

Interview with Jerry Jordan

Conducted by David Wheelock, Robert Rasche, Julie DeClue

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Wheelock: All right, April 11th, 2012. My name is David Wheelock. I'm a member of the Research Division at the Federal Reserve Bank of St. Louis. My colleagues and I are here today to interview Jerry Jordan, a one-time economist and Research Director of the Federal Reserve Bank of St. Louis for the Bank Centennial Project. I'll let my colleagues introduce themselves.

Rasche: I'm Bob Rasche. I first came to St. Louis Federal Reserve Bank as a Visiting Scholar in 1971 when Jerry was a member of the Research Division at that time, and I subsequently came back in 1999 as the Research Director, a position I occupied until I retired a little less than a year ago.

DeClue: I'm Julie DeClue. I'm with the Facilities Department here at the Bank, and I'm a member of the Centennial Committee.

Wheelock: Okay, I'm going to ask Bob to set the stage and kick us off here, and again, welcome Jerry to the Bank for a return visit, and I hope you enjoy your day.

Jordan: I will. Thanks for inviting me.

Rasche: Jerry, we appreciate very much the fact that you've gone to the effort to come here and take the time to participate in this and sort of preserve some of the, what I guess we both can refer to as somewhat ancient history of this institution at this time. I thought maybe I'd start out by talking a little bit about the context of the economy and monetary policy and so on at the time you came here, and so I'll toss in a few of the thoughts and recollections I have, and then I'll toss the ball to you, and let you add, subtract, correct my memories or whatever. Okay?

It seems to me that shortly before you came here, one aspect of economic policy in the economy was what everyone called the "high water mark of applied Keynesianism." There was the 1964 Kennedy/Johnson tax cut, there was the wage/price guideposts as promulgated by the Council of Economic Advisers in the Kennedy and Johnson administrations as a way to sort of control inflation, or to prevent inflation from building up when they wanted to stimulate the economy to get lower unemployment rates. That's one aspect of it. Another aspect of it in the early 60s, early/mid 60s, was a resurgence in interest in money and monetary policy, which prior to that time had been very much below the radar screen, both in policymaking and in academic discussions. There was the Friedman and Schwartz volume on monetary history of the United States, which reinterpreted the Great Depression, and the role of the Fed in the Great Depression, it attributed a causal role to the Fed in the Great Depression. There was

Friedman and Meiselman's paper for the Commission on Money and Credit, on the relative stability of monetary velocity and the expenditure multiplier. That's going on in the professional literature. Homer Jones came here as Research Director in 1958. St. Louis began, the Research Division began to publish estimates of the money stock, and in the early 1960s, about the time shortly before you came here, there was the AER, *American Economic Review* issue totally devoted to the rebuttals to the Friedman-Meiselman paper and then Friedman-Meiselman's rejoinder, what became known as the Radio Debates. Darryl Francis became the President here in 1966, and then there was the famous or infamous (depending on your perspective) issue in November '65 when McChesney Martin raised the discount rate, and the Fed Board of Governors raised the discount rate over the loud protests of the Johnson administration, and my understanding is that gave McChesney Martin an invitation down to the ranch in Texas.

And then finally, there is an awakening of inflation which had been in the one percent range in '64, and by '65, depending upon what basis you measure it, it gets to the high twos or even three percent. So that's some of the context that I remember from the mid '60s and just prior to when you came here to St. Louis. So maybe you would like to correct me or add to it or whatever.

[Part 1, 00:04:35]

Jordan: No, everything you said is correct. But let me supplement it with a few other things that were going on in that period. First, the reason it was called the Radio Debates was Friedman-Meiselman, FM, and the other people on the other side, the Keynesian, were Ando and Modigliani from MIT. So that was the AM of the FM, which gave it that label. And it was a debate about the thrust of monetary policy versus fiscal policy, but really without any kind of empirical work, maybe some casual charts or something. But it was mostly just academics talking in the classroom sort of debate. The other thing that was contributing to that debate atmosphere was Milton Friedman versus Walter Heller in a small book that was published where they each argued in a classroom type fashion how they saw the world, and how the whole broad, what we call macroeconomics, worked.

The one thing you didn't touch on that I think did contribute a lot to the discussions at the time was international. We were still under a fixed exchange rate system. There was a debate underway among academics about whether the world should remain under fixed exchange rate systems. Remember that we were only two decades out of World War II, and we had the brand new currencies of — new in the sense of less than 20 years old — the Japanese yen that had been fixed by MacArthur and the guys at 360 yen to the dollar, because 360 was the most divisible of numbers, and it didn't really matter anyway. And the Deutsche mark had been fixed by the occupation forces at four to one.

And both of those were under stress because of the US inflation that had been undertaken in the 1960s — Vietnam and the Great Society — and Johnson's refusal to consider the possibility of raising taxes to pay for it because he didn't want Congress to debate whether the war effort was worth it, or whether the Great Society was worth it,

which they would have had to do if you were going to propose a tax increase. So, he said, "No, I'm going to do this, and I'm going to get the Federal Reserve to pay for it." And one of his advisors explained, "But that will just cause inflation, and inflation is just a different way to tax." And Johnson responded, "I understand that, but it's a tax that no one has to vote for," except, of course, the FOMC in effect had to vote for it.

So that was adding a dimension to the domestic debates that were going on because the United States was supposed to be the anchor of stability of the international monetary system, a willingness to exchange our currency for ounces of gold, and Johnson simply unilaterally refused to do it. And basically, he told the Europeans, "If you want your gold, send in your navy to get it. We'll be waiting for you, because it's our gold and you can't have it."

So, I think that that contributed a lot to the tensions around the world as well as within the United States about how economic policy was viewed.

Within the Federal Reserve, for the most part, I think that people were viewing the world through a lens of interest rates. No one was paying attention to any kind of quantitative measures, and the ideas that Homer Jones and Darryl Francis supported at this Bank of looking at aggregates, looking at bank reserves, looking into money supply, was just out of tune with what everybody else was saying. And as they started to say money mattered, the response from New York in particular, but from others in the system, was, "Well, if money matters, then credit counts." And it added to the public kind of aspect of the nature of the debate that was taking place.

[Part 1, 00:08:52]

Wheelock: You came to the Bank in 1966?

Jordan: '67, summer of '67.

Wheelock: How did you happen to come to the Bank?

Jordan: Actually, I drove.

Wheelock: From California?

Jordan: From California, yeah.

Wheelock: You were a student of Brunner's at UCLA?

Jordan: Yeah, what happened was in the spring of '66, I was working on my dissertation and Milton and Rose Friedman came to visit for a quarter. He was on, Milton was on sabbatical from the University of Chicago, and so Homer Jones as Director of Research at St. Louis Fed, who had been both a teacher of Friedman's at Rutgers University when Milton was an undergraduate, and then later went back to Chicago to write his dissertation under Friedman. So, Homer owned the distinction of being both a teacher of, and a student of Milton Friedman, which must be about as good as you can get. And Homer took a leave of absence from the St. Louis Fed, and came out to spend a couple

of months at UCLA while Milton and Rose were there. Milton conducted a money workshop as he typically did at the University of Chicago, and it was those students who were at the dissertation stage would present their thesis proposal in a Friedman workshop type seminar, which I'm sure was very unique in the approach he took. And I presented my dissertation in that workshop, and attended, was registered, even though I was at the dissertation stage for credit in that quarter course. And so, I was able to become acquainted with both Milton and Rose, and with Homer Jones.

So, then the next winter, when I entered the job market, and was interviewing at the Reserve banks, one of the banks that I interviewed was St. Louis, although St. Louis did not make me the best offer. And so, I was all set to go to Kansas City until Homer Jones made a counteroffer and agreed to match Kansas City. So, I came to St. Louis instead. But it was true that my professors, both Karl Brunner and Armen Alchian at UCLA and Milton Friedman, strongly urged me to consider St. Louis as the better place to go for the kind of work I wanted to do.

Rasche: So, coming here, what were your expectations of this environment, intellectual and otherwise here in St. Louis?

[Part 1, 00:11:37]

Jordan: From what I knew from Milton Friedman, and from Homer, it was going to be a place where I would have more freedom to do the kind of research that I wanted to do. Karl Brunner at that time had been on leave from UCLA and [visiting] other schools. He had spent some time at Michigan State, Wisconsin, and then settled at Ohio State. And he suggested that this would be a better place for me to be to continue the kind of work that I had been doing as his graduate assistant for three years, and to complete a dissertation.

I didn't have much expectation about the other people that I would be working with at the Bank because I didn't know anyone else except for Homer. I did expect the facilities as far as computers and research assistants and all of that to be better than it was, and so that was somewhat of a surprise to find out that we didn't even have a memory typewriter, let alone a computer that could compute anything. So, one of the first things that we had to do that summer was to invent remote processing. Maybe Al Gore invented the internet, but St. Louis Fed invented remote processing because we would punch up some cards and call Yellow Cab and give them to the taxi company who would deliver them out to McDonnell Douglas out at the airport that had a computer that was the same as what I'd been using at UCLA. And so, we were able to start doing computer processing by way of Yellow Cab Services.

The other thing that we had to do that first summer was to hire some recent college graduates that had good math skills so we could teach them to write Fortran programs after we explained what Fortran was as a computer language. And we were probably the first Reserve bank that was, outside of New York maybe, that was both using a

mainframe computer, although one located 20 miles away, and had our own staff of people that could actually punch up the cards to start to do some empirical work.

[Part 1, 00:13:55]

Wheelock: What was it like working for Homer Jones? Was he a hands-on Research Director, or was his philosophy just to go out and hire the best people and then let them do their own thing?

Jordan: Homer drove everyone absolutely crazy. I think part of his method was to really make us angry. He was a total agnostic as far as both theory and empirical evidence. He would needle everyone to “prove it to me, where’s your theory, say it better, where’s your evidence, prove it to me.” His background — he had worked at the FDIC with Clark Warburton and was a professor [. . .] and student of Friedman — was to focus on empirical. And he wanted the data. He had started collecting, as Bob mentioned, the money supply statistics, and putting out things just to make it available, even before I arrived that summer of ‘67.

His philosophy essentially was, if the people see it often enough, they’ll come to believe it whether they understand it or not. And so, the weekly release, *Monetary Trends*, *National Economic Trends*, all of this was a conscious effort to simply get it out [in front of] the people, and if people start seeing and hearing enough about all these various statistical relationships, it will start to, over time, gradually, to make sense to a lot of people. And he would press the economists, both in the way we wrote the cover page to the *US Financial Data*, the weekly release, the monthly releases, to try and put in a little bit of economic theory. It wasn’t very sophisticated, but it had to be more than pure descriptive of ‘this went up and that went down.’ It had to be derived from a relationship that could be supported, at least theoretically, and possibly empirically. Homer didn’t do research himself. He did not write. I’m not sure he could have written a grocery list. He did not compose anything. But he was a tenacious editor. He edited everything, and especially in the early years, nothing went out of the Bank that didn’t have Homer’s red pen scrawls all over the early drafts before it got to the final production stage. The weekly release, the *Monetary Trends*, the monthly *Review* articles — every one of them had gone past Homer several times or else it didn’t go out of the Bank, and that pretty much was his biggest function was supreme editor of making sure that there was no mistakes.

Another part of his philosophy that I learned from, benefitted from, was the accuracy of the English language, punctuation, as well as the numbers, because he would say, “If you can’t even spell correctly, why should people believe that you can report the numbers correctly; and if you can’t report the numbers correctly, why should anybody believe that your theories are any good, or your evidence is any good?” So, he was very, very exacting in no mistakes. Zero tolerance for any kind of mistakes.

[Part 1, 00:17:42]

Rasche: The famous, or infamous, depending on your perspective, Andersen-Jordan equation got published in 1968. When did you guys start working on that? What are the antecedents of it?

Jordan: Well, I would say almost from the time I got here. The Friedman-Meiselman article was already out. The Radio debates, Friedman-Meiselman/Ando-Modigliani, was in the profession. UCLA had hosted two conferences on targets and indicators. Karl Brunner and Allan Meltzer had arranged to invite in people from both sides of the debates from MIT, Harvard, Yale, the big names in the profession [such as] Tobin, as well as Modigliani and those type of people were all involved. And then, those more associated with Chicago or UCLA and Texas A&M and a few places.

At that time, the Economics Department at UCLA was referred to as the “University of Chicago at Los Angeles” because of the views of so many of the people that were at UCLA was in sync from a theoretical framework standpoint with those people that were then prominent at the University of Chicago — Friedman, Stigler, and the others. So, we had been exposed to the ideas of the debate. We wanted to contribute, in part because Homer wanted to be a part of the debate because of his relationship with Milton Friedman. I certainly did because of my relationship with Karl Brunner who was a major participant in it. And we had the staff and the potential to do the empirical work. It’s really important to understand how major it was to be able to have access to a computer and be able to assemble the data and run it through computer programs, to torture the data until it confessed to something or other, and then the ability to get it out as we did with the *Review* because otherwise the production process, to get articles into the major journals of the profession, was very long. The ability to even get access to computers and somebody to punch up the cards and type up the manuscripts, the drafts — we didn’t have even memory typewriters, but we had a lot of secretaries that could within a couple of days time type up a new draft of an article, for us to edit it again — and then we’d give it back to them, and two days later, we’d get it back. And people in the academic profession didn’t have that available to them. The professors, they didn’t, for the most part, didn’t have the computers, they didn’t have the research assistants, they didn’t have the secretaries, and so we were able to organize the resources in a way to get a much quicker turnaround and a response, and get it out to a very large audience.

I think that a couple of things that made the effort successful — and we might ought to talk a little bit about the role of the President of the Bank as well as Homer — but it was a philosophy of writing for the educated layman. Homer’s idea was that we should aim to get our work cited, not just by the profession, but in *The Wall Street Journal*, in *Business Week* magazine, *Fortune*, and the other business publications. So, this idea of “don’t target to compete with the *American Economic Review*, but target to have something to say that readers of *The Wall Street Journal* would be interested in” was an objective.

We benefitted enormously from Andy Andersen's writing abilities because the rest of us couldn't write at all. Homer couldn't, I couldn't as a new graduate student. Most of us didn't have the ability to put verbs and nouns in the same sentence, let alone make it coherent. And Andy had both wonderful expository skills, and a very good judgment about what worked and didn't work with the level of the audience that we were trying to reach. And so, while Paul Samuelson, probably one of the most prominent economists at the time, said, "Economics is the profession that sings for its own applause." And we weren't willing to sing just for our own applause. We wanted to reach a much, much larger audience, including a lot of people that were not trained in economics; and in succeeding in doing that, I think that we altered the way that the debate was conducted because very few other people were even attempting to sing outside the profession.

[Part 1, 00:23:03]

Rasche: What was going on sort of when you arrived, and let me put that in a little context. I was in Boston from '66 to mid '67 as one of the people working on the Fed MIT Model project, and I first met Andy when he came several times to visit Frank Modigliani to talk about some sort of modeling work that was going on here in St. Louis. I can't remember exactly what the modeling work was. But I've been thinking about it probably for days. But that was the context in which I met Andy. So, there must have been — and you didn't come here until, I thought you had come here in '66. You didn't come here until '67. So, this precedes that. So, there must have been something going on when you arrived?

Jordan: Well, not empirical. There was no empirical work underway. There was an attempt to start from the top down in the sense of the idea of national income accounts, and how could you derive from that something that related to the Federal Reserve in terms of either quantities, the money supply, or in terms of interest rates. But it was still mostly in the inherited theoretical framework that we referred to as Keynesianism. It was not at all what we later labeled "monetarism." It was not even in the same context of the historic[al] quantity theory of the relationship between money and economic activity. So, until I arrived with the benefit of having participated in those targets and indicators conferences at UCLA, having been exposed to Milton Friedman and his theoretical approach [of] how to formulate a falsifiable hypothesis, something that's testable, and as a student of Brunner, it wasn't couched in that kind of a rigorous testable kind of thing. But very little in the profession was at that time.

We also benefitted at the outset by Al Berger joining the staff from Purdue who had been a student of Bob Baseman's and had also been very much involved in helping the profession understand how to test things, and how to formulate theoretical propositions that could be subjected to empirical testing to find out whether there was confirmation or not. But that was all very, very new for a lot of people. We benefitted because there were so many of us that were very recently educated. We had just come

out of graduate school, and there was a common core at the time — a couple of people from Missouri who were students of Pinkney Walker, who were Chicago kind of economic theory; three people from Minnesota, Keith Carlson, Andy Andersen, Mike Keran, all had been students of Oz Brownlee's, who was also an Alchian-Stigler-Friedman type theoretician. [. . .] Roger Spencer came the next year from Virginia, a student of Buchanan's. A group of people that all had been trained in a very similar way as far as thinking in macroeconomic issues about how the world worked. And so, there was a commonality that made us all cooperative colleagues on all of the issues, and it made for very, very lively discussions.

[Part 1, 00:27:26]

There was another element that I think none of the other Reserve banks were doing at the time with the exception of New York, and that was visiting scholars. From the time I arrived here, we were inviting in people, [. . .] from around the United States, to come in for a very short period of time, a few days, a week, sometimes for up to a year. We started inviting people from around the world, and people from other central banks, from universities in Europe, and none of the other Reserve banks were doing that. So, it generated a very lively interactive environment of intellectual excitement because we were constantly preparing for another visitor coming in, discussions about what the previous visitors had been saying, and could we write articles about it, and we were inviting in a lot of people that we disagreed with — imminent names in the profession like James Tobin, and Fed Governors — Andrew Brimmer, Sherman Maisel — would come in and give a seminar. And when you could get a Governor of the Fed or a top professor from Harvard, Yale, or MIT, let alone Chicago and other schools, to come and sit around the table with some fairly new PhD students right out of graduate school and really mix it up, it was a very exciting environment. And then because we had the ability to use the computer and research assistants and data available to us to test out the propositions of what they said, pretty much every time somebody came in to visit wound up in another article being written, either showing that we could confirm or not confirm what they had to say.

Wheelock: Was Friedman a regular visitor or ever a visitor?

Jordan: Friedman was a visitor a number of times. I wouldn't call it regular, but he did come down and give seminars. Sometimes it would only be four or five people sitting around the table. There were occasions where we would invite him when some of our new recruits were still working on their dissertation, and he would conduct in effect a mini Chicago money workshop to go over their thesis. He would have them sit at the end of the table, or sometimes stand at the blackboard, and talk about what it was they were working on, and Milton would instruct them on how to do it better.

[Part 1, 00:30:08]

Rasche: After the Andersen-Jordan equation got published, the fur started flying so to say, and you got a lot of criticisms. One of them I remember was by Frank DiLeo and Jack Kalchbrenner, which ultimately was published in the *Review*, and then you guys got to respond to that. So, this is a replication of the Radio Debates in your own publication. What was Homer Jones thinking about publishing critical comments by outside economists in the *Review*?

Jordan: Well, anything that generated active debate and maybe pushed out the boundaries of our knowledge, Homer was in favor of because he really was an agnostic. "All right, you've got an opinion, that person's got an opinion, let's get it out there and see what people have to say about it." I think most critical to that was Darryl Francis. As President of the Bank, had he not been willing to take the peer pressure from his colleagues among the other Presidents and the Governors about the maverick reputation that the St. Louis Bank had developed, it wouldn't have happened. Darryl truly was, as I thought of at the time, the Harry Truman of the Federal Reserve System. He lived what was meant by the "Show Me" state philosophy. He really believed, "well okay, let's shine some light on it and let's see," and he would stand his ground. He didn't need a sign on his desk that said, "The buck stops here." Everybody knew that with Darryl, and he wasn't willing to be intimidated though the pressures were at times very considerable — especially after Arthur Burns became Chairman of the Board of Governors — to stop what we were doing at this Bank. And that was only in February of 1969, two years into the development of this maverick reputation, just a year after the article that Andy and I wrote, and there was a lot of pressure on Darryl to change the course of the Bank, to go back to focusing on regional agricultural issues or what were the crops doing this year, something like that, and Darryl resisted those pressures. He supported Homer as his Director of Research, and he supported the rest of us, but that meant that we had to be open and receptive to our critics. And so, we were very happy to publish in the *Review* opinions of people that didn't agree with us, and mix it up. It was interesting that the reputation of the *Review* was spreading rapidly worldwide at that time, and the volume that we were publishing was just exploding — a tremendous worldwide circulation.

[Part 1, 00:33:25]

As a bit of digression, quite a few years later, after the Berlin Wall came down, I was at the Kansas City Fed conference in Jackson Hole, Wyoming, and they had a visitor from what was then known as Czechoslovakia, who was the new Finance Minister, and they had just had their velvet revolution, and liberated themselves from the Soviet Union. And he approached me and said, "Are you Jordan of Andersen-Jordan?" I said, "Well yes." And he says, "I learned about you at the central bank of Czechoslovakia because you provided me with free propaganda." I said, "In what way was it propaganda?" He said, "Well, the censors didn't know that something that came from another central bank, a Reserve bank, was propaganda, was full of ideas, and we had no money to buy anything. We couldn't get *The Wall Street Journal* let alone your journals or magazines

or anything. But you would send it to me for free, and I could sit in my office in the central bank of Czechoslovakia, and I could read all of the articles that you were writing.” And he said, “You indoctrinated me because you published Milton Friedman; you published Friedrich von Hayek. And I learned about these people by reading the *St. Louis Review*, and that’s how I came to all the views I hold.”

Rasche: That’s interesting. I hadn’t heard that story before. Let me sort of stick something in here that I’d planned to ask you later, but I think now is a good time. Research at this point is a relatively small unit within the Bank, and so now you’re creating this controversy. You’re getting pressure from around the system and so on. How did the rest of the Bank — you explained how Darryl supported you — but how did the rest of the Bank view what you [inaudible] were doing?

Jordan: Well, there were a lot of very lively conversations in the dining room. Economists regularly would go into the dining room with all of the other officers from the Bank, and talk about what we were doing. But we would also be learning about what was going on in supervision. And not all of our research was focused on macroeconomic issues, monetary policy. We also started getting into banking issues, supervisory matters, and some of that was generated by lunchtime conversations. At that point, we still had a few very senior people who had been young employees at the Bank in the Great Depression of the 1930s. So, we were able to hear stories about what was happening in the Depression with bank failures, the role of the Reserve banks in the supervisory arena as part of a debacle for the country in both macroeconomic terms and in banking. And from these people who are now very senior — but at the time they were young in the 30s — the Federal Reserve was only 20 years old, and the First and Second Banks of the United States didn’t last longer than 20 years. So, the very survival of the central bank as such was still in question when they became employees at the Bank. So, I think that that influenced us, but I think for them, maybe the idea that we were engaged in these sort of larger issues, and mavericks within the Federal Reserve System, I think was a source of pride in the Bank.

[Part 1, 00:37:25]

Wheelock: No resentment though or feeling that, gee, if those guys would cooperate a little more with the rest of the system, that would ease our budget pressures, or—?

Jordan: No, I don’t recall anything like that. I think that the only tensions that surfaced was in the area of the computer, because the Bank had a very small 1401 computer that would crunch out the budget numbers and a few other things like that. Younger people today would have a hard time even imagining just how small this computer was because everyone talks about gigabytes and all of that of memory. And so, this was a 16k processing machine, card punch machine. We didn’t even have a tape drive on the thing. And Research wanted to have computer capabilities in-house, and not just call Yellow Cab and send our work out to McDonnell Douglas. So, we were pressing for more computer powers. I knew it existed because I had worked on a big machine at UCLA —

big in the sense of it was a 64k machine that took up the size of a basketball court. And the rest of the Bank didn't see the justification for that. And so, we started lobbying within the Bank, and with President Francis, to have more computer power. The rest of the bank's point of view was, well, if Research wants it for their purposes, they'll have to pay for it because it cannot be justified by the rest of the Bank. They agreed with the philosophy that probably there's about a thousand people in the whole country that would ever need to use a computer for any purpose. So, I was then made Director of the Division of Data Processing, Planning, and there was a third department, I think Accounting was even a part of it, in 1969 for the purpose of installing a [third] generation IBM type computer, building a computer facility, recruiting a computer staff. And so, having an economist put in charge of all of these people that were otherwise, in charge of automation, data processing, accounting, maybe was not comfortable for everybody, and not everybody decided that this was continuing to be a good place to work, and so there was a lot of turnover in those areas of the Bank.

[Part 1, 00:40:12]

Wheelock: What about the Board of Directors of the Bank. Were they supportive—?

Jordan: Board of Directors was very supportive, and that was critical. And that's where I learned the importance of a Board of Directors to a Reserve bank's personality, and its true independence. Darryl was very conscious of the need to have a Board of Directors that was supportive of our independence, and he worked hard at maintaining that. There was a lot of interaction between the economists and the Board of Directors. We made presentations at all of the Board of Directors meetings. We attended the luncheons with the Board of Directors, sat at the table with them to talk about the research we were doing, and what we were engaged in. And at that time, there was a very significant controversy within the Federal Reserve System about independence that had gone on from the beginning of the System. And the New York Fed was experiencing the same thing with their directors, and Board of Governors.

[You're looking now at the centennial, but remember that the system at that time, was just] a half a century old, and it was less than two decades after the famous accord with the US Treasury. The relationship between the Reserve banks and the Board of Governors was in turmoil, and the New York Fed, Jerry Corrigan who later became the President of that Bank was an economist and secretary to its Board of Directors, and was working on a number of issues about what is the role of the Directors vis-à-vis the President and Chief Executive, and the economists of the Bank, and where do they derive their authorities and powers and responsibilities. And so, we at St. Louis found common ground on one of the rare occasions with the New York Fed on especially this issue of independence and the importance of a Board of Directors in preserving the independence of the individual Reserve banks. And not all the other Reserve banks agreed with us [. . .] at that time, or subsequently, but it was something I learned here

from Darryl and this Board, and it was something that I tried to maintain and enhance when I was at the Cleveland Fed later.

Rasche: By 1970, the Andersen-Jordan equation had become one piece of an Andersen-Carlson model. How did that evolve? Let me try and give you a little more context, I guess. One of your visiting scholars was Bill Yohe at Duke I believe, and he published an article in the *Review*, I think jointly with Denis Karnosky who I guess was a student of his at Duke, about interest rates and the idea of nominal interest rates versus real interest rates, and at least the rudimentary idea of inflation expectations in effecting long term interest rates, and that became part of the thing. And then there was another equation that was built into it that I would characterize as a type of a Phillips curve equation, although probably no one at St. Louis at that time would mention the word Phillips curve. But it related, it was not the traditional Phillips curve of Samuelson-Solow, it was more of a Friedman-Phelps type Phillips curve in that it related inflation to a measure of expected inflation, and a demand pressure term. So how did, what was the thinking that caused all that to evolve as you can remember it, if you can remember it?

[Part 1, 00:44:17]

Jordan: Well, I do, and I guess one response is it wouldn't have happened if I hadn't been running Data Processing, Planning and Automation at the time. I remember my first reaction of reading an article in *Business Week* magazine after the first November '68 article. *Business Week* said, "St. Louis builds a model." And I saw that, and I thought, I'll be damned. I'm going to find out what this is all about and put a stop to it. And they were referring to my paper with Andy. I didn't know it was a model, but it turned out that people viewed that any set of equations, or even a single equation put to data, somehow served to be described as a model.

There was some focus on bits and pieces. The Bill Yohe-Karnosky thing, Yohe was one of our visiting scholars that came and spent a year, an academic year on leave from the university, one of many that helped sort of stir the pot here, intellectual ideas, a freshness to the way we thought about things, and that was before Karnosky then went to Duke and was a student of Yohe's. So, they benefitted from that. Roger Spencer from Virginia was also interested in and working on the Phillips curve. Milton Friedman had written his presidential address for the American Economic Association in December of '67, published in '68. Phelps also wrote about that time. So we were, especially with Roger and Keith Carlson to some extent, doing some empirical work, testing some of the propositions that were in the Friedman-Phelps work. There were quite a few people both in the United States and in England at that time that were starting to try to do some empirical work about this relationship between inflation and unemployment because it had become a big issue in the 1960s, the idea that there was [. . .] a social/political tradeoff between inflation and unemployment that policymakers had to contemplate. But most of the empirical work was fairly elementary. It might involve only drawing some charts, and drawing the attention of the reader to something going up or

something going down on the charts. So, St. Louis was able to add an extra dimension to the ability to do some empirical testing of the ideas that were in there — which way did the causality go, how did the expectations fit in, and could you quantify it in a way that expectations fit in? The idea of putting it into a framework of a model probably reflected the sort of work that was being done at the Board of Governors, at Penn, at MIT, of wanting to have some structure, showing a number of relationships instead of what we originally started out to do was simply test propositions that were a part of somebody's model, but not the idea that we were going to build the model ourselves.

[Part 1, 00:48:04]

Rasche: How did all this research play into Darryl's FOMC participation?

Jordan: Most of Darryl's statements to the FOMC were prepared in advance. He often went scripted, which I'm not sure was always the most effective way to do it. We spent a lot of time with him in advance preparation of an FOMC meeting, and he would go with a text that he would often read in the meetings. Probably much more significant were his speeches. Much of the research that we were doing found its way into public addresses that Darryl would deliver. I'm not sure that some of his audiences out in Chillicothe or Cape Girardeau or something would appreciate some of the speeches, but we worked hard at trying to put into those speeches the fruits of our research, the theories and the empirical evidence, so that we could then publish those speeches in the *St. Louis Review* as a speech, and reach a much bigger audience than otherwise. We could write in a way, in a speech, that you wouldn't do in an article that was meant for an audience of other economists, and it would often get picked up and at least quoted at length, if not run entirely by major publications. If it was not *The Wall Street Journal*, then we would get it into a publication known as *The Journal of Commerce*, or the *Christian Science Monitor*, or there were a number of places that would use these, including a lot of foreign publications.

We found later that economists at other organizations, especially central banks around the world, might be trying to influence their bosses, and they could get them to read a Darryl Francis speech, whereas they might not be able to get them to read an article written by economists at the Bank. So, we used the speech as a way of trying to broaden the audience that we were reaching with the same thing. So, in that sense, we were trying to write at different levels for different people that we were targeting. And without Darryl's willingness to give those speeches, and let us run them under his name, we wouldn't have been as effective. The Board of Governors at that time required that all articles written by the economists at the Reserve banks be submitted for review by the really good people on the Board staff to correct all of our mistakes and point out our faults before we published them, and that was not true of the President's speeches. And so, we were able to write and get out some ideas through the President's speeches that we might have to fight long and hard about with Board staff in articles written by the staff.

Wheelock: Was Darryl Francis, you know, he was not a trained economist.

Jordan: He was.

Wheelock: Oh, was he?

Jordan: He had a master's degree, he wasn't a PhD, but he had a master's degree in Ag-Econ, and he had a very good grounding in micro economic fundamental principles.

Wheelock: So, he had a pretty good understanding of the research that you were doing?

Jordan: Yes.

Wheelock: And could communicate that effectively to an audience?

[Part 1, 00:51:01]

Jordan: He was a very effective communicator, and that was one of the challenges of writing a speech in his style of speaking. And for my own experience, I found that once I had settled on a theme or a topic, that I would be able to achieve it best if I would outline it and then at a time late in the evening or on a weekend when I was all alone and no else around, turn on the tape recorder and give the speech as best as I could in his good St. Joe, Missouri style of speaking, transcribe it, clean it up, and that would become a speech. As you've probably observed, most people coming out of graduate school, not only can they not write the English language very well, they certainly can't write speeches very well. And in a reference Ronald Reagan made at one time to a draft of a speech [from] the State Department — George Schultz brought over from the State Department to give to the President — President Reagan said, "This was written to be read, not to be heard." And that was the problem with most economists I found, including my time at the Cleveland Fed, is they could not write something that was to be heard. And so, the challenge was to write things that Darryl could stand up and deliver orally very effectively, and that didn't sound like he was reading the text of a *Review* article.

Rasche: Was the speechwriting for Darryl an interactive process? Did he come to you guys with ideas that he wanted to talk about and then—

Jordan: Sometimes. But it was often, he would tell us he had a speech to give, and we would sit in the conference room and we would talk about ideas. He would give feedback on things that sounded interesting to him. Sometimes, if we were pretty strong about wanting him to give a certain speech, we would have to talk him into it and explain how the speech would work, and whether it was suitable for that audience. But there were times also where he said, well you know, I really want to talk about this problem of inflation or something else that he thought was suitable. It would depend a lot on whether he was speaking to a larger professional organization, and he started getting a lot of invitations to speak before groups that typically Reserve bank Presidents at that time might not have been invited to. A lot of the people wouldn't even know what a Reserve bank was, let alone what the President of the Bank did. It's very different today

to get a President of a Reserve bank. Everybody would love to have them. But a lot of people were getting their very first exposure to the head of Reserve bank, and Darryl I think found that sometimes intimidating to be asked to speak before major organizations. Sometimes banking organizations of a scale that maybe they would have only had a Fed Governor out from Washington, or the New York Fed speak, but to ask the President of the St. Louis Fed to speak was a very significant development for him.

[Part 1, 00:55:30]

DeClue: You were saying that with the written articles, you couldn't get information out, so you did it via the speeches that were written. But I guess what I want to ask is, some of the information that you were getting out with the speeches, did you get a lot of push-back then from the Board of Governors after Darryl gave those speeches?

Jordan: Oh yes.

DeClue: What was that like?

Jordan: Well, Darryl's approach, his sort of independence, allowed him to be willing to say, well okay, they have a different view, let them give their own speech. But I'm sure that he took a lot more remarks from the other Reserve bank presidents or from governors than he was always comfortable with. Probably the greatest stress that he came under was being called to testify in Congress. Sometimes with just a few other Reserve bank presidents, sometimes with all, because he knew that he was going to be singled out.

DeClue: Because he was the one that was differing opinion than everybody else around the table?

Jordan: Right. He was the maverick, and the members of Congress and some of their staff, some of whom we knew personally, we knew what the questions were going to be. Or, I guess the staff learned what would be good questions that maybe they could ask to facilitate an interesting congressional hearing. And there were some individuals in Congress that were very anti Federal Reserve, especially in the House — Chairman Patman was very critical of even the existence of the Federal Reserve. And there were some others on both sides. And sometimes they found it convenient to show the dissention, disagreements within the Federal Reserve. Fortunately, in the first years, Chairman Martin of the Board of Governors believed that having differences of opinion within the Federal Reserve was a strength, and he tended to be more, if not supportive, at least tolerant of the idea that a maverick Reserve bank could have another point of view, so that when he was called to testify and someone would be critical of, "Well, why do you have not only the seven governors, but these 12 Reserve bank presidents coming in to do monetary policy? You all sing from the same songbook." And he said, "No, no, no. We don't all have [one] view. There's Darryl Francis from the St. Louis Fed, he has a different point of view." And Martin would cite that as it shows that we had diversity. It was only 18 to 1. *[They laugh.]*

[Part 1, 00:58:30]

Wheelock: So, picking up on a couple of the themes there, you mentioned Congressman Patman of Texas, later day was Henry Gonzales, I remember when I was first at St. Louis, Gonzales was taking the Patman role to be kind of critical of the Fed. Did Patman single out St. Louis as being a particular problem or was he just generally anti Fed?

Jordan: No, it wasn't a problem from Patman's standpoint. It was actually the others that were the problem, and he welcomed the idea that there was another perspective. His version of Texas populism was pretty much anti banking. He just didn't really trust bankers. And he would have viewed anything that the central bank did as being suspect because I think, probably like Ron Paul later, was doubtful that the Federal Reserve even was constitutional, and whatever they were up to was no doubt no good, whether he understood it very well or not. He had been influenced in the early 60s by Karl Brunner and Allan Meltzer at UCLA [and] a commission on how the Federal Reserve conduct itself. Those were the days when interest rates were the primary instrument for monetary policy, and being a sort of a Texas populist, he always wanted low interest rates regardless, and the idea that the Federal Reserve would raise interest rates and cause the cost of credit, especially to farmers, to be higher was something that he didn't like, was critical of.

I think probably the one episode that both was critical to the kind of work that we were doing at the outset, and to Patman, and congressional oversight, was the so-called mini recession of 1966. That was a period when the Great Society [and] Vietnam expenditures by government were creating tensions, stresses within the economy, and inflationary pressures building, internationally the dollar was under pressure, and fiscal was characterized as being very stimulative, and yet the Federal Reserve's philosophy was that the role of the central bank was to preserve the purchasing power of the currency. And so, the Federal Reserve did engage in some restrictive monetary policy actions, and caused aggregate demand growth to slow, and inflationary pressures to ease, but as a part of that, it involved the very visible increase of interest rates which raised congressional criticism.

That episode served to teach a lot of people that monetary policy had the ability to counter what everybody in the profession viewed as [. . .] fiscal stimulus, and yet monetary restraint was effective. So at least in one direction, there had been almost a laboratory test, an empirical test, of an idea. That was important because just a few years later, Lyndon Johnson — as he was leaving office, not running for reelection — got Congress to vote to pass a 10 percent surtax on all personal and corporate incomes. [It] was characterized at the time as a massive dose of fiscal restraint, and some people called it overkill or at least over-cool in terms of fiscal restraint. And the Federal Reserve sought to cushion this restrictive action [by] the fiscal authorities by monetary ease and the economy boomed, not contracted. And so, we had within a few years' time demonstrated in real time both the ability of monetary policy to restrain the economy in

the face of what the profession thought was fiscal stimulus, and then the counterexample, to stimulate the economy in the face of what the profession thought was fiscal restraint. And both of those events served as the backdrop to the kind of article that Andy and I wrote in 1968, and the later articles. So, we were trying to provide empirical support for some propositions that were being tested out in the policy arena by the FOMC versus the fiscal authorities at the time.

[Part 1, 01:03:31]

Rasche: You've mentioned the prominence of nominal interest rates as a focus of monetary policy in the late 60s, early 70s. I remember, and you probably remember it and we may be two of very few people who remember the term "even keel" as an aspect of monetary policy, and an aspect of monetary policy that was very troubling to people here in St. Louis in the late 60s and early 70s. Do you have any perspective on that?

Jordan: Yeah, the "even keel" phenomenon tied together monetary actions, whether or not it was an intent of policy, to what the fiscal authorities were doing in a way that I think only came to be understood gradually over a substantial amount of time. There had been a view that whatever the Federal Reserve was doing in terms of setting discount rates or the federal funds rate was something quite independent of what was going on in government fiscal matters — spending, [. . .] taxing, [and] deficits.

But during the 1960s, and then continuing in the 70s certainly, there were significant periods of time of substantial deficits being incurred by the government, and the FOMC made a decision that there would be no intervention in the markets, no change of policy in terms of targeted interest rates during the same period of time that the Treasury was selling securities. But with the size of the deficits that were being incurred at the time — the amount of borrowing that was going on — the Treasury was in the market all the time. So, in spite of the desires of the majority [of . . .] members of the Open Market Committee, it was simply not possible to be very aggressive at times of altering money market conditions or changing interest rates because the Treasury was in the market and we had to "even keel" — we had to be steady.

So, it was trying to sail a sailboat where the winds are shifting dramatically, but you're committed not to change the rudder at this moment in time for whatever reason, and you get blown way off course. And we had argued that, especially in the '67-68 period, when the deficits were so large and coming continuously, that monetary policy was becoming de facto more expansionary even though the intent, if anything, (as stated) was to become more restrictive. But in the face of what the government was doing, the US Treasury was doing, there wasn't practical independence.

I think for a lot of us at that time, it reinforced the idea that ultimately monetary policy is a fiscal instrument. Later, Arthur Burns would characterize it as "we are independent, but we're independent within government; we're not independent from government." And fiscal ultimately is going to dominate. And for my own view on it, if a society is

unwilling or unable to achieve and maintain fiscal discipline, then it's unlikely to have the political will to maintain monetary discipline. Monetary policy becomes just another way to finance government.

[Part 1, 01:07:09]

Rasche: Once you had developed Andersen-Carlson model, you had a device which you could use for short term forecasting, and you did use for short term forecasting. Was there a way, a sense in which that caused a movement away from sort of the traditional monetarist focus on long term relationships?

Jordan: It was inadvertent in this sense. While we were trying to demonstrate that monetary policy, as quantified by aggregates — the monetary base and various measures of the money supply — could be powerful, it was not our intent to demonstrate that this was a new tool for discretionary, activist, stop-and-go monetary policies. But that's how it came to be viewed by others.

We were more, I think, of a mind that monetary policy not only was powerful, but also the timing of its effect — how it interacted with other things going on in the economy — was of such great uncertainty that it was not an instrument that you wanted to go messing around a lot with. Steady as you go was probably the best.

It wasn't a view that we could "do good" by manipulating monetary instruments, but we needed to avoid doing harm, and so I think that most of us would have thought at the time the appropriate course of action would be a very steady growth of the money supply to do the least damage as possible, while other policymakers, regulatory, fiscal and so on, would try and achieve their objectives without interference from the monetary authorities. However, others in the Federal Reserve System, including governors and members of Board staff or probably staff at other Reserve banks, thought that the strength of monetary policy impulses that we were able to demonstrate empirically gave them an opportunity to push and pull on a different lever to achieve their objectives. And so, to the extent that this kind of monetarist activism surfaced within the Federal Reserve, it was never a part of our intent.

Wheelock: So, getting back to the use of the St. Louis analysis and monetary policy, specifically the FOMC meetings, did you attend many FOMC meetings and see the interaction that Darryl had with the other members of the committee, and how that went?

[Part 1, 01:10:14]

Jordan: I attended only one, maybe [. . .] two in a year, as a substitute for either Homer while he was still Director of Research, or for Andy when he became Director of Research. [. . .] It started to become [. . .] that once or twice a year a different member of the staff would have the opportunity to attend. The rules at that time for the FOMC were that the President of the Reserve bank could only have one person accompanying him, and so typically, almost universally for all of the Reserve banks, it was the Director of

Research, unless there was a health issue or something else, that Director of Research was the only person that ever attended an FOMC meeting. And so, the rest of the staff was left with only secondhand reports about what actually went on.

For many of those years, even transcripts were not being maintained and widely available, or what became [. . .] referred to at the time [. . .] the Memorandum of Discussion. And they were only available with substantial lag after the meeting in any case, partly because of technology of producing [them] at the time. So, St. Louis probably was the first Reserve bank that started a regular program of having someone other than the Director of Research accompany the President on a periodic basis to broaden the number of people that had had the opportunity to sit at the table, or actually behind the table, and experience the dynamics of what actually took place in those discussions. And I think everybody that went to a meeting for the first time was very greatly influenced by it, because it was something that was, I think, not possible to imagine — the tensions, the pressure, the atmosphere in the room, in the way the meeting is conducted — unless you have actually sat there and observed it.

Wheelock: How did it change from the '70s to later when you were President of the Cleveland Fed? Was the dynamic of the meeting the same, or had it changed because of differences in leadership?

Jordan: Well, it changed a lot because of who was chairing the FOMC. We used to say about William McChesney Martin, Chairman for 19 years, that he never lost a debate because he always voted last, and Arthur Burns never lost a vote because he always voted first. But by the time Alan Greenspan became Chairman with briefly Bill Miller and then for two terms Paul Volcker in there, I think that the conduct of the FOMC had evolved very considerably from what it was like under Martin back in the 60s. The degree of professional input from the Board staff, the preparation of the Presidents by the Reserve bank staffs, was very different by the 1990s from what it had been 15 or 20 years earlier. The whole profession of course had developed a lot in that time. So, I certainly saw the discussions and the amount of economics that was involved in the discussion very different when I went to Cleveland than what I saw quite a few years earlier when I was in St. Louis.

[Part 1, 01:14:17]

Rasche: My recollection, '71-72, of the department here and interactions with Darryl was that both pre-FOMC briefings and post-FOMC debriefings was a pretty open and lively discussion involving everybody, including visiting scholars at that time.

Jordan: And it was very controversial within the Federal Reserve System because no one else did that.

Rasche: Okay, because—

Jordan: Not New York, none of the other Reserve banks, and the idea that we were allowing staff at all levels, including visitors, to sit in a debriefing session to get feedback from the President and the Director of Research about what went on in an FOMC meeting, who said what, and we would all laugh about how ridiculous it was. That was not going on anywhere else to my knowledge. It later started to occur at other Reserve banks, but that was Darryl and Homer valuing the staff's working on real issues of policy. I think the majority of economists at Reserve banks around the country at that time really had no idea what went on in an FOMC, what the issues were, what positions their President took, what advice the Director of Research was giving to their President about a meeting. Whatever research they were doing, it had nothing to do with monetary policy and the open market committee.

Rasche: Again, my only experience at that time was with St. Louis. So, I don't have perspective on the rest of the System. Did a lot of, was St. Louis subject to a lot of pressure to abandon this sort of openness?

Jordan: I don't know how intense that pressure was. There were questions raised about how appropriate it was, the confidentiality, whether that was being maintained and respected. Fortunately, we didn't have any incidents that would serve the purpose of somebody that didn't want that much openness. There were no leaks about what somebody had said or anything like that that caused us an embarrassment or any problem that I recall. And so gradually, and it probably was very gradually, [at] other Reserve banks, the Presidents would start to learn from Darryl about including all of these other people, and why he did it. I don't know how much interaction Homer had with the other Directors of Research about what we were doing and why and how we were doing it. It wasn't his personality to be that collegial with his fellow Directors of Research. So, he may not have shared with them a lot about the environment he was creating here as opposed to what was going on in their shops. But as I recall System committee meetings, among other young economists at other Reserve banks, they were increasingly aware of what was going on here, and profoundly jealous.

[Part 1, 01:17:41]

DeClue: Yeah, I was going to ask. What was the relationship between the economists here and those at the other Reserve banks? Did you ever talk with them or collaborate with them?

Jordan: Well, there were constant System Committee meetings of economists specializing in topics of monetary economics or fiscal economics or regulatory supervisory issues. These were going on all of the time. And so, we all had the opportunities to become acquainted with people at other Reserve banks that were doing similar kind of work to what we were doing. That had been going on I think even before I came in 1967. But after that, it became very, very frequent, and I think that some of the things that they learned that we were doing at the time, such as visiting scholars. How was it that the St. Louis Fed could invite in somebody from the Deutsche Bundesbank in Frankfurt or this

or that reserve bank around the world, other central banks, or academics? They liked the idea, but they didn't understand how and why we were able to do it and they were not.

Prior to that time, as you can imagine, visitors to the United States from other countries, especially if they came from Europe or Asia for an IMF meeting or World Bank meeting or something else going on in the US, they would be in Washington and/or New York, and if they had time for a longer visit in the US, they would fly out to San Francisco. And the idea of stopping off in the heartland of America simply wouldn't have occurred to them to make the time to do that. I think our reputation for the kind of research that we were doing — being the maverick, coming up with different ideas, challenging the conventional wisdom — gave a lot of people a reason to stop off in St. Louis and find out what was going on. And so, we had [. . .] pretty regular visitation[s] from the Germans, the British, the Spanish, the French, sometimes for just a few days at a time. But often, these resulted in someone coming for a matter of months or an entire year.

The first that I recall was from Mexico that a young economist from the central bank of Mexico, Banco de México, took a sabbatical leave of absence, and came and spent almost an entire year in St. Louis. We provided him with an office, and he did some research, but he mainly was learning how to do research. And as a result of both the kind of work that Andy and I had published — but also the work that I did in the automation area of the bank — I was invited to then go to Frankfurt for most of a year because the Deutsche Bundesbank in Frankfurt wanted to install a new computer because they had a computer that was about like the one we had here in St. Louis when I had arrived a couple of years earlier. And they couldn't do research — they couldn't collect the data, they couldn't punch the cards and write the programs, and they wanted to learn how to do this.

And it stemmed from a visit we had had from the then Director of Research at the Deutsche Bundesbank — a younger economist who later became the President of the central bank of Germany — because he had stopped off in St. Louis (on his way to go mountain climbing in the Rockies or something), to find out what we were doing. We got acquainted and so he invited me to then come to Frankfurt to teach them how to program the computer, and collect the data, and formulate the hypotheses to do research.

That was controversial because New York and Washington, for historical reasons, said, "No; relationships with foreign central banks belongs to the Board of Governors and the New York Fed." And the idea that a Reserve bank out in St. Louis would be invited to send an economist to visit with another central bank was questioned as to whether this was even appropriate. There was certainly somebody at New York or Washington that could do the job just as well, but Darryl stood his ground and said, "No, the invitation went to one of our economists, and I'm going to sponsor it, [to] support it," and so I was sent to Frankfurt. And then after that, we had regular visitation exchange[s] where our

economists would go and spend time at other Reserve banks and they would come to St. Louis. And New York and Washington did not always welcome that intrusion on their turf, so to speak, with other central banks.

[Part 1, 01:23:09]

Wheelock: So, Homer retired around 1970 or so, and Andy Andersen became Research Director I believe at that time. Did you—

Jordan: Yeah, but Homer retired, but he still came in every day. *[They laugh.]*

Wheelock: So, there wasn't a major shift I take it then with change in regime as Research Director?

Jordan: No, no, I don't think that Homer's handing over the Senior VP job to Andy changed very much except Andy moved up to the second floor and wasn't around the department as much, and we didn't have to worry about Homer's edited copies of everything we wrote, including — he often must have been doing this in the yard under the tree, because he would have grass stains on all the galley proofs that he was working on. And so, Homer became less significant in agitating on the quality of the writing, and the quality of the empirical work, but by then I think that we had pretty well established a pattern of how to do research, the emphasis on the quality. We had all learned the Homer Jones lessons of zero tolerance for mistakes, including punctuation, and so he had done his job of setting the environment that lasted long after he was Director of Research.

Rasche: A slightly different area. Late '60s, you guys started publishing the Monetary Base which is now known as the Adjusted Monetary Base. This clearly built on earlier work by Karl Brunner and Allan Meltzer, and contrasts to the Friedman-Schwartz concept of high powered money. How did you get into the Monetary Base business?

[Part 1, 01:25:09]

Jordan: Well, I was a student of Karl Brunner's and so I had drawn the charts for him and put the tape on them because we didn't have computers to draw charts in those days. It was a concept that I had had to learn thoroughly as a graduate student, and yet arrived at the Fed as a young economist instead of finding a closet where we kept the IS and LM curves that we were going to be shifting around, learned that what the Federal Reserve thought it was doing in policy was raising and lowering the overnight interbank rate, which was also being debated in the profession as to whether this actually did anything, was connected to anything important. And the idea of not looking at quantities was puzzling.

And so, we thought, well, let's give them an alternative; that in addition to bank reserves somehow measured, that the balance sheet of the central bank — what Karl Brunner and Meltzer had called "source base or extended base" when you apply adjustments to reserve requirements. Milton Friedman had called it high powered money. James Tobin had called it demand debt of the government sector. So, it was a

concept that was already in the economics profession, but was not at all a concept that was being used by the central bank itself. So, we had classrooms around the country teaching about an important concept of central banking, but was not being used in the central bank.

And following Homer's philosophy of "if they see it often enough, they'll come to believe it whether they understand it or not," we decided, well, we needed to start publishing the Monetary Base. So, because we had the ability to collect the data and to assemble it for publication, we obtained all of the necessary data to produce the time series and started putting it out in the publications. We did have to give it a name, and so in that article in the summer of '68 we settled somehow on the term "Monetary Base" because we didn't like any of the other labels that were then being used.

A side story of after we started doing that, Karl Brunner was visiting in England, and he met with a member of the Parliament who was interested in some ideas of what we were doing, and Karl was a great ambassador for the research data for the St. Louis Fed, and our publications, and he traveled the world giving seminars telling people to read our publications and look at our data, and this is where to find something valuable to them. So, he's meeting with this member of Parliament who wanted to learn more about it, and he told her, Mrs. Margaret Thatcher, that she should tell the central bank, the Bank of England, that they should look at the Monetary Base. And a couple of months later, Professor Brunner was meeting again with Mrs. Thatcher, and she said she had met with officials at the central bank, and they said they had looked everywhere and England does not have a monetary base. But in time, they discovered one, and started to publish it themselves.

That was part of my job when I went to the Deutsche Bundesbank in 1971 was to derive from their accounts the monetary base which they call central bank money, because before I went there they also did not access their own data to compile it in a way to have a data series that simply represented the central bank's balance sheet as a factor that they should look at. They essentially were doing — like the open market committee — looking at measures of pressures in the money market.

[Part 1, 01:29:19]

Rasche: You mentioned Homer's longstanding associations with Milton Friedman both as a teacher and as a student. My sense was that by at least 1971 when I arrived here, that Karl Brunner and Allan Meltzer were the dominant intellectual influence here in the Research Division. And while Friedman was respected, he didn't have sort of the standing that Brunner and Meltzer had. Do you think that's an accurate reflection?

Jordan: Yes, it is, and it's because Karl Brunner and Allan Meltzer, from the time they started collaborating, were very focused on doing empirical work. And they, more of their publications were supported by access to statistical series, whether it was in tables or graphs, but somehow trying to bring evidence to bear. Milton's work was much more

presentations of theoretical concepts, and far less sought empirical support for what he was doing, at least at that time. And I think all of Karl Brunner and Allan Meltzer's work was more oriented towards what was empirically testable than many of the people in the profession at that time. It became much more common for a lot of people to do empirical work in the subsequent decades. But they had to be viewed as pioneers in the 1960s and carrying into the 1970s at doing empirical research. And St. Louis was oriented empirically, in large part because of Homer and his emphasis on data. And so, it was just a natural fit.

Wheelock: Another one of Brunner's students who showed up here in the early 70s was Ted Balbach. I understand you had a relationship with Ted going back to your undergraduate days. Were you instrumental in Ted coming to St. Louis?

Jordan: Yeah, Ted Balbach was my teacher in my last year as an undergraduate in what we had come to call Cal State University at Northridge, changed its name after I left there. And he encouraged me to go to graduate school and go to UCLA, introduced me to Karl Brunner who happened to be looking for a research assistant. And so, Ted was instrumental in the fact that I went on to study economics, but also linked up with Karl Brunner as a student, and later as just a friend and colleague. So, when Mike Keran, who was our international economist in the early '70s, took a leave of absence to go work at the Treasury Department in the incoming Nixon administration, the new Treasury team that was being put together there, we invited Ted Balbach to take a leave of absence from his university and come back and spend an academic year doing the work of an international economist in Mike's absence. And then Mike returned, Ted and Rachel went back to California, but then Mike Keran was hired to become the Director of Research at the San Francisco Fed, and so we asked Ted to resign from his university and take a permanent position. So, after a year back in California, he returned to the bank and finished his career here.

[Part 1, 01:33:14]

Mentioning Mike going to Treasury, that was preceded [by] another incident where St. Louis was controversial. When the Nixon administration came into office in early 1969, like any incoming administration, [. . .] the Council of Economic Advisers [was] populated mostly by the outgoing administration's people. And so, the new incoming Council — Paul McCracken, Herb Stein, and Henry Houthakker — didn't have a staff of their own. They had the staff that was left over from the Johnson administration.

And so, they were looking for some support and they invited Andy Andersen and Elaine Goldstein from this bank to come down and give them some help in doing the kind of work that the Council needs to do while they recruited a staff. Because most people that staff the Council of Economic Advisers are academics who are on leave of absence, but because of the academic calendar, it usually means they're going to be there summer to summer. So, they had to live with the Johnson administration Council [staff] for a

number of months until they could recruit their own people. So, Andy and Elaine served that function for them as a transition, and then that led to Mike going to Treasury.

Until those events with the Nixon administration's Council, I don't think there had been another occasion where Reserve bank people outside of New York had gone to Washington. Certainly, Board of Governors had long had a practice of their staff would take leave and go to work at Treasury, or vice versa, or New York people. But the idea that people from a Reserve bank, especially a quite small Reserve bank in St. Louis, would be invited to come in to be the senior staff for the Council of Economic Advisers for the President was not only unprecedented, it was controversial, because like our relationships with other central banks, this was getting into the turf that belonged to New York and Washington, and they did not welcome that event. But once it happened, then I think it became much more common after that, both for Reserve bank economists to go to work at the Council or at the Treasury, or to work as Board of Governor staff on leave.

I think that St. Louis was probably the first Reserve bank that sent a staff economist to Washington to serve as an incoming Governor's staff assistant because Governors often would come in and even though there were a very large number of economists at the Board of Governors, none of them were available to serve a Governor. If the Governor was an economist who wanted to do some research or have access to economic information, they really had no resources available to them. So, I think St. Louis probably started that practice also of sending people in there. I don't remember when the first year was that we did that, but I suspect it was the first in the entire system that was willing to give someone a leave to work among the Governors' staffs in Washington. I hope it continues; I don't know.

Wheelock: This is probably a good spot to stop, take a break, have some lunch. If you are willing, we'd like to resume after lunch with a few more questions.

Jordan: Certainly.

Wheelock: All right, thank you.

[End Part 1 of Interview]

[Begin Part 2 of Interview]

- Wheelock:** This is David Wheelock, the Research Division of the Federal Reserve Bank of St. Louis. It's April 11, 2012. This is part two of our Centennial Committee interview with Jerry Jordan. Let the other interviewers introduce themselves.
- Rasche:** I'm Bob Rasche. I was a visiting scholar here at St. Louis in '71-72 when Jerry was in the Research Division, and in 1999 I came back as the Research Director.
- DeClue:** And I'm Julie DeClue, I'm with the Facilities Department here at the Bank, and I'm a member of the Centennial Committee.
- Wheelock:** Jerry, if we could sort of pick up where we left off before lunch. In 1971, August of 1971, President Nixon closed the gold window, and effectively took the US off the gold standard, off Bretton Woods, and at the same time instituted a wage and price control regime. And I believe the Committee on Interest and Dividends was chaired by Federal Reserve Chairman Burns. What was it like in St. Louis at that time? What were the discussions in St. Louis with regard to wage and price controls, with regard to closing of the gold window, or even about the Federal Reserve Chairman heading up a committee on interest and dividends in charge of wage and price controls?
- Jordan:** I would characterize the general response was shock and outrage, that it was so contrary to any ideas that we had about a market economy or a democratic republic, that it was hard to put into words just how disappointed we were both in the administration and in the Federal Reserve's role in it. Only Paul McCracken, the Chairman of the Council of Economic Advisers, resigned on the spot. The other two council members decided to continue to stay to limit the damage. George Schultz was in the Labor Department, Secretary of Labor, [and] decided to stay to try to limit the damage.
- We shared the view of Milton Friedman who then broke his relationship — a long-term personal relationship with his teacher, Arthur Burns — [when] he publicly wrote in *Newsweek* magazine that this was an outrage and wouldn't work. So we started writing articles trying to — from a theoretical perspective — put some data to it to empirically and support the view that the controls would be distorting, that they would not be effective in dealing with the problems emerging in inflation, and try to prepare the groundwork for when the controls ultimately broke down. [We knew that ultimately] people would try to restore a market discipline, as opposed to government edicts on setting wages and prices. So, the whole thing, it was an education, an opportunity at least to educate the public about how bad it was to have government involved in the economy in that way. And we tried to play a part in the educational process to a very large audience that we had by then as to why this was really a very bad idea.

[Part 2, 00:03:43]

Wheelock: Did President Francis give a number of speeches on the topic?

Jordan: Yeah, Darryl was very outspoken [. . .] about inflation being a monetary phenomenon — which the people in charge of the control programs didn't want. Arthur Burns had been giving a number of speeches where he said inflation was caused by what he called wage push, basically labor organizations. Arthur was trying to create the idea that it was the Wagner Act [. . .] of the 1930s, organized labor strikes, pushing up costs of businesses, forcing businesses to raise their prices, and that ultimately government had to intervene in labor markets in order to address the problem of inflation. And until they could do that legislatively, then the President needed to wave the magic wand of controls in order to stop it. And it was extraordinarily popular at the time.

The people seemed to want to believe in magic wand approach to stopping [inflation], and labor was very sophisticated and very far-sighted. The heads of the biggest unions, George Meany and Walter Reuther saw the handwriting on the wall as to what was at work. They could hear in Arthur Burns speeches and John Connally — who was Secretary of the Treasury — an effort to get a political solution to problems as they saw it. And [the unions] decided to cooperate, and they joined the Pay Board to assist in the governmental effort to determine appropriate wages so that they could not be cast as the [villains]. They didn't want to be victims, but they [also] didn't want to be the cause of the breakdown of the controls [and] anger the public and cause Congress to respond to the public's desire to find out who's to blame for the controls breaking down. I thought that the unions acted very intelligently in a political environment where the game was rigged against them, and survived to play again in the longer run by cooperating in the short run.

It was also difficult internationally to accept what was going on, because the pressures had been coming, had been building for a number of years, especially in Japan and Europe, to do something about the US deficit spending, and the inflationary pressures that were [increasing]. Milton Friedman at that time published an article about inflation just being a different way to tax. He called it the unlegislated tax of inflation. Germany and Japan were accumulating surplus dollars, but didn't feel comfortable pressuring the United States to exchange those surplus dollars for gold, which the Bretton Woods agreement fixed exchange rates allowed because, you know, we were only 20 years since the end of the war, and they were not—, they were trying to be cooperative and not adversarial. So, it was the British and the Belgians that had stepped up and said to the United States Treasury, “we want to redeem gold, turn in our dollars for gold as the treaty allows,” and the US refused and closed the gold window, and in effect we forced the strong currency countries that wanted less inflation, Germany in particular, to revalue their currency upwards rather than the United States devalue.

[Part 2, 00:07:22]

What happened starting in late '70, Arthur Burns had been in the Chairman's job just about a year, the 1970 midterm election had not gone very well for the administration

— midterm elections typically don't go very well for the new administrations — and the US had had a mild recession. The recession had occurred as a result of the aftermath of Lyndon Johnson's surtax at the end of his administration, and the on-again/off-again stop/go policies of the Federal Reserve. So, there was a mild, what we call a saucer shape, recession at that time.

And looking ahead then to a presidential reelection campaign in 1972, the Federal Reserve in early 1971 engaged in a very, very expansionist monetary policy. We wrote about it at the time as being 'pedal to the metal' monetary policy, just let her go. The data at the time was reporting a shocking increase in the growth of the money supply in the first half of '71, and we wrote about it extensively. All of our publications, *US Financial Data*, *Monetary Trends*, and the articles in the *Review* prominently featured just how expansionary monetary policy was, and that, with a lag, this was going to cause substantial inflation.

We got out to the middle of the summer of '71, and through our efforts, and the efforts of some others, especially academics like Milton Friedman, people were starting to see the implications of this monetary policy, and that left them with a dilemma. Either stop the monetary growth and run the risk of a return to recession by the time of the 1972 presidential election, or continue with the high monetary growth and have soaring inflation by the time of the election. And seeing that those two alternatives politically were very undesirable, they went for the magic wand of the wage-price control programs — to freeze everything in place, to float the dollar so that they didn't have to turn gold over to the Europeans, and get past the election. And effectively, that's what happened, that all of 1972, the pressures were allowed to build up, the distortions were allowed to increase. Real economic growth, as reported, soared. I think it was something like nine percent GDP during that presidential election year, but that's because the pressures that stimulative monetary policy were causing were not allowed to show through in prices. And so output soared, but we knew it would break down. We wrote about the fact that it was going to break down. And the result was going to be both substantial inflation and unemployment, and that this was a big mistake and we were going to pay a very heavy price for it.

And that's where our relationship with Washington — in particular, the Board of Governors, both the Governors themselves in some instances, and the Board staff — became very adversarial because they were trying to be cooperative and supportive of the control programs, the government policies, the Treasury policies, and we were not. We were trying to be open and honest and call it like we saw it, and it was very different than what they saw. That's a longer answer than you wanted.

Rasche: What conflicts emerged in the FOMC as a result of these dual hats that Chairman Burns wore? One hat, he's the head of the FOMC, the other hat he's — maybe triple hats, okay — He's the head of the FOMC, he's the head of the interest and dividends, and he's an ex-counselor to the President.

[Part 2, 00:11:28]

Jordan: Well, in '72, I don't really know because I was on leave of absence from the Bank at that time. So, I wouldn't be the best one to talk about what that was causing within the FOMC. I think that there was increasing attention on alternative ways to measure monetary policy, monetary growth, which St. Louis had been talking about for some time, versus the interest rate approach of other Reserve banks or the Board staff. And these were showing conflicting signals about the thrust of policy actions. And so, I think that probably it was starting to split down the middle between those that sided with Darryl Francis about how important money was and we should pay more attention to it, versus those that wanted to adhere to the focus on interest rates. Because inflation was being suppressed because of the controls, and because interest rates were being artificially influenced by the environment we were in, domestically and internationally, it wasn't a good period to get resolution to the conflict. It was only after the controls broke down in '73 — and inflation took off, and we went back into recession and unemployment went up — that the debate became more grounded in what was actually being observed because it was no longer being distorted by controls.

Rasche: In '73-74, we had the oil embargo, a very big outburst of inflation, gas shortages and so on. What was, I wasn't here then, so I don't know. What was the St. Louis view of how this oil shock hit the economy?

Jordan: Well, there was— two components to that. There was later in '73, and carrying into '74, the restriction on oil supply, which had a transitory effect on the measured rate of inflation, of influencing temporarily the price level. But we were writing about how the underlying monetary component of inflation was starting to show through with the kind of long lags that we observed in the data at the time, that the inflationary pressures of '73 were really just letting the monetary stimulus of '71 and '72 start to show through. So, we wrote about how much of the inflation was caused by the oil prices, versus the part of the inflation that was the monetary component. We also wrote quite a bit about the changes in international terms of trade where the oil price shock [. . .] amounted more [of] a wealth transfer from American consumers, people that bought gasoline, to the countries that produced and exported oil to us. So, we were trying to cast it in a broader context.

We drew at the time on such things — Milton Friedman called it the permanent income hypothesis, Professor Modigliani called it the life cycle hypothesis — but a way about talking about how wealth is created over time, how growth especially per capita income could be influenced by both domestic policies — taxes and regulatory energy policies — and foreign developments. In this case, [it was] the 1973 war between some other nations in the middle east and Israel that resulted in the curtailment of supply. And we would try [to] explain [what happened as partially] an economic event triggered by political developments — as opposed to that component [of inflation] that was influenced by domestic monetary or fiscal policy. [We] tried to let our readers see that

these things could be separated analytically as to what was within our own control by good or bad policies, and that part that was beyond our control because it was international geopolitical developments.

[Part 2, 00:15:54]

Rasche: Sort of a broad-brush question. You've talked about, and we pestered you a bit about the intellectual environment here in St. Louis. How would you characterize the intellectual environment in the system in the late '60s, early '70s, say, compared to more recent times? And I realize you haven't been in the system for a while. So, compare it to during your tenure in Cleveland.

Jordan: Well, I think that might be best represented by the fact that the serious economics at the Board of Governors including model building and testing of propositions was done in a special division, called Special Studies. It wasn't a part of the central, main-line economics. The Board of Governors hired a very large number of economists that had no involvement at all in issues that came before the open market committee. They had no bearing on what policymakers had to decide on on a regular basis. And I think that that was probably also true for the most part at the New York Fed. Whatever they were doing, it didn't have any relevance to their President when they went to a meeting. So, I think that that changed in the few decades after that.

Our focus in St. Louis was much more on trying to be useful to the President in the decisions he had to make. And we would sit around and say, "He has to go down there every few weeks and at the end of the day he has to hold up his hand and vote. So, what have we got to say that's useful to him in doing that?" And I think that that was rare in the Reserve banks, and probably mostly nonexistent at the Board of Governors. I think most Governors went into the FOMC meeting with virtually no input at all from the economists that worked at the Board of Governors other than whether they had read the blue book, green book, and any briefing papers for the meeting, but without any preparation. So, I'm sure that we were sending our President off much better prepared to engage in the important decision that had to be voted on, than just about anybody else.

I think because the competition among peers, that over the subsequent years, the other Reserve bank Presidents and their successors wanted to build up a staff that was able to help prepare them to also sit at the table and engage in a serious way as a policymaker, and with varying success some of the Governors also sought to get independent input to help better prepare them to sit at the table and to engage. And I suspect before the late 1960s, very few of the Presidents and the Governors actually went to an FOMC meeting prepared to engage in a conversation among economists about what is the best policy or not. They'd have a casual discussion about God knows what, but it had little economic underpinnings to it.

[Part 2, 00:19:21]

Rasche: Follow up question, which you can duck if you want. To what you just said, a number, a considerable number of participants around the table have been professional economists by training in the last couple of decades. But a bunch of, but others have not. Do you think it's necessary to have sophisticated — or sophisticated may be the wrong word — a substantial knowledge of economic theory and the economics literature in order to adequately participate in those discussions?

Jordan: No, and I think St. Louis demonstrated that in that both Darryl Francis and his two predecessors that I know had some abilities with the jargon of economics, but were not really trained researchers. They were not academics themselves. So, they weren't former professors or anything like that. But they were able to absorb what the staff was able to tell them and use it.

And most importantly, Darryl Francis' immediate successors were not trained in economics, and yet they were able to, after a short time at the Bank, with regular interaction with the staff, with the culture that was here at the time, they were able to become adequately prepared, in both the jargon of economics and economic theory and evidence, that they were able to perform at the FOMC much more strongly than they would have been without the staff input to help prepare them. When you have a Reserve bank President that really has maybe never even had a class in undergraduate economics within a year of becoming President to basically have been trained and educated as an economist able to perform shows that it's not necessary for the principals — the Presidents and Governors themselves — to have a strong background, as long as they have access to good economists on a frequent basis to help prepare them.

Wheelock: I could turn back to the 1970s for a moment, thinking about the oil embargo and oil shock. Early '70s, there was apparently we now know a substantial slowdown in productivity growth occurring early in the '70s perhaps because of oil shocks. It's been argued that one reason why the monetary policy ended up being excessively stimulative in the '70s was that the Fed was slow to pick up on the fact that productivity had slowed, and the potential output growth had slowed. Was there a discussion about the impact here of the oil shocks on potential growth, on productivity growth, and how that might affect optimal monetary policy in the '70s?

[Part 2, 00:22:40]

Jordan: Yeah, it was an attempt to cast the dialogue in a different context in that changes in international terms of trade between countries that produce and export oil out of the middle east or elsewhere, and those of us who were primarily importers of oil who pay for the oil by exporting wheat or other commodities or goods, that the change of the terms of trade amounted to a redistribution of wealth. In a permanent income kind of hypothesis sense, it wasn't an all at once, one-time big transfer from the US to OPEC, but rather something that was manifested and distributed over a substantial length of

time in the form of what we measured as being lower productivity growth, but lower growth in per capita, real standards of living.

These things are subject to fairly long lags, and so this Bank wrote about, talked about these measures of productivity reflecting a fundamental underlying phenomenon that could be analyzed, but the data didn't reveal it without digging a little bit deeper as to what was going on. But I think a part of that was also an intellectual dispute in that many of the people that advocated more expansionary monetary policies were still wedded to the idea of what we call the Phillips curve — the tradeoff that they perceive between inflation and unemployment — and the unemployment rate was high or rising, and they thought that the way to get the unemployment rate down was more expansionary monetary policy to gin up aggregate demand, to create opportunities for employment and cause lower unemployment.

Many of them advocated and apparently believed that inflation would only be triggered and move to a higher level after some threshold of low unemployment, or rapid employment growth would occur. Even in the late '70s, there were prominent political leaders — and maybe their economic advisors — that were saying that excess demand could not cause inflation because the unemployment rate was still above the non-inflation accelerating rate. There was a different name for it that Modigliani gave to it before NIRU, it was simpler than that. So, I think it was a debate within the economics profession, and that then carried over and became a debate within the policy circles, both monetary and fiscal, as to whether we needed to worry about rapid money growth.

1977-78 saw extremely rapid monetary growth and St. Louis was very vocal, outspoken in talking about the inflation threat that came from this excessive monetary growth. Many other people were discounting or outright dismissing that on the grounds that the unemployment rate was also high, and they didn't believe that it was possible to have fast and rising inflation while the unemployment rate was still well above anybody's idea of full employment or even an acceptable level of unemployment.

Wheelock: So, if I understand what you're saying, you would argue that the inflation of the '70s arose because of bad economics within the system, rather than say political pressure on the Fed from Congress or the administration, or simply a view that we prefer lower unemployment rather than, you know, we're willing to accept higher inflation. So, it was just a misunderstanding of the dynamics of inflation and so forth?

Jordan: I think it was mostly a misunderstanding of the dynamics of inflation. I don't know to what extent that there would have been pressures at work in that '77-78 period. But I think that both the administration and the Treasury department firmly believed that inflation would not become a problem until unemployment declined from where it was at the time they came into office. And those people that were pointing to the rapid monetary growth as being a risk of higher inflation were a distinct minority.

[Part 2, 00:27:45]

Wheelock: So, by the end of the '70s and into the early '80s, Chairman Volcker came in and changed the policy, instituted the very tight monetary policy to combat inflation. We saw the beginning of the deviation of velocity, monetary velocity, which up to that point had seemed to be quite stable, and hence, useful for employing a monetarist understanding of inflation. Of course, you were no longer in St. Louis at that time. But how did the Volcker inflation, and more specifically the subsequent realization that velocity was deviating from, or apparently deviating from long run behaviors? How do you see that as affecting the long run of legacy of monetarism and of the work done here in St. Louis when you were here?

Jordan: Well, I think that we have to start back in the earlier '70s, in the immediate aftermath of the wage-price control program breaking down. Especially with the inflation in the '73-74 period, the argument about how much of that was caused by the prior excessive monetary growth — and therefore it was a domestically generated inflation — and how much of it was the international — the consequences of the real effects, the shock effects of curtailment of oil supplies and the shift to higher price level. That debate was very useful, not only within the economics profession but then to a broader audience, in talking about what were the causes of sustained inflation over a longer period of time.

So, a lot more attention started to be paid to money supply measured in different ways and the monetary base, and what the open market committee could really control — the monetary base as opposed to interest rates or other things that we were doing. Because we had some allies not only in the academic profession, but some who were working in and around government, including staffs in Congress that were paying attention, it gave rise to a desire to at least constrain the growth of the money supply, if not outright target it. So, the Congressional committees — after a number of hearings — finally passed a resolution instructing the open market committee to at least have targets for the money supply, and to periodically come to Congress and explain if the growth of the money supply was outside those targeted ranges.

And that, I think, came directly from the work that had been done at this Bank a few years earlier, and some of the people that had followed our work were in influential positions in Congress to help organize and conduct those hearings. That was not an entirely successful experience because of the practice of the FOMC to shift the base for their monetary targets at every meeting, to shift around as to which money supply was most important. One time, I think they added 16 different measures of money that they claimed to be targeting simultaneously, and they could always find one or two or more that were within the target bands even if the others were out.

That legislation— those resolutions then became legislation mandating more attention to the growth of the money supply. But unfortunately, it was combined with another political agenda of simultaneously targeting unemployment. And so that was the origins of the so-called dual mandate — that the Federal Reserve, in conducting monetary

policy or formulating monetary policy — was to pay attention to both the unemployment rate and the inflation rate. And because monetary policy works with a much longer lag, and an uncertain lag, on inflation, than it does on real output or employment and unemployment, it became really a mandate that was impossible to fulfill within a timeframe that any politician cared about.

The behavior of both money multipliers and money velocity on economic activity was being influenced by the real shock effects internationally. It was being influenced by the inflation and the distortions that was there, and then especially by the [. . .] legislation in 1980 and again in 1982, the Monetary Control [. . .] and Depository Deregulation [Act].

[Part 2, 00:33,21]

What had happened is— we wrote about this in the early '70s of the problems that were coming, that the interest rates offered by savings and loan associations and particular credit unions had been controlled by law — and at one point they gave the thrift institution a quarter point advantage over the commercial banks in order to attract deposits. But inflation was causing the whole thing to become unsustainable because of, simply, market interest rates were so much above these controlled interest rates.

We had contributed, if not actually coined, the ugly word of 'disintermediation' — about the problem of people taking their money out of the bank and putting it someplace where they can get a higher interest rate to protect them against inflation. So, by late 1979, early 1980, Congress was getting more and more anxious to deal with what they perceived to be a lot of problems out in the economy at the same time — high inflation, high unemployment, very high interest rates, a very adverse environment for home construction or other interest rate sensitive sectors.

So, there was both— there was legislation coming through both the Senate and the House of Representatives at the same time. One was focused on fixing the problems of the distortions in the depository institutions by deregulating the kind of instruments that could be offered, [especially] the interest rates that could be paid, and the other legislation on monetary control. And these [two bills] actually had very little to do with each other. But one was in the House and one was in the Senate, and neither was holding hearings on the legislation coming through the other branch of the Congress. So, because of the crisis kind of atmosphere that we found ourselves in — with Volcker having come in as Chairman of the Fed and deciding to target bank reserves and let interest rates go to whatever level would clear the market — Congress wanted to be a part of the solution and they wanted to vote to pass something. But they didn't have legislation that had passed both houses of congress. So, they simply slapped the two halves together, sent it to a joint conference committee as a single bill, and voted to pass it, even though hearings had never been held on the entire legislation, and one

[body] had not even read the legislation that had come through hearings on the other side of Congress, and it took them several years to figure out how it even worked.

It was one of those sort of historical accidents in politics, that without having carefully studied all of the political dynamics of the time — and coming with the reelection campaign of President Carter, and the challenge by Ronald Reagan — it would be hard to understand how and why Congress could slap together and pass pieces of legislation that no one had read or no one had understood. Well, now we see them pass a lot of legislation that no one has read and no one understands, but back then it was very unusual for the Senate to agree to something that the House had passed, but they had never even held hearings on it, and vice versa.

[The legislation] was [. . .] distorting in what it did to reserve requirements, what it did to authorized types of deposits, what it did to interest rates that were permitted to be offered on instruments by the different type institutions, [all] stretched out over a six year period of time for full implementation. It had enormous implications for the channels through which monetary policy worked through the banking industry.

And I think even within the Federal Reserve, it probably took most of the '80s to start to really understand the new framework of the financial system and how their policy actions work. By the 1990s, we were sufficiently far into the long term, sustained disinflationary process. [The mission was] to restore the idea that increases in inflation and interest rates are transitory, they will go down, and to counter the prior regime that prevailed by the end of the '70s, that declines of inflation and interest rates were temporary. Most people were acting on the belief they'd go back up. We had to defeat that idea. Probably Volcker's eight years [as] Chairman, followed by the first few years of Greenspan at the end of the '80s and in the early '90s, had gone most of the way of reversing the way the American people thought about the inflationary process. [Sometime in the 1990s, people] came once again to believe — like they had in the '50s and the early '60s — that low inflation not only could be achieved, but it could be maintained by good economic policies. It took us, I would say, the better part of two decades to undo the damage that had been done in the late '60s and through the decade of the '70s. So, the whole period from '64-65 to 1980 was just very, very bad economic policies pursued for much too long, and it took us until sometime in the 1990s to correct that.

[Part 2, 00:39:02]

Wheelock: Are you optimistic now that we have conquered inflation permanently, that we've learned the lessons of the '60s and '70s?

Jordan: We've learned absolutely no lessons. We're screwed.

Rasche: We've sort of pestered you with a whole bunch of thoughts that occurred to us about St. Louis in the '60s and '70s, but we probably don't see the full picture. So, is there

anything reflecting on what we've asked and what you've said that you think you would like to add, that we've omitted?

Jordan: No, I guess, sort of to summarize the intellectual environment, I think that the willingness to be open-minded — both on economic theory and then to resort to empirical evidence, to test rival theories — was fairly unique and fundamental. It really was a search for what worked, and a disinclination to accept the inherited wisdom that came before, a willingness to challenge the views of others, and to allow our views to be exposed to the challenge of others in I believe an honest, open effort to improve our understanding of the way monetary and fiscal policy actions influence economic activity, and a willingness to be wrong if that's the way it turned out.

And whether there's any long-term legacy from that, I think it's too soon to judge, especially in view of the fiscal policies that have been pursued in just the last few years, and whether monetary policies, independent of that fiscal environment, can maintain both a low inflation regime and at the same time prosperity and rising standards of living. If I were doing research again now, I would be willing to test those propositions. I would be open to the possibility of being very wrong about fiscal dominating monetary. I would be willing to look at evidence to the contrary that says that monetary policy can facilitate a non-inflationary environment in the face of the fiscal imbalances, and at the same time foster rising standards of living and prosperity. But I'm a skeptic. I would have to— it would have to be demonstrated by evidence and experience.

Wheelock: Julie or Bob, additional questions for Jerry? We certainly appreciate your willingness to come to St. Louis and visit with us today.

Jordan: My pleasure.

Wheelock: We've had an excellent session and look forward to reviewing the tape and making good use of it. So, thanks again.

Jordan: I'm sure the centennial will go very well for the whole system.

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