacity or potential, that productivity was growing rapidly and the perceptions of the marginal returns to capital were higher than were generally understood.

MR. SMALL. Larry, you were deeply involved in that. What are your views of that period?

MR. SLIFMAN. I agree with Dave. Even though I was involved with Carol Corrado in a lot of research that he then used to publicly support his arguments for the step-up in productivity growth, just by personality I was more cautious about how to interpret the data that Carol and I were putting together and the analysis that Carol and I were putting together. I would often say to him that I was not sure that there was sufficient evidence yet to make that call. He was convinced that it was and he was right.

MR. MARQUEZ. There is an angle here that may be too implicit. The reason for the attention on productivity is because the unemployment rate was declining, which was normally the antecedent to increasing inflation. So there was something at stake other than just being correct.

MR. STOCKTON. Absolutely. That was the call, on inflation, during the second half of the 1990s as to whether monetary policy needed to be tightened promptly as the unemployment rate came down. So the analysis was important. It involved more than just trying to guess the potential growth rate of the U.S. economy. There were important policy implications to the analysis as well. If you read the transcripts of FOMC meetings from that time, it wasn't just the staff members who were behind the curve on the acceleration of productivity. Many FOMC members, with a few exceptions, were behind as well. They were skeptical with the unemployment rate falling as much as it had that higher inflation was not in the offing.

MR. SMALL. You haven't talked much about financial contagion. At that time there were some S&L and banking problems.

MR. STOCKTON. I've said this publicly on a number of occasions. In the early 1990s, in the wake of the credit crunch, the Federal Reserve System and the economics profession in general did tons of work on credit channels. There was a volume published by the New York Fed that included contributions from people throughout the System, lots of work done by people here at the Board.[6] That work tended to show that there was an important credit channel that mattered. Regrettably, a decade later, none of that was reflected in the models that we were using. It seemed like we thought we had learned something, but then we did not take those lessons and build that into the models we were using. Now you can argue we didn't change the models because that was a relatively mild experience. Or more likely, credit crunches were viewed as more episodic and not part of the continuous linear-type model that we tended to rely on. That was unfortunate, because we would have been better prepared for the financial crisis had that work been reflected in some way in our work-horse models. As I noted earlier, models

[6] *Financial Innovation and Monetary Policy Transmission*," proceedings of a conference sponsored by the Federal Reserve Bank of New York, April 5–6, 2001 (New York: Federal Reserve Bank of New York).

serve as intellectual organizing devices, and had credit played some role in those models, people might have been asking more questions about those channels in the 2000s.

MR. SLIFMAN. Do you recall whether the staff even presented any of that kind of research? I'm thinking of the stuff that Simon Gilchrist did when he was here at the Board. Did the staff present any of that either to the Board or the FOMC?

MR. STOCKTON. I don't recall, but I'm guessing we probably did present some of that work in a Board seminar or briefing. And there were plenty of Federal Reserve conferences organized around that question. So I don't think this was a matter of it not reaching the attention of the policymakers. It was just a collective sense the credit channel was not central to macroeconomic outcomes outside of special circumstances. Once we got beyond the credit crunch in the early 1990s, the economy was improving, and it seemed like there were other issues, including productivity growth, which moved higher on the radar screens and on the research agendas of people here.

MR. SMALL. During the productivity boom, the unemployment rate was falling, and there was a question: Was the NAIRU going down with it because of productivity increases, or did this fall in unemployment require a tightening of policy to get it back up to a higher NAIRU? That led also to the tech boom in the stock prices, which came down rather abruptly in the early 2000s. Do you remember the lessons that were taken away from that event about how markets work, how policy responds?

MR. STOCKTON. Sure. I expected the effect of that stock market decline of the early 2000s to be larger in its macroeconomic effects than was the case. Whether that was because monetary policy acted so promptly to offset the effects, I do not know. I also expected there to be more political backlash against the Fed for having caused not the bubble but the crash in those

equity prices. Both the macroeconomic effects and the political backlash were milder than I anticipated, and I suspect milder than many others anticipated as well. There was a considerable amount of discussion both within the Fed and in the academic community about the appropriate response of monetary policy to concerns about asset price misalignments. The prevailing thinking at the Board was that it would be difficult to identify those asset price bubbles with any confidence, and then it would be difficult to calibrate the appropriate response of monetary policy even if you suspected that a bubble had formed. The prevailing wisdom—captured to some degree in work by Ben Bernanke and Mark Gertler, and certainly characterizing Greenspan's view—was that the appropriate response was to not to lean against those bubbles but to act aggressively when the bubbles burst, to provide a significant monetary policy offset or buffer to that shock. The relatively mild recession that occurred in the wake of a significant decline in stock market prices in the early 2000s probably reinforced a perception that that strategy was the right one.

MR. SMALL. Going into this period of productivity growth, one could model the effects of higher productivity simply as affecting aggregate supply, so aggregate supply would be greater than aggregate demand. The work that the staff had done on forward-looking models, and the FRB/US coming online, showed there's a wealth effect that can affect aggregate demand. That became a big deal: Did aggregate demand or supply shoot up more? Do you remember this debate? In helping with Bluebook simulations, I remember feeling lucky that the FRB/US model came online just before that, so we could analyze productivity shocks.

MR. STOCKTON. I agree with that sentiment. I associate this line of thinking principally with John Williams, but others were probably working on it as well. Flint Brayton and Dave Reifschneider were also using FRB/US to look at how big productivity shocks affect

relative movements in aggregate supply and aggregate demand. That balance, in turn, had policy implications as well. One also had to be careful to distinguish one-time jumps in the level of productivity from persistent changes in the growth of productivity. All of these matters factored into the staff's thinking about the macro consequences of productivity developments at that time. I remember going to both Greenspan's office and some of the other Governors with John Williams to explain our analysis of the supply and demand effects of productivity shocks.

MR. SMALL. And earlier work by Steve Braun had helped frame why.

MR. STOCKTON. Steve's work was especially important in thinking about how productivity changes would affect the NAIRU. We had long debates about whether to use an "effective" NAIRU (one that is affected by changes in productivity growth). In the second half of the 1990s, the question was whether a favorable productivity shock could lower the effective NAIRU because wage norms might not adjust quickly in response to that improvement. Sluggish adjustment of wages could allow price inflation to be lower for a given the level of unemployment than it would be ultimately when the wage adjustment was complete. When everybody understood exactly what productivity growth was going to be, those expectations were reflected in nominal wages and real wages, and then the NAIRU would revert to its previous level.

MR. SMALL. We came out of the stock market crash of the early 2000s knowing how the world worked, with flexible markets that would solve all our problems.

MR. STOCKTON. That's overstating anybody's degree of confidence. But that episode, and both the response of monetary policy and the subsequent favorable response of the economy probably did boost confidence more than was warranted. It probably left policymakers feeling that it was possible to come in and mop up after the demise of asset bubbles and at least limit

their adverse effects. But new puzzles were emerging. That upturn generated a jobless recovery, and there was a lot of thinking about how much structural change was taking place in the U.S. labor markets.

Terrorist Attack on the United States on September 11, 2001 (9/11)

MR. MARQUEZ. You were the division director during 9/11. Karen Johnson was in Europe at the OECD. Did you meet her there?

MR. STOCKTON. Karen and I were in different meetings, but we were both stranded in Paris for nearly a week right after the attacks. Steve Malphrus arranged to get me on a military plane out of Ramstein, Germany that following weekend, but my even more resourceful wife secured a ticket for me on the first United flight out of Paris back to Washington Dulles. So I didn't need to travel to Ramstein.

MR. MARQUEZ. Say you go into a Greenbook forecasting cycle. You have a view of the next two quarters. You have a view of the longer horizon. Then this event comes in. Keeping the prevailing views is a little odd, but how do you adjust, which way, by how much?

MR. STOCKTON. For the first Greenbook after the attacks, we briefly considered writing something completely different because the world seemed as if it was going to be so different. Why not go to some other kind of narrative about what might be going on and what might transpire. The thought was maybe we should write something about what's going to happen given the disruption of the transportation sector, for example. Ultimately, we wisely decided not to do that. There were macroeconomic issues here that could be analyzed with our traditional theoretical and empirical models, and the best service we could provide to the FOMC was to give our best guesses as to the consequences and risks that followed from the terrible attacks.

In the end, we concluded that we were better served by sticking with the framework and tools that we had, recognizing there were going to be some surprises, and there were. We expected a much more significant macroeconomic hit to the U.S. economy from 9/11. But by the turn of the year, we were surprised at just how resilient things were. We thought there would be a big hit to productivity and productivity growth because the United States was going to be spending enormous amounts of resources on security that wouldn't show up as final goods and services. But if anything, productivity was stronger than we expected. The contraction of employment didn't really gain much momentum. All in all, the economy was considerably more resilient than we had anticipated.

Communications Policy under Greenspan

MR. SMALL. During Greenspan's Chairmanship, there were a lot of developments on communications policy. Do you have any perspectives on the communications efforts, whether that made markets react differently, whether it improved things?

MR. STOCKTON. In general, the staff was very supportive of better communications, in part because the analytical framework that we were then using suggested you could achieve better monetary policy outcomes by making clearer to markets and the general public what monetary policy was doing. So the steps seemed helpful but incomplete. In this case, the staff's thinking on the benefits of clearer communication was probably equal to or ahead of the FOMC's.

Financial Crisis in the 2000s

MR. SLIFMAN. If one looks at the transcripts for FOMC meetings in 2005, 2006, and 2007, those transcripts suggest that the staff underestimated—some might say acutely underestimated—the drop in house prices that ultimately occurred and the extent of the

consequences of that drop in house prices for the financial system, for the macroeconomy. What were the reasons that the staff underestimated the magnitude of the drop in house prices and then underestimated the consequences?

MR. STOCKTON. The single most interesting transcript of this era is the briefing on housing at the June 2005 FOMC meeting. That transcript most clearly illustrates what we were seeing, what we were expecting, and the issues we were thinking about. It illustrates both a failure of imagination on the part of the staff about how far and quickly house prices could fall. And it illustrates inadequate communication between the banking supervision and regulation piece of the Fed and the macroeconomic monetary policy piece of the Fed concerning the consequences for financial institutions of a steep drop in house prices.

The Board staff presentation suggested a housing bubble had likely developed. Josh Gallin presented a lot of evidence to suggest that you couldn't rationalize what house prices had done. Dick Peach at the New York Fed took a different point of view. And the transcripts indicate there was sympathy for both positions. The Board staff did well on that part of things, but then the next step in that was looking at what the financial vulnerabilities were going to be. The staff could see some vulnerability on the household side, but thought those vulnerabilities would likely be manageable. There was clearly an inadequate understanding of the broader financial interconnections of the financial institutions with the mortgage market.

We analyzed the consequences of a decline in house prices of 10 percent because that looked like a really big decline, something we'd never seen before. In most past episodes, nominal housing prices would flatten out for a long period of time and the economic fundamentals would grow into those nominal prices. We asked the bank regulators what would happen if house prices went down 10 percent. They looked at individual banks and said, "These

banks are well capitalized. There will be some hit to their balance sheets but not too bad." This illustrates two problems: (1) the failure of imagination of the staff to think that house prices could go down 30 percent, not 10 percent, because that just looked wildly large; and (2) the failure of communication and analysis between the monetary policy and supervision areas. Maybe if the supervisors had been asked about a 30 percent decline in house prices, they would have been more concerned. But even then, I suspect, because they were looking at individual institutions, not the system as a whole, they would have underestimated all the financial interconnections that became so critical.

So we said that financial vulnerabilities were not acute. We then used FRB/US to simulate a house price decline of 10 percent. The effects of that shock mostly worked through wealth. Then we amped up the effects by assuming that there would be some negative confidence effects. Those model results suggested tenths off GDP growth, not the integers off GDP growth that occurred in the wake of the crisis.

MR. SLIFMAN. In part because there was no fully developed financial sector in the model.

MR. STOCKTON. There was no financial sector in our model or those in widespread use in or out of central banks. That was an interesting period of Board staff analysis because it got right the concern about a bubble in housing, but it failed to understand what the consequences would be of the demise in that bubble.

MR. MARQUEZ. A little while ago you were talking about studies done in the 1990s on financial structure, but then somehow we forgot about them or moved on. If staff had incorporated those developments into FRB/US, would there have been something useful to learn?

MR. STOCKTON. There could well have been something useful. You would have changed some of the questions that were being asked and perhaps thought more thoroughly about the role of credit and credit growth driving macroeconomic outcomes. I suspect it would not have enabled us to forecast a 30 percent decline in house prices or to have foreseen all of the financial market developments and interactions, but it would have caused us to pose different questions. The model often provides a framework to pose certain kinds of questions. Issues outside the model sometimes tend not to be considered as thoroughly.

MR. SLIFMAN. Is FRB/US too important?

MR. STOCKTON. I don't think so. It's essential. But as the crisis progressed, we needed to develop tools that were mostly outside FRB/US to consider, for example, the effects of bank credit on activity. Some of those models were simple VAR models that included bank credit. Some of the tools we developed focused on correlating the errors of FRB/US with measures of financial stress, bank credit growth, and other financial variables. The objective was to see whether these variables could explain why we were seeing much greater-than-expected weakness in spending. So I don't think FRB/US was too important. But it was incomplete.

Functions of the R&S Division

MR. SMALL. There could be a perception that forecasting is the primary function of R&S. As division director, were there new areas you got into?

MR. STOCKTON. That would be far too narrow a view about the focus of the division's work. Certainly forecasting, and research that was being done to support the forecast, is an important element. Research agendas driven by issues that might be important to the forecast were clearly central, but there was a lot of work on economic measurement being done in our Industrial Output Section, the Flow of Funds Section, and the Survey of Consumer Finances.

Most of that work wasn't directly in support of the forecast, even though we tried as best we could to use the insights you could derive from those data.

Then there were a large number of people in the division thinking about financial markets. Some of that was directly important to the forecast, but some of it was important in informing our broader understanding about financial market developments. Another important piece of the division was microeconomic research in support of developing frameworks for banking supervision and regulation. It's a big division.

MR. SLIFMAN. The division was involved in some of the Basel work on banking regulation.

MR. STOCKTON. Yes. Pat Parkinson and Myron Kwast were directing a lot of work that was supporting the Basel process.

MR. SLIFMAN. You were the keeper of this group of high tech, Ph.D. economists, so when issues even outside your domain came up, people came over and tapped your expertise?

MR. STOCKTON. Yes, we provided a lot of support, especially to the Division of Banking Supervision and Regulation, though maybe not always at its request. Obviously, R&S worked closely with Monetary Affairs and with International Finance on many macro-modeling issues.

Financial Crisis in the 2000s, Continued

MR. SLIFMAN. Let's go back to the financial crisis. In 2008, things start to get pretty dicey. What are your views about how the Board and the FOMC responded to the financial crisis?

MR. STOCKTON. One has to give enormous credit to Ben Bernanke, and Tim Geithner and then Bill Dudley as presidents of the New York Fed, for erring on the side of being

aggressive in responding to the crisis, and I'd say often ahead of the staff. I remember sitting in meetings, hearing some of the requests coming, in late 2007, and thinking, "Wow, would we really implement the Term Auction Facility?" Bernanke drove a process that included a lot of innovative approaches to the crisis that were important in limiting its fallout, and he did so with considerable focus. Other central banks, like the ECB (European Central Bank), underreacted to the crisis, while others, like the Bank of England, reacted aggressively but in fits and starts. The Fed, under Bernanke, could be characterized as responding aggressively, with incredible innovation that has continued right on through this year.

MR. SLIFMAN. To many outsiders, it appeared that a lot of what was done, especially in 2008, seemed to have been made up on the fly. Is that perception accurate?

MR. STOCKTON. A lot of what was being done was being driven by events that nobody had fully anticipated, if you want to call that "on the fly." "On the fly" sounds more haphazard and reckless than I would characterize the policies. Yes, some policies were driven by events. Some were influenced heavily by Bernanke's overall framework about the role of banks and bank credit and financial markets, based on his earlier work on the Great Depression. He didn't want to repeat the errors of the Federal Reserve of the 1930s.

So we weren't always fully anticipating events, instead we were often responding to them. But if you look at the actions taken, there was a certain coherence across the various programs that were designed to keep financial markets functioning and to improve the provision of credit and liquidity in the economy. And while they all had different alphabets and different approaches to different markets, it wasn't a scattershot approach but much more of a coherent strategy that had to be applied sequentially. For a time, you'd wake up every morning, and some

new market you never even knew existed was finding itself in some trouble, threatening overall financial stability.

MR. MARQUEZ. From your interactions with the Chairman, did you sense some second-guessing on his part about the route to follow? The magnitudes and the frequency with which these measures were implemented would have led someone to think not so much a fly-by-night operation but—

MR. SLIFMAN. I didn't say fly-by-night, I said on the fly.

MR. MARQUEZ. Did you sense that the Chairman was saying in effect, "I have a hunch. That's all I've got. This is a first-time-in-the-United States episode. I don't see an alternative, but that doesn't mean that I am sure that this is going to work." Did you sense some uncertainty about the efficacy of these implementations or these tools?

MR. STOCKTON. I don't think I personally witnessed that. I'm sure he was not absolutely certain he knew exactly how each one of these programs would work and whether they would be successful. But his natural inclination throughout this period was to err on the side of action rather than inaction.

MR. SMALL. We've been talking about the lender-of-last-resort and emergency lending through the discount facilities, by and large. But there's the other issue of the zero bound. Coming out of the Volcker years, we had 4 or 5 percent inflation. Back then, that might have been seen as the new norm, which probably means you weren't thinking seriously about the zero bound binding with 4 or 5 percent inflation. But now—

MR. STOCKTON. The first time I can remember the zero bound being discussed or mentioned was a comment by Larry Summers in a panel discussion at a Cleveland Fed conference on inflation.[7]

MR. SLIFMAN. When was that?

MR. STOCKTON. That was the early 1990s. We had been thinking about defining the appropriate inflation objective: downward nominal wage rigidity and measurement error being the two principal reasons why you might not want to have a target of zero inflation. That was the first time I remember the zero lower bound being raised, and it got people thinking about that possibility. That was early on, because there was an issue in the 1990s about how low should inflation go. Was 2 percent too low or too high?

MR. SMALL. I remember some early work by Jeff Fuhrer and Brian Madigan in a paper on the zero bound.

MR. STOCKTON. And, around 1999, we held a conference on monetary policy in a low inflation environment that was published in the *Journal of Money, Credit and Banking*.[8] That began to address all these issues in a more systematic way. I would hope people are still thinking about that because one of the questions that the crisis leaves unanswered is whether 2 percent is too low, and do we find accumulating evidence that 2 percent is too low in light of two decades of low inflation during which we hit the zero bound twice? I don't know the answer to the question. Two percent may be just fine, but it certainly seems like the events of the last decade or so leave open the question of the appropriate inflation target.

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[7] Lawrence Summers (1991), "Panel Discussion: Price Stability: How Should Long-Term Monetary Policy Be Determined?" *Journal of Money, Credit, and Banking*, vol. 23 (August), pp. 625-631.
[8] Jeffery C. Fuhrer and Mark S. Sniderman, eds. (2000), "Monetary Policy in a Low-Inflation Environment," *Journal of Money, Credit, and Banking*, vol. 32 (November).

Inflation Target

MR. SMALL. Do you remember much about the evolution towards an explicit inflation objective?

MR. STOCKTON. I wrote a paper on defining an explicit inflation objective with David Lebow and John Roberts around 1989. Mike Prell called it a quixotic effort, looking at and trying to define what price stability might be for monetary policy. We were of the view that you should have an inflation target. Chairman Greenspan wasn't enamored of a numerical objective and preferred to frame it as a rate of inflation so low that it didn't factor into the decisionmaking of households or businesses. I gave many FOMC briefings on this exact subject. There was an evolution in thinking within the Federal Reserve System, even though the Chairman's thinking was not changing much beyond his more qualitative characterization of price stability.

Ben Bernanke's appointment as Chairman was a significant step forward because the System began to think more seriously about making some kind of statement about its quantitative inflation objective. That didn't come to full fruition until January a year ago when the FOMC issued its one-page "Statement of Longer-Run Goals and Monetary Policy Strategy." So that's a long evolution from—

MR. SLIFMAN. 2007.

Composition of the Board

MR. SMALL. Generally, do you think the turnover on the Board plays an important role in bringing in fresh ideas?

MR. STOCKTON. I spent most of the past year at the Bank of England reviewing its forecasting and monetary policy process. The Bank of England has some external members on its MPC (Monetary Policy Committee), but the core of the MPC consists of people that came up

through the ranks of the bank itself. That has some strengths, but it also has some weaknesses relative to the Board. There is an advantage to Federal Reserve Board members predominantly coming from outside. The Board staff has an important institutional memory and has an important role in providing an analytical framework that can serve as a common point of departure for Board members. But the fact that the Chairman and most of the Board members are typically appointed from outside the Federal Reserve System brings some fresh perspectives. Sometimes those are more fully formed, academic, coherent frameworks. And, in some cases, the Board has benefited by just having smart, non-economist business people or other folks that come in with different perspectives, people that can question the prevailing wisdom. That's true for the FOMC as well.

That takes me back to your earlier question about whether FRB/US is too important. Quite often, Federal Reserve Bank presidents or Federal Reserve Bank staffs might have different models of the economy. That's helpful in providing different perspectives and forcing everybody to think about ways in which they could be wrong.

Financial Crisis in the 2000s Continued

MR. SLIFMAN. Earlier in our discussion, you suggested that, prior to the crisis, both the staff and the Board paid at best minimal attention to issues of financial stability. Am I right in my understanding of what you said?

MR. STOCKTON. I would say "insufficient" rather than "minimal." But when Bernanke was appointed Chairman, even before the financial crisis took place, he was organizing a multidivisional effort to think about financial stability issues, including briefings and presentations to the Board. That's another example of how turnover and individuals coming in from the outside occasionally can be important in ensuring alternative thinking is introduced.

Unfortunately, that came late in the game and didn't prevent our failure to fully anticipate the coming crisis.

MR. SLIFMAN. In the aftermath of the financial crisis, how do you think things have changed in the amount of attention and resources that the staff and the Board give to issues of financial stability?

MR. STOCKTON. There's been an enormous step-up, which was under way by the time I left, to increase resources and attention devoted to financial stability, including the establishment of the Office of Financial Stability Policy and Research. That office was intended to give some institutional structure to the data collection efforts on financial stability-related issues—a huge amount of work has been done across the entire Federal Reserve System. Briefings on issues related to financial stability were given both to the Board and to the FOMC.

That was all helpful and needed to happen. The bigger question is taking the time to figure out what lessons we should learn from the crisis. That's going to be a longer-term effort and one for which the payoff is not entirely clear. That said, if I were still here, I would be supporting that work. Fundamentally, central banks still don't know how to respond to emerging asset price misalignments. For example, suppose we get another year or so of 10 percent increases in house prices with an unemployment rate that is still in excess of 7 percent, what should the Federal Reserve do in response to that? That's not an easy question to answer and it's one that we could be facing in relatively short order. While house prices undershot on the low side, they didn't undershoot that much, if you look at house prices relative to rents. And that is the reason price increases have pushed us back into a comfortable territory of what one might characterize as fair value. But from here on out, housing prices only ought to increase about as much as rents are increasing, and it's not clear that that's going to happen.

A lot of staff has been hired to work on financial stability issues. A lot of reports are going to be written. But we still face challenging questions, and questions that aren't just in the future but may be relevant now. You can see that reflected in concerns being expressed by some FOMC members. Governor Stein's recent speech gave a little window into how the Fed might be thinking about this.[9] But his work doesn't provide answers to questions about how to calibrate interest rate increases to fill in the cracks of the financial regulatory structure, how you would make that decision, and how you would weigh the trade-offs. This is an effort that should not be allowed to flag, as the effort from the early 1990s somehow seemed to have flagged. It would be a real mistake if 10 years from now we look back at the models that we're using and we don't see some clear imprint of financial crisis on those models. But that's a big agenda.

MR. SLIFMAN. You've highlighted one issue that comes out of the crisis and that is how monetary policy should respond. In the past, the Greenspan view was that monetary policy shouldn't respond to relative changes in asset prices and that, if you get a bubble in one asset market, you just wait it out; it's just a relative price change. I think I'm characterizing the Greenspan view correctly. Governor Stein has suggested that maybe there are times when monetary policy should respond to these asset price bubbles. That's one big area that still needs to be understood better, and you've highlighted that.

The other one, though, relates to the work that the staff does on issues like stress-testing of banks. Do you think that's just an effort that sits off as an annex somewhere, or is that getting incorporated into thinking about monetary policy as well?

[9] Jeremy C. Stein (2013), "Overheating in Credit Markets: Origins, Measurement, and Policy Responses," speech delivered at "Restoring Household Financial Stability after the Great Recession: Why Household Balance Sheets Matter," a symposium sponsored by the Federal Reserve Bank of St. Louis, held in St. Louis, Mo., February 7.

MR. STOCKTON. I don't know the extent to which it's being directly incorporated in thinking about monetary policy, although Governor Stein's speech and other comments by FOMC members suggest this is clearly on their minds.

The stress-testing type example indicates how the Fed has changed. That is a clear example of the macro people interacting with the bank supervision and regulation people to think about the interaction of those issues. That's a potentially fruitful collaboration because it's going to force the macro people to think about financial considerations—in this case, not just credit extension but overall financial issues. And it's going to force the bank regulators to think about the macroeconomic environment in which they're conducting regulations. Governor Tarullo had set up a group that would incorporate people from the three economics divisions at the Board, bank supervision and regulation, and people from around the Federal Reserve System in this coordinated council. Those kinds of interactions are going to help.

The area of macroprudential policy still seems like it's in its infancy in thinking about what tools need to be developed. When you see these asset price misalignments, Chairman Bernanke's first instinct is not to use monetary policy but to think about strengthening macroprudential policies that will limit the vulnerabilities. Working on that may be just as important as thinking about the work on how monetary policy itself should respond to asset price bubbles, but both those areas need attention. I may not be as fully versed on the current literature, but it still seemed pretty rudimentary in the thinking about monetary policy strategy for dealing with financial instability.

MR. MARQUEZ. Do you think that the financial crisis changed the functioning or the structure of the U.S. economy in such a way that stories that we used to tell or estimates that we used to use are no longer relevant, that it's almost as though we're starting afresh?

MR. STOCKTON. I guess not, which isn't to say that some fairly radical and significant adjustments to the modeling framework aren't warranted. But I still think many macro relationships are much as we previously thought: consumption depends on income which is probably related to output and interest rates; exchange rates affect external balances; interest rates affect housing demand, and so forth. But what about the crisis? I do think the crisis has some difficult implications for the modeling effort. If these are largely episodic events, it's harder to think about how to get those productively into a model and keep monitoring them. And it is hard to know whether the kinds of financial instabilities that we dealt with in this last crisis are the ones that are going to drive events in the next crisis.

MR. MARQUEZ. Nevertheless, some people may be arguing that the level of potential output, with the understanding that it's hard to measure, may have been influenced by the crisis. Potential output is a different word for the natural unemployment rate. So the notion is that, yes, consumption depends on income; but for what we do, the Phillips curve is really a big deal, right?

MR. STOCKTON. Has the financial crisis affected the structure of the economy in terms of potential output, natural rate of unemployment? The area that I would focus most of my attention on would be the labor market and trying to understand how big a permanent imprint will have been left. That's important. The role of finance in shaping potential output is also an interesting one. Has the role of credit as an intermediate input into the production process been impaired in some significant way and could that be affecting the level of productivity growth over the intermediate term? Then there are issues about longer-term technological progress and multifactor productivity. But those problems aren't related to the crisis about which there's considerable uncertainty.

There was some research done at the Board and elsewhere that was looking at whether the ease in the provision of credit could explain some of the decline in the saving rate that occurred from the mid–1980s. If in the years ahead there is less willingness to lend to households, that effect might work in reverse. Reduced credit availability might lead to an increase in the equilibrium saving rate.

MR. MARQUEZ. Yes, that's right.

MR. STOCKTON. It's possible.

Relationship between R&S Staff and Board Members

MR. SLIFMAN. What is your view about the balance of power between the staff and the Board?

MR. STOCKTON. There's a good balance for the most part, if one includes the Chairman as part of the Board. The staff was always responsive to the requests of the Board members. And the Board valued, for the most part, the independent perspective offered by the staff. Relative to my experience at the Bank of England, there is a value in having an independent view provided by the staff that isn't directed by the Governors, including the Chairman.

MR. MARQUEZ. Do they have the same division of labor as Federal Reserve Board staff?

MR. STOCKTON. No. The staff at the Bank of England are much less an "independent" entity. For example, there is no staff forecast. Basically, the staff at the Bank of England takes the MPC's previous forecast and updates it for incoming data and changes in fundamentals. The staff never briefs the MPC on the staff view of the economy. At the Board

and the FOMC, that was my job, and the job of the other division directors, Karen Johnson, Nathan Sheets, Vincent Reinhart, Brian Madigan, and Bill English.

MR. SLIFMAN. More generally, could you talk about the independence of the staff at the Board.

MR. STOCKTON. In my 11 years as division director, I never had Greenspan or Bernanke call me and say, "Could you change that number a little bit this way or that way. It would make my life a little easier at the upcoming meeting." We didn't ever have to respond. We were independent. The transcripts will reveal that there were plenty of times we faced a lot of skepticism by some FOMC members, or maybe even most FOMC members, sometimes correctly and sometimes incorrectly, about the staff forecast. That independence is an important and valuable characteristic for the Committee. I would always tell every new Governor or FOMC member, "My job is not to convince you that the staff's forecast is right. My job is to present a coherent, consistent framework and story, and something that would serve as a common point of departure for the Committee to help organize their discussion." They could then say that they think the staff is wrong about the following sets of things, which they often did. That independence is important because I don't think I ever even heard any Bank president or Governor say, "You're just forecasting that because that's what the Chairman wanted." Maybe some of them thought that on occasion, but that was never expressed to me, even in indirect ways.

MR. SLIFMAN. Beyond the forecast and FOMC issues, what about research that was being conducted by staff members? Did the Board members ever try to get the conclusions changed or suppressed or anything like that?

MR. STOCKTON. No. Topics were generated by Board interest; the staff researched issues that they were raising. But we would only do that if we thought these were issues upon which research could shed some light. I don't remember getting any pressure from Board members about results of papers, or getting any pressure whatsoever, or even having to internally edit the results of papers for anything other than content. The independence of the staff's research was considered sacrosanct.

I remember a couple of occasions where somebody might have written a paper before they arrived here. For example, there was one paper on the rates of recidivism of Italian prisoners. It was written by a Fed staffer with an outside co-author before he joined the Board staff. I thought, "Do we really have to put that in the Fed's working paper series?" I could picture that being viewed as outside the Fed's remit, and might signal that the Fed was devoting resources to this question, which it was not. Still, if you look at the subjects that papers have covered over the years here, we interpreted our research remit pretty broadly.

MR. SMALL. There used to be a review process by Board staff of Reserve Bank official publications. Would you comment on what that was, how that evolved, and where we are now?

MR. STOCKTON. The Board staff used to review all the articles that were being published in the Reserve Bank bulletins or reviews. I thought that was a bad policy. It was a considerable expenditure of Board staff time that could be applied to doing their own research, instead of reviewing the work of Reserve Banks. I've always thought the Banks should have both the independence to produce what they want, to review it, and to have the accountability for it. If they put together something of bad quality, the first response of the Board members should not be to call me and ask how did I let that happen—how did this piece of research get in—they

should call the Reserve Bank president and the research director at the Bank and ask how did they let that be published.

However, the Board members typically didn't want to have those kinds of unpleasant conversations with Reserve Bank presidents. They were much happier if I was the person that would have to do that conversation. Ultimately, we persuaded them that that wasn't a good use of Board staff time. In any event, as more and more electronic forums became available for posting material, it was harder and harder for the Board staff to monitor everything that was being done by the Reserve Banks.

MR. SLIFMAN. In the past, division directors at the Board have sometimes been called barons.

MR. STOCKTON. I always thought of myself as a player/coach, rather than a baron. Part of my job was to help other people on the staff better communicate their ideas. I didn't view myself as controlling information that should go to Board members, except for some basic quality control. One of the ways this place worked well, in contrast to what I heard happens at many other government policymaking institutions, was that there wasn't a lot of strategic behavior on the part of division directors, or Board members for that matter, in manipulating and controlling information. That certainly was the case in my dealings with Karen, Nathan, Vincent, Brian, and Bill English. We would always share information and keep each other in the loop on what was going on.

Occasionally, some Board members felt that they weren't fully engaged on all the issues that they wanted to be. But it typically wasn't the staff that was preventing that engagement. We had a pretty good record of open service to the Board, so the notion of barons—if it existed—wasn't a good description during my period of dealings with the Board or the other

division directors. Historically, maybe there were division directors with stronger personalities who played bigger roles than my colleagues or I did.

MR. SLIFMAN. Did a Chairman or a Board member ever try to accomplish something that was thwarted by the staff?

MR. STOCKTON. I don't think so. I can't remember ever deliberately attempting to thwart something that a Board member wanted to accomplish. That would have been outside the culture of this place. The staff had a service orientation that was attentive not just to the Chairman but to other Board members. Obviously, the Chairman, in some sense, had first call on staff resources. That seems appropriate, given the disproportionate responsibilities carried by most Chairpersons. There certainly were times when we might have to negotiate with Board members about prioritizing work. One of the difficulties of being a division director here is that you have 7 bosses, if you count all 7 Board members, and 19, if you consider your role on the FOMC. Quite often you would get either conflicting assignments or ones that couldn't all be accomplished simultaneously. In those cases, you'd go to the policymakers and ask for some help getting things prioritized. People were pretty reasonable.

Working with Fed Chairmen and Other Board Members

MR. SLIFMAN. Do you remember the first time you went to the Chairman's office?

MR. STOCKTON. I do. It was with Mike Prell. I was drafting testimony for Paul Volcker, and we wanted to get his views about what he wanted to say. That was nerve-wracking, even though the meeting was entirely uneventful.

MR. SMALL. Do you remember the response to your first draft? Did Volcker respond?

MR. STOCKTON. The first response I received was from Mike Prell. He suggested quite a few revisions. We then got some fairly extensive comments from Volcker as well. That was all part of the writing process that takes place here at the Board.

When I came here, one thing that surprised me was the importance of words, of just how critical the use of words was to policymakers and the staff. I remember standing out in the hallway and seeing Mike Prell and Don Kohn arguing over a specific phrase in a testimony. At first it seemed like they were overdoing it. But later on, I'm sure I was doing the same thing with Vincent and Karen as well, and for good reason. Precise communication is important, and sometimes lack of precision can cause confusion or misinterpretation.

MR. SLIFMAN. Do you remember when you'd finally moved into the inner core of folks who would go to the Chairman's office and interact on a frequent basis?

MR. STOCKTON. That was when Greenspan arrived because he had such a different approach to interacting with the staff than Volcker did. Greenspan wanted lots of people in his office that knew something that would be helpful to him. Then, once he determined that you were knowledgeable, one could find oneself in that office all the time, on various projects and issues or whatever. He liked having a lot of staff to support him. That coincided with my promotion to Chief of Economic Activity and Assistant Director. And, in return, Mike gave me a lot of responsibilities along the way. Larry, it might have been your absence in China that put an extra special burden on me and stimulated a lot of visits to the Chairman's office.

MR. SLIFMAN. It was my pleasure. You worked with Fed Chairmen Volcker, Greenspan, and Bernanke, and with many Governors. There were a lot of different styles, backgrounds, and personalities. How did you adjust to differences in styles? Volcker and

Greenspan, as you've already suggested, were completely different in their style and ways they interacted with the staff.

MR. STOCKTON. Bernanke as well. Bernanke actively promoted staff discussion and debate. Unlike Volcker and unlike Greenspan, Bernanke was using a view of the economy, a model so to speak, that was similar to that of the staff. He spoke the sort of academic language that most of us were familiar with. In some sense that made the conversation easier. But he operates at an extremely high intellectual level so it was challenging because he was quite capable of spotting and noting flaws in one's logic or arguments but in a productive way.

MR. SMALL. Do you have any lessons observed about Governors coming in with certain preconceptions of staff. Which ones figured out how best to use Fed staff, how not to use Fed staff, and how they may help this place work for them?

MR. STOCKTON. That's a good question; what characteristics would have been associated with those who were able to integrate themselves with the staff? People who recognize that the staff wants to provide them with high-quality service, and who view the staff as willing supporters versus potential adversaries, tend to receive better service over time. On a number of occasions, there were Governors who came to the Board suspicious or skeptical of the staff and the staff's independence. They may have believed the baron-type characterization of the staff as an independent power source attempting to exert its own influence on the policy process. Those people tended not to want to work as closely with the staff because they were suspicious about whether they would get what they wanted.

MR. SMALL. Does working with the staff mean buying into the staff and buying into the Phillips curve and the NAIRU, et cetera?

MR. STOCKTON. I don't think so. We did a lot of work on commodity prices and commodity price targeting. That work was outside the framework that we were normally employing. Some of the work was done by John Rosine and Peter von zur Muehlen for Governor Wayne Angell. They explored both measurement issues and some of the analytical issues of how targeting commodity prices might affect macroeconomic performance. So I think the staff has provided support to Board members on issues outside our preferred economic framework.

MR. SLIFMAN. From which Governors did you learn the most?

MR. STOCKTON. Don Kohn would be the person that I'd point to, and not necessarily in terms of new analytical tools, but a way of approaching the work, asking the questions, thinking about what was important to the conduct of policy. I learned from Don, first, in his role as a member of the staff, and then when he was Vice Chairman. Professionally, he was the policymaker that probably pushed me the farthest.

I learned a lot from Greenspan as well. He forced great attention to understanding the data. He taught us to have a healthy skepticism about the official data, and to be willing to look for conformation or for tensions in those data by using other approaches to measurement and nontraditional sources of data. That was helpful in thinking about the economy. It wasn't always so much an alternative model, but it was a form of data forensics that provided additional perspective on economic developments. I learned a great deal from him about economic measurement.

From Bernanke, I learned a great deal about financial markets and the role of liquidity and credit and about how financial markets affect the economy. He is a creative person.

Forecasting

MR. SLIFMAN. You said you've got to know where you are to know where you're going. Some people have called that forecasting the present. If you look at the Greenbooks from that period, the ones that are publicly available now, there was a lot of space in them devoted to the current economic conditions. Why do you think it was important to devote that much time and space and resources of energy to forecasting the present?

MR. STOCKTON. We may have been devoting a little too much to the near term. One of the changes we made, when we went from the Greenbook to the Tealbook, was to cut back on some of that very high-frequency analysis. But fundamentally, the detailed analysis of the incoming data isn't so much about refining your estimate of GDP growth in the current quarter by a tenth this way or a tenth that way. It's more about looking for bigger patterns in the data that might reveal the underlying stories of what's happening to the economy. For example, are we seeing signs of an investment pickup, and what would you be looking at? Not just looking at orders and shipments data to try to calculate what current-quarter spending on equipment and software is, but asking how anecdotal information from businesses is lining up with what we're seeing on orders or what we hear about capital spending plans from the limited surveys that are available. All of that is part of building pattern recognition and a developing a "machine" that tries to detect the bigger stories occurring in the economy; it's more than just getting the current quarter right. Some of this digging through the data might have been lower valued, but you sometimes need to weed through those things on occasion.

MR. MARQUEZ. Do you think this idea of looking for patterns in the data is completely neglected in Ph.D. programs?

MR. STOCKTON. Maybe not completely neglected, but I suspect underappreciated. When I came here from academics, I witnessed this incredible attention to data at the Board; you weren't just pulling five time series off the computer and running a regression on those data. For example, it was understood at the Board that you can never learn much from 70 quarters of aggregate time-series data. Looking for additional sources of information was greatly valued. That aspect of macroeconomics sometimes receives less attention in academics. Part of this process is just understanding and appreciating the extent of your ignorance. Part of this data forensics is directed towards questioning whether the stories that you're telling about the economy have support beyond the aggregate data.

MR. MARQUEZ. I agree with that. Ph.D. programs in economics rarely cover anything remotely close to that.

MR. STOCKTON. Little time is spent trying to understand the data themselves. And my guess is a lot of Ph.D. theses are written where the authors don't fully understand what the flaws in their data [are]. One of the values of the Board staff is that there were lots of people who understood, in great detail, the strengths and weaknesses of the data being looked at and reported on. And that was true even before Greenspan. I was struck by that when I walked in the door here and realized how little I knew about productivity measurement and labor market data.

MR. SLIFMAN. What is your favorite story or memory about Chairman Volcker?

MR. STOCKTON. It was the time he started a fire in a cigar tray during a briefing. It rattled the concentration of the briefers at the end of the table when he tried to put out the fire that he had started. He had ignited the cellophane wrapper.

The other memory I have is when I got on an elevator with Joyce Zickler, Chairman Volcker, and Senator Bill Bradley. My head came up only a little bit over their belt buckles, and Joyce is shorter than I am. It was quite a contrast of height there.

MR. SLIFMAN. What's a favorite memory or story about Chairman Greenspan?

MR. STOCKTON. I'm not sure I've got a great story for Greenspan or Bernanke off the top of my head, partly just because I spent so much time with both of them that there were many, many stories.

Transparency

MR. SMALL. You talked a lot about forecasting, a lot about the Greenbook and its successor, the Tealbook. Transparency is one of the big developments in monetary policy that Greenspan and Bernanke pushed. So if you have this well-thought-out forecast, why not share it with the public earlier than five years? Why not help inform the public of your thinking and maybe get pushback from the academics on whether you are putting this forecast together correctly?

MR. STOCKTON. There's certainly nothing sacrosanct about five years. The advantage of the five-year lag was that it places staff analysis of fiscal policy outside the presidential electoral cycles. You wouldn't get drawn into someone saying "see the Fed staff said this particular fiscal stimulus plan was going to be worth nothing or whatever." That potential for controversy might have influenced the degree to which the staff could feel free to offer their unfettered views. But I'm not sure that's necessarily the controlling factor. Another reason for some delay gets back to this issue of whether the staff should be an independent power source. Suppose the staff forecast was released at the same time as the FOMC's forecast. One of two things could happen. One, staff members might find their forecast drifting towards the

consensus of the Committee to avoid creating any uncomfortable public disagreements, in which case the value of the staff forecast would go down. Two, there could be problems when people outside the Fed say, for example, "Your staff is telling you this. Why are you guys doing that?" Maybe that would be fine for the process, but I'd worry about the feedback onto the forecast itself because the independence and the freedom given to the staff to call it exactly they see it is very valuable to the policymakers. The staff's forecasts get plenty of outside attention, and it is something still held out as a benchmark against which many academics and outside forecasters judge their own forecasting records. The historical record is always going to be there. Every statement made at an FOMC receives enormous scrutiny. That one day a year, when the transcripts of every forecast are released, was always marked on my calendar.

The Structure of the Board

MR. SLIFMAN. What makes the Board work well or not so well?

MR. STOCKTON. The culture of the organization has been sustained over the years as one that promotes collegiality, conversation, and cooperation on the policy side, and in terms of the way the Board is managed. One of the weaknesses of the organization, from a management perspective, is there isn't always an administrative governor that takes a deep interest in the way the Board is run. Most of the management decisions are left to the collection of division directors. At times, that makes it difficult for the Federal Reserve Board, as an organization, to prioritize and allocate resources or to reallocate resources when they need to be reallocated. I'm not sure exactly how to solve that. Some central banks have administrative governors who take executive responsibility for the resource allocation questions but are not involved in the policy process. That would be a big change here.

MR. MARQUEZ. Do you think that Larry's earlier question about the barons could be reflected in this separation of authorities? For example, you can imagine that the administrator governor says "Mr. Stockton, this is really nice but no more computing time for you." And you say that's just not going to work. Could that be an interpretation of barons?

MR. STOCKTON. Yes, it could be. But I wouldn't call that being a baron. I'd call that arguing at a high level about organizational issues. One of the complicated aspects of the internal structure of the way the Board is managed is precisely this issue. There isn't a CEO who is making final decisions on management issues. In fact, if anything, there tends to be a reluctance on the part of the members of the Board to want to get involved in management issues. Obviously, they're here to address bigger analytical and policy-related issues, and they're happy that somebody else is taking care of all the difficult and unpleasant management issues—even if those "somebodies" are the division directors. It isn't necessarily an optimal arrangement.

MR. SLIFMAN. What is your view about the role of the Board within the larger federal government inside Washington?

MR. STOCKTON. That particular arrangement is pretty sound. The Board is a policymaking institution. It has open communication with the Treasury, the Council of Economic Advisers, and other agencies and offices. And there's a lot of accountability in providing information to the Congress.

MR. SLIFMAN. Are you concerned that an entity like FSOC, the Financial Stability Oversight Council, takes away something from the independence of the Board as an institution?

MR. STOCKTON. I'm not. But I do have questions about how FSOC will operate in a true crisis period, about whether there will be too many decisionmakers to take timely action when it's required.

MR. SLIFMAN. That's a slightly different piece.

MR. STOCKTON. That's slightly different than what you were asking. It isn't so much a concern about FSOC taking away the independence of the Fed, but how it might impair the Fed's decisionmaking process. I'm not sure whether the FSOC organizational structure will ultimately survive. Given the financial crisis, the modest organizational changes made to the financial regulatory apparatuses in the federal government are surprising. I would have expected a more profound restructuring to have occurred. So that too remains an open question.

MR. SLIFMAN. You were talking about the Board vis-à-vis other agencies, and most notably the Treasury. Was that a good arrangement?

MR. STOCKTON. Yes, that's a fruitful arrangement, at least during the years during which I was division director. If you went back to the late 1980s and early 1990s, I'm not quite sure whether the relationships between the Fed and the Treasury were always so cordial and productive. But certainly in the 11 years that I attended Treasury lunches every week or every other week, there was good interaction and full discussion. Even when there were differences of opinion about a policy, those differences were recognized and better understood through open communication.

MR. SLIFMAN. Do you think it's optimal to have seven Governors? Do you think it would be better if there were five, nine, just a Chairman?

MR. STOCKTON. A committee structure is probably preferable to a single decisionmaker—a Governor or whoever. While a Chairman leads, he or she can be influenced

by other members of the Board in ways that are probably helpful to the quality of decisions over time—not always, but probably on average. Is seven the right number? As a staff person, it always felt great when there were only five. There were fewer speeches and testimonies, but sentiment was not driven by what leads to best public policy. Importantly, reducing the number of Board members would not be appropriate, without reducing correspondingly the number or influence of Reserve Bank presidents.

MR. MARQUEZ. You have to have a majority vote, so if you put it at four you don't have a majority.

The Structure of the FOMC

MR. SLIFMAN. The FOMC consists of many people, making consensus more difficult. Would the Federal Reserve better off with a system that had fewer decisionmakers?

MR. STOCKTON. It would be hard for me, on the fly, to come to any firm conclusion about that question. The Federal Reserve has existed for 100 years. It might be time, at least at some point, to think about the optimal structure. Now would not be the best moment for that political conversation to take place. But 100 years from now, should we still have the 7 Board members in Washington? Should there be 12 Reserve Banks, and should they be where those Reserve Banks are currently located? Is there some potential for changing the structure of the Federal Reserve? That's open to question. I would be supportive of some effort eventually to look seriously at that question.

Getting back to the question you asked. Nineteen FOMC members is a large number, and one could imagine operating an effective decisionmaking committee with fewer people. In producing my report at the Bank of England, I attended all of the relevant meetings. The Monetary Policy Committee (MPC) of the Bank of England consists of nine members. They

meet in a small room, with few staff members. And actual conversation takes place in that room. They talk to each other in ways that can't happen in a room with 19 decisionmakers, 6 or 7 staff members at the table, and many staff members hanging around the edges. I was impressed with policy dialogue that took place at the MPC meetings. The Bank of England example demonstrates that it is possible to improve discussion, dialogue, conversation, but still receive the benefits of a committee orientation—taking the decision out of the hands of a single individual—while operating with a smaller committee.

MR. SLIFMAN. Is it easier to reach consensus with only 9 members, relative to 19?

MR. STOCKTON. In general, yes, I think so. But the objective should be good decisions, not necessarily consensus. The Bank of England has a process that purports to reach consensus on the outlook but leaves the policy position to each individual member. On the forecast, they call their process "best collective judgment." They produce their fan charts in the inflation report by reaching consensus about the forecast. There are plenty of flaws in the way that operates as well as a certain lack of transparency. But set that aside. While they reach "consensus" on the forecast, they take great pride in voting on the policy itself and being accountable for their policy vote. But in this arrangement you're never quite sure whether the difference in the policy vote reflects a difference in the view of the forecast or in their policy preference. Nobody is forced to explain that. Still, the conversation is more productive because of the smaller group of policymakers, and I think it's easier to reach what appears to be consensus. I'm not sure that, in all cases, that's what the policy process should be driven toward—consensus—because, as the crisis illustrated, sometimes different points of view need to be given voice to just try to round off all the edges. That's an issue both for the MPC at the Bank of England and the Federal Reserve. So a smaller group of policymakers might better

facilitate reaching a consensus, but it isn't the only consideration in designing an optimal institutional arrangement for making sound monetary policy.

MR. SLIFMAN. Do you think it's the size of the FOMC that inhibits conversation or is there something else about the nature of the FOMC meetings that inhibit conversation?

MR. STOCKTON. Fundamentally, size is important here. If you think about being in the Board Room without all the staff in an executive session with the seven Board members prior to an FOMC meeting, they're just talking about policy, conversation takes place, and they talk to each other in much the same way as the MPC members. With 19 people, it's harder to organize that kind of discussion. But there could be some cultural aspects as well.

Chairman Bernanke has promoted more active discussion amongst the FOMC members. And they have responded. But it still isn't as spontaneous as occurs with smaller groups of people.

MR. SMALL. How much did Board staff help Governors with their FOMC presentations?

MR. STOCKTON. Not a lot with the presentations that they made. We assisted many of them on the forecast side, helping them think through what they wanted to submit as their forecast. When Larry Meyer joined the Board, he wanted to meet with the staff independently, with a couple of other Governors, to talk about forecast issues, in a more relaxed conversational format than is possible in the Board Room with all the staff there. And that was continuing when I left the Board, both the meetings on the forecast and meetings on monetary policy, though obviously with a different set of Governors. Those were ways in which the staff could interact in a more intimate fashion. I was down in the Governors' offices a lot, talking about the economy, our forecast, their thinking, hearing their views as well. But I deliberately avoided going to those

meetings that occurred right before the FOMC meeting, because I didn't want the Board members to think that I might be inhibiting staff members from expressing their views about the economy, or to be the one to dominate the conversation; then it just becomes another conversation with Dave Stockton. So, by all accounts, the Board members found those meetings helpful. It was one way to create a more intimate environment for conversation outside of the Committee structure itself.

Bank Supervision and Regulation

MR. SLIFMAN. The Board is involved in bank supervision and regulation. Do you think it's appropriate for a central bank, the Federal Reserve, to do both monetary policy and supervision regulation?

MR. STOCKTON. This is not an area of expertise of mine, so my remarks should be taken in that spirit. I was skeptical before the financial crisis. I thought the arguments made by the Board to support its continued role in supervision and regulation sounded weak to me, and I didn't see any great value. But the crisis changed my view about that. In thinking about these interactions between financial stability issues, monetary policy, and bank regulation, it seems like some role should be played by the central bank. Whether the role that we have attained is the right one is open to some question. One could imagine the Board being involved and responsible for institutions of systemic consequence, not necessarily regulating community banks. So could one imagine a reshuffling of regulatory responsibilities? Do we now have exactly the right set of institutions to supervise? I'm not so sure. There may be counterarguments that I'm just not fully tuned into. But if I were thinking about an agenda for the future of the Federal Reserve, I'd have the scope of our regulatory responsibilities on the agenda.

This isn't just an issue for the Federal Reserve; it's an issue for virtually all of the major central banks, which are now being asked to do a lot. I wonder about the capacity of institutions and individuals in those institutions to handle the broad range of responsibilities that have been given to them. That's going to be an issue with the Bank of England, which is also now taking on its banking regulatory responsibilities. And it's an issue here as well. I'm sure people specialized in organizational science can think about how big a portfolio of responsibilities is feasible for a single institution to operate effectively.

I worry less about the issues that were raised for separating supervision from central banking earlier on. Specifically, the oft-stated concern that central banks might use monetary policy to produce certain private banking outcomes—I'm not so sure that's something I'd worry so much about.

Congressional Oversight

MR. SMALL. What do you think of Board members' views on congressional oversight—how helpful it is in helping us do our function and stay independent? How respectful is that relationship?

MR. STOCKTON. The Federal Reserve has considerable accountability to the Congress. I don't see that accountability as raising a major issue from the perspective of the Fed. In the last few years, there has been a much greater push in the Congress to trim the independence of the Fed. That's going the wrong direction. To be sure, the Fed should always be evaluating its level of transparency and accountability. I suspect that for a long time, the Fed may have had a view that "we won't give anybody information unless we absolutely have to," whereas you might want to switch that mindset to "we should be giving everybody full information and transparency" and only pull back from that commitment when absolutely necessary to maintain

our effectiveness. Nevertheless, the Fed has become more transparent over time. This evolution of transparency is a reflection of both an evolution in academic thinking and an evolution of the Fed's culture that has promoted greater transparency. My view is that the Fed is quite responsive to congressional requests.

MR. SLIFMAN. Someone could have a picture in their mind of Paul Volcker testifying with the cigar, the smoke, and the mumbling, or of Alan Greenspan and the constructive ambiguity and opaque statements at times. And they could say that conveys a lack of communication and mutual respect. What you read in the press often doesn't reinforce the idea of accountability and communication. One could counter that there's a lot more to the relationship. There are other issues behind the scenes, the staff's work or the Chairman bringing in congressional members on such a frequency.

MR. STOCKTON. That would be a harder criticism to level at the Bernanke Fed, and, even then, I don't see that as a regime shift. There's been this continued process of increased transparency. If you went back to the 1970s and 1980s, central banks in general thought mystery and surprise were important tactical tools to be used. People have changed their thinking about that. I view the Fed as having been on a long evolutionary road, with changes that continue right up to now. Even some of the criticism of the lack of communication earlier is probably unwarranted.

If you think about Volcker and what he did to bring inflation down, the historical record would show that he was direct in his public communication about how painful that was going to be. People can argue about whether monetary targeting was actually based on a deep intellectual commitment to monetarism or was largely a vehicle for boosting interest rates. But I don't think that Volcker ever shied away from the important message that this was going to create high

unemployment and it was going to be painful, but it was necessary to bring inflation down. That was fundamentally being transparent.

Life after the Fed

MR. SLIFMAN. When did you retire from the Fed?

MR. STOCKTON. October 2011. I announced I was retiring in May. It was a long goodbye.

MR. SLIFMAN. Why did you choose to leave the Board?

MR. STOCKTON. I was eligible to retire and the economic incentives were to do so, but that was only one factor in my thinking. I was interested in taking on some new challenges. And I was young enough to take advantage of opportunities. Another lesson of the crisis in my mind was that organizations need to have refreshment at the top on a routine basis, and I had been the director of R&S for 11 years. Ten years is probably about the right amount of time for any executive to run an organization. You tend to implement your biggest changes early on. Then there's a tendency towards greater stasis over time, which can be broken by getting somebody new to take over. Before Mike Prell left, I accumulated ideas that I was anxious to implement. And I'm sure David Wilcox was thinking about what he would want to do differently if the opportunity arose. So, from a lot of perspectives, it was the right time, and I don't regret that.

MR. SLIFMAN. What have you been doing since you left the Board?

MR. STOCKTON. In addition to some public speaking, I went to work for Macroeconomic Advisers in February a year ago. I did that for several months and then was asked by the Court of the Bank of England to conduct an external review of their forecasting process and performance during the crisis. So I moved to London in June of last year, and was

basically there from June to December writing this review. After I came back, I resumed my work with Macroeconomic Advisers. I'm also now a senior fellow at the Peterson Institute for International Economics, where I'm heading up their global economic forecasting effort and serving as their principal U.S. economist. While I was at the Board, I was mostly focused on domestic economic issues, whereas Peterson is focused primarily on international economics stuff. I'm learning a great deal, so it's a lot of fun.

MR. MARQUEZ. What do you miss the most from the Board?

MR. STOCKTON. The people, the amount of human capital here is impressive. You appreciate it when you're here, but you really appreciate it when you're not.

MR. MARQUEZ. When you need it, and it's not there.

MR. STOCKTON. Partly, it's the need, but partly just sitting down at the lunch table and being able to talk to someone who's a state and local finance expert, the next day it's somebody doing monetary policy or the Brazilian economy or whatever. And the passion that people bring to their work here is impressive. It all sounds a little corny, but it's true. Some of the criticisms that were leveled at the Fed about only caring for financial institutions and not caring about people are grossly inaccurate. I never had any meeting in my office where somebody said I'm worried about the welfare of some bank's shareholders, but I had plenty of people, once a month sitting in my office, concerned about what was happening in the labor market, especially when employment was dropping 600,000 a month. It was a passion for the analysis and a recognition of the importance of the work. I miss that.

When I spent six months at the Bank of England, I realized how much I missed being around people who were passionate about central banking, monetary policy, and economics.

The Structure of the R&S Staff

MR. SMALL. I want to ask a question about managing your research staff, hiring, and new and competing schools of thought. How did you manage the intellectual human capital that you just talked about, keeping it fresh, keeping it competitive, and keeping it up-to-date? Here are some kids who are more reasonably tooled, someone not necessarily that you plug in right away, but is there a danger that you will become too much of a venal production line?

MR. STOCKTON. There's always that danger, and those dangers are more acute than they were before the financial crisis because so much of everybody's effort has been focused on solving immediate policy problems or issues. One of the great strengths of the staff in the crisis was not the specialists that we had, it was the generalists that were able to think through how to design programs and think about the macroeconomic consequences of those programs. I worried about the Fed moving more in the direction of a staff of specialists versus the generalist type of staff. So it's going to be important for the Board to manage its way back to what we had, which was this nice combination of people letting their research programs inform their policy guidance, advice, analysis, and related work. That was a strong feature of the staff here.

The Bank of England rotates staff through jobs about every two or three years, so there are few specialists. That's good for the 10 or 15 people who will ultimately be part of the senior management structure, who will have gained broad experience with all aspects of the institution's responsibilities. But for the other 200 people or so, that frequent rotation can have costs. Relatively few people are doing basic research there, and the people that are doing basic research tend to be removed from the policy process, whereas at the Board, there were fewer people in white lab coats and most economists had both research and policy-related responsibilities.

How does the Board manage to provide that blend of responsibilities? In brief, they provide the resources to do research, hire sufficient talent, and provide compensation to retain many of the best people. The Board will need to remain mindful of the importance of remaining competitive for the best talent if the culture of excellence is to be preserved.

MR. SMALL. Although it faces challenges in this area, do you think the Board has understood this and has delivered as best that it could?

MR. STOCKTON. I think the Board understands. It certainly has provided many more staff resources. The salary issue has always been a more complicated one, balancing the political risk involved with raising salaries with the ability to pay people what is needed to remain competitive. There probably isn't as keen an appreciation by the Board members of these challenges as compared with division directors. One of the strengths of drawing people from the outside is that they come in with different views. But one of the weaknesses is they sometimes don't fully understand the pressures that the organization faces. The job of the division director is to communicate those pressures to the Board, but the receptivity varies over time depending on the predilections of individual Board members.

Former R&S "Think Tank" Group

MR. SLIFMAN. I have a question that I should have asked earlier. You mentioned, briefly in passing, what you called "the people in white lab coats." When you first came here, there was an internal think tank group within R&S—people in white lab coats, to use your phrase. The division moved away from that and made a conscious effort to disband that think tank as a separate unit and to disburse those people and those resources throughout the division. Were you involved in that process at all?

MR. STOCKTON. Probably to the extent that I complained a lot about it because I didn't have a white lab coat. Early on in my career, I wanted to do research and then felt overburdened by current analysis. So to the extent that people listened to me and to others who felt the same way, we played a role in that change. As division director, I maintained the structure of integrating research into sections involved with current analysis rather than separating it into a special unit.

MR. SLIFMAN. Do you think that it has a negative effect on recruiting, getting the best talent to come to the Board?

MR. MARQUEZ. The dispersion or the concentration?

MR. SLIFMAN. Not having a think tank.

MR. STOCKTON. It could. It's not clear which way it cuts in the aggregate. If you are able to hire and retain high profile researchers with few policy responsibilities that might help the effort to hire high-quality new Ph.D.s who aspire to research careers. The downside is that only a small number of people could be hired into those "white lab coat" positions. Most other economists would be assigned to jobs doing current analysis, with little time for research. That structure runs the risk of stirring up resentment on the staff, which can be detrimental to productivity. So a structure in which the great majority of staff share both research and policy responsibilities ameliorates these problems. Within that type of structure, the division would still retain flexibility about how to develop and deploy staff. People that are capable and productive in doing important long-term research will naturally, over time, get more time to do it than people that are less productive researchers. At our division strategic planning meetings, occasionally I would raise this question about whether we wanted to return to a structure with an

established "think tank" within the division. But there was really little taste for it, at least among the officers of R&S.

MR. MARQUEZ. Thank you for taking the time to participate in this interview.