Federal Reserve Board Oral History Project

Interview with

Karen Johnson

Former Director, Division of International Finance

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Interviewers: Jaime Marquez, David H. Small, David Howard, and Steven Kamin

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.

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MR. MARQUEZ. Today is May 12, 2010. This interview is part of the Oral History Project of the Board of Governors of the Federal Reserve System. I am Jaime Marquez of the Division of International Finance (IF). I am joined by David Small from the FOMC (Federal Open Market Committee) Secretariat in the Division of Monetary Affairs (MA) and by David Howard, previously a deputy director in IF[, as well as by Steven Kamin, a current deputy director in IF].

We are conducting an interview with Karen Johnson. This interview is taking place in the Special Library of the Federal Reserve Board in Washington D.C. Karen came to the Board in 1979. She was the division director in IF from late 1998 until 2007. She retired from the Board in February 2008.

Thank you, David, and thank you, Karen, for taking the time to be interviewed.

Educational and Professional Background

MR. MARQUEZ. Starting with your background, what attracted you to economics?

MS. JOHNSON. Before college, I had no concept of economics as a field of study. I went to Radcliffe College in Cambridge, Massachusetts, thinking that I would major in mathematics.

I come from a small town with one public high school. I'd been a good student particularly in math. When I got to the big city, I discovered that I didn't exactly understand what mathematics was. I felt totally in over my head. My small high school didn't offer calculus. I took calculus during my freshman year at Radcliffe. The level of abstraction in the approach to mathematics at the university level was so different from what they taught in my high school that I immediately felt that I wasn't going to be able to manage. But part of that

feeling was just the normal freshman angst in adjusting to a new environment and being in the middle of the pack as opposed to being one of the top students.

Some of my friends had taken introductory economics as freshmen, which I had not done. They talked about it. It seemed interesting so I took introductory economics. I was charmed by it. It seemed like a form of applied mathematics that had all of the dimensions that interested me. Like all introductory economics, especially then, they didn't use much mathematics, but if you knew some math, you could see the math implicit in what was going on. And economics was relevant to the world. Economics was a tool one could use to do good in the world. It involved some history and some political science but in a way that had some so-called rigor, and it had analytical content as opposed to just verbal whatevers. So economics seemed to suit me, and I never looked back. If I had studied anthropology, I might have discovered those things about anthropology, too, and never looked back. But as it happened, I went into economics, loved it, and stuck with it.

I graduated from Radcliffe with a degree in economics. After three or four years in Cambridge, my view of the world changed. It was that kind of place. To this day, I feel that people who arrive in Cambridge, Massachusetts, and never leave shouldn't be trusted because their view of life is warped. [Laughter] I know quite a few people who, at a young age, went to Harvard and never left.

Somewhere along the line, I decided I would go to graduate school. Having breathed the oxygen in Cambridge and consumed that water long enough, the idea that education and academics was the greatest calling became part of my thinking. I applied to a bunch of graduate schools and ultimately went to MIT.

I found graduate economics an order of magnitude different from undergraduate economics but, in much the same way, it met the kinds of priorities that I had. Now the mathematics was more evident. The analytical rigor was somehow more important. And professors were more careful than undergraduate economics professors. In undergraduate economics, they try to be interesting or relevant, but careful wasn't necessarily high on the list. In graduate school, these things get reordered. I enjoyed the company of my fellow students more in graduate school than I did as an undergraduate. I felt that I had a great deal in common with them in how I thought, what I thought was important, and what excited us intellectually.

Being an economics major at Radcliffe was an odd experience. When I was there, the women were housed separately from the men.[1] Once Radcliffe disappeared and the undergraduates all became Harvard students, things changed, in a way.

There were very few women in economics at the time I was an undergraduate. It's a big school, but it was possible to know the other women. There were so few women that early on you could pretty much figure out who in your class or the class ahead of you was also in economics. There weren't too many women to turn to for assistance, but you could get to know the guys well enough to study together. Unlike all your other classes, I could walk into an economics class and easily be the only woman in the room or one of two in the room. Even the math classes had far more women in them than my economics classes.

MR. HOWARD. There were probably few female faculty members and role models.

MS. JOHNSON. I remember at least one faculty member, but she was not the ideal role model. Regrettably, whether it was a math, history, or economics class, most of the professors

[1] Radcliffe College was a women's liberal arts college and was the coordinate college for Harvard University. Radcliffe conferred joint Harvard–Radcliffe diplomas beginning in 1963. A formal merger agreement with Harvard was signed in 1977, with full integration completed in 1999.

were men. And it is true still. The whole student body had fewer women than men in those days, but in the economics classes, there were almost none.

MR. SMALL. Were you a woman with a proclivity towards mathematics?

MS. JOHNSON. Yes, I think so.

MR. SMALL. Did that separate you?

MS. JOHNSON. The other women in economics had the same proclivity. At that time, the cutoff point on the SATs of Radcliffe women was higher than the Harvard men and the grade point averages of Radcliffe students were higher. The respective size of the classes was such that you cut off the Radcliffe women long before you cut off admitting men to Harvard. That's probably changed now because admission is at least 50–50 and maybe even a higher percentage for women today. I don't know, but it's certainly not what it was in the 1960s. All the women in economics were similarly mathematically inclined, quantitatively oriented, and serious students. People in other disciplines were aware that there were few women who majored in economics.

I had a good friend who was majoring in mathematics. Her father was a professor at Berkeley. She understood what the serious study of mathematics was about, and she was good at it. You could go to the people who were majoring in mathematics, of whom there were more, and get help with the mathematics if you needed it. But there weren't too many others in economics. We all got to know each other and maintain some relationship. I've lost track of several of them, but I know where a few of them are. Many of them went on to graduate school. Many of them taught and became professional women who are more or less my generation. We have seen each other or at least heard about each other through the course of our professional lives.

I was in college from 1963 to 1967, and then I went immediately to MIT in the fall of 1967. There weren't a lot of women there either.

MR. MARQUEZ. And you finished in 1973?

MS. JOHNSON. Yes, it took a while. There were maybe 4 or 5 women out of 35. Classes at MIT of entering graduate students are quite small—much smaller than at Harvard. Classes generally are smaller when you are a graduate student. All your classes are in the same discipline and with the same people. You get to know the faculty members in a way that as an undergraduate in a university setting you rarely do. You get to know the other students. I had a nice class of people, some of whom have gone on to become quite famous. Bob Merton and Bob Shiller, for example, were classmates of mine at MIT.[2] We talked to each other over lunch. Like all graduate students, you studied together, you tried to learn together, and you argued about points. Economics seemed to be that nice fit of a social science that wasn't just soft. As an undergraduate, we read Alfred Marshall's *Principles*, and you'd read what economics had once been. At the time, I'm sure people reading that thought it was technical, but with hindsight we all thought it was ludicrously verbal, fuzzy, and confused, even. After eight years of breathing the air and drinking the water, and particularly the intensity of graduate school, an academic career was just taken for granted—all serious people wanted academic careers, as far as I was concerned. [Laughter]

MR. MARQUEZ. Within MIT, you went towards monetary economics. What did you find attractive about monetary economics as opposed to development or micro or industrial organization?

[2] Robert C. Merton is an economist, professor, and a Nobel laureate in economics. Robert J. Shiller is an economist, professor, and best-selling author. [In 2013, Shiller was awarded the Nobel Memorial Prize in Economic Sciences.]

MS. JOHNSON. I had taken money and banking as an undergraduate. I found the fractional reserve banking, money supply multiplier relationship to be the neatest little trick I had ever heard of. [Laughter] I thought this was amazing. These people—it's like a magician! You start over here and you end up over there—and look what they did! In reality, this hypothetical mechanism probably never described the way the world worked—it certainly doesn't describe the way the world works now—but it made for fun learning to think about that process. In general, my intuitions are much stronger where macroeconomics is concerned than micro. And I could learn microeconomics; I did learn various fields. I never took industrial organization or narrowly applied micro topics. That was never where I thought my talents lay or where my interests lay. And, broadly speaking, macro seemed to me to be comprehensible, interesting, and filled with questions that needed answering. Money and banking was just a way of being in the general area of macro and yet narrowing it down to something more specific, as opposed to somehow just being in the field of general macro.

MR. HOWARD. In graduate school, a lot of times you are focused narrowly on classwork, dissertations, or whatever. But while you were at MIT, Paul A. Samuelson and Robert M. Solow were there.[3] Then inflation got out of control, and Bretton Woods collapsed and was gone.

MS. JOHNSON. It was exciting times. The idea of being a macroeconomist then was terrific.

MR. HOWARD. Do you remember debates, seminars, or classes with those guys?

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[3] Paul Samuelson was the first American to win the Nobel Prize in economics. He was considered to be one of the founders of neo-Keynesian economics. Also he was instrumental in turning MIT's economics department into a world-renowned institution by attracting noted economists to join the faculty including Robert Solow, Paul Krugman, Franco Modigliani, Robert C. Merton, and Joseph E. Stiglitz, all of whom went on to win Nobel Prizes.

MS. JOHNSON. Sure, but the most important event that colored my graduate experience is the Vietnam War. From about the second year, after we had taken our general exams and were writing or thinking about writing dissertations, the issue of not being drafted out of school and sent off to war preoccupied all the male members of my class, and different people did different things. We lost a couple of really bright guys who felt so vulnerable that they ended up changing careers, moving to Canada, or doing something, and never finished their degrees. I've lost track of those people—but at the time we all thought that was a shame. It pervaded life. In my own family, I have an older brother, for example. In general, that period of time in the United States was dominated by concerns about the Vietnam War, as well as things like the civil rights movement and so forth that were also going on.

Solow and Samuelson were huge figures in the economics department at the time. They were in their prime. Samuelson was no longer writing the pathbreaking articles that he had written as a young man, but he was clearly the elephant in the room. Solow was this wonderfully gifted teacher who could help you understand anything. Along with several other famous people who went on to win Nobel Prizes, they really determined the nature of the atmosphere being a graduate student at MIT. They attracted other wonderful people who were my fellow students, whom I found equally interesting to talk to, and equally stimulating. It made for a nice environment, and I'm sure it still does, with a different cast of characters. I'm sure most graduate schools function that way.

MR. MARQUEZ. You mentioned that the Vietnam War colored your experience, but not necessarily the choice of field, correct?

MS. JOHNSON. Right. That had been done long before. When I made that choice, those issues weren't in the picture. Four years went by, and I wasn't done. And, by that point, I

had two kids. So I took a job teaching at Wellesley College. It was then that Bretton Woods fell apart. We had the wage–price controls, and we had a host of macroeconomic things going on. I transitioned from being in an environment where I saw my graduate students a lot to one where I was the one doing the teaching—to undergraduates. I was in the midst of all this. We would have programs for the students, and we would talk. There were constant conversations and debates about the wisdom of this or that: anti-inflation policy, what to do with oil. And then the oil-price shock hit toward the very end. The sequence of events, starting with the breakdown of Bretton Woods a little bit before 1968, then inflation issues and the oil shock, came at the time in my life when I was in graduate school and when I was still on the East Coast and I was teaching at Wellesley. It made it all seem exciting, relevant, and lively. Milton Friedman was still a force to be reckoned with. He had adopted a somewhat contrarian position, shall we say, relative to the East Coast types, and those debates were lively. Monetarism hadn't really evolved yet because the inflation rate hadn't gone up by 1969, 1970, or 1971. That wasn't so much the issue. But Friedman had long been in the debate about the structure of the consumption function. He had long been in the debate about other things, exchange rate choices—the East Coast people all had views on that—and he was writing and speaking. It was heady times. It was a good time to be an economist, or exciting to be an economics student, anyway.

MR. HOWARD. In the early 1960s, Samuelson was a consultant to the Council of Economic Advisers (CEA) and, from 1961 to 1962, Solow was a senior economist for the CEA. There was a relatively stable Phillips curve.

MS. JOHNSON. Samuelson never left Cambridge; he was an ad hoc adviser to President Kennedy, so that was a short period of time. I don't know that he played that role with Lyndon Johnson. Bob Solow became the fourth unappointed, unofficial council member. He came to

Washington and became chief of staff, or they gave him some title. He was kind of the fourth guy. Arthur M. Okun or Walter W. Heller were the people who loomed large at that time.[4] The debate over the Kennedy tax cut loomed huge. That was heralded widely as the first Keynesian macroeconomic policy, the first move done explicitly for the purposes of influencing the economy, as opposed to being done because the government needed the revenue. It was as if macroeconomics had come into its own. We had all the answers. It reminds me of how we felt right before this recent crisis broke [laughter], and right before things went to pot on inflation in the decade of the 1970s. For a brief shining moment, things were good and we thought we had all the answers. Of course, things got really bad. And it's a little like how the profession evolved between the Great Moderation and inflation targeting.

MR. HOWARD. Did you get a sense of where Samuelson and Solow thought it went wrong? Do you remember class lectures or debates?

MS. JOHNSON. I don't think so. There were lots of debates about the appropriate response, say, to the oil price shock, which was how people saw the problem initially. The role of the breakdown of the Bretton Woods system and the consequent dollar creation process or money creation process in contributing to high inflation problems of the 1970s was not focused on at the time. There were big debates about how to think about the oil-price shock and what the right monetary response was to the oil price shock. The fights were more about price controls versus no price controls than they were about one theory versus another in understanding the economics of the time. Solow and Samuelson probably felt that they had a pretty good understanding of what was going on. My guess is that, by the middle of the 1970s, everybody was puzzled, in particular because nobody understood—and I think to this day, don't

[4] Walter W. Heller was Chairman of the CEA from 1961 to 1964. Arthur M. Okun became a member in 1964 and CEA Chairman from 1968 to 1969.

understand—why the rate of productivity growth slowed as much as it did. To begin with, people didn't appreciate that fast enough. The data didn't enable you to understand in real time that productivity was declining on you. And we never did come up with a good story on why productivity got that low at the time. So we didn't feel like everybody else was wrong and we knew the answer.

They were genuine puzzles; there were people who would debate and recognize that some of their understanding of the Phillips curve, for example, couldn't possibly be helpful. Edmund Phelps wrote not too long after that, and people began to get an understanding of how to go back and fix their understandings. I think this is the way people learn. I didn't take the course, but Samuelson from time to time taught the history of economic thinking. In that context, my guess is that he had recognized this cycle of learning something, of being, as a profession, excited about the new insights that it offered, carrying it too far, being too confident, then seeing the problems come. That's how the world has worked for a long time, so he probably understood that to a degree.

If you think about what these people had gone through—the Solows and Samuelsons of the world had been young at the time that Hicks produced his synthesis of Keynesianism. In many respects, Hicks should be credited more than Keynes because Keynes is almost impenetrable unless you've been taught it through the lens of Hicks. But they had gone through that, and Hicks had explained what this was really all about. It was exciting and wonderful. Then Phillips puts this empirical spin on it. Then you push it too far and it kind of falls apart.

In many ways, in a smaller context, Bob Solow had played that role. Growth theory was being born at the time that I received my undergraduate degree and moved into graduate school. Harrod, Domar, and people like that were writing these strange articles that had clumsy

representations in equations.[5] Then Solow put it all together and explained the general model—how this is a special case when you make this assumption and that's a special case when you make that assumption. It's a tinier piece of economic literature than what Hicks did, but Solow had played that role for growth theory. Then, all the rest of us can do the footnotes. Most research is taking some brilliant breakthrough and writing a whole bunch of footnotes to it.

In 1960, 1961, 1962, people had a lot of confidence about Keynesian tax cuts, fine-tuning the economy. "Fine-tuning" was not a dirty word then; it was precisely what you were trying to do. I remember the definition of 4 percent as the target unemployment rate was explained as an intermediate target: "We're going to get the unemployment rate down to 4 percent. Once we reach that, we're going to think about how much lower we can get it." People understood that it was politically absurd to announce zero as the target, but they were on thin ice with announcing that any unemployment should be the objective of the government, so they characterized 4 percent as an initial, intermediate goal, and then we would go from there.

People believed, and I believed, that with the right science we could manage the economy so that it operated just fine forever. We would become like the physicists who ran the electricity grid. You didn't think about them. They no doubt developed new instruments that enabled them to improve on the wiring or the transformers or the generation process, but nobody paid much attention. You just let them do it, and we had electricity. That was the attitude in economics. We'd found these key breakthroughs that enabled us to manage the economy. It was quite shattering when it all seemed to fall apart in the early 1970s. It was shattering for everybody.

MR. MARQUEZ. And it is at this point that you go to Stanford.

[5] Sir Roy F. Harrod in 1936 and Evsey Domar in 1946 independently developed a model to explain an economy's growth rate from savings and productivity of capital. Solow extended the Harrod–Domar model.

MS. JOHNSON. Yes. I lived in an academic environment because I was seriously considering an academic career. I also thought that the lifestyle of an academic would allow me to manage work and family life since I had two children. I was quite wrong about that. It was difficult to manage a family life and succeed at work. At that time, you had to work many hours to succeed in academics. Maybe others could successfully balance the two, but I certainly couldn't.

But I thoroughly enjoyed my time at Stanford. And if I had it to do over, I wouldn't do it any differently. I met a lot of new people who hadn't been drinking the water in Cambridge for eight years. They had different ideas, and that was a good thing. Stanford was a serious academic place; the students were of high caliber. I know people that have become disillusioned with teaching, in part, because they are at a school where the students aren't particularly fun to teach. It wears them out quickly. At Stanford, they were all bright people. Lewis Alexander was a student of mine at Stanford as an undergraduate.[6] He introduced me to his parents at some graduation event. Later, when I was at the Board, I remember seeing the name in some context and that he graduated from Stanford. Somebody asked me about him and I remember saying how strong he was. When he was a senior, he took my graduate-level intro macro course and was one of the best students in the course. He made an impression on me. I don't remember too many other undergraduates from Stanford, but I got to know him and remain friends with him forever.

MR. MARQUEZ. In the early 1980s, there was something about sovereign debt. He was doing some work on his thesis and was brought here to give a seminar.

[6] At the time of this interview, Lewis Alexander was a Department of Treasury adviser and former chief economist of Citibank. At one time, he was a deputy director in the Board's Division of International Finance.

MR. SMALL. As a first-year faculty member at Sanford when you arrived, was your mission just to do a good job and publish or were you bringing the new economics or more rigor into the first-year program?

MS. JOHNSON. I don't know by whom—possibly by faculty members at MIT—but I was warned that the Stanford economics department had a reputation of being hostile toward women. I was adjusting to many changes, including moving from the East Coast to West Coast and from being a student to being a faculty member. I kept wondering at what point you cross that threshold from being a student trying to learn and understand things to this font of all wisdom that faculty members had always seemed to me to be.

The senior faculty at Stanford was a colorful group of folk. I made close friends with some of the junior faculty members. Having gone to Radcliffe as an undergraduate, and then to MIT as a graduate student where there were few other women, I had mastered the issue of dealing with a mostly male environment. It didn't faze me. I just laughed at some of the foibles of the various people that I met there. In the course of my six years there, there were one or two young female assistant professors that I tried to befriend and help. There were no senior women. I can't remember whether there were even any female visitors the whole time I was there. It was a very male environment.

The student body was mixed. Many undergraduates were female. One graduate student that I became friendly with was Andrea Kusko. And when I came to the Board, Andrea was here (now a senior economist in the Board's Division of Research and Statistics). Guillermo Ortiz, who went on to become governor of the Bank of Mexico, was one of my students. It was a strong department that attracted good students. Some of them went on to become prominent economists. I maintain professional relationships with some of them. Teaching at Sanford was

good training. I would encourage most graduate students, perhaps not all, but most graduate students, to take an initial teaching job to round out their training.

MR. MARQUEZ. While they're finishing their thesis.

MS. JOHNSON. While they are finishing their thesis or even right after finishing—like a post-doc kind of thing. In my experience, teaching something is the best way to learn it. Having to solidify my understanding of macroeconomics by teaching it at the undergraduate level and the graduate level for a few years served me well. I don't at all regret the years I spent at Stanford. I enjoyed the teaching. Because I like teaching I have always thought that when I left the Board, I'd teach at some college for a while.

MR. SMALL. Do you remember any gems of wisdom that you taught from textbooks or advanced graduates' papers?

MS. JOHNSON. I was just doing my job and getting through. Remember, I had two little kids at home, so I didn't have that much time to be too philosophical. [Laughter]

MR. SMALL. Do you remember your textbooks?

MS. JOHNSON. I was always a big fan of James Tobin.[7] There were some Tobin articles. Of course, there were always the landmark Samuelson articles. The one I particularly loved was the one about interest with or without the social contrivance of money.

MR. MARQUEZ. That's the overlapping generation one.[8]

MS. JOHNSON. Yes, overlapping generation. I loved that article. In just a few pages, the article hits you in the face what you're really talking about. I also loved Tobin's debt management article. But it was not so clearly written, and you had to keep rereading it to

[7] James Tobin (1971), *Essays in Economics, Volume I: Macroeconomics* (Amsterdam: North-Holland Publishing).
[8] Paul Samuelson (1958), "An Exact Consumption-Loan Model of Interest with or without the Social Contrivance of Money," *Journal of Political Economy*, vol. 66 (6), pp. 467–482.

remember it. Portfolio balance then blossomed at some point, but this early Tobin debt management article had the seeds of portfolio balance as a way of thinking about macroeconomic—the financial side of a macroeconomy in it. I do not remember even using a graduate textbook. We taught from articles. You had this long reading list of journal articles.

MR. SMALL. Do you remember, for example, Robert Lucas?

MS. JOHNSON. See, Lucas would have come along more or less contemporaneous with that, so I'm sure in the years that I was still teaching, he was not on the horizon.

MR. SMALL. You mentioned Ned Phelps.

MS. JOHNSON. Yes, right.

MR. SMALL. There was the Friedman presidential address on the natural rate.

MS. JOHNSON. I think that was 1968. Those debates were still very lively. We had textbooks for the undergraduates. I had a relationship with a couple of publishers. I would either read or comment on a book or, in one case, I did a workbook for a publisher for a textbook. One of the textbooks that crossed my desk in draft form was Dornbusch and Fischer.[9] I remember being very impressed with that as an undergraduate textbook, but it was in the drafting stages when I was teaching. It's now probably no longer available. I was the research assistant on one edition of Samuelson, which I proudly have on my shelf with an acknowledgement in front.

It was a world that was caught up in too much inflation and oil price shocks and a macroeconomy that was disappointing. It was also a world that was caught up in Richard Nixon and Watergate and trying to get out of the Vietnam War. These things are all one big mix of the time that I was at Stanford.

[9] Rudiger Dornbusch and Stanley Fischer (1978), *Macroeconomics* (New York: McGraw Hill).

MR. MARQUEZ. As you saw your time at Stanford coming to an end, you had a choice of going elsewhere to pursue an academic career.

MS. JOHNSON. It was clear that I had not published enough to get tenure at Stanford, but you go through the process partly because you can't avoid going through the process. The AEA (American Economic Association) had standards and mandates, so they have to process everybody. I could have resigned, but I went through the process. Part of the reason you do that is to let people generally know that you might be available because these letters are going out and questions are being raised. I talked to other schools. I remember going to the meetings of the AEA in Toronto when I was about to receive my MIT degree and interviewing with a bunch of schools. I must have interviewed with the Fed. I don't think I actually came to the Fed and gave a presentation the first time around, but I probably don't remember all the places I went the first time around.

In 1979, I got a phone call from Dale Henderson. Dale would keep lists. He knew six years was the right time. [Laughter] So he would call old friends, go back to his list, figure out whose six years were up, and contact them. And he contacted me. I very much needed a job. I had talked to schools but, at that point, the range of schools that might be interested in me was a lot different from the range of schools that had been interested the first time around.

MR. MARQUEZ. David, you were here at that point, correct?

MR. HOWARD. Yes. I was on the recruiting committee. [Laughter]

MS. JOHNSON. The Fed was at the top of its quality of researchers and people. And I'd figured out that the flexibility implied by being an academic wasn't quite as simple as I had imagined. Managing family and work would be easier to do with a job where the hours were more routine. The expectation that you were going to work all the time you weren't sleeping

wasn't quite so great. So it seemed that the decision I'd made six years earlier ought to be flipped. I would have an easier time managing my overall life here at the Board than I would if I took another academic job, especially if I took another academic job where I didn't already have tenure, and my chances of getting tenure were good only at a lower school. I didn't have enough publications for even a middle-range school. It turns out that I wasn't really cut out to be an academic-style researcher. I devoted way too much time to teaching and students. I let myself get distracted all the time. I didn't have the narrow focus on research that it takes to be a successful academic. And maybe I didn't have the right intellectual tools, either. Anyway, coming to the Board seemed the right compromise then, where six years ago I had made the decision that a different compromise was the right compromise.

Joining the Board

MR. MARQUEZ. In 1979, you arrived at the Board to work in IF. Edwin M. "Ted" Truman was the division director at that time.

MS. JOHNSON. Yes.

MR. MARQUEZ. Coming to the Board, did you experience another round of adjustments?

MS. JOHNSON. Oh, sure. Most academics are basically self-employed. They set their own agenda. Few of them—at least the ones that I knew—went to one school and stayed. They were mobile people whose loyalties to a particular institution were only as long as they were there, unlike an alumnus whose alma mater is forever. Academics were more interested in their own career, their own publications, their own selves more than they were the context in which they were working. I don't think I ever coauthored anything at Stanford. Everything I tried to

do at Stanford was just me, which is an indication of how isolating and how male-dominated Stanford was.

At the Board, you're supposed to work with people, and that took some getting used to. And having people comment on your writings took some getting used to. Charlie Siegman would send a document back to me indicating that a sentence wasn't correct and so forth. [Laughter] The collaborative nature of the Board work was a new experience for me. It was difficult at first, but after a while I concluded that I was well suited for the job. This made a whole lot more sense as a way of doing economics for me than trying to go back into an academics mode. And to have done that, I would have had to behave here the way an academic would have behaved. I would have had to isolate myself as much as possible; write, write, write; do all the research I could; and try to get an important article out in order to attract an academic offer.

MR. MARQUEZ. You already knew that from Stanford.

MS. JOHNSON. Yes. I wasn't interested in doing that. After I got over the shock, I liked the fact that instead of working all by yourself or with one other author and focusing solely on getting publications out, we were doing other things. Early on, I decided that whatever I was going to do, I wasn't looking to use my time at the Fed as a stepping stone to get back to academics.

MR. MARQUEZ. Your teaching, for example, or your views on monetary policy, particularly your theoretical views, do not emphasize the openness of an economy. That has changed over the years.

MS. JOHNSON. I was every bit as closed-economy-minded as the rest of the monetary economists. I had taken international as a field in graduate school, but for six years at Stanford, I

taught what amounted to closed-economy macro with this little extra sector of "X minus M" they'd throw in every once in a while.[10] And rarely in my money and banking courses did international issues come up, even given all that was happening. Thinking about it now, it's startling in a way. Bretton Woods had broken down; exchange rates were in principle flexible. In my courses at Stanford, we spent very little time discussing exchange rates, et cetera, and I had control over how much talking about exchange rate determination there would be. Much of monetary economics was still thinking about the United States as a closed-economy system. We left the issue of exchange rate determination to those who taught the international courses. Most money and banking textbooks might have had a chapter at the end. That was partly because you were driven by a gold-dollar-mixed standard in which U.S. interest rates were decided by the Fed. Everybody else had to grapple with interest parity, but the United States didn't have to grapple with that. So books about the U.S. financial system, books about how monetary policy worked in the United States, textbooks from that era didn't devote much time to the topic, so neither did I.

When I came to the Board, I worked in the international division largely because Dale Henderson is the person who called me about a job. I was courted by both the Division of Research and Statistics (R&S) and IF, and I chose IF. I had enough background study in international, the world was open enough, and I basically learned from my colleagues like David. I absorbed all I could, worked with people, and began to be more and more open-economy-minded. But that took some time for me.

MR. MARQUEZ. Until recently, the conduct of monetary policy in the United States has been largely a closed economy.

[10] Exports minus imports.

MS. JOHNSON. It has been gradual. Nothing changes overnight in this world.

MR. MARQUEZ. One episode that I think brought about the issue of openness was the first debt crisis with the LDCs (less developed countries). It created a sense that we were exposed to developments abroad in a way that we did not think we were exposed.

MS. JOHNSON. I think we should separate the issue of financial globalization—the opening up of capital flows—and what that did to the world and what it did to how we think broadly from the macroeconomic issues of an open-economy model. There have been huge changes in the way the profession and the financial sector think about global capital flows and linkages across financial markets in the transmission of shocks. I'm not so sure that's the same thing as what a real-sided macroeconomist would say is the difference between an open-economy real macro model and a closed-economy real macro model—they're not independent, but they are distinguishable. We've come much farther in understanding how much the U.S. system is exposed to the consequences of financial events around the world than we have in believing that macroeconomic equilibrium in the United States is importantly determined by a fiscal policy shock in a trading partner.

MR. HOWARD. Yes. The international influence has become more apparent to policymakers over time, but I think they were aware of the importance of it long before they started thinking about open-economy macro models. The Board created IF about 60 years ago. For the Fed, among others, the world changed when Bretton Woods fell apart, and that was some time before Karen.

MS. JOHNSON. Before the collapse of Bretton Woods was the emergence of the Eurodollar market. Many an hour was spent in graduate seminars trying to figure out how to think about the Eurodollar market, what it meant for the control of monetary policy. Did it limit

a country's ability to control monetary policy, reduce it, turn it to zero? What is the Eurodollar market? And that had happened early on. It was one of these financial developments in the actual world that showed the U.S. economy is open rather than a real-side issue focused on how you build the right model. For example, people didn't take Mundell–Fleming seriously in how you think about the United States.[11] They thought the United States was sufficiently big, that a Mundell-Fleming approach—namely that you could have fixed exchange rates and independent fiscal policy or floating exchange rates and independent monetary policy, but you couldn't have both an independent fiscal and monetary policy—did not apply to the United States. I don't think anybody took that seriously as a restriction on the United States, although they might have taken it seriously as a restriction on small, open economies. It was the size of the United States that probably exempted us from this conclusion, or the fact that they couldn't find any significant empirical reason to take the Mundell–Fleming model seriously as a way to think about U.S. macroeconomic policy. But issues like the Eurodollar market, the role of gold, the breakdown of the Bretton Woods system, international capital flows, all came on one right after another and forced people to realize that we operated in an increasingly open financial world.

MR. SMALL. What I'm hearing is that, if you take a standard Keynesian model, almost the type we have now, it's largely consumption flows, investment flows, and international didn't figure too much into that. You talk about the shocks in financial crises that come on later on. What about when Volcker became Fed Chairman in 1979, and there was a huge dollar crisis?

MS. JOHNSON. Again, at the time, the rest of the world was seen not as going to have a big impact on the United States, but as a kind of macroeconomic laboratory where policy is being based on other ideas and other policies are being pursued and one can learn from them. In

[11] Editor's note: The Mundell-Fleming model, named after economists Robert Mundell and Marcus Fleming, is an open-economy version of the IS-LM model.

the United States, in the closed-economy world, it was Milton Friedman on the one side versus James Tobin and Paul Samuelson on the other. By the time you get to about 1979, it's the Bundesbank on the one side and the Fed on the other. And then, God help us, we got Margaret Thatcher, and it was the Bank of England doing one thing and the Fed doing something else. In that sense, I have always been an advocate that one role of IF is to be a conduit of those experiences and that analytical framework and those ideas to the Board and to the rest of the staff here. Things being done elsewhere, ideas being explored and the evidence that can be mustered for or against, are valuable resources for learning. Such lessons can be more important than the fact that Germany's practicing this kind of monetary policy and we're practicing that kind of monetary policy, and when you write the equations down, you get this outcome. It was more that Germany was putting forward a point of view that we needed to consider, not so much that its policy was going to affect us by so many tenths of a percentage point of real equilibrium this year.

MR. HOWARD. That was the prevailing view in IF. Whether or not we were fooling ourselves is another matter. We viewed ourselves as doing basically what Karen just pointed out. We always modeled and forecasted the external sector, the X minus M part, so we had that base covered.

MS. JOHNSON. When I came to the Board, because of my background in teaching money and banking at Stanford, I was assigned the cross-country topic of monetary policy. This was a time when other people were setting money growth targets, and we were deep in the weeds of arguing about it ourselves. I arrived here just weeks after Volcker and just before the new operating procedures. I was trying to get up to speed quickly, particularly in briefing the Board twice a year on monetary policy as implemented in the other major industrial countries. That

was a tremendous career boost and learning experience in that it put me at the Board table as a brand new member of the staff. That might not have happened with such speed for somebody else. One of the reasons they did it was because I had six years of professional experience at a major university. They weren't going to give somebody who'd written my dissertation that kind of role in the division. They were acknowledging the experience that I had in academia. It quickly brought me up to speed on what other countries were doing, which I had been vaguely paying attention to but not focusing on as an academic. Then Volcker came up with these new operating procedures. That was dead on. I felt immediately involved in all the debates going on here in a way that was instrumental in making me quickly feel that this was a lively place and this was a place where important ideas were being discussed. People were trying to be "careful" is the word I used, but rigorous. That was an eye-opener.

During the 1970s, partly because the Fed was failing to control inflation and partly because of the personalities involved, academics were rather scornful of the Fed. It was commonplace at major universities to think that the Fed didn't understand this or that. Milton Friedman was basically saying the Fed didn't understand anything. I wasn't a Friedman-ite. The Tobin–Samuelson view was not quite so critical of the intellectual apparatus that the people at the Fed were using. But in the implementation of policy, there were lots of people who were quite critical and even disrespectful of what the Fed and the Fed's staff was like. When I came to the Board, I discovered that there were smart people here who were trying to be very rigorous. They were thinking hard, and that was something of a transformational experience also. Between Jimmy Carter's aborted attempts to talk people into lowering the inflation rate, it wasn't hard in 1978, 1979, to be scornful in general of the policies the United States was using to fight inflation.

The Rise and Fall of Japan's Economy

MR. MARQUEZ. From FOMC transcripts, I have noted that there was a recurring theme—an interest in Japan. I don't know whether it was because of the circumstances or because of an innate interest.

MS. JOHNSON. It was largely personal. Neither the Board nor the division director ever communicated to me to make Japan a focus. Even now, in retirement, I have toyed with the idea of some longer, deeper work on the parallels between Japan's experience and the U.S. experience, but I haven't started that work or decided if I will ever do it. In a different generation, my guess is you would have looked at the U.K. economy, a dominant global economy about which people were trying to find the secrets for why they were doing so well, then it went down after the Great Depression to the pits.

When I was young and filtering all these ideas. Japan was succeeding beyond everybody's expectations. Japan had the answer, and we wanted to show exactly how Japan had the answer. To some degree, it was an industrial policy—Japan, Inc.—a top-down approach to what you invested in, how you organized industries and firms, the links between the financial sector and the real sector, and their work ethic. That Japan had done all of this from 1945 on, when the country was basically in ruins, was itself staggering. I had lived through the 1950s as a kid, where the "Made in Japan" fights had been fought and Japanese goods were thought to be inferior. Suddenly Japan was producing the best cars and the best electronics, and its steel industry and shipbuilding industries set global standards. How did Japan do this? Japan was riding high until the economy collapsed in the bubble. In the 1970s, Japan had high inflation. Japan was thought of as among the high-inflation countries in the post–oil shock world. It had never adopted monetarism in any significant way, and it didn't have a strong central bank. It

wasn't like the Bundesbank, where you could look to them for important articles or leading a separate intellectual school in Japan, though the central bank was in the background and pretty much a secondary player. But the Japanese economy was fascinating. I probably would not have become as interested in it if it hadn't had the bubble and it hadn't collapsed. The fact that it went from one extreme to the other was interesting. I thought there was a lot there—and I still think there is; we just haven't mined it properly.

MR. HOWARD. It wasn't just your personal interest; it was a major issue. Japan is a major trading partner of the United States, for a start, and it has all of these extraordinary features to it. It had huge success and the big bubble. Japan grappled with liberalizing their financial system, and then it went into the tank and had these horrible problems—still does—with what to do about monetary policy. So I think it's intrinsically important, and you would expect the International Finance Division to put it front and center.

MR. MARQUEZ. My sense is that one reason why Japan managed to call your attention is because after Japan was doing so well for so long, it had a bad moment; monetary policy responded in a way that at the time people thought was okay. Then, from reading [in the FOMC transcripts] your answers to questions from the different Governors and Reserve Bank presidents, my sense was that there was an element of mystery left over that had to do with monetary policy.

MS. JOHNSON. Yes, absolutely. If you were to sample the profession widely, you'd get a host of different points of view. And I don't know that the division had ever tried to come to a consensus point of view or even a majority feel this way. I am not, and never was, a real expert on the Japanese economy. I don't understand exactly how pre-1990 Japan worked as well as it did. There are very careful studies done by people trying to discern how much higher, say,

the saving rate in Japan was than in the United States. For example, when I was more a macro person than an international person, the debate about the consumption function loomed large—trying to figure out how the lifetime average propensity to consume, which is the bedrock view of the lifecycle hypothesis, manifested itself in Japan in a higher saving rate than the United States. You could correct for demographics and this and that, and you could narrow the gap, but you could never really resolve if that theory is true in both places. How do we reconcile the data to it?

When the Japanese stock market was going to 40,000, the Fed Vice Chairman, Manny Johnson, assigned a project to IF to do much the same sort of deep analysis to understand Japanese stock prices. At the time, you look at that and you see price–earnings ratios in Japan that seem vastly different from price–earnings ratios here. If you dig deeply, you realize that many of the Japanese stocks are held in a close circle of crossholding situations with other firms. And you might persuade yourselves to treat those stocks as if they were held by the firm's treasury as opposed to being held by the public and so forth. Maybe there's additional accounting issues—I don't know.

Anyway, Bonnie Loopesko was assigned to work with Lawler.[12] They wrote this huge paper. I think Volcker was the Fed Chairman. Manley Johnson wanted to give a speech. I think he was even a coauthor on the first version. I don't remember the details. Anyway, they did a tremendous amount of work trying to reconcile and understand the behavior of the Japanese stock market and the behavior of other stock markets, particularly the United States, and understand that. These kinds of questions were floating around about Japan aside from issues of whether we should pick winners and subsidize shipbuilding or get out of shipbuilding because

[12] Patrick Lawler, Bonnie Loopesko, and Marc Dudey, "An Analysis of Some Aspects of the Japanese Stock Market" (Mimeo), November 10, 1988.

the Japanese were taking over shipbuilding, or issues of what to do about the car industry. Those were much more pragmatic, but these other topics were the ones where the Japanese economy was just different, and yet it was succeeding. At the time, Japan was thought to be the most successful economy in the western tradition. All that goes along until, suddenly, through pressure from the United States and possibly others, we produce a version of the Big Bang in Japan, and we force them, or they choose to give into the pressure, to liberalize some of their financial regulations (we the United States, not we the Federal Reserve). The Japanese banks became even bigger. There was a long period of time in which everybody worried that the 4 biggest banks in the world, or among the top 10, were four Japanese banks.

MR. HOWARD. Right. I think the top 4 were Japanese.

MS. JOHNSON. Not only was the Japanese auto industry, steel industry, and shipbuilding industries outcompeting us, their financial industry was outcompeting us. Then there was this bizarre period in which Japanese asset prices behaved one way and yet goods and services prices and inflation remained very low. Then we had the bubble and the collapse, and we all started fighting about what Japan should do. IF, amongst ourselves and with other interested parties, had lively debates about what should be the right course of action for the Bank of Japan. Some people in R&S decided they had views on what Japan would do, and we would debate them, too. It was a fascinating experience of how all these different pieces happen one after another and how we got Japan where it is.

So aside from the sheer interest in this set of events, Japan matters because by some measures it is the second-largest economy in the world, although you have to treat Europe as a bunch of small economies to come up with that number. We know that China is very cognizant of the experience of Japan and wants to make sure that it takes the good lessons from Japan but

doesn't follow the bad experiences of Japan. So understanding and debating about what actually happened in Japan is of more than intellectual interest; it's of interest in understanding today's world and what to do in today's world. I think we all were prone to run that one up the flagpole.

Monetary Unions: The Case of the Euro

MR. MARQUEZ. How did the introduction of the euro affect the international division and your work as division director?

MS. JOHNSON. I wasn't entirely objective. I was inclined to weight the arguments in favor of the euro and was quick to minimize the arguments against the euro. In the division, we had a variety of views. All the Board members had views as well. The Federal Reserve, rightly, in my opinion, tried hard not to appear to make a public, overt statement that took sides or supported any particular pro or con argument because this was another country and another sovereign area. The Fed wanted to maintain appropriate interest but not appear to control or influence attitudes about another country or set of countries.

I remember having debates within the division. For part of that time I was chair of the briefing committee. We would organize briefings for the Board members on different pieces of the single market, a topic that preceded monetary union, which was an add-on to the single market, and that many people thought was a step too far and other people thought was important and useful. And we all waited for 1992 to come because that was the benchmark date at which the single market, per se, would come into being. Now, of course, it's ancient history; that was almost 20 years ago. There was a cottage industry of people in the United States who struck out to do different studies about the wisdom of the Delors Report (on economic and monetary union) and what to do. This is a question with many levels of interest. There is the purely analytical issue of what are the criteria by which you might decide to form a monetary union, and how does

the euro area, as we now know it, measure up to those criteria? We did some of that work. What are the institutional arrangements you should contemplate, given that you've decided to make a monetary union, and what decisions should you make about your institutional arrangements? We did some work on the former. We did a lot more work on this issue in part because the Federal Reserve, being a federal system, was of great interest to the Europeans. We would have visiting scholars come from central banks that were going to write. We had academics. We interacted with them in different ways. We wrote papers about comparisons of this, that, or the other thing. We did a fair amount of—is the Federal Reserve a good model for a federated European central bank or is it not; why this, why not that? This was before I became division director, partly starting with my original role doing the monetary briefings.

I always saw one channel of work that the division did as something I called comparative central banking. We became a storehouse of human capital for the Board on different aspects of central banking that could provide a range of experiences—in the context of what David and I were talking about earlier, that our job was to scan the rest of the world and find useful information and interesting episodes to inform them. We had at least two go-rounds with people in the Congress, coming up with either budget questions or governance questions. We did survey-type work where we'd figure out how all the other central banks of the world do X, Y, or Z, and we put together big databases and produced papers. This comparative central banking is something that has many guises, many topics under it, and this was part of it—the voting structure, for example, of the FOMC versus the voting structure of the European Central Bank (ECB) or the ownership structure of the Federal Reserve versus the ownership structure of the ECB. You could go on and on. It was why we were in international; we found this stuff attractive to work on.

The ECB came into being as an institution in 1999 just about the time I became division direction. Prior to that, there was the EMI (European Monetary Institute), which had been the nascent ECB as they were answering some of these questions. We had always found that one needed to be careful in dealing with the numerous central banks in the ERM (European Exchange Rate Mechanism)[13] system to avoid being caught in the middle of a purely European fight. We had spent an absurd amount of ink on the various ERM crises that had come along, not to mention the infamous United Kingdom sad exit from the ERM.

The ERM crises would liven up our lives every time they happened. We'd work furiously to figure out the analysis of where we are going to be, where the Italians were going to be, how much were exchange rates going to move. And there were big issues about whether the DM (deutsche mark) was going to appreciate or the French franc was going to depreciate and how the hell you could tell the difference. [Laughter] Those struggles posed practical problems to the United States. I came to agree with the idea that movements of the dollar exchange rate for various reasons having to do with our monetary policy or political events, such as when the dollar moved significantly and fairly rapidly, created pressure in the ERM; so there was a link between the dollar and ERM events. Whether the dollar was going up or down, life in the ERM was a function of what was happening, to some degree, to the dollar–DM rate. That led to an added layer of complexity about how our relationships with the counterpart central banks worked. Whether they blamed us for causing the problem or whatever, that was an added level of complexity. So when the institutional solidification came about by form in the ECB, in that sense it made some of the dealings of the Fed and our division more straightforward. We now had a clear set of single counterparts—we could identify colleagues at the ECB who did this, and

[13] An agreement among countries to keep changes to their mutual exchange rates contained within a band.

this, and this. There was that one institution. It was responsible for dealing with harmony within the system—

MR. MARQUEZ. With the ECB.

MS. JOHNSON. —with it. In that sense it makes a practical simplification, but it also eliminated some of these troublesome—

MR. MARQUEZ. Multiplicity of exchange rates.

MS. JOHNSON. —and so policy issues. Yes. But, it's now an open question or a partially open question of how successful that has been. There were, for example, a set of papers that I organized. Ted Truman was division director. We did maybe six or eight papers on monetary union in Europe that were put together as a collection of sequentially numbered IFDPs (International Finance Discussion Papers), and people put them out. We were always trying to be careful not to cross the line on what's appropriate for the Fed to say about events going on in somebody else's turf, but we had more than an intellectual interest. We had a deep interest in this experiment that was the ECB.

Globalization and International Banking

MR. SMALL. Did you get much involved in cross-country issues about banking supervision and regulation and structure?

MS. JOHNSON. Probably more so when Ted Truman was the division director. In the 1980s, there were tremendous cross-country issues associated with working out that crisis because it was a bank-dominated crisis. The Swiss were big players, and the Germans and Japanese were players to some extent.

MR. HOWARD. It wasn't really supervisory.

MS. JOHNSON. It wasn't supervisory so much as it was strategy in putting together packages in which banks would be—

MR. MARQUEZ. Is this associated with the debt crisis?

MS. JOHNSON. The Latin American debt crisis, right.

MR. HOWARD. Yes, coordinating the official responses.

MS. JOHNSON. How one managed the issue of forbearance with one's domestic banks at the crucial moments in some of those crises was a delicate issue. I'm sure there were conversations at the highest levels in confidence about who was thinking what on a given day because, depending on how you thought it wise to handle that, there could have been different outcomes for the domestic banking system, never mind for the rescue of Country A, B, or C. But those were not things that would have hit my desk. They probably didn't hit IF too much. There was tremendous expansion of both the International Development Section and the International Banking Section for a time. Then it would shrink back down to monitoring developments in these countries. I remember that, for Mexico, part of handling these episodic crises depended, or was influenced by, or at least had as a concern, public opinion in the countries in question on whether they would tolerate an IMF [International Monetary Fund] program and how views were vis-à-vis their own governments and their own programs. We would send people—Yves Maroni would go to Mexico—to do this. They would read the local press and keep Ted Truman and presumably then the Chairman informed about which way the wind was blowing in some of these countries during the most intense periods in these crises.

MR. MARQUEZ. It's too bad that the internet was not developed then. [Laughter]

MS. JOHNSON. Yes, it would have made life a lot easier. But the internet didn't exist then, so these people would go to a country and then call up on the phone. You could get the

information, but they wanted it in real time and you needed to do it that way. One episode that did occur: In 1999, we had Russia and we had Argentina and Brazil as the tail end of what had been the Asian financial crisis. We had some elements of bank coordination that were part of the rescues. Individual banks that had exposure to an indebted emerging market country were willing to maintain that exposure if they could be assured that other major lenders would maintain their exposure. But what you didn't want to be was the last guy holding the bag for a country where other people had managed to get out on favorable terms. Part of the role of the IMF and these bank committees early in the crises, in which I didn't really have much of a role, was to invent a structure that would create assurances to the banks that there was Fund money so that they knew it was possible for the country to follow a certain course of action. There was a committee where people made pledges to each other. Then, after a time, the language of some of these instruments had cross-pledge-type covenants in it. In 1999, this problem arose again, and there was an organized effort, which occurred right as I became director, to provide sufficient information that individual commercial banks felt confident that other foreign commercial banks were not finding an exit, but that didn't violate some norms of proprietary information or some willingness on the part of the individual banks to share information with their competitors. There was some coordination among the central banks of the world to help this process, and we helped it for a while. It largely succeeded in meeting the needs of the different lending commercial banks to believe that they, too, could participate because they felt that there were safeguards that others were participating. That went on for a brief period. I'm not so sure we would do it quite that way again. These things tend to be one-off, they work for a given problem in a given moment. I don't know that we'd do it the exact same way if it were to come up again.

It may yet in the Greece case, although it doesn't seem to have specifically yet. But what do I know? I'm just reading the newspapers.

That characterized the first year of my time as division director. It did have a banking aspect to it, which was something that I had never been involved in. As I went through the division, I was very involved in the rest-of-world industrial countries. Then my responsibilities broadened to include the emerging developing countries, and I tried hard to learn stuff about them. Then I shifted responsibilities with Peter Hooper and was responsible for our forecast for the U.S. external sector. I had this tremendous education, which I desperately needed, on capital flows and how to think about them and how to even measure them, but I never was much involved in the division's work on international banking issues. That was always an area where I depended on other people in the division to have the expertise to help me think about that and to do what we needed to do at that time.

MR. SMALL. You saw financial crises in the 1980s and 1990s under two different Chairmen, Volcker and Greenspan. Were there differences in their philosophical approaches?

MS. JOHNSON. Yes, but it was part and parcel of the differences in the crises and the differences in the tenor of the times. In the 1980s, the whole world's approach to managing that crisis—and Paul Volcker because he was Paul Volcker—was a hands-on programmatic approach to figuring out a way to get countries to get through a process and emerge out the other end while minimizing the damage to the industrial countries' banking systems, and, by doing so, minimizing the damage to the industrial countries' economies. It was through the channel of the banking systems, so individual banks had idiosyncratic problems. I'm sure Volcker and the people who worked with him had an understanding from the point of view of the United States,

bank by bank, of their relationships with the individual countries and their exposures and how all that worked.

The crises of the 1990s started with Asia before it became Brazil, Argentina, and Russia. It started in a world in which the economies themselves were deemed to be more successful macroeconomic entities than had been the case for Latin America in the 1980s. You didn't exactly have this petrodollar recycling overtone to it anyway. More importantly, it was not bank-based; it was more diversified in who the creditors were—or who had initially been the creditors. In these crises, who owes the money and who the creditors are shifts over time as countries issue guarantees, countries substitute sovereign debt for private debt. All kinds of things happen. In the beginning, it was a broader range of creditors than just the banks, and it was a broader range of debtors than had been the case in the 1980s. But there were also countries that, if they went into severe macroeconomic distress, would have had a bigger impact on the rest of the world, not so much because they would have damaged the banks but because they were a large, successful part of the global economy. So there was a bigger macro element and a smaller finance element to the crises in the 1990s than there had been to the crises in the 1980s.

And the solutions had to be different. The solutions had to be more market-based because everybody had become a bigger believer in the wisdom of capital markets and markets in general than they were in the mid-1980s, so the tools that people were inclined to use were somewhat different. You get reforms of the banking system but not specific bailout mentality a bit more in the 1990s. But there was still pressure, say, on Indonesia to close failing banks. And there was an IMF that was producing many of the same kind of performance criteria that the IMF always produced. Many people fought the IMF for—out of habit, so to speak, or old-fashioned

ways of thinking—forcing policies on some Asian countries that they didn't need or that hurt them instead of helped them. But those countries all bounced back quickly. Another fundamental difference had been the degree of openness. In IF, we made a lot of the fact that the Asian countries in crisis were far more open than were the Latin American countries that had been in crisis. Open economies react differently to different policy incentives. That was a big part of the 1990s crisis as well.

MR. SMALL. In the 1980s—I've heard this said—you could call the debtor countries, say Mexico, and you could call a handful of creditors and that would contain 90 percent of the debtors and creditors and you could say: "Okay boys, sit down," and do a deal. But later on everything is so diverse—the debt is being held widely out in the markets.

MS. JOHNSON. Right. But in the 1980s, the threat was to the banking systems of the industrial countries, and you had to somehow figure out a way to solve that problem. Rightly or wrongly, some of us came to believe that the problem manifested itself because the capital flows that were engineered largely by banks had enabled the Latin American countries to postpone adjusting to the change in relative prices associated with the rise in oil prices for too long. Their real economies had not adjusted to a different mix of relative prices, so they had to keep borrowing and maybe even keep borrowing more, and we had shielded them inappropriately from adjustments that they needed to make. The underlying long-term problem was this adjustment to higher energy prices and a different mix of primary product versus manufactured export prices.

MR. MARQUEZ. And we had regulations on interest rates that were making them negative, the real interest rates.

MS. JOHNSON. That's right. Therefore, that encouraged them to borrow more. Then the problem was that it was concentrated in the banking system, and we couldn't afford to let our banks fail because of this. And the justification for rescue packages—apart from the negative externalities that would be involved in letting the banks fail—partly was this idea that somehow we had contributed—we collectively, the world's international financial institutions, the world's major central banks, the world's major governments—because we'd been part and parcel of allowing these countries to be shielded from having to adjust in the name of recycling, and we did it for too long.

In the 1990s, the fundamental problem was understood differently. Whatever else might have been true, these were economies that had exported quite successfully. They had huge export sectors, and they had competed openly in the world's markets successfully. On the other hand, we had pressured them to open their financial sectors. Now we all thought maybe we'd poked at them too soon. The Thai financial system wasn't ready for this; we had encouraged countries to allow capital imports—maybe we shouldn't have. When Malaysia responded by putting restrictions on capital, we got upset. Stopping capital flows was still thought to be a bad thing. We invented these new explanations about crony capitalism and the decisions made within these countries that were somehow fundamental to their becoming more indebted than they should be. We still thought that their tendency to want to control their exchange rates contributed importantly to the manifestation of the crisis and that Korea's pseudo-pegging of its exchange rate was a factor.

MR. HOWARD. That's what the Thailand thing was all about.

MS. JOHNSON. Right. But it wasn't so much about recycling petrodollars as it was protecting export sectors. And there was this irony that the export sector was what we deemed to

be the great success of these countries. We kept telling the Latinos they should be more open, they should follow the Asian model. For a while, we called them the Asian Tigers. They put so much importance on their export sectors that they came to have policies vis-à-vis the exchange rate that we thought were misguided. That was part of the deal. That hadn't been in the picture in the 1980s much, although it probably was in the 1995 Mexico crisis. Whereas in the 1980s, people would say: "If only it was bond markets instead of the banks, we could just let them fail." It could be the 19th century all over again. We wouldn't care because it would be the bond markets and it would be widely diversified, and people would just have to take their lumps. We wouldn't have to bail out these deadbeat countries because we feared for our banks; we could just let the chips fall where they may. When the 1990s came along, we kept saying, if only it was more concentrated we could get everybody in a room, we could solve this problem. We had these problems about too many creditors. You couldn't form a committee. There were free riders. There were people taking special exits. The grass is always greener. If you're in the middle of the crisis, the exact opposite of whatever's going on is what you wish for. Somehow, that seems easier. I don't know whether we've learned from either experience at this point.

MR. MARQUEZ. During these episodes, did you feel comfortable talking to people outside the Board to get a different view?

MS. JOHNSON. Yes, in a limited sense. People here who are privy to confidential information are kept few in number, as is appropriate. So if we had some seminar speakers come in, or if you read an article written by somebody and you wanted to talk to them, or if Dornbusch, who was a big figure in the Latin American crisis, was available, you could go around town to something put on by somebody and you could engage in conversation. Most of us working on the crisis, myself included until I got to be division director, didn't feel that we

knew clearly when we were and weren't privy to truly confidential information, but the economics of it, the analytics of it, could be pretty widely discussed.

The academics took on board and had opinions on the crisis of the 1980s and the 1990s. Some people were arguing that these countries should just default. At one point Ken Rogoff, when he was an academic, recommended default.

MR. MARQUEZ. Were you thinking [Jeffrey] Sachs?

MS. JOHNSON. Maybe. I think I'm confusing who it was. But some academics, looking back on the 19th and the early 20th centuries, thought that countries reacquired access to the world's capital markets faster than in any given moment one threatened them with and that their best interest would be served by defaulting. That was argued loudly, and there were other points of view. This place is remarkably encouraging of open debate, considering all the possible arguments, and seeking out and inviting in academic consultant types. I don't remember the 1980s, but David might even have organized an academic consultants' meeting or two on these kinds of things.

MR. HOWARD. I did one on currency unions. Ken Rogoff was the featured speaker. But I don't remember.

Year 2000 (Y2K)

MR. MARQUEZ. You became the division director in 1998. That's when the Russian debt crisis begins. And, as we move into 1999, we have the Y2K preparation.

MS. JOHNSON. Yes. Those are two very different events. In the first few months, people would ask me, "How is it going?" In the caboose of a train, there's a small platform and a railing. You used to see pictures of Presidents who campaigned by train standing there with the flags and waving to people as the train slowly pulled out of a station. Well, I felt as if there'd

been a freight train moving past at a rapid pace, and that I had grabbed a hold of that railing and I was flipping in the breeze in the back of this freight train that was barreling down the track for the first several months. [Laughter]

In retrospect, I did not do as many of the things that I would have said a new division director should do upon taking that position, establishing a new team, and talking to people. Nathan Sheets [Director of the Division of International Finance, 2008–2011] did a better job, or at least he did some things that I thought, why didn't I do that? I should have done that. At the time every minute was full; I would never have figured out when to do that.

Ted abruptly left the Board to join the Treasury Department and work on these very same problems. I took on a lot of responsibilities that were new to me. The crisis was still occurring, and new things were happening. And there were issues about the crisis for which I previously had no responsibility. Argentina, Brazil, and Russia became part of the mix. Everything was happening so fast. People were already working to figure out what went wrong. The original G –22 was formed on an ad hoc basis. It started turning out working groups and reports, and that was done in central banking circles as well. There was a lot going on, and that was all policy implementation and analytical stuff. The U.S. economy was doing quite well. This is 1998-1999. The U.S. economy still seemed to be the golden boy. The U.S. budget deficit was turning to surplus. We had this widening current account deficit, and we had a very strong dollar. The goal was to keep the U.S. economy on a good path while fending off all the problems these other people were having.

Y2K just loomed. It was a totally different kind of animal, but there were the same potential risks, the unknown features of it. It was not just in the United States, it was worldwide. We were preparing for it, but what if some small country somewhere couldn't? Could Thailand

or the inability of, say, Ecuador to spend as much money on new computers as we were spending on new computers, somehow rise up and bite us?

MR. MARQUEZ. Ecuador is dollarized, by the way.

MS. JOHNSON. Yes, but I don't think it was at that point. That was later on. There were many people outside of central banking circles, outside of government circles, writing a bunch of "what ifs" about how various things might contain CHIPs (clearinghouse interbank payment system) that didn't have the capacity to function after Y2K. CHIPs were crucial for this, that, or the other thing that most people didn't even know were there. These devices could somehow fail, and we wouldn't have any warning—and so issues arose about stockpiling currency in case people had to switch to currency because electronic money got log-jammed. What if all the oil wells of the world shut down because buried deep somewhere in these wells, which were quite self-contained, was a little chip that went [out] at midnight? Would communications break down in some ways that nobody had anticipated? There were all these unknowns. Lots of resources were put into trying to imagine "what if." Our role was largely to worry about how the rest of the world was handling this. Were there going to be shocks emanating throughout the global system from one or more places around the globe where these problems were unable to be addressed ahead of time, and so they manifested themselves? In retrospect, I think one of the reasons we had as strong an IT (information technology) bubble as we did stemmed a bit from the Y2K. If you're going to have to fix the damn thing anyway, why don't we just get a new one, and that sort of thing. More spending occurred in response to the new innovations in IT than might have otherwise in such a concentrated period of time because of the looming Y2K problems. It was the most rational way to deal with that problem, given the

new innovation and given the timetable. And there were other countries that didn't seem to be as excited about this or doing as much as we were.

MR. HOWARD. We were mortified.

MS. JOHNSON. Yes. So there was this big plan. Instead of going to some big fancy New Year's Eve party—which I probably wouldn't have done anyway—I spent that night at the top of the Martin Building in the command center. I remember watching monitors there. We had people on 24- or 48-hour shifts. We started with the New Year in New Zealand. I remember seeing fireworks over the Sidney Bridge, and the celebrations in the streets. Then they had people who'd try to use ATMs: "Oh, it still worked! It's working in Wellington; it's working in Sidney; it's working in Athens," as the clock went around. [Laughter] Faxes would come in from different people we'd lined up to tell us if there were any problems. In the end it was a big non-event in that nothing dramatic happened. But it took a lot of resources, and it posed problems that we hadn't really confronted quite that way previously.

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

MR. MARQUEZ. Talk about 9/11.

MS. JOHNSON. For many people, me included, it is a "Where were you when Kennedy was shot?" kind of memory. On Sunday and Monday, I had been with Chairman Greenspan in Basel. Monday afternoon, I got on a plane and went to Paris for a WP3 (Working Party No. 3) meeting. I was going to be the delegate at the table. Ordinarily, Vice Chairman Ferguson would have attended the BIS (Bank for International Settlements) meeting, and because there was a WP3 meeting, he too would have gone to Paris and would have been the Fed delegate. Instead Greenspan attended the BIS meeting, but I no longer remember why.

It wasn't the norm for me to be the delegate at the WP3 meeting in Paris, but division directors have filled in often at WP3, and I was filling in. Lunch at the WP3 meeting was to start around 1:00, which is 8:00 a.m. New York time. I was attending a closed-door lunch listening to the luncheon speaker and eating lovely French food. I was enjoying the fact that I was the principal delegate because that didn't happen often. If I were the staff person, I would have been eating lunch someplace else and missing out on all this. After lunch, we exited the room where the lunch was being held. In the lobby of the OECD (Organisation for Economic Co-operation and Development) building, there was a big TV immediately outside the room where the WP3 lunch was being held. I think that by the time we walked out of the lunch room both planes had hit, but my memories are enough confused that that might not be the case. We walked over to the television and saw the buildings burn. We were astonished. We didn't know what to think. We stood there for a while and eventually the meeting was resumed.

The meeting was supposed to resume a little after 3:00 p.m. (10:00 am EDT) and then adjourn. It was awkward. Everybody was having a hard time focusing on what we were supposed to be talking about. Every once in a while someone would come in with some new information. And one way or another it was announced to the group in the room that the twin towers of the World Trade buildings had collapsed.

The WP3 meeting is supposed to be followed by a meeting of the G–10 deputies, which now no longer exists. The G–10 deputies were to meet starting half an hour after the WP3 meeting. There was a half-an-hour break while they rearranged the nametags in the room. At this point, people were really not interested in sitting around the table, but we were trying to do it. People were talking on their cell phones. I don't think BlackBerrys existed at that point; at least I didn't have one. Word came about the fourth plane being unaccounted for and crashing in

Pennsylvania. It seemed as if someone was shooting planes out of the sky and flying them into buildings. We were totally confused. Nobody in the room understood what had really happened. I had no idea how to think about what had happened or who had done it. It was emotionally very upsetting.

Eventually, the guy running the G-10 deputies meeting gave up and adjourned the meeting. He also cancelled the dinner that would have occurred that night; people clearly were not interested in that. The team of U.S. people all went up to the U.S. offices at the OECD building. There we learned that our flights for the next day departure had all been canceled.

I had one beneficial bit of good luck that made a huge difference for my emotional well-being. David Stockton had landed in Paris that morning. He was coming for the EDRC (Economic Development and Review Committee of the OECD) review of the United States. I managed to reach him by phone at his hotel and made plans to have dinner with him. It was such an enormous relief to spend time with another American who similarly was grappling with the attack, not understanding what had happened, feeling very far from home, and at the same time also feeling a responsibility to what was going on here.

I went back to the hotel. I speak a little bit of French, and the French were aware of what had happened. Some of them spoke English, but I felt that nobody understood quite what I was experiencing.

I was trying to reach David Howard in Washington. He was in charge of the division because I was out of the country. I had a cell phone and the hotel phone. I was trying to figure out how to make long distance calls from abroad cheaply. The circuits were overwhelmed. I got through to David at least once and that was an enormous relief. There was a lot going on here, and he was trying to take care of it, and he took care of it beautifully.

MR. HOWARD. It was dramatic. We just tried to follow what was going on. A lot of people left, but a few of us stayed. The idea was to do our job, and I thought the job was to maintain an information flow to Vice Chairman Ferguson about what was going on in the world economy. We tried to monitor as best we could with newswires and things like that.

MS. JOHNSON. And there was some delegated authority you ended up signing for.

MR. HOWARD. Yes, I performed one official act in Karen's absence. One of the big questions was dollar funding abroad for various banks. Our markets were just shattered. We used the swap line to ensure that our foreign counterparts in the central banks would have dollar funding if they needed it. We had to resurrect some of the swap lines. The IF division director has delegated authority from the Board to okay these things.

MS. JOHNSON. These are the accounts you need to have at the Federal Reserve Bank in New York in order for the swap line to work.[14]

MR. HOWARD. They have to be approved by the division director, but since she was not available, that was my job.

MR. MARQUEZ. You were approving the amount as well?

MS. JOHNSON. No, just opening the account. The Board has to set the amount. Many central banks have what you might call normal business-type accounts, and you have delegated authority for those guys, too, but this was a special occasion, and the same authority applies.

MR. HOWARD. Yes. So that was basically my only official act. Virgil Mattingly, the Board's general counsel, explained to me that I had to do it. [Laughter]

MR. MARQUEZ. You were deputy director, correct?

[14] Editor's note: To facilitate the functioning of financial markets and provide liquidity in U.S. dollars, the Federal Reserve approved the establishment of temporary reciprocal swap arrangements with the ECB and the Bank of England on September 12 and September 14, respectively. Additionally, on September 13, the Federal Reserve and the Bank of Canada agreed to a temporary augmentation of the swap facility already in place.

MR. HOWARD. Yes.

MS. JOHNSON. Previously, David had been officially and legally designated as the person in the event I am not available. There probably is a third person who is authorized to do this in the event neither of us is available. There were other, more normal business occasions for using this authority. It wasn't set up with this thought in mind because people hadn't anticipated something quite this dramatic. So this was in place for other purposes, but it applied in this case as well.

Then the issue for me in Paris was how on earth do I get home? We know how Chairman Greenspan solved that problem, but I wasn't with him.[15] Dino Kos (then executive vice president of the marketing group at the New York Fed) had been in Basel with me. He had gotten on a different plane from the Chairman's; Dino was going to New York. He may have left earlier. His plane crossed farther into the Atlantic than the Chairman's plane and was not turned back. It landed in Canada. He was bused from some place in Newfoundland back to New York with all the other people on his plane. [Laughter] There had been stories on television about the hospitality of Canadians toward people on stranded planes, and he enjoyed some of that hospitality.

So there I am in Paris, and David Stockton was there. The decision was made that the EDRC review would take place. All the relevant people were in Paris, so why not have the meeting. The next day, they all attended the meeting at the OECD. I had no role in that meeting. I was told to just enjoy the day in Paris and check in in the afternoon. The secretarial staff at the OECD were Americans who'd been working there for years, and I'd gotten to know them. I spent the day wandering around Paris, looking for places and things in Paris that I particularly

[15] Chairman Greenspan's initial commercial flight to the United States had to return to Switzerland because all planes within the U.S., or headed to the U.S. from overseas, were grounded. He later returned by military plane.

loved that might comfort me. I filled the day; it was a beautiful day in Paris. I kept telling myself: What could be so bad about being stranded in Paris and having to go visit wonderful—

MR. MARQUEZ. D'Orsay museum? [Laughter]

MS. JOHNSON. Yes, right. How bad could it be? But it wasn't a good day. At the end of the day, I checked in at the OECD. No flights were available, and there were no prospects of a plane the next day. At that point, I think that perhaps I could reach, if not David Howard, then Margarita Serafini, my secretary, who I was calling. Things had calmed down a bit. You could make phone calls; you could reach the United States. I made the decision to backtrack to Basel. The hotel staff was telling me that I could only stay one more night in Paris because other guests had reservations. They probably weren't spending a lot of time asking themselves whether those reservations were from Americans who weren't going to be able to fly in and hence I could have the room; they just knew they were booked. The people at the OECD had been very nice, but they had a host of people to worry about, some of whom were U.S. Treasury, who were a much higher priority for them than the Federal Reserve because some of the people there who staffed the OECD were Treasury employees.

If I went back to Basel, I would be in the world of central bankers. The staff people at the BIS were always very good to the central bank folk who come to the various meetings. They had always been nice to me. I knew their names, and they knew me as a person. They would work with Margarita to help me get home. Also if I went there, I'd have an office with a computer that was hooked into the Board's computer. I could get up in the morning and have a place to go. I could email. I could read documents. I could interact with people. My phone connections would be better. We even had a special phone line. And I could probably get a hotel room with an open-ended ability to stay.

Margarita reserved a hotel room for me. I got on a train and went back to Basel. Margarita kept working on a plane reservation because the planes weren't yet flying. There were a group of people in Basel, including Chris Cumming from the New York Fed—we all shared our stories over time. They had been in Basel for a meeting subsequent to mine. They'd gotten stranded at Basel and decided to go to Paris as a more likely place to get a flight. [Laughter] Maybe I got her room. Anyway, we did not overlap. I did, however, overlap with Rich Spillenkothen. He was in Basel. So, at that point, David Stockton, Rich Spillenkothen, and I—heads of three very relevant Board divisions—were all stranded in Europe over September 11 to September 13. That seemed a big deal to me. Virgil Mattingly [the Board's general counsel] was at the Board, and David Howard was here holding down the fort.

MR. HOWARD. And, most importantly, Vincent Reinhart [Division of Monetary Affairs director] and Louise Roseman [Division of Reserve Bank Operations and Payment Systems director] were here. [Laughter]

MS. JOHNSON. Right, they were crucial. Greenspan made his way back. Ned [Governor Edward] Gramlich was driving in Arizona.

MR. HOWARD. He was reachable by phone. Only Vice Chairman Ferguson was at the Board.

MS. JOHNSON. Yes, that first day, it was very thin.

MR. SMALL. Was Don Kohn at the Board? He was an adviser [to the Board] then.

MR. HOWARD. Don was not here that first day, but I think he was later on because I authorized the swap lines in Don's office, and Don was there. I was with Vincent Reinhart the day we heard about the attack. He was the director of the Division of Monetary Affairs. Don

might not have been here the first day, but he was here the next day or the day after that. He was involved.

MS. JOHNSON. In any event, Margarita, being the capable person she is, got me a reservation out of Zurich. I was not in Basel long. I got up early, jumped on the train, and ended up in the Zurich airport. I didn't have a ticket on that plane, but I had a reservation, and I talked my way on to the first plane leaving.

The airport was packed because this was the first day planes were flying to the United States. I had a business class ticket and a government credit card. I managed to get a hold of this lady at the airline ticket counter who agreed to accept the ticket. I don't even know if it was on the same airline. She gave me a boarding pass, threw up her hands, and I went running for the plane. I told her I'd take any seat. There were people who had reserved seats on that plane, and they were entitled to their seats. They were desperately trying to make sure they got those seats lest they join the crowd of people, all of whom are waiting to catch an empty seat somehow. Because it was the startup of resumed flights, some people were caught unawares. It was an early morning plane out of Zurich, so if they weren't in the right place at the right time, and they didn't react fast enough or they didn't get there, they missed the flight. Then there are the people who never made it to Europe who weren't there to take their return flights. There were the people rethinking the situation and deciding that flying to the United States on the first plane out didn't seem like a good idea. They didn't show up either. So there were some empty seats, and a bunch of us got on. On the plane, you're all looking around and wondering if there's anybody who looks funny. It was a tense group of people on that plane.

I landed in Atlanta on a Delta flight. I had a ticket from Paris to DC, and I came from a plane from Zurich to Atlanta, so you can understand why the ticket agent was staring at this

ticket puzzled about what the hell to do with it. [Laughter]. But she took it and handed me a boarding pass. I was prepared to buy a ticket to Washington. I had this wonderful government credit card whose purpose was to help me get through these things.

It was chaos at the Atlanta airport. There were any number of possible flights back to Washington, but the issue was mustering up a crew of both pilots and flight attendants. Planes and people were in the wrong places because it was the first day that flying resumed. People were screaming and yelling at the ticket agents. Eventually, I got on a plane and arrived home that night.

It was an astonishing event. Even though I wasn't personally the target of anything, you felt like you were. Every American I'm sure felt like they had been the target that day. It was just harder to be that target so far from home.

MR. MARQUEZ. Were you worried about your kids?

MS. JOHNSON. They had long since graduated from college. Wherever they were, they weren't in D.C. and New York. My poor mother was worried because I was who knows where. I had asked David to call her and tell her that I was fine. Again, I wasn't the target, but we all felt like targets. We all felt very vulnerable, especially those of us who got on airplanes. We were glad to be done with that part, and I was so glad to be home.

Current Account Deficit of the United States

MR. MARQUEZ. Steve Kamin, the IF deputy director, has joined us.

Karen, while you were division director there was a widening gap in the U.S. current account. To what do you attribute that and how did you study that issue?

MS. JOHNSON. My interest in the topic began while Ted Truman was still division director. He asked me to head a group that did the first division project on the sustainability of

the U.S. external balance. That assignment was given to me fairly soon after I switched responsibilities with Peter Hooper, so I was in the learning mode in thinking about both the real side and the financial side of the external relations of the U.S. economy. We put together a good group of people. I thought the project shone a great deal of light on the problem. There was ultimately a second project that Bill Helkie more or less oversaw, which also shone light. Over time there was considerably more work done in the division—much of it done by or at least supervised by Steve Kamin, which is why I [think] it's helpful to have him here.

While I was division director, we needed to monitor the unfolding of this process, and its tentacles reached out in many ways. To some extent there were issues associated with Governor Bernanke, when he was still a Governor, and then as Chairman. There were points of view that Chairman Greenspan had. There were a variety of sub-themes in this question that constituted a substantial part of the work that was done while I was division director.

The first project impressed upon me certain important things. The sequence of events that we call the U.S. external imbalance was not necessarily the result of a single cause; many things contributed to these events turning out the way they did. While this aspect of the question is messy and while it may complicate the debate on the topic, it might also just be true. One of the sad things about the debates that we have in economics—particularly the ones that matter to public policy and that end up on the evening news or in the financial pages of newspapers—is that people want to make them simple.

I always thought that one of the major contributions that the division had to offer to this debate—whether the debate was internal with the Board and the FOMC, or within official circles such as, at the BIS, the IMF, and other places where we played a role, or in public in speeches and papers that we put out as research papers and the like—was to remind people that this was

not a horse race. These were not mutually exclusive factors. There were a series of exogenous events that needed to be brought into the explanation in order to understand why the events of 1995 onward were what they were.

That insistence on not letting it become a debate about one point of view versus another, but rather a coherent story that embraced all the relevant key facts, was one piece of it. The other was that in so doing, then, it had to be a general equilibrium story. I was particularly wary of any argument or even any sentence that started with an endogenous variable. Explanations about the exchange rate or its role were always tenuous because the exchange rate itself, given our choice of how we ran monetary policy, was the result of forces. It was not a truly exogenous variable. So it was necessary to be mindful of telling a story that held up to general equilibrium thinking. For example, there are people who looked at the identity between the external balance, the private sector balance, and the public sector balance and said—we have the external balance over here, and it equals the sum, appropriately signed, of the two domestic balances, so we'll do X to close the domestic balance, and that will immediately move the external balance, pretty much dollar for dollar. That kind of reasoning is invalid. It leads to mistakes, and trained economists ought to be able to agree that those arguments don't hold and to set them aside. That doesn't mean that all trained economists come around to my way of thinking about the problem, but we can avoid some mistakes. I tried hard to make what we did as a division, how we expressed our views to the Board, and how we helped Board members express their view to the public meet this test: that it was general equilibrium robust and it carefully distinguished between things that were are truly exogenous and things that were endogenous and reacting to other forces.

There was a tremendous amount of work done. In some cases, it was comparisons with previous episodes, like the 1980s. When we look on it now, we think that was just a minor run-up and run-down in the balance. But, at the time, we thought it was very serious. Other efforts involved projections going forward about what might be the best possible outcome and was there some way that policy might help us achieve a good outcome, and projections of what would be the worst possible outcome, and what kinds of steps might one take to lower the probability that this worst outcome would be the path that the economy would go on. We profoundly disagreed with some of the views taken by noted academics, people at the IMF, or economists that we would cross in various meeting circles, whether it was the OECD, the BIS, or whatever. Probably in the grand scheme of things, the differences between ourselves and such a person are actually small relative to the range of the wide debate in the whole. We had intense conversations with the people at the IMF over how a change in U.S. fiscal policy would result in a change in the external imbalances among the countries being considered. Over the details of the magnitude of that effect and the channels by which it worked and the name you wanted to give it and how you presented, we went tooth and nail with these people. In the broad spectrum, we thought almost identically about these issues. We were talking parameters here and timing there. You step back from it. You realize these people are really your allies. They think a lot like you think, but at certain moments of time that little difference in the manifestation of that loomed very large.

MR. KAMIN. The difference was like between 0.2 and 0.4 for what fraction a fiscal adjustment would translate into current account adjustment. I don’t quite know what the formula is, but I never thought it was an important difference. [Laughter] We have plenty of people in

our division who, for the intellectual thrill of it, if not for its ultimate effects on the welfare of the American economy, were eager to engage in this debate.

MS. JOHNSON. I was not so prone to thrills that my deep feeling could be attributed to that. [Laughter] But this was a place where a lot of people made one of two mistakes. They failed to think about it in general equilibrium terms as opposed to partial equilibrium terms. Many people thought of fiscal policy as a substitute tactic to approach a solution to narrowing these imbalances that would thereby make it unnecessary for the exchange rate to adjust, whereas, in fact, the way fiscal policy turns into an adjustment of the external balance as opposed to the domestic balance is through the exchange rate. Making people understand that a truly exogenous fiscal action has to be seen as inducing a change in the exchange rates, which then induces a change in the external balance, I consider to be one of my great crusades. I'm not sure that I succeeded in persuading anybody who didn't want to see it that way, but I never gave up.

MR. SMALL. I'm surprised to hear you say there weren't differences when you stepped back because I would have thought one camp would have said—we're in a flexible exchange rate regime; let them take the burden; let them do the work. Why do we need a policy response at all? I would have thought there would have been some in that camp and some in the other camp that said—that's ridiculous; it takes too long; it ignores factors.

MS. JOHNSON. The issue was really not that the key people at the IMF and I persisted in somehow overstating the importance of our differences. My concerns were about the use that I saw being made by people who were outside of our circle of like-minded economic analysis, who used these results when they appeared in IMF publications to justify assertions that we could fix the problem if we just changed our fiscal policy. I stressed that a fiscal action would entail a change in exchange rates. There are very few people outside the United States who are

as indifferent to the exchange rate as are we, whether you're talking about the people in this building or the man on the street. Many of the colleagues that I would meet in circles, particularly Europe but elsewhere, had studied in the United States and they knew this. You could stop a lot of people on the streets of the United States and ask them to quote you any exchange rate with the dollar that you cared to name, and they would not know the first digit. They would not have a clue. Most other countries, there's one or two exchange rates that even the average person on the street knows, follows, is aware of, sensitive to. This goes back, in part, to some of the issues we were talking about earlier about the closed economy and the post–Bretton Woods setup. Most Americans—even the ones who are convinced that China's exchange rate policy is destroying our way of living—have no idea what the first digit of that exchange rate is. In any event, in a lot of the debates over the adjustment, the hidden agenda is how that adjustment is going to be distributed amongst and across our trading partners. The form it takes is a debate about whose exchange rate is in disequilibrium, whose exchange rate is in equilibrium, and how much work the exchange rate has to do. They all somehow believe that if their exchange rate doesn't get disturbed much that will minimize any adjustment that they have to make.

MR. KAMIN. The policy debate, to the extent it ever got to that, is whether or not we needed to do fiscal adjustment in order to do our part of current account adjustment. Basically, we supported fiscal adjustment. The IMF supports fiscal adjustment. Everybody supports that the United States should fiscally adjust. So from the standpoint of the most objective man on the street, we weren't arguing at all. It was largely intellectual.

MS. JOHNSON. I think it reflected the hidden agenda—an attempt on the part of all the different participants to make it somebody else's problem and not their problem. In 1999, when

I became division director, the United States was running a budget surplus. I was already five or six years into being a major contributor to thinking about the work the division was doing and asking people to contribute and contributing myself in some limited ways. We should have been running a bigger surplus if extra imbalance was somehow reflecting an underlying problem that we wanted to address. But it was hard to see in 1999 that U.S. fiscal irresponsibility was a big piece of this story. Subsequently that changed, but I take that as the case in point. It can't be much about U.S. fiscal policy if the problem got big during a period of time in which our deficit went away and grew to be a surplus and then it got worse when the surplus went away and grew to be a deficit. Fiscal policy cannot be the linchpin, or at least not all by itself, given that fact.

MR. MARQUEZ. Much of the work that we did in those special studies of the current account emphasize many angles to the problem, for example, the work that Cathy Mann and Helkie-Hooper did in the mid-1980s.[16]

MS. JOHNSON. Or even the work Peter, you, and I did.

MR. MARQUEZ. The first current account project was raising the profile of the financial account. The United States has to do this or that, otherwise the current account is going to bring us to hell; there was a 30-year litany of warnings. Do you think that we were saying anything new about the U.S. current account that wasn't said from 1985–2000?

MS. JOHNSON. No, I don't, but I come back to my point about the context in which you say it. The strength of that first group was your contribution from the trade side, and your leaving an impression in my head that the Houthakker–Magee result,[17] to the extent to which it

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[16] Editor's note: Ralph C. Bryant, Peter Hooper, and Catherine L. Mann (1993), *Evaluating Policy Regimes: New Research in Empirical Macroeconomics* (Washington: Brookings Institution Press).

[17] "Even when the world grows faster than the U.S., the U.S. trade deficits tends to widen. This is the consequence of what apparently is a greater appetite for imports by U.S. consumers and business than foreigners' appetites for U.S. exports. This so-called Houthakker–Magee effect has been a feature of U.S. trade for the whole of the postwar

persisted, was a stylized fact that was going to have important empirical implications and be a piece of the story.

Then there is the savings/investment aspect of the story. It had to do with the productivity shock in the United States. I'm not going to remember the timing well, but there was the note written by David Bowman and Dale Henderson. Part of the problem with productivity shocks is that they have an ambiguous theoretical result on the exchange rate in most of the literature, so you've got famously the Balassa-Samuelson result.[18] But you can—if you introduce a shock to productivity slightly differently—depending on how many sectors you posit in the economy and depending upon whether the productivity shock equally hits the tradable sector or the nontradable sector—if you distinguish those two—get a different result.

That meant that we needed to think about the forces that were driving the attraction of capital coming into the United States, on the one hand, and the implications of a positive productivity shock in the United States. Would the result be a higher stream of income, and hence increased present consumption—therefore altering the savings and investment balance? Such effects all have to be integrated into the picture too. And the Balassa-Samuelson implication of what the shock does to relative wages and therefore the relative price of tradables versus nontradables has to be factored in too.

A paper by Bowman and Henderson, in a given structure, explored some of those things. We had the empirical work on trade modeling. All international economists know that the

period, but the implications for the trade deficit are more apparent since the breakdown of the Bretton Woods system of fixed exchange rates." Catherine Mann (1999), "On the Causes of the U.S. Current Account Deficit," Briefing for the Trade Deficit Review Commission, Peterson Institute for International Economics, Washington, August 19.
[18] Editor's note: The Balassa–Samuelson effect, proposed by economists Bela Balassa and Paul Samuelson, implies that an increase in the productivity of the tradeables sector (relative to nontradeables) in one country induces a real appreciation of its exchange rate.

Marshall-Lerner conditions can matter in this game.[19] Then we all learned that the coefficient on income on the import equation can matter in this game, although I am a little uncomfortable in that imports and that coefficient are not primary parameters. They are the amalgam of a bunch of other parameters that you've put together and aggregated because imports are a mixed bag of intermediate products and final products, so that coefficient, which is hard to understand, hard to explain, hard to have a priori views about, can be anything. Depending on how complicated you make the model out of which you aggregated that import equation, it can have different meanings. We used to fight with the people in R&S about whether or not having that coefficient greater than 1 violated fundamental principles of economics.

MR. MARQUEZ. The R&S staff like a coefficient of 1 because FRB/US needed to have the import-to-GDP share stabilized in the long run.[20] If you had that greater than 1, the import-to-GDP ratio was going to grow, and it was going to eat up all GDP.

MS. JOHNSON. Yes, but, on the other hand, if you're Singapore, that makes no sense at all.

MR. MARQUEZ. Well, that was not acceptable to R&S. [Laughter]

MS. JOHNSON. I understand. But aside from interpersonal debates we might have had with other modelers, imports are this odd thing. They are not all final goods. Hence their relationship to income, if you had a fully articulated, general equilibrium, Arrow-Debreu kind of model, and used that as the basis for aggregating an import equation, you'd discover there's a whole bunch of parameters in there that you're now putting together. This is not to mention the fact that the parameter might not be stable, which is a problem, and we did time-varying stuff

[19] Editor's note: The Marshall–Lerner condition describes the conditions under which a depreciation of a country's exchange rate induces an improvement in its trade balance.
[20] Flint Brayton, Eileen Mauskopf, David Reifschneider, Peter Tinsley, and John Williams (1997), "The Role of Expectations in the FRB/US Macroeconomic Model," *Federal Reserve Bulletin*, vol. 83 (April), pp. 227–245.

too. My personal prejudice is that there are three ways of thinking about this problem: There is the trade balance part, there is the savings/investment part, and there is the capital flows part. They're all relevant. While we have a tendency—because they are linked by identities—to say that we could just focus on one and it answers all the questions, that's a huge mistake. So, in doing this project, I learned a lot about the trade equation part of the deal and the long-standing debates for 20 or 30 years that people have gone into doing empirical studies of U.S. trade. We did a bunch of stuff comparing other countries, and followed the reasoning that if we had trade equations that looked like this, somebody else must have a trade equation that somehow fits in because our exports are their imports and vice versa. I learned a tremendous amount from the people who work on capital flows about how to think about the capital flows, how we measure the capital flows, and how they fit in. Finally, the macro stuff, which is the fundamental savings and investment part, which is where the global savings glut comes from, needs to be considered. And I always thought our various projects and the work that we did to put this together or the work that we did for that IMF seminar, which I thought was another biggie, correctly reflected that we thought about each of those pieces. We insisted that they then be endogenously solved together. We didn't exogenize in some convenient way that enabled one of them to drive the system and the other two to become irrelevant. You get all kinds of useful appreciations of how complex this thing really is by doing that.

MR. MARQUEZ. The novelty is insisting on general equilibrium aspects?

MS. JOHNSON. Yes, and the eclecticism of having a representation that has all these. When you do that, you can say to somebody who wants to insist on a particular interpretation that their view is taking this model and making this parameter a zero and that parameter a 1 plus endogenizing this variable; but why should we do that? There's no reason to think you should

do that. Insisting on the complexity can be frustrating, because then you get cloudy results or results that would depend a bit on these parameters. People would become impatient when you say, this result is sensitive to some particular assumptions because they want a nice, clean, simple answer. It doesn't necessarily mean that's the way the world is made.

MR. MARQUEZ. A moment ago, you emphasized the general equilibrium aspects of studying the current account. The presentations that the division or that you made to the Board were based on the USIT (U.S. International Transactions) model, which is not a general equilibrium model.[21] Why not use a general equilibrium model?

MS. JOHNSON. I think we did in some sense. By combining our work with R&S, we approximated a general equilibrium model. We ended up with a more detailed and more complete picture of the U.S. economy than we could encompass if we tried to do a pure general equilibrium model–based forecast. We iterated with them, and we ended up with a consistent forecast if not a general equilibrium–derived forecast. It's true that to the extent to which we do things like a standard set of shocks or a standard set of extensions of policy options, we then get on much thinner ice. I was always leery of those extensions and that work. One of the reasons I'm leery of that is that we oftentimes jump ship on the model. We would do a Greenbook forecast combining one set of algorithms, approaches, relationships, and embedded parameters, and then we'd do an extension doing a different model. I was proud of that, but we didn't have the capacity to do all the work that it takes to produce a Greenbook forecast over and over again to do multiple shocks or multiple extensions or policy alternatives, full-blown Greenbook treatment over and over and over again. There was not the kind of staff time and preparation.

[21] The USIT model is used to generate projections for nearly all of the entries in the U.S. balance of payments; see William L. Helkie and Peter Hooper (1988), "The U.S. External Deficit in the 1980s: an Empirical Analysis," in Ralph C Bryant, Gerald Holtham, and Peter Hooper, eds., *External Deficits and the Dollar: The Pit and the Pendulum* (Washington: Brookings Institution), pp. 10–56.

There wasn't even the brainpower in a forecast that detailed and that judgmental to come up with a detailed picture of every variable in the U.S. economy, if monetary policy was tightened 50 basis points or if it was loosened 50 basis points. What claim could we make that we knew exactly how every single item in the U.S. economy was going to change in response to that? So we used more aggregated models. We used different models, and sometimes they had slightly different properties. But I think the actual baseline Greenbook model wasn't one in which there was this odd partial equilibrium piece that wasn't consistent with the rest of it. Enough steps were taken to mimic a general equilibrium outcome. I think the FOMC and the readers, other readers of them, understood the difference between the extensions and in the shock versus control elements that we would report to them and the basic forecast. We tried to be honest about it, but it is certainly a fact that we used many tools, and the answers are tool-dependent.

MR. MARQUEZ. We like detail, and the USIT model has detail. We also like general equilibrium, and to have a general equilibrium model with a lot of detail is tricky. At some point did you decide that, for this application, detail is of the essence, whereas for this other one, detail isn't all that relevant, so we use the USIT model where it's for this other case? For example, if you were engaged in studying the redistribution—that was going to absorb the U.S. current account deficit, the notion that we have exports of computer, non-computer—detail would not be of the essence, whereas if you wanted to study the sustainability of the current account over the next two years, maybe detail is of the essence.

MS. JOHNSON. You're quite right. Detail is certainly part of the story. The equations that we used in the first project were aggregated—even the USIT equations were aggregated. But another feature besides the level of detail that mattered is the extent to which, in the regular Greenbook forecast, there were exogenous variables. As a matter of forecast procedure, we did

exogenize certain things in the forecasting world that would be very uncomfortable exogenizing over long periods of time—the price of oil comes to mind as a variable that we treated as exogenous when we did Greenbook forecasts. There are others, too. When we sat down that first time and in subsequent times, one of the reasons that we aggregated as we did was to avoid making difficult assumptions about exogenous variables. One of the reasons why this thing we call the long-term model—I don't even know if it still exists—was aggregated more than the USIT model was again the question of the exogenous variables.

MR. MARQUEZ. It's dormant.

MS. JOHNSON. Okay. I used to depend on it a lot. We buried some of the unfortunate exogenous variables into an aggregation into which they disappeared. That is reason for thinking that the results of this model are conditional on that being a good thing to do and not giving you a false insight. You had to worry about that. But I thought that was a smaller sin than writing down some explicit exogenous forecast for some price for the next 20 years. I wasn't about to do that. So you have to make these tradeoffs. All models are simplifications. The question was: Have you made the simplifications in places where it won't destroy the usefulness of looking at the results, or have you made the simplifications in such a way that you have conditioned the results, you're just getting back what you put in? A model can be useful, but you have to know what you've got. Sometimes these models are just articulations of a point of view you want to express. You make the simplifications and you put in the variable so that you get result X. You can't claim your model told you X. You can only claim that the model helped you describe X, but you started out to get X and you got X. Other times, you could say, let's put in some parameters that we think are empirically relevant. Let's make some simplifications that enable us to do this, and then let's see what comes out and be genuinely informed by it. A lot of the

things we did in some of these current account sustainability things were of the second kind. So one of the lessons I learned from those is that you have to pay attention not only to the endpoint. We had 50-year horizons, I think, at one point.

And it would be perfectly reasonable to have an endpoint condition met, given the ability of economies to adjust, but still have it be true that the path went through a territory that you didn't think would be sustainable, that the financial markets would see those events and something discontinuous would happen. I thought it was useful to show that—a lot of people would say—and even we to some extent would say—"This is a market phenomenon. The markets can handle it. Let the markets take care of it." If you could show that, under reasonable parameters, the markets would handle it by producing an interval of time in which the U.S. debt-to-GDP ratio got so high that you think it would be startling to investors and might not be sustainable, even if it ended up back lower—that is an important result. That was useful to know and to see whether or not reasonable models would generate paths that were not straight lines or gently sloping curves but things that moved around. Something like the debt-to-GDP ratio had so many moving parts to it that it certainly could move around. I thought that was a useful insight, not that I thought I was predicting exactly what was going to happen; it was just giving me another bit of information about the sorts of risks about which one had to be mindful.

MR. MARQUEZ. Was there tension between research that was done in the division? I am thinking Meese–Rogoff is Exhibit A;[22] and Exhibit B is the treatment of the dollar in day-to-day work. In some of your remarks in FOMC presentations you noted this tension. You would note that we have this path for the dollar. We have this path for the current account. And a Governor would say, "But Karen, you know the two of them are not compatible." How did you

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[22] Editor's note: Richard A. Meese and Kenneth Rogoff (1983), "Empirical Exchange Rate Models of the Seventies: Do They Fit out of Sample?" *Journal of International Economics,* vol. 14, pp. 3–24.

reconcile this, or did you reconcile that the exchange rate, at least according to Meese–Rogoff, was not predictable, but we had to have a stand on what future exchange rates would be—given that they are endogenous variables.

MS. JOHNSON. Yes, I know. And another case would be the oil price. I didn't mention the exchange rate; I mentioned the oil price instead. I don't think there's a good answer to this question. You make whatever practical compromises you feel you can support to a given question at a given time, trying to explain your rationale to the audience. So we struggled—— and we could see what other people do. So you get the IMF, who has to do a forecast estimate, assumptions about both policy—which is truly exogenous, but you don't know what policies are going to be—and exchange rates, which are endogenous variables. The OECD faced this dilemma as well; we knew what they were doing. There were a lot of people who would make what they would call technical assumptions, and they would keep everything unchanged as of March 30, for an example, and just run that out. The advantage of that is everybody knows what you're doing, and you basically don't have to explain or you're not liable in some sense for deception.

MR. MARQUEZ. You're not concealing anything.

MS. JOHNSON. Right, but you're also not being very helpful. And the problem reemerged, and has reemerged more recently in other contexts. Or at least it's similar to the problem that people who are doing inflation targeting based on a forecast face when they have to do something about future policy in order to generate the inflation forecast. The solutions that have been implemented here such as in Sweden or in the United Kingdom, etc., about whether you take the Fed funds futures curve or whether you hold the rate unchanged, etc., etc.—it's not unlike the problem R&S grapples with on fed funds in their forecast. We had a result that we

were all trying to bask in the reflected glory of—this Meese-Rogoff paper. It would suggest that you should treat the exchange rate as a random walk and that you couldn't do any better than forecast an unchanged exchange rate, basically. Yet we would have meetings and forecast the exchange rate, or if we had to close a model for the purposes of some analytical work, we would often rely on uncovered interest parity. But that equation didn't win the horse race either. We tried to do it in sensible ways. When we thought that we could justify an assumption—well, certainly if there was a policy path that R&S gave us that provided a reason for thinking that an unchanged exchange rate wasn't likely—we could lean on that, but that seldom happened. We seldom forecast an exchange rate that moved enough over the horizon of the forecast that it wouldn't, up to one standard deviation, qualify as a random walk. We were not exactly bold exchange rate forecasters. [Laughter]

Ted Truman once explained to me that our job was to give the best possible analytical presentation to the FOMC that we could, to arm them with as much useful insight into the forces at work that they were dealing with, as we possibly could. We had many other responsibilities, but that was a prominent responsibility.

One of the preeminent responsibilities of the FOMC was U.S. inflation. In that vein, it was irresponsible of us to condition the forecast on an exchange rate path that helped keep the inflation rate down. That might be unwarranted in the sense that a reasonable man could think of reasons why the exchange rate might not do that, even if the best exchange rate forecast was unchanged. So, particularly at times when the dollar was thought to be very strong, not unlike in the case of our forecasted commodity prices and oil prices, but anyway—as it would go up, up, up, we would always forecast, "Oh, it hit its peak yesterday; it's going down." Six weeks later: "It hit its peak yesterday; it's going down." [Laughter] And we would do it again and again and

again. But the argument certainly could be made that we thereby reminded the FOMC not to rest assured that the exchange rate would contribute to containing U.S. inflation forever. There might come a time when the inflationary consequences of the exchange rate would go the other way. And the FOMC should at least be thinking in those terms. Now, that doesn't give you intellectual solace at all moments in time for all the forecasts we did for all the games that we play, but it was one of the arguments.

MR. SMALL. When you were talking about the general equilibrium—the current account and the general equilibrium model—you talked about a picture that there are many factors leading to the current [situation.] So just from someone reading the popular press—in that story, you didn't end up naming the one, two, three, or four which you thought most important empirical factors—so reading the popular press, I'll throw out some that—toss them all out. At this point, what do you think the causes were? From the popular press, I can think of one: Americans are very productive, and when their productivity goes up, people want to invest in the U.S. market. Another one is Americans are spendthrifts. Both personally and through their government, they run large deficits; they want to buy all this stuff from overseas. We work hard; we play hard. The third is that it's not the Americans, it's the Chinese. They have this surplus, it's got to show up elsewhere, and as long as the country is willing to accumulate foreign reserves, you can't force them to revalue like you can force them to devalue when they're running a deficit. The Chinese can just keep on accumulating this dollar stuff forever and keep their currency weak.

MS. JOHNSON. These are not—

MR. SMALL. —that are endogenous, they're—

MS. JOHNSON. No, those are largely exogenous, and they're all—what's the word I want? They're not either-or; they're and, and, and—all of those things contributed. All of those things are, to some degree, empirically relevant factors.

So if I were to produce a story—as I did with Steve [Kamin] several times and as we did when we were trying to do good outcomes and bad outcomes—I would start this process back in 1995. That's a point in time when the U.S. current account imbalance—if you look at this chart that Jaime Marquez carefully produced—wasn't large. You could go back to 1991, but 1991 is cyclically, I think, extreme, so I always started at 1995. That was a flat point in the deficit, which was about 1.5 to 2 percent of GDP. Anything subsequent to 1995 is too late. Reasonable people might even argue you should go back before 1995, but I want a story that goes back at least to 1995.

So yes, I want the productivity shock, and I want an argument that says, if the U.S. economy experiences a productivity shock, people want to invest in the United States. That raises the rate of return on real capital in the United States, and it attracts capital flows into the United States, the consequences of which are probably to put downward pressure on interest rates. But whether interest rates actually fall depend on what else puts pressures on interest rates.

Yes, I think the U.S. economy has a tendency to save less than some others, but that's a tricky parameter to stick in there. We've got this lifetime average propensity to consume in the back of our heads, so how can it be that U.S saving stays low? Or individual Americans, how does it turn out that we save less? For China, in particular, we came up with stories about the failure of its insurance markets to enable them to pool risk efficiently. But you throw that in. Particularly for a five-year forecast, you just put it in there, just saying that we save X, they save Y, as a parameter thing.

Other people might exogenize their exchange rate, so you want to exogenize the Chinese exchange rate and have them not have monetary policy but rather take the exchange rate as a given. We certainly leaned very hard on the role the exchange rate plays even endogenously, but if you throw it in as an exogenous factor in a couple cases—because there are lots and lots of exchange rates, not just one—that gives you some mileage.

I would also throw in the Houthakker–Magee parameter, which is the elasticity of imports for income. I would throw in the price elasticity of exports and imports into the equation. That's been debated a fair amount among empirical international economists.

In the capital flow story, you could throw in something that's like [the work of Ricardo] Caballero and these other guys about which countries produce attractive, liquid, diversified financial assets that other people want to use. So, in other words, we could be a financial intermediary for the savings of other countries, which we could either take in and then put back out or we could take in and keep. The model or the facts tell you that. But we have a stronger production function in the financial sector than other people.

We could put all these things in there; they're all relevant; they all contributed. And they don't necessarily indicate anything wrong with the global economy, even if they generate imbalances. That's just the global economy's way of allocating comparative advantage.

MR. MARQUEZ. Reading your FOMC presentations, I think you were the first one to bring TIC explicitly into the discussion—not just what the financial flows in the aggregate were.[23] Your presentations were disaggregated in TIC. My sense was that, then and even now, you don't view the role of the financial account as merely balancing the current account. Much of the work in our division, i.e., that the planets revolve around the current account of the United

[23] TIC is the acronym for the U.S. Department of Treasury's International Capital System.

States, operationally and Greenbook-wise, leaves me with a tension. It's almost as though we have taken one of those identities that you say and say, okay, one has got to be the negative of the other one, so we don't have to worry about it.

MS. JOHNSON. The traditional answer to that question was that since the identity has to hold ultimately, you would look at those empirical relationships for which the evidence was most robust and for which the demands of putting in right hand–side variables for some forecast or something were more intuitively accessible to you, and you would look at the implications of how that came out. For example, some people argue that savings and investment are what's important. The exchange rate doesn't affect savings and investment, so the exchange rate isn't really part of the deal; the deal is fiscal policy or the deal is the behavior of American households or tax policy maybe, but it can't be the exchange rate because savings and investment have to be there, and there's no exchange rate in there. My answer to that is: It has to be there. You might not be smart enough to find it, but it's there. It is there because it's in income, because income then depends on the exchange rate. You can get away with saying of the three—of the pieces of any identity, right—so you can solve that identity and have one of the three pieces on one side and two of the other pieces on the other, and you can solve them for any one of the pieces.

Oftentimes people solve it for X minus M, which is close to but not the same thing as the current account balance. And you could say, I'm doing that because the empirical evidence on that is the most robust, and then I'm going to use that. That's not the same thing as saying exports and imports determine—

MR. MARQUEZ. The final outcome.

MS. JOHNSON. Because just as the exchange rate has to be in the savings rate, the tax on capital has to be in the export equation somewhere. You just don't see it. So you can buy it that way.

That said, two things, I think, shaped how all this evolved: I believe bringing TIC over from Treasury—which was essentially done under Truman, but it happened basically as he put his hat on and walked out the door, so in many respects the job of integrating TIC fell to me—mattered. It presented us with a wonderful opportunity. Prior to that, we weren't paying as much attention as we might have, or we didn't have access to the information the way we do now. Although there were people like Allen Frankel in the old days in the financial half of the USIT model—Frankel was upstairs—and there were Louis Stekler and people like that who were very knowledgeable and looked at those numbers, I think the rest of us—I know that I personally—found the whole thing to be this very strange box. I would just go to them and I'd ask a question, and I'd try to remember the answer for a day. Then I'd go back to where I was before. I never really made enough of an effort to get control over that material until it became a bigger part of what was going on in the division. The driving factor for the FOMC with respect to TIC is that a lot of people in this debate—or one of the strands in this debate—was that the U.S. current account deficit was being financed by public behavior as opposed to private sector behavior. So they would point to an accumulation of reserves by public entities as *the* reason. Now, obviously that immediately violates my basic principle that you can't pick one, you got to have them all. But more importantly, if you understand the capital account, you realize that it overexplains the current account. You can combine the numbers in the capital account to the net public behavior, to the net long-term behavior, to the net of short-term private and long-term public. You can net any way you want, and then you could say that's the reason. It's a big

mistake. Isolating pieces of that account and calling them the driving force, based on your own whim, strikes me as a big mistake.

MR. KAMIN. We also got involved in the issue of disorderly adjustment and what that meant. We were standard setters in that regard because we looked at that issue through various means and found that the historical experience in industrial economies didn't seem to support much evidence that these adjustments were disorderly, though we always, in our boilerplate for Governors, wrote down that we cannot discount the possibility that it could occur.

MS. JOHNSON. Right, and that led us to have an unjustified reputation as being Pollyanna about this.

MR. KAMIN. Yes, although the subsequent events so far have fully supported.

MR. HOWARD. But there is a risk— [Laughter]

MS. JOHNSON. Yes, there still is the risk that I call the "Wile E. Coyote" risk—that you run off the cliff without realizing it, and then you drop. But the stuff that Joe Gruber did for Greenspan gave us more insight. Can you remind me of the details because Greenspan, too, came to look at the financial stuff somehow? He was fond of this perspective because he had this home bias idea.

MR. KAMIN. Yes, so Gruber did a lot of work looking at the variance of current account deficits across countries and different measures of that to support the Greenspanian theory that the improvements in international financial intermediation were allowing countries to run bigger imbalances than they did before. My recollection is that the data are mixed on that point, but it seemed plausible at the time, and it still seems plausible.

MS. JOHNSON. That is a view that says the capital account acts as a binding constraint on the current account. If you release that constraint, the current accounts of the world will

widen. If you tighten that constraint, they will perforce have to narrow, as opposed to a truly general equilibrium—where whatever happens, happens.

MR. MARQUEZ. That's Howard's paper.

MS. JOHNSON. That, I'd like that too.

MR. MARQUEZ. That's Howard's 1989 *Journal of Economic Perspectives*.[24]

MS. JOHNSON. Okay. This topic encompassed virtually the entire division, and the various task forces that were put together reflected that. We had to have a trade person and a financial person. We had people from TFS, the Trade and Financial Studies Section of IF, contributing too. The banking people and the micro people didn't have a huge role to play, but it was a way of calling attention to and using the ability of the different parts of the division to contribute more insightful bits about a piece of the story and then create a whole from that. And to use the econometrics, people had a role to play. Like I said, the theory people at TFS contributed, the country people and the exchange rate people had something to say. It couldn't have been a more IF-designed issue. It lasted the entire time, and is ongoing. The problem and my work on it started before I became division director. And the fact that it is ongoing suggests, as Steve mentioned a minute ago, that our Pollyanna attitude that this is just the world's economy being the world's economy and people should relax a little bit wasn't entirely misplaced. On the other hand, the housing market people probably said the same thing before. [Laughter] I remember some R&S briefings on how the price-rentals ratio could be explained, and it was equilibrium. The same thing is true here.

We may yet encounter some sequence of events that produces a discontinuity in the exchange markets, and we will call that the external adjustment problem coming home to roost

[24] Editor's note: David H. Howard (1989), "Implications of the U.S. Current Account Deficit," *Journal of Economic Perspectives* vol.3 (Fall), pp. 153–165.

when it happens. But it's been ongoing for quite a while, and it has redeemed our view that it has not only a variety of contributing factors, but given the length of time that it has unfolded, some exogenous factors have come and gone and some new ones have joined the party. And you might ask, can't we get one or two that would go the other way? [Laughter] We may have. We may now be seeing some doing that. We certainly did when the U.S. economy went into recession. But it was always one of my caveats for defining a solution to the external adjustments problem that it couldn't just be a recession, it had to be a sustainable long-term outcome. You might have a difference in potential growth rates that maintain themselves—that's fine as a possible long-term outcome. But just recession, that was not an answer. But you might be seeing some kind of adjustment in the Chinese savings rate that reflects the maturing of their financial markets and the higher standard of living. The poorest of the world's poor save because if they don't, they die tomorrow. When you get to the point where there is a market economy, where there are some safety nets, where you're not all in the same agricultural cycle together, where there's some ability to get help if you happen to become unemployed, savings rates can actually go down. China might be getting to a point where its savings rate is going down.

My favorite possible resolution was always that this hard-to-fathom productivity shock would pop up someplace else, and you'd get the process that had driven the United States from at least 1995 to 1999 would start to happen someplace else. Then you'd quite spontaneously get the world's markets moving capital into that economy, and undoing some of what had happened to the United States, and that could be a cure. That could still happen, but we haven't had any evidence, really, of that.

Relations with Other Central Banks

MR. MARQUEZ. Would you talk about the role of IF in dealing with other central banks? Has that relationship changed over time?

MS. JOHNSON. It has, in part in response to some of the major developments when Ted Truman was the division director. A huge change occurred when we took up our seat on the board at the BIS and made a greater commitment. That would be perceived by others as making a commitment to the BIS over and above what we had up to that point made, and that we needed to follow through on that. I think there's some truth to that.

In the early days of my time here, Henry Wallich was the international Governor. He went to most of the meetings, including the BIS meetings and the OECD meetings. He didn't attend the Latin American meetings. The other Governors were quite happy to have Henry fill that role. I'm sure the travel in those years was more arduous than it is now.

MR. HOWARD. Henry Wallich probably enjoyed it, though.

MS. JOHNSON. I think he enjoyed it, yes, but other Governors weren't sorry that he was the one doing it. Did Ted arm-twist the Chairman into going once a year or something to the BIS, or did he not ever go? Did Arthur Burns ever go to the BIS?

Wallich had health problems, and he left the Board [in 1986]. Subsequently, I remember conversations with academics who were seriously in the running to be on the Board or thought themselves to be seriously in the running. For a while, the idea of an international Governor hung on, and people would say, "I might be interested if I could be the international Governor." But I think the notion of an international Governor went away, and responsibilities were spread among more people. People wanted to make sure they weren't being left out completely. The Chairman and the Vice Chairman started going to BIS meetings. This coincided with decisions

on the part of the BIS to transform itself. I don't know the extent to which the United States contributed to that. [The] BIS began to expand its membership; it began to be less a European club and more a world place. The Basel committee was becoming more prominent, and people were fighting to get on the Basel committee. Countries would petition to be admitted to the Basel committee. At the same time, the Euro-Currency Standing Committee changed its name and became the Committee on the Global Financial System. The BIS began doing more research and calling attention to its research. And it played a role as a source of bridge loans and various rescue packages in the 1980s and 1990s. It transformed itself and took on a more global role.

The global economy was transforming itself *pari passu* with all of this, and the Board and the division increased its commitment to and energy spent on these issues and on the BIS at the same time.

The BIS meetings were restructured. During my time, the BIS formed committees and made some decisions. [The] BIS committed a certain amount of money to changing its meeting rooms around so more people could be accommodated. [The] BIS expanded the board of directors ultimately. It expanded the membership in some of the key meetings. There was a tension between having small versus large groups. Small groups can be more productive, and the people in those groups valued those opportunities. But the people who were outside complain bitterly. They struggled with making the groups bigger or having both small and large groups. I found myself very involved. I chose to do it personally and probably more than was necessary. It was an explicit decision on my part. There were things I could have delegated but chose not to. I don't regret my decision, but I can understand that somebody else—Nathan Sheets now and anybody who succeeds Nathan as division director—might make different decisions.

I came on when the Euro-Currency Standing Committee was changing its name and its focus. It was at the time of the Asian crisis. Studies on what had gone wrong and the right solution were being worked on. The BIS had instituted a Working Party on Monetary Policy in Asia and a Working Party on Monetary Policy in Latin America. I don't know how long before 1999 that was done. There had been prior meetings because they would talk about this was the third or the fourth. It had been going on only for a short while. I used those meetings as a device to get to Asia and Latin America, where I had limited travel. Most of my travel abroad had been to Europe. I'd been to the BIS any number of times as a staff member for occasional events, but I'd never been to Latin America, Mexico, or Asia. I wanted to strengthen my knowledge of and my ties with those central banks. The BIS meetings were a good way to do that in that they rotated them among the central banks in the areas. Ultimately I went to Mexico City, Cartagena, Brasilia, Santiago, Lima, and possibly one or two other places that slip my mind at the moment. I went to Buenos Aires. I never went to Montevideo, but Uruguay was not a member of the BIS. Don't hold me to that because many countries come to the annual meeting of the BIS annual meeting who are not BIS members. There are some occasions in which their actions are open to nonmembers. I went to different places in Asia. In visiting the central banks, I made contact with a governor, a director of research, or whoever the right person was. I'd spend two or three days with people from the other central banks in the region who came to the meetings. That struck me as a good thing. Although people come and go and these relationships aren't long-lasting, I got some business cards and phone numbers, and on occasion used that to call people when I had particular questions. I would see some of those people at the regular meetings in Basel with some frequency, too, depending on whom they were and what their role was. So I

went to those meetings and viewed that as part of this extension of relationships with other central banks.

Partly I was schooled by Tom Connors, David [Howard], and Ted that a lot of these countries have an asymmetrical relationship with the United States, and hence their central banks have an asymmetrical relationship with the Federal Reserve. The Chairman might agree to an appointment at the time of the annual meetings and they might even be included in a G–10 meeting. Smaller countries might be included in some meetings where the Chairman shows up. What the United States does is huge from their point of view. What they do, we hardly even think about. I thought that part of my role was to once in a while go and hear their views and thoughts, contribute to a conversation about the problems as they see them, take the time to visit their central bank, and go to the dinner their governor organized. I thought it was a nice thing to give them some time as an acknowledgement that this institution understood it was an asymmetric relationship, but we weren't totally blind to them. We were prepared to be helpful. We were going to treat them with respect; that was part of my role. R&S staff, in particular, was too busy, I guess, to be interested in playing this role. It was sad.

Another occasion in which the chance to build a relationship would be if someone came to visit us. Charlie Siegman left soon before Ted Truman. Both Larry Promisel and Charlie Siegman [Senior Associate Directors in the Division of International Finance] took a buyout deal. From Charlie, Tom [Connors] took over as the person who organized the appointment of foreign visitors. Tom maintained a good relationship with the Board members and got them to agree to meet with foreign visitors by making the process easy for them. He gave them information at the right time that they could quickly absorb, have the meeting, and be done. He made the Board members understand who was important to see and who was not important.

Oftentimes, the Chairman's secretary would say to Tom, "Does the Chairman need to see this person or not?" If Tom always said "yes" then he'd be useless. He would sometimes say, "If the Chairman's busy, I'll ask the Vice Chairman or I'll see a Governor." He would be responsive to help them understand which persons or potential foreign visitors were the most important and which ones were in the gray area. He would allocate the ones that were below the gray area to somebody else.

For foreign counterparts in the research divisions, we would try to get them appointments with staff economists and R&S staff. They didn't always have time, and a lot of them didn't have interest, either. That made me more determined that IF had a real responsibility. I realized I was imposing on R&S staff and asking them to talk to these people, but IF had a responsibility to maintain a collegial relationship with the staffs of other central banks. I personally saw quite a few people. I showed up at the BIS and tried to use that time constructively to talk to different people, maintain relationships, and put a positive, pleasant face on the Federal Reserve as opposed to making it seem like we were running roughshod over people. For the most part, everybody else who showed up at the BIS did the same thing—Roger Ferguson and Don Kohn when he succeeded him as Vice Chairman. They were respectful of the central bankers from other parts of the world and willing to work cooperatively with others on various committees and on various decisions.

In general, many people in this institution work at that, but I think the international division has a particular responsibility. We don't have a setup that creates a division that is devoted to external relations. There are a lot of central banks, especially of smaller countries, where international relations with others—the IMF in particular—loomed large. So there's a whole team of people who interact with the IMF. And, in the days before the ECB, if you were a

European central bank, you probably had a group who interacted with the other European central banks. Not in the economics of it but more in the politics of it, the international relations of it. We don't have any of those people, really. We have Tom Connors and before him Charles Siegman. But we've never had an IMF program. The Treasury holds the governorship of the IMF, not the Fed. The Fed is not required to have a division to maintain international relations, but I always took that responsibility to be an explicit part of what the IF division should be doing.

Advancement of Minorities at the Board

MR. MARQUEZ. What are your views on initiatives at the Board to advance minority representation on the staff? For women, the Board was perhaps not all that different from MIT or Stanford a long time ago. It's a little different now. Who in IF took that initiative?

MS. JOHNSON. For many years, Dave [Howard] and I ran the recruiting committee. We had multiple two-year stints running the recruiting committee. Overtime, the guidance on this subject changed in ways that I found hugely frustrating. Much of that guidance came from changes in the way affirmative action or equal opportunities were being kicked around in the society at large and in the way that the laws were understood. What you could and couldn't do seemed to me to change all the time.

When I first came to the Board, there were more women here than I was used to in a given economics department. At Stanford, there was rarely more than one other person and myself and it was not always the same person. When I arrived at Stanford, there was probably somebody older than me and, by the time I left Stanford, there was somebody younger than me but only two other women faculty members. There were plenty of women students.

I think the previous generation—the generation of Alice Rivlin—viewed government as a place where a woman with a Ph.D. could get a job comparable to what a man with a Ph.D. could get, which you couldn't do in academics. They just didn't hire women. There are all sorts of horror stories of women who are now in their 70s getting law degrees and not having any interviews and getting Ph.D.s and not being able to get real faculty jobs, having to be hired as instructors or something. In the 1940s and 1950s for sure, it was a very different world. Somebody like Marina Whitman (economist and professor), Alice Rivlin, and others have stories to tell.

But when I came to the Board, there were quite a few women, and they were very helpful to me. Immediately, there was this sense of reaching out and bringing one into the nexus of people. And there were enough people that you didn't feel that you had only one mentor. There were any number of different women in the division and the Board as a whole. There were people like Eleanor Stockwell in R&S who had been here forever. In that sense, I found the Federal Reserve easy to join and participate in. I felt nothing particularly different about being a woman on the staff.

Working up through the ranks was different. There were far fewer women at the higher levels than there were at the entry levels. But at the time that I was made a division director, there were several, which was quite remarkable. Louise Roseman was the director of the Division of Reserve Bank Operations [and Payment Systems]. Dolores S. Smith was director of the Division of Consumer and Community Affairs. There were probably at least one or two others. Several had been recently appointed at that time. I think Lynn Fox organized a dinner for me. Subsequently, I think we had a dinner for Sandy Braunstein who reached the level of division director in consumer affairs in 2004. There were enough female directors to fill a table

at dinner. That was a real transformation for which I credit Alan Greenspan. I always believed that ultimately the Chairman makes the decision on division directors. That was especially true the farther back in time you go. Now, as with everything, the processes have become more open, the criteria have become more objective, and the stages of interviewing and whatnot have become an explicit procedure. When I first became division director in 1998, the way you promoted officers was a much more behind-closed-doors, soft process than it is now. I figure that nobody became division director if the Chairman objected to that person. He might have said, all three candidates on the shortlist are fine with me, and let somebody else make the final decision, but nobody became division director over his objections. The fact that so many women ended up being promoted I always thought was to his credit, and I appreciated his being willing to take that action.

Social Internal Dynamics at the Board

MR. SMALL. Do you remember whether outsiders were seriously considered as division directors?

MS. JOHNSON. My memory is that it was always being brought up but that it never panned out. When I was involved in the recruiting for economists, people wanted to bring in new blood and beef up the research component. Reserving the highest positions for outsiders always raised concerns about what that approach would do to staff morale. You weren't going to be able to work hard and be seriously considered for promotion without any absolute constraints on you. The culture of the Board is that we promote from within. In the research divisions, we promoted from within and to deviate from that would be difficult. The Board has deviated from that approach lately in the Congressional Liaison Office; it has been bringing in outsiders.

It was presented to me that Peter [Hooper], Lewis [Alexander], and I were the candidates for division director of IF, and it was a question of choosing among the three of us. There may have been a broader list and greater deliberation before it got to that point that I didn't see. When Ted Truman thought about retirement from the Board when he became eligible, a bigger group had been configured. There were six or eight people among the division officers. There was about two and a half years to go before Ted was going to be able to retire, and from that group would emerge the next division director. That was almost said out loud. Cathy Mann left shortly thereafter, basically saying that she wasn't prepared to wait around that long for a small probability of becoming division director. [Laughter]

Outsiders weren't seriously thought about, but they have been more so lately. At the very least, the process is viewed as holding up internal candidates to that standard, so we might end up promoting the strongest internal candidate if that person measures up to the other one or two external candidates that we imagined we might approach, even if we didn't approach them.

MR. SMALL. Let's say that a well-known, highly respected academic was thinking of taking a job as a division director at the Board or one of the research divisions because they believed it to be a powerful job where, as one of the three barons (a director of one of the key Board divisions), they would help run the show and advise the Chairman. But that may not be true. The Chairman and the other Board members are the decisionmakers and, by the time you get down to the division director, you're kind of a glorified research assistant. You get your marching orders from the Board or the Chairman to go do your task. How do you view the division director job?

MS. JOHNSON. It depends on who the Chairman and the other Governors are, and I think the Chairman dominates that. I had little exposure at high levels to Chairman Volcker.

The style, personality, and views of Chairman Greenspan and Chairman Bernanke affect the administration of the Board, how the staff is utilized, how staff members might see the pluses and minuses of a given job, and how the job of being a Governor might be viewed by the people who are candidates to be Governor.

It must have been a different world to think about becoming a Governor in the days when Paul Volcker was fighting off internal mutinies, from the days when Alan Greenspan was first starting out, and from the days when Alan Greenspan was toward the end of his 17 years of being in charge. Those things matter to what the job of being a Governor was like. Hence, I think they must matter also to what the job of being a division director is like. For example, if you think that power has been shifting toward the Chairman and you want to make the Board a more open, inclusive, and collegial place for the Governors and the FOMC, it's probably going to mean that on balance, the division directors have less of an explicit role and more of a supporting role. Either the Governors or the Reserve Bank presidents, or both, have more of a role. There are only so many decisions to be made. There's only so much time at a given meeting. To some degree, you are marginally increasing one role and marginally decreasing another role as these things shift over time.

Arthur Burns, I'm sure, governed differently from the way Miller did it. Miller did it differently from the way Volcker did it. I was here for all of but two months of Paul Volcker's Chairmanship and I was here the entire time Alan Greenspan was here. The Greenspan of the first few years was different from the Greenspan of the last few years. Early on, he was less certain in his own mind. When I first became a division director and worked for Greenspan, it was late in his time. I was desperately trying to learn what he expected of me, and I had to live up to what he expected of me. I was on a steep learning curve, and I was tentative in the

beginning. By the time he left, our relationship had shifted, and I was more willing to challenge his thoughts. I was more willing to offer him alternative views or explanations. I was more willing to suggest things that should be in testimonies or speeches or raise questions about things than I'd been early on. When I was first director, I needed Greenspan to teach me what the division director should do. By the time we get to the point where Ben Bernanke became the Fed Chairman, I viewed my job as helping him learn how to be Chairman. I knew how to be division director. I was open to him telling me that he wanted things done differently or he wanted certain specific things. I'm not trying to suggest who the boss was; it's clear who the boss was: It was Chairman Bernanke. But I felt I had a responsibility to bring certain things up with him and to explain the historical situation about events, organizations, or persons, and to some degree attempt to persuade him if I thought that the circumstances called for the Federal Reserve to do something in the international arena and give him the reasons why. I would not have done that early on with Greenspan; and by the time Greenspan was ready to leave, it was moot. Things had shifted because of where I was in my term as division director early on versus toward the end, and then when each of them was a brand-new or experienced Chairman. I remember seeing Ted interact with Paul Volcker early on and being a little puzzled. But looking back on it, it was the difference between a brand-new Chairman and an experienced division director versus an experienced Chairman and a brand-new director. These things evolve.

Functional Structure of the Fed

MR. SMALL. Do you remember conversations with counterparts at other central banks about their structure?

MS. JOHNSON. A little, but not much. The ECB is the institution most parallel with ours. It has a huge economy. I had known some of the people at the ECB for a long time and

could have those conversations with them. We would make some comparisons about our structure versus the ECB structure. A conversation with people from the Bank of Canada was a much smaller deal all around.

MR. SMALL. Currently there are proposals before the Congress to restructure or reinvent the Federal Reserve System? Are there things you'd change?

MS. JOHNSON. We talked about the debates when the ECB was being structured. I said then, and I still believe, that if we had it to do over again now as opposed to 1913, 1933, or 1935, we would structure the Fed differently. The ECB was structured in the world as it was in 1999. If we were restructuring the Fed today, we wouldn't have the Districts we have now. Why would you have San Francisco having this immense District and Cleveland having the District it has? Our Districts reflect 1913. If you have an institution that's already been built, has a long history, and has working relationships, why should you change it and under what conditions should you change it? That's a whole different question than whether you would do it differently if you were building it from scratch now.

Like the people who care a lot about the Federal Reserve and the United States, I share some concerns about what is perceived as the democratic deficiency of having the Reserve Bank presidents, these strange entities, make public policy but not being part of the political process and not being accountable to government. I question the status of the District banks as quasi-private nonprofit, Section S corporations, or whatever the heck they are under U.S. law. They're not public entities, but they're not maximizing shareholders either. I wonder about the ability of a FOMC as large as the one we have to come to one mind.

MR. SMALL. Do you think it's good to have free market research departments in one of the Reserve Banks criticize and challenge the Fed staff?

MS. JOHNSON. At some level. For example, in the days when monetarism was becoming a serious thought in our profession, did it help that St. Louis became a bastion of monetarism? I wasn't here much of that time. I didn't come until 1979. By the time I came, St. Louis had already staked out this claim. I don't know how much inside the halls St. Louis' role forced people here to ask questions or produced in the FOMC arguments that might not otherwise have been made, but I would think yes. [Laughter]

On the other hand, do I think that these other views loom large every FOMC? No, I don't think so. The profession doesn't have quite the deep divides that existed when St. Louis was doing what St. Louis was doing. Could it have been just as effective if St. Louis had been a major academic institution and not a part of the system? Was the University of Chicago doing the exact same thing? We needed a bunch of people to do it, but it didn't matter whether they did it in academia or they did it inside the Federal Reserve System. I don't know that we could judge that, exactly. I know that it is confusing sometimes to outsiders, whether they are in the markets or they are in foreign central banks if the District Banks act too independently and start expressing conflicting views. From the history of the 1920s, we know that was a big issue until the New York bank became dominant, and then we had the formation of the FOMC. This confusion of who was in charge and who spoke for the Federal Reserve loomed large. From time to time, I think there's been a little bit of that. There are people who try to make more of differences within the Federal Reserve than makes sense. But, on the whole, the marketplace of ideas and the opportunity to hear different voices are valuable and useful contributions from the District Banks.

If you already have a system in place, and you aren't starting from scratch, the hurdle for making changes should be high. At this point, there's nothing about the way the system now

works that strikes me as so obviously a bad idea that sensible people ought to be worried about it and ought to be changing it.

MR. MARQUEZ. My understanding is that the system is working the way it was designed to work. If the presidents of Federal Reserve Banks represent the private interests of their region—

MS. JOHNSON. They don't represent private interests. I firmly believe that each of them, in making monetary policy, take a national perspective. That point was deeply discussed when the mirror was held up to the Fed for the purposes of the ECB. The people at the ECB, or the European Monetary Institute, were worried about bringing together the equivalent of Reserve Bank presidents and having them debate monetary policy from the perspective of what their country needed, as opposed to from the perspective of what, on average, would be the right monetary policy for the whole area. I think the Reserve Bank presidents make policy for the entire United States, not for their Districts. On the other hand, they bring to the table information they have gleaned from their areas. They want that understood, and they want to share that. And they want the people in their Districts to feel that they have a relationship with that District bank and that the District Bank will voice their concerns and the way events are having an impact on them. Now, that's a very fine line of distinction there, but I think it's a valid one.

The 14-year terms for members of the Board of Governors were intended to have a Board of experienced people. People probably at the end of their careers, who had done other things and then come here as a last job, who would be removed from the political process because of their protection and their long terms, and who would stay here for long periods of time providing stability and wise counsel. We haven't done that for a real long time, but I don't know that there's anything bad about it, necessarily; it's just not quite what people had in mind. It seems to

me that the structure of the terms, being fixed in calendar time, and so if you had five people, it would be 10 years, and if you had four people, it'd be eight years and so forth, that structure works well. It provides a structure and order that keeps things from being chaotic with people coming and going.

I've been always torn about this idea that the President of the United States ought to have the chance to replace the Chairman of the Fed one year after he's been in office. This implies we should somehow rearrange the way appointment of the Chairman works to make it more political. I see the logic for and against that idea. I have never been persuaded that, on balance, it'll be a good thing. Plus given that the term only lasts four years, a President has an opportunity to replace the Fed Chairman pretty soon no matter what.

MR. MARQUEZ. Right, unless they start the same date.

MS. JOHNSON. And even then, the device of having a vacancy every two years enables a President to appoint someone who can force the issue. That seems like plenty of political leverage; we don't need yet more political leverage. If we truly were in a parallel universe, we probably wouldn't think that 14-year terms now would make sense. Alan Greenspan stayed 17 years, but generally we realize that nobody's going to stay here 14 years. For the most part, we aren't hiring people at the end of their lives anymore. The idea that this institution is going to be removed from politics is mostly but not completely true. If I were to draw a comparison, I think the Supreme Court has a more contentious political element to it at the time that new justices are appointed than does the Fed. We have succeeded in making it a more competency, apolitical thing. But once you're a justice, I think you are more removed and will likely stay the rest of your working life, than are Fed Governors, who tend to come midstream in their careers, and they are going on to something else, et cetera.

Interactions between the Staff and the Board

MR. MARQUEZ. What are your views on the relationship between Board members and Board staff with the division director in between?

MS. JOHNSON. There have been episodes in which individual Governors have interacted with the staff in a way that created problems. In my 29 years here, there have been many Governors, and they interacted with the staff differently for widely differing reasons. Some had particular intellectual ideas that they were trying to push; some wanted to further their own careers in some ways; and some were positioning themselves vis-à-vis the Chairman. There is a risk that staff members might be put in an impossible situation by a Governor who has his or her own agenda and somehow thinks there's a way that one or more members of the Federal Reserve staff can help with that agenda. It's the division director's job to keep an eye on that. Somebody who's reached the point of being division director should be close enough to the Chairman that if there is a real problem, the division director could go to the Chairman and the problem could be solved quietly. That's not necessarily easy, but it shouldn't be impossible.

Governors come from all walks of life. They have a secretary, and some had an assistant. I didn't know if some of them didn't know they could ask for an assistant. I know that the issue has arisen in the research divisions that if individual Governors were utilizing a whole lot of staff time, a resource constraint issue would arise. The Board has this wonderful large staff. But the 300-person staff of economists in these divisions are not an individual Governor's personal staff. How different Governors develop a work style that meets their needs but is consistent with the rest of the Board is tricky. It's made trickier by the fact that the Reserve Bank presidents, who are their colleagues on the FOMC, each have a good-sized personal staff. When the question came up, for example, that the way we were going to solve the question of

communication/inflation targeting/whatever it is we're going to do next, which resulted in the idea that we'd do more forecasts and that individual forecasts would be reported more often, the Governors were at a disadvantage where that's concerned vis-à-vis the Reserve Bank presidents and to some degree the Chairman, who could ask more of the staff than any one Governor feels comfortable asking of the staff. It's an ongoing process. The staff director of management, the administrative Governor, the Chairman, and the personalities of the individual Governors enter that mix differently at different times, and different people find different solutions to it at different times.

Speech writing is also a troubled area that most of the division directors manage as best they can on their own. For example, I never thought that I needed to think about creating some kind of quantity in my own head about how many resources each Governor might get for speechwriting purposes over a year, say, and was somebody exceeding their limit. Now, I think I had an easy job because there weren't that many Governors giving internationally focused speeches. Most times, we welcomed a Governor to give a speech with an international orientation. If we had a Governor going to an international meeting, it was probably because we went to them and asked them to go. We would tell the person that we would help. We'd provide a briefing book or write talking points. We were looking to be supportive. We recognized that it was to our advantage to have Governors willing to do some of these things—notwithstanding, I said a minute ago, that people were a bit competitive for some of these roles. I could imagine in R&S, it's a whole different question. I see it as a structural issue.

The institution has never really had to address the question of parity among Governors in demands on different staff or different purposes. There are no guidelines of which I'm aware. There's no person who sits around and worries about it. If people were giving lots of speeches

and they all wanted complicated help from R&S, I could see where that would be a problem. It was not likely to be a problem in IF. It is a gray area where not much is specified. The system just pragmatically solves that problem. If certain Governors are pushing certain viewpoints, it could create some stress if the staff were being asked to endorse that view and do a lot of work on it. It's one thing to give them what we've got; we'll answer the questions you've got. But if the staff is being pushed to endorse the viewpoint, I could see where that could become a problem. But, in my time as division director of IF, I ever felt we got that far.

Research and Day-to-Day Policy Work

MR. MARQUEZ. We talked about the exchange rate and about the USIT model versus a general equilibrium model. In IF, there is sometimes a tension between what we call current work and research, in particular with the type of research that may be valued most. When you were division director, you didn't seem to have any litmus test on what research should or shouldn't be conducted. What was important was that the output of the research answered a question that was relevant for the conduct of monetary policy.

MS. JOHNSON. It was not without episodic flare-ups in the division on how much research is valued versus how much current work or policy work is valued. Before I became a division director, and while I was the division director, I observed how the experiences in the division are viewed by people in different positions. People would come to me and complain about how much more work section A was doing than section B. A section chief might well come to the view that their people are being asked to do more than the people down at the other end of the hall.

MR. MARQUEZ. Is this your perception as division director now?

MS. JOHNSON. Yes. People doing one kind of research might voice a complaint that I may not hear. You listen and try to work through the people who are confided in by others, so they'll come and tell you. The people doing one kind of research feel that they're being undervalued versus people doing another kind of research, or the people doing research will feel undervalued relative to the people doing current work, and so on. There's a lack of available ways for people throughout the division, and ours is a fairly small division, to fully understand what everybody else is doing. And there is a tendency in human nature to downplay what the other guy is doing. There are circumstances in which it's evident that people are working hard. During the midst of the crisis in Latin America in the 1980s, we all could see certain people running around like crazy and working all the time. But lots of the time, people assume that other people aren't working as hard as they are. And the more we try to make evaluations objective, the more this problem seems to be brought out in the open and confronted in conversations among the officers or between sections of the division about who was really doing what. I always struggled with what the right management tool was to deal with this. I didn't want to defend other parts. I didn't want to feel that I owed an explanation to a given section chief who believed his or her people worked harder, by listing all the work the other guy did or by offering to provide evidence. I wasn't going to surrender my role as the person who supposedly saw everything and made judgments. I was happy to confirm by saying, "Your guys work hard, but I see more than you do, and in my judgment, those guys are working hard, too." But I wasn't going to feel that I had to provide them with enough evidence to show that it was or wasn't true.

The research question loomed large all the time, and it too was a moving target. From time to time, I would get reports from certain people in the division who felt comfortable coming

and talking to me: The people in charge are awful, and I'm unhappy. Sometimes the problem was judgment about the quality of different research output or the balance of research versus other work or that research isn't valued. There was a perennial issue of whether TFS is or isn't a special job. And there were troubles keeping people in that section or keeping out some people who wanted to get in but whom the rest of the section didn't want.

When we were making PMPs (Performance Management Programs) more formal, it was explained to me that some academic departments award points for certain journals. An article in the *Journal of Political Economy* got a certain number of points, but an article in the *Journal of International Policy Analysis*, if there is such a journal, got fewer points. There were class-A journals, class-B journals, and class-C journals.

MR. MARQUEZ. The New York Fed was doing it. They were also counting the number of citations of a paper. So if you got a paper published in a lousy journal, but it was a seminal paper that was widely cited, that compensated somehow.

MS. JOHNSON. That's the obvious problem of starting off by putting the journals in groups. I tried to help people understand the multitude of ways that one could succeed in this institution. When I was recruiting, I would tell people that, in part in contrast to an academic job, there were numerous career options here. You had two different models in academics: the university model and the college model. Everybody in academia who was paying any attention whatsoever understood that. There were the occasional universities like Princeton that muddied the water by being in the middle, but beyond that you understood those two models. But here at the Board, there were a multitude of ways to succeed. Not all the people who got promoted did so because they did the same thing. Some people were promoted because they were good researchers. Some people were promoted because they were good at current work. Some people

were promoted because they showed tremendous initiative and kept coming up with new ideas. Some people were promoted because they were good at leading groups. If you put them in charge of a group, you got a good product out the other end. Some people were interested in and willing to make their policy relevant. Other people didn't want to do that, but their policy was attracting attention throughout the profession and, therefore, enhancing our reputation and making it easier for us to attract and keep good people. Even in the research track or in the policy track or in an in-between track, there were multiple ways to succeed. I thought that was a strength in that you could come here and you didn't have to be just one thing or another thing; you could even experiment. I always encouraged the people who came right out of graduate school to focus on one of the research tracks. Don't close the door, until you really know that you want to give up on an academic career. As a very young, new person, you should be doing more of that than somebody who's been here 10 or 15 years, who knows what they like about this place and are prepared to make those judgments, close some doors, and maybe open other doors.

The downside of that is if there's all these ways to succeed, how do you evaluate them? There's this point problem. At what pace is he moving on track A and I'm moving on track B? How do you tell me that he's ahead of me? How do I know what I have to do to catch up to him? If there are only so many promotions, is it evident how you're rating these different ways of contributing to the division?

History and human resources are pushing us to be more objective. But the more you go down this road, the harder it is to be objective about these things. There's a tension. For research, the most important thing to do is to not eat the capital stock. In tough times, you might eat a little of the capital, but if you eat all of the capital, it's going to come to a bad end. Eating

all the capital is never the right thing to do. So, depending on the resources you have at hand, you encourage the people who are contributing to the capital, who are the capital, to stay. You encourage them to do what they do best. If you're lucky enough to have ample people, and people whose interests coincide with yours, they're going to flourish here. And if you keep enough of those people and you lose some of the people for whom that's not true, you don't cry too much. I always thought that it was a big mistake to feel that you should never lose anybody; that it was a negative every time somebody voluntarily left. You had to be realistic. You were not going to keep all the strong people. On the other hand, if you started losing large numbers of these strong people, you were doing something wrong. You needed to be somewhere in the middle, that you weren't so catering to the idea that a strong person might leave that you were distorting the division and your management and your treatment of that individual to keep that person at all costs. On the other hand, if you found yourself losing your strong people pretty consistently, you had a big problem.

There were times that you were not sure if you were doing this right. Shortly after Ted Truman left, there were times when the division came under a lot of pressure for various reasons. There was a time when the IMF was really eating our lunch.

There was a time right after I took over where we lost a bunch of people all at once. I was nervous. But I tried to think of it in these terms—this isn't the ideal place for everybody, so you mustn't view everybody who chooses to leave as somehow a disaster or a failure or the wrong thing. If you find yourself keeping the people you don't want and losing the people you do want, then you have a real problem, and you have to do something about that. To some degree, the problems were shared in general.

So we go back to where we started with the issue of what kind of research does the division support, what kind of research does one reward, and how much research does one think you actually want people doing? We all had the stories we like to tell about projects that led to important published articles and the multiple byproducts of research into answers for current work and attention in the Board Room. Far more common is research that doesn't have quite all that going for each and every paper. In part, it depended too on what was hot in the international economy. If things calmed down a little bit, you hoped people would quickly turn to their research, and when things heated up in a particular area, you hoped people had projects that they could afford to put on hold for a bit without their having big cost to them.

MR. MARQUEZ. What you're describing now also applies with the same force, even if you were in the academic world where you have all the freedom to do it. You submit the paper—it's now in the hands of the editors, judging whether or not this is worth publishing. And that's subjective. To some extent, we go through the same thing. There's going to be someone who says this is good; that's not the issue; the issue is that this is better.

MS. JOHNSON. Right, exactly. It's frustrating. When I first became professionally active as an economist, the Committee on the Status of Women in the Economics Profession (CSWEP) didn't exist. I remember the AEA [American Economic Association] convention. I was working and still teaching at Wellesley. Carolyn Shaw Bell was on the Wellesley faculty. She was the mode of force, the instrumental person who ultimately was present at the creation of CSWEP, which was a result of a fight at a plenary session at an AEA meeting. I heard all about it when she got home, and I saw her role as she set up and became the first chairperson of

CSWEP.[25] So there was awareness of and sensitivity to whether women were being unfairly treated one way or another.

I think after that, but I could be wrong, pressure built within the profession—the academic side of it, anyway—and people started doing, at least for some articles, anonymous refereeing. This cavalcade of people claimed that, if certain person wrote something, they get published. The newcomers, the young people trying to break in, faced a much higher hurdle at getting an article published in a major journal. How about making referees review articles with no authors' names attached? People pointed out that an experienced reader was going to be able to detect who the author is based on the subject matter, the references listed, and the approach taken. But they were not going to force people to write the article differently to make them less traceable. They were only going to black out the name on the front page and hope for the best. Still, people thought it made some difference, and they went for that.

MR. MARQUEZ. That wouldn't work today because all you have to do is Google the title of the paper and out comes—

MS. JOHNSON. —the author. At the tail end of my tenure as a division director, the issue of webpages for economists came up. How do you balance here at the Fed the research role and the norm of having a webpage, getting your papers out, having a blog and having everybody being Nouriel Roubini [professor, New York University, Stern School of Business], write large or write small, however you want to do it, versus being a staff member at the Fed, where everything is supposed to go through channels. Plus if your name's attached to a webpage, it can't this and that, and a disclaimer must be evident, and you can only do certain things—all these can be issues for the staff. The problem must only have gotten worse since I

[25] CSWEP is a subcommittee of the AEA charged with addressing the status of women in the economics profession.

left. Every academic I know has his own webpage. Many papers are widely read long before they ever hit a refereed journal. In fact, I'm surprised there are still refereed journals.

MR. MARQUEZ. I haven't had that problem. In the division, the problem we have had—and I only know because I talk to those guys—is with the IFDPs (International Finance Discussion Papers)—and this will also be true for the working paper series of the other divisions. We don't have copyrights. So here is what has happened: Rob Vigfusson, one glorious day, gets a request from a publisher to go over his paper that is going to appear in this book. He didn't submit the paper to the book; he doesn't want his paper in that book, but he has no control over it. The editor or publisher asked him to make sure that the transcription was correct. That has had an effect.

The webpages are now blogs, and you can now just put photographs of your little dog and so on. Maybe if I were 20 years younger, where all my competitors have that and I want to have that, then that will be an issue.

MS. JOHNSON. Well, what if you'd been out teaching for six years and you had already established one, and we try to hire you here and say, by the way, we've got these rules.

MR. MARQUEZ. That's a good point.

MS. JOHNSON. It's the difference between being this self-employed academic in charge of his or her own future and allocation of time and having certain responsibilities to their so-called employer versus being here. As you recruit, the role for webpages and posting your work on those webpages is going to be an issue.

MR. MARQUEZ. The 508 requirement that any website has to be accessible to people with disabilities is discussed a lot. What that means for IFDPs is, if you have a chart, there has to be an embedded description of the chart.

MS. JOHNSON. Programs exist that take text and read it out loud. You supply the text. Everything has to be able to be represented by text. Then a blind person can install the software on their computer, and the software will read the paper to the person. When it gets to a chart, the software will skip to this embedded explanation and read the explanation to them. Two years ago we were grappling with this for the backward-looking things. There were some policies adopted, but then we agonized over how many backward files do we need to do this to, or can we not put them on the web? You wanted to put them up, but they wouldn't then be compliant. There's some kind of reasonable man test or something. We kept trying to figure out whether we'd met certain exemptions or, if not, at what pace could we do it, or was there some limit to how far back in time we had to go in order to comply. It is difficult.

MR. MARQUEZ. We're not done.

MS. JOHNSON. No, by no means.

MR. MARQUEZ. And the Humphrey–Hawkins presentations now have to be compliant. All of it. It's not limited to research; it's everything, including the transcript of this presentation.

MS. JOHNSON. But this is text, so it's not such an issue.

Life after the Fed

MR. MARQUEZ. Are you happy since you left the Fed?

MS. JOHNSON. I have thrived in retirement. It's not without its wistfulness. It's been an exciting time in economics since I left. On occasion I think about the FOMC having these conference call meetings and making all these decisions. The Fed is being very activist. All this stuff is happening, and I'm not part of it. I'm missing out. But then the other part of me says that you're not working seven days a week. You're free to say what you want. You have options that you didn't previously have. I'm not entirely immune to the seduction of being part

of an international crisis, running around at international meetings, and talking to people about them.

So far I have enjoyed retirement. There are parts of my former job that I miss, not just the shot of adrenaline that you get from an exciting event but also the genuine serious conversations that you have with people about world events. Being at the Board puts one in a good position to have those serious conversations readily. You have them over lunch. They're part of doing your work. I have to make an effort to have those conversations now and, therefore, I have fewer of them. I still do some work, and I've gotten a few things distributed.

Washington's a good place to live. I had toyed with what I might do by way of location over some horizon. I had this fantasy that I would clean out my house. [Laughter] But when nothing happened on that score for over a year, it was clear that that decision wasn't anything that was looming soon, although I will say that in the last six months I've done quite a bit. The media in Washington is helpful, but the Internet is every bit as helpful. I go away for the summers now. I'm a little bit disadvantaged but not a lot disadvantaged. I still log on every morning, and I still get all the news. I can still buy the *Wall Street Journal* in a hard copy if I want to. I'm still reading things off the wire services every day and checking on what the rates are doing. The internet makes it possible to do that whether or not you're employed and wherever you are, and that's a big plus. Five years ago, it might have been harder. Ten years ago, it certainly would have been harder. Now it's easy.

When I'm in Washington, I accept invitations to hear certain speakers or participate in certain events. I try to hold up my end by occasionally being the speaker or by showing up so that they keep inviting me. The speakers are actually worth listening to more often than not. I also run into fellow economists, which I now don't do very much unless I make an effort. Then I

get to have conversations with them about what they think about what happened in Greece or something like that. Otherwise I end up having those conversations with my son over the telephone, and that's frustrating. That doesn't really accomplish anything. [Laughter] So Washington is a good place for that. Being division director, I was offered a chance to become a member of the Council on Foreign Relations (CFR), which has proved to be a valuable resource. They have a lot of speakers, so I go from time to time.

MR. MARQUEZ. This is in New York?

MS. JOHNSON. It's both in New York and Washington. I never go to New York. Although New York has a few enticing things that are not duplicated in Washington, I don't go there; it's not worth the trouble. They have a new building here in Washington on F Street. They have small groups as well for some purposes, and I have been included in some of those for some purposes. That's another environment in which I get to have a conversation with people. And they have posted two papers for me on their webpage. And who knows, if I ever finish this third paper, which has been sitting in a quite undone state for some time now, maybe I will offer it to them first. It's a way of staying in touch, seeing people, and feeling obliged to do stuff. I have a small consulting business that makes a tiny bit of money.

MR. MARQUEZ. So you have got tenure. You don't have to raise kids. You still have to worry about promoting and maintaining yourself active and connected. So you've completed the circle.

MS. JOHNSON. Yes. On the other hand, I'm my own research assistant and my own secretary. Those are not pluses; those are minuses. [Laughter] I'm woefully inadequate as a research assistant. I don't know how to draw charts, and I make terrible tables. [Laughter]

Surely, there must be some software miracle that you invoke and it makes tables look better, but no, they are just ugly. So obviously there are things you give up.

I am now not censored by being a member of the Federal Reserve staff, so I have written a couple of op-ed–like pieces that another friend with a different webpage has seen fit to post. For many years, we all had opinions. Within the Fed, Lord knows, if you sat down to lunch any day here, no one hesitated to share their opinions. But it was expected that one would be circumspect outside the Fed. Even within the BIS, people were circumspect about their own country and their own central bank. We might all freely talk about the advantages of rational expectations as an idea, but we didn't freely talk about policy steps that had just been taken. People put up their guard for those kinds of topics. Now, without anybody caring, and absent committing libel and slander, I can express opinions. These op-ed pieces are more opinion than they are analysis. I don't know that too many people are reading what I'm writing. One of the reasons I'm having a hard time making myself sit down and work on this paper that is a third done is—so what if it were done? I'm not sure that anybody will read it. But then I read something in the newspaper that gets me going, and I say, God, somebody's got to understand the difference between this and that, and I have to finish that paper. Then I go home and do a little more work on it. I have managed to get a couple of conferences where the cost is small; money can be the issue. So like coming here to a conference at the Board, that's no big deal. Generally speaking, a conference held in town is a two-thirds-of-a-day conference where you get lunch or something.

I have managed one way and another to make contact at UVA (the University of Virginia). That's manageable because I'm cheap from their point of view. I drive down. They put me up overnight and I participate in the conference by talking on some panel. It's a perfectly

reasonable place and perfectly reasonable people. This last one was with Frank [Warnock], and the one before that was at a place called the Miller Center. It's loosely speaking attached to, or at least sometimes works with Darden [The University of Virginia School of Business]. I think that link was in fact from the first CFR paper. I think somebody there had read that paper because the topic that I spoke on was more or less the topic of the paper. Now I know a few people down there, so who knows, maybe something else will happen. It's a very low-intensity thing. I contrast myself to what I see Ted Truman doing, and I am miles apart. He's basically working full time. I contrast myself with Mike Prell [former director of the Division of Research and Statistics], whom I see fairly frequently. Those people have continued to work—a larger fraction of their average day is spent on economics than is the case for me. But I'm pretty happy with the balance I've struck.

Years ago, when I thought that I would probably retire before age 67 or 68 and maybe even before 65, which is what happened, I thought it would be too cold turkey to stop working. I thought I would become a visitor someplace where all they would expect me to do is teach, someplace pretty. I would go do that for a semester or every year in the spring for two or three years. I thought, well, that's constraining. [Laughter] I have any number of former Fed colleagues who teach in an adjunct sense. Universities are always willing to get people to come and teach on that basis. They are desperate for cheap help, so I can do that.

I retired in February 2008. I left the Board at the end of 2007, but I was a full-time Fed employee until February 1 or 2 of 2008 because I had unused annual leave. It was too late at that point to scrounge up a spring teaching job. I decided to relax a bit. I enjoyed being on annual leave, but, come spring, no doubt I would be feeling at loose ends. I thought about lining up

something for the fall. If you teach a course, that's kind of constraining. You've got to go! Every week, you've got to go to this place and teach this course. [Laughter]

I haven't taught, and I am astonished, frankly. I thought I would. But I am a delighted guest lecturer, so if you know anybody who wants a guest lecturer for a one-time-only guest lecture, I'll prepare. I write it out. I'll even hire somebody to turn it into PowerPoint if that's your thing. I'm happy to be a guest lecturer. And I consider myself extraordinarily fortunate in the financial relationship that I have with the Federal Reserve because of the time when we were hired and because of the rules that applied when we were hired. I feel that I'm indulging myself in a lifestyle that I fully know many people cannot indulge themselves. I try to not lose sight of that. Most of the things that I do for UVA or various people around town, I do gratis. I don't ask for money for the papers. They did pay me a little bit for one paper, but it was a different relationship. If I'm still interested in economics, it's as much a service to me as I am a service to them. I try to concentrate on nonprofits. I'm not particularly interested in expanding my role vis-à-vis profit-making entities. I don't know that I could if I wanted to. As I said, I have a small consulting business, and I do get paid. Those people are in the real world. I'm happy to accommodate anything associated with any kind of nonprofit, whether it's a think tank or a university because that's my way of giving back to the fact that I happened along at a time when I am now able to retire in a fortunate situation.

MR. MARQUEZ. Karen, thank you much for your time, and David, thank you much for your time.