Federal Reserve Board Oral History Project

Interview with

Jared Enzler

Former Associate Director, Division of Research and Statistics

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Interviewers: David H. Small, Janice Shack-Marquez, Jaime Marquez, and Robert Drzyzgula

Federal Reserve Board Oral History Project

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

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

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

Contents

MR. SMALL. Today is Tuesday, June 26, 2013. This interview is part of the Oral History Project of the Board of Governors of the Federal Reserve System. Today we are interviewing Jerry Enzler, who is a retired Board employee. I am David Small from the FOMC (Federal Open Market Committee) Secretariat in the Board's Division of Monetary Affairs. I am joined by Janice Shack-Marquez, a deputy director in the Division of Research and Statistics (R&S), Jaime Marquez, a senior economist in the Division of International Finance, and Bob Drzyzgula, lead research systems analyst, also from R&S. This interview is taking place at the Board.

Education and Professional Background: Positions at the Board

MR. SMALL. Maybe you could start and tell us a little bit about your upbringing, your academic background, your education, what skills you brought to the Board on day one.

MR. ENZLER. Well, I'm a farm boy from the State of Washington. I grew up in a small town there. I went to Washington State University, where I started as an engineer but finished in economics. Then I went to the University of Pennsylvania, where I got a master's degree in economics. I went through prelims and started a dissertation but never finished it, so I'm not a Ph.D. By the time I got to the University of Pennsylvania, they were beginning to use computers, and so I gained some familiarity with them during my time there. Probably that's all you want to know, because the next thing [that] happened is, I came to the Board.

MS. SHACK-MARQUEZ. What year was that?

MR. ENZLER. I worked as a summer student at the Board in 1963. I liked the place very much, and so when it came time to be looking for work, I was much interested in going back there. And then I joined the staff in the fall of 1966.

MR. SMALL. What section and into what position did you come?

MR. ENZLER. I went into what in those days was called the Business Conditions Section. It was the section that handled the Indexes of Industrial Production and Capacity Utilization and things of that sort. My first task was to put out the capacity utilization statistics. I did that for a year or so.

MR. SMALL. Did you know the importance of that statistic at the time? There's a lot of debate now about full employment and potential output, but, at the time, that was one of the few measures around and an important measure of economic activity.

MR. ENZLER. There weren't many in those days. Manufacturing capacity utilization—that's how the Board got into it. The concept that we used had been developed by Frank de Leeuw, who was the only modern economist in the Division of Research and Statistics in those early days that I was aware of. There were other young ones who I was not aware of, because the Business Conditions Section was physically isolated in the FDIC (Federal Deposit Insurance Corporation) building near the White House.

What the Board did in those days was not so much forecasting and then making policy based on the Greenbook analysis. The Greenbook only went ahead one or two quarters, and so the forecast was largely just an embellishment on current conditions. But the Board members were interested in current conditions, and so they were very interested in the capacity utilization concept.

MR. SMALL. So capacity utilization was industrial capacity, not labor force utilization?

MR. ENZLER. Yes, that's right. All I was doing was a capacity utilization statistic that was completely consistent with the manufacturing portion of the Industrial Production Index. It didn't go beyond that. It had no relation to labor conditions.

Evolution of Staff Analysis

MR. SMALL. Do you know if, at the Board, there were a lot [of] notions of full employment or potential output off [of] which you'd base other utilizations?

MR. ENZLER. Of course, we were all aware of the unemployment rate. And there was interest in capital utilization. That is how de Leeuw got tasked with developing the Manufacturing Capacity Utilization Index. But I don't think there was much consideration of just how the two utilization rates were connected to inflation. They just thought that if either of the two utilization rates was too high, it would cause inflation to break out. But they also worried about it breaking out due to considerations other than utilization rates and over which they had no control. So they wanted to keep the utilization rates moderate. I don't think the Board had much insight into where or how much inflation would eventuate from too-high values of the two utilization rates. And [there was] very little belief at all that policy actions had inflationary consequences if they could keep the utilization rates below their upper limits.

I remember being told by one officer that all effects of monetary policy were worked through the economic system in six months. There was no need to forecast further out.

MS. SHACK-MARQUEZ. And when did the change in perspective occur?

MR. ENZLER. It took a long time, I'll tell you—many, many years. Gradually, the Board staff began to forecast further and further ahead. And, of course, the further ahead you forecast, the more inflation begins to show up, because the policy things you do now do have an effect on inflation, but it's usually some years down the road. But the first three or four years I was there, there was just almost no discussion of that.

The staff in those very early days had what they called a "stage of processing" view of inflation. Basically, what they did was to look at things like commodity prices, and if

commodity prices were rising, they realized that the increase was going to affect future inflation. They spent a lot of time worrying about that. And, indeed, you can predict a lot of future inflation with commodity prices, and it is useful information for short-term price forecasting, either in a model or nonmodel forecast. But the real causes of longer-term inflation are much deeper.

At first it seemed like the staff was rather indifferent to the current rate of inflation, or there wasn't anything they could do about it, so they spent their time trying to judge what current conditions were and modify current conditions to get them in line with what they wanted to get current conditions to be. But they really didn't think about the future inflation rate as being much of a consequence of that. And that, in my opinion, is what got us in trouble during that period. They were focusing on getting output to where they wanted it, and they just did not really understand the longer-run inflationary consequences of the policy actions that might result.

When overheated conditions due to the Vietnam War resulted in rising inflation, then staff came around to a modern view of the matter rather quickly. The Board took much longer. It caused some stress between the Board and the staff. It was resolved when Paul Volcker ordered an elaborate presentation to the Board about their views on inflation and its causes. That was done in the early 1980s. The main presenter was Dave Stockton, and he did a really remarkable job. From the questions we got at the end of the meeting, I thought the presentation converted the whole Board. They realized they were going to have to hold the unemployment rate far above normal for an extended period of time to wring the high ongoing inflation rates out of the economy. Just returning output conditions to normal would not do.

MR. SMALL. Do you think that the modeling effort had an important role in bringing the staff up to speed, in that if you were going to have a model that endogenously determined

output and inflation, you were forced to try to model the linkage and the structures that tied those things together.

MR. ENZLER. We certainly did model it. At the beginning, we didn't quite have it right either. We did, in fact, have a nonvanishing Phillips curve in there in the first three or four years I was running it.

But, yes, I think the model played a role in getting the staff to look at it properly. But so did the rapid flow of newly minted Ph.D. economists to the Research Division staff. That also had a lot to do with it. The change would have happened anyway, but I think the model accelerated it some.

Evolution of Economic Modeling and Forecasting at the Board in the 1960s and 1970s

MS. SHACK-MARQUEZ. Would you be willing to tell us the story of the model as you understand it? Like, how did we get into this business? Who was involved in it? What was it like?

MR. ENZLER. In 1965, there were a series of papers written on policy instrument alternatives by Albert Ando and Franco Modigliani and by Milton Friedman and David Meiselman that came to be known as the "AM–FM debate" because of their initials.[1]

The Board in those days had academic consultants meetings in which all those characters were regular participants. The Board had an interest in this, because that debate had to do [with] whether monetary or fiscal policy was the most important in determining output, with Ando and Modigliani being on the side of fiscal policy and Friedman and Meiselman being on the side of monetary policy.

[1] The debate was whether an appropriate model for the U.S. economy should be based on the Keynesian investment multiplier or the quantity theory of money.

In the course of that debate, Ando and Modigliani suggested that they could probably build an econometric model to demonstrate their view, and that they thought it would be of interest to the Board. The Board indeed was interested and financed it through the Social Science Research Council, as I remember. That's where the initials MPS come from: it's "M" for MIT where Modigliani taught, "P" for Ando at University of Pennsylvania, and "S" for the SSRC. And so construction began on it, I would guess, in 1967. I was not in on the very early stages.

I got recruited into that in 1968, when the Special Studies Section was formed at the Board and took over as the main site for the development of the model. Frank de Leeuw was the section chief, and Ned Gramlich, who you know in another guise as a Governor, was one of the key players. Those two fellows, plus Ando and Modigliani, were the ones who really developed the model. Most of that work went on in 1967 and 1968.

MS. SHACK-MARQUEZ. That's awesome. And, just morbid curiosity, was it all in FORTRAN?

MR. ENZLER. Yes, it was. The original code was written by Bob Rasche, who was at the University of Pennsylvania and later went to the St. Louis Fed. And then, when the main development site shifted to the Board, I became the code writer, because I already had FORTAN programming skills, and that's how I got into the model. At first, I was just writing the FORTRAN code.

MR. SMALL. Was it on punch cards?

MR. ENZLER. Yes. In those days, it was. In fact, however, because the Board's computer system was really just getting under way, we got time on a commercial computer that was located up on K Street. It was a Control Data computer. Yes, indeed. We did it all on

punch cards, but, of course, at that machine. I could actually run the punch card machine, and so I ran the thing and took the stuff off the printer and so on. The machine was lightning fast. The Board's machine at no stage ever was anything that you would describe as lightning fast.

MS. SHACK-MARQUEZ. So you would do the punch cards and then have to go up to K Street to run your programs?

MR. ENZLER. We did the punch cards up there, too. I just camped out there.

MS. SHACK-MARQUEZ. So you just lived up at K Street? I'm just trying to picture what the whole thing looked like, in terms of how you got that to work.

MR. SMALL. What was the effort required to update a simulation, in terms of getting the new updated data, punching it in, getting the computer to run, and churn it out?

MR. ENZLER. You're talking about in the development stage or after we got going?

MR. SMALL. In the production stage. Say you moved ahead a quarter and said, "Well, we need a new simulation for this FOMC meeting."

MR. ENZLER. Oh, yes. That was not difficult at all. Your questions suggest that you think it's quite a bit more difficult than it was. No, it was not difficult. And once you got it to run on the machine, there was really no problem. The problem was getting access to the machine. In the early days, you would put a run in, and you would go back the next morning and pick it up. And that's not exactly the way you want to proceed. And even as the years went by, we always had trouble getting the runs done quickly enough. But once they got on the machine, the machine was capable of handling them quickly.

At one point, I know, you suggested that you'd heard stories that we could only run one quarter ahead or something. That's not true. It never took an amount of time to do a simulation

on the mainframe that was of any consequence at all. The problem was getting the mainframe to do the darn thing. Too many user competitors.

MR. SMALL. How did you intersect with Greenbook forecasting?

MR. ENZLER. We would bring a model forecast to each Greenbook meeting, and that model forecast was our own forecast. It bears some relation to the staff forecast, because we talked with their people, particularly about variables that were exogenous to the model and so on. But we always brought a model forecast in there. And those things were always discussed seriously, because they wanted to know what the model had to say.

However, as a forecasting device, a model by itself is not really adequate for making a forecast. There are always just hundreds of idiosyncratic things going on at any particular period of time that have an effect on the forecast. And we would have no possibility of matching the accuracy that would come from out of the Greenbook forecast for the first quarter or two ahead.

Where the model began to come into play was, once you got out beyond the near term, the model had a way of holding the world together and holding it together in a way that was consistent with the major behavioral relationships. That was much harder for the Greenbook people to do, because with the Greenbook forecast, you had one guy that did consumption, another guy that did inventories, and so on. And it's always a challenge to make sure that they're all using the same inputs. And so people paid a lot more attention to the model once you started talking three or four quarters ahead than they did for the first two quarters. We really couldn't compete on those first couple of quarters.

After the Greenbook meeting Cort Peret, one of the officers in R&S, would combine everything into what we called the "judgmental forecast." Then we would adjust the model in such a way that it reproduced the judgmental forecast exactly. And, finally, we would run policy

alternatives. We might have based the original judgmental forecast on, say, 4 percent money growth, and then we would use the model's multipliers to see what would happen if you had 5 percent.

MR. SMALL. So you were the father of add factors?

MR. ENZLER. Well, I didn't invent them, but I'll guarantee you, they were used in every model in those days. And yes, indeed, the add factors are a very big part of it. Even when we did the so-called model forecast, there were always things out there that you saw going on. You've got to do something about them. You can't just let the model generate its own forecast. And I will repeat: Using the model as a pure forecasting device, it's really not up to the job, or—it's not really up to the job for the generation of models that we're talking about. It requires add factoring. It just can't be done otherwise.

MR. SMALL. When you strung the forecast out a couple [of] quarters, how did you model monetary policy?

MR. ENZLER. The only alternative we had in those days was that we always did the policy forecasts using money growth as the policy instrument, because it was not explosive in the model. We couldn't use interest rates as the policy instrument. If we did, the model tended to be explosive. If the policy rate was set too low, output growth would push down unemployment, which would, in turn, increase the inflation rate. That, in turn, would reduce real interest rates, causing things to go even faster. Soon you got hyperinflation. If the initial policy rate was too high, the opposite happened.

But the model was stable if we used money growth as the policy instrument. In that case, if the rate was set too high, the economy started growing too fast, which would drive up interest

rates, and that would start to slow things down, and so it's not explosive. So we always used money growth as the monetary policy variable.

MS. SHACK-MARQUEZ. It's the late 1960s, and you're camped out at K Street, running the model. How were the Greenbook forecasters doing?

MR. ENZLER. First, let me say, we moved the model back from K Street when we came to the end of the development period. I believe that the very first forecast I did was on the K Street computer, but then we moved it back to the Board.[2]

MS. SHACK-MARQUEZ. What I was trying to figure out is, you got this big model that now I understand you've developed at K Street. You've migrated it to our dinosaur mainframe here at the Board. Are the Greenbook forecasters working on the Board mainframe at this point?

MR. ENZLER. At the beginning, they're not. Cort Peret played the role of the model in the Greenbook forecast, or what we called the judgmental forecast. You would have specialists that worked on all parts of the judgmental forecast, as we talked about before. Somebody would be working on consumption. Somebody would be working on cars. Somebody would be working on inventories and so on. And then they would all send their results to Cort. And Cort would add up the GNP, and the wages, prices, and productivity people would have given him forecasts of prices, wages, and unemployment and so on. Cort tried to integrate all that stuff. In the early days, he did it with one of those old junkity-junk, you know, calculators that we used to have on our desks in those days, not on the computer. After a few years, that did migrate to the computer, but Cort still put all the judgment in at that point. All the computer did was just execute the identities. As far as I know, it didn't do anything beyond that.

[2] See Flint Brayton, Andrew Levin, Ralph Tryon, and John C. Williams (1997), "The Evolution of Macro Models at the Board," *Carnegie–Rochester Conference Series on Public Policy,* vol. 47 (December), pp. 43–81.

MS. SHACK-MARQUEZ. And so the people doing the sectoral analysis, like the labor person, they were just using their instincts to think about their forecasts?

MR. ENZLER. Yes. That, of course, is the problem with a judgmental forecast. You can't be sure that all those specialists are on the same wavelength when they start. And, yes indeed, that's exactly the way it worked. Each one used his own instincts. And then Cort would try to integrate this stuff. And then part of what was done at the staff meetings was to say, "Well, Cort, I don't think you integrated this quite right. Let's do something else." But, yes, that's exactly how it worked. Each of those specialists did a forecast on his own, and Cort banged it all together. And then the staff meeting was to do the final reconciliation.

MS. SHACK-MARQUEZ. So that was the GNP—was that still called the GNP meeting?

MR. ENZLER. Yes. It was still called the GNP meeting.

The Rise in Inflation in the Late 1960s and 1970s

MR. SMALL. You come to the Board in the mid–1960s as a young man. Inflation is starting to rise. There's the Vietnam War. There's the Great Society. There's all kinds of things going on. But inflation is getting to postwar historic highs and getting pretty sustained rather than just a temporary spike. What was the model saying about the causes of that? What were some of the debates between the Greenbook people and the model people about the causes?

MR. ENZLER. I don't think there was much debate. To the extent that inflation was increasing, at the staff level, we were all well aware that this was due to the economy being overheated. And that, in turn, was because of the Vietnam War. There really wasn't all [that] much difference between us and them in that regard. We had a little bit different outlook, as I've already described, on what the causes of the inflation were, because we did tend to look at things

like Phillips curves more than they did, whereas they tended to look much more at things like floods and droughts and weather. And all kinds of things got into their analysis that didn't get into ours.

But there wasn't a huge debate about inflation up until the Nixon price controls.[3] At that point, everybody was confused. And I don't know what you can say about it. You see, once you put price controls into effect, the model's wage and price equations no longer have a great deal of meaning. They generate a number, but that's not what comes out, because the wage and price controls dictate otherwise. And so we went through two years there in which it was very difficult. The price and wage forecasting was very difficult. And there wasn't much inflation because of the price and wage controls.

Now, I can remember that the price equation in the model at that point was showing that the price controls must be having quite a sizable effect. Prices were coming in much lower than the equation predicted. And I can remember writing a memorandum to Charles Partee—he wasn't a Governor then, he was the managing director for research and economic policy—saying that when these price controls are released, it looks like we're going to get a bout of inflation that might be 6 percent or so, which in those days was considered a lot of inflation. And he took that under advisement, but I remember that he wrote me back, saying something like, "Yes, you might be correct, but I'm not going to put that in a forecast to the Board."

So I just offer that. We suspected that there would be quite a bit of inflation. And as a matter of fact, when the price controls were removed, we did indeed get a lot of inflation. At that point, it became perfectly clear in the data that the argument between vanishing and nonvanishing Phillips curves was over.

[3] The Nixon wage and price controls were in effect, in various forms, from August 1971 to April 1974.

Before the run-up in inflation, I had started estimating Phillips curves that were structured just slightly differently. They used a longer distributed lag on prices in the wage equation. And I found that the data were quite consistent with the nonvanishing Phillips curve if you used that structure. And so when the price controls were released, I immediately switched over to these equations and never changed again. The debate was over.

MR. SMALL. This was the old debate, whether the coefficients summed to 1 or not?

MR. ENZLER. Yes, that's exactly right. As a matter of fact, I might say, Ando and Modigliani both thought that there was room for a long-run tradeoff there. I never did, but that caused me to have some new equations all ready to go so that when we got the first solid evidence that, indeed, the Phillips curve was of the vanishing variety, I was all primed and ready to go. Ando and Modigliani by then had accepted the idea.

I don't think the fact that the model was late in converting to a Friedman–Phelps type [of] Phillips curve had anything to do with the mistakes in policy that the Board made during that period.[4] Even if I'd had the thing right, they were just off in some other world where things like inflation didn't matter very much. And they weren't doing their forecasts far enough ahead for them to have much of any consequence anyway. You know, it takes inflation a while to develop under a Phillips curve, but the way the Board looked at it, where it was due mostly to exogenous factors that were pushing it this way and that way, they just had a tendency to try to control those variables they could control, which was output, and to ignore variables which, by the time anything happened, they might not have to do anything anyway. So they would just mostly ignore the rate of inflation.

[4] See Milton Friedman (1968), "The Role of Monetary Policy," *American Economic Review,* vol. 68 (March), pp. 1–17; and Edmund S. Phelps (1967), "Phillips Curves, Expectations of Inflation and Optimal Unemployment over Time," *Economica,* vol. 34 (August), pp. 254–81.

MR. SMALL. So, one fairly common story is the 1968 LBJ temporary tax increase. The model said it would not significantly strain aggregate demand, and, therefore, the Fed could be a little looser with monetary policy. But then Friedman comes along and says, "Gee, you modelers don't understand the difference between temporary and permanent policy changes."

MR. ENZLER. I am only sort of vaguely aware of that incident. It happened before I joined the model staff and certainly before the model went into production mode. I asked Frank de Leeuw about it years later when he visited the Board, and he doesn't know just what it refers to. He said he did some runs of the consumption function for Dan Brill, a senior adviser to the Board, at about that time, and maybe the episode arose from that. It is possible. Models can be wrong.

By the way, Frank may still be alive. You might want to see if you can contact him. Do you know him?

MR. SMALL. No.

MR. ENZLER. Well, the last I knew, I was able to look him up. The name is de Leeuw, D-E, and then L-E-E-U-W. And he was a considerable power in those days, and the leader of the model group. And he was the only one who could conceivably be involved in such a thing, and he said, "No." He could never relate to that episode. He didn't know what they were talking about.

MR. SMALL. And what about the 1968 Friedman–Phelps natural rate or the 1972 Lucas expectations and the neutrality of money? Did the modeling group try to take those on?

MR. ENZLER. The Friedman–Phelps natural rate we did accept, though not until 1974, as I explained earlier. I know that rational expectations (a Lucas version) appeared in 1972.[5] The model did not have any rational expectations in it, and we did not incorporate it at that point.

We were not exactly hostile to the notion of rational expectations. I mean, one can't even hear the rational expectation story without knowing there's some considerable truth to it. But it was very difficult to incorporate such a thing. And, in fact, we never did. And when the model died in 1988 or so, we still hadn't modified it so that it incorporated any rational expectations notions.

I realized in those last years we were going to have to do that, but it's such a massive undertaking that I thought we would have to expand the model staff for a while, much like was done when it was developed. And so I thought, well, in a couple years, we'll do that. But we never really did. After I left, a different rational expectations model replaced the MPS.

MR. SMALL. The problem was more of a technical one, just solving the model under rational expectations?

MR. ENZLER. Well, first, there's a technical problem, yes. Solving a model under rational expectations is not a trivial matter. But, second, there was certainly a belief among those of us who were running the model that it probably, for most of the things we did, was not something you had to worry unduly about. Some economic agents formed expectations rationally; most did not.

Now, I certainly wouldn't say that about the world today. Today the sectors in which rational expectations are definitely important, like the financial sector, have become giant things,

[5] Robert E. Lucas, Jr. (1972), "Expectations and the Neutrality of Money," *Journal of Economic Theory,* vol. 4 (April), pp. 103–24.

much bigger than they were in those days. And so I certainly wouldn't advocate anybody building a model today that wasn't fully consistent with rational expectations.

We thought it really was not worth the effort and wouldn't be for some time. And I don't really think that's wrong, but I imagine that most people do think it was wrong.

MR. SMALL. Did you get to a point in the mid- to late 1970s where you said, "My goodness, inflation expectations have become unhinged now"?

MR. ENZLER. The Phillips curves that we used worked reasonably well all through that period. We did not have much trouble with those equations. We did have trouble in another sense. We always used money growth as the policy variable in the model for reasons I explained earlier. But the exogenous money growth forecasts that we put in there were almost always too low. I mean, money growth was not the variable of interest [that the] Board and staff wanted to concentrate on. What they would do was, they had an idea for the path of interest rates they wanted to use in creating the forecast. Then they first would ask us to simulate how much money growth would result from that. But then, when we would ask what money growth to use in the forecast, they would give us lower money growth than we had told them to expect. That went on forecast after forecast. So during that period, we were generally using money growth rates that turned out to be, in retrospect, much too low. We complained about that a great deal, but to no avail.

So, yes indeed, that did cause us to underestimate both output and inflation in the 1970s. But had we used the right policy variables to begin with, we wouldn't have. Doug Battenberg, another economist in the group, and I went back and worked very hard on that question. And if you went back and put in the historical money growth numbers—this is after the fact—there was no consistent tendency for the model to underestimate output or to underestimate either output or

inflation. But given the inputs that we kept getting for monetary policy, we did indeed make mistakes of the sort that you're describing quite consistently during that period.

Resources for Board Computing

MR. SMALL. I'd like to push on a little bit through time, but before we go further through the timeline, were the computer people just geniuses and delivered to you all the computing power that you needed? Or how did you push, or what was delivered to you, or how did you request more? How did your resource needs with computer power and programmers develop over this period?

MR. ENZLER. Okay. We're off on a little different subject now. The computer was an enormous frustration all during the period through 1987 when I left. And I might say, if you will allow me to get off on a tangent here a little bit—but I think you'll see why in a minute—the computer was eventually developed at the Board for things like processing the money supply statistics. That's why the division that ran it was called it the Division of Data Processing. It wasn't the Division of Information Technology or something like it might be today.

And they saw themselves as running a sawmill down there where you push logs in one end and something comes out the other. They got data from the District Banks, and they pushed it in one end, and out came well-formatted monetary statistics at the other end, making armies of statistical clerks unnecessary. And you ran those programs over and over and over for years and years and years. The ratio of programming to execution was very low. There was a lot of execution, but very little development.

Research work is the other way around. We do all development, or almost all development, and then the production period is very short. Whatever you get, you run it once or twice, and that's the end of it. It gets put in a drawer and put away. So the computer system was

designed all wrong for us. And we fought for years and years with all kinds of different strategies, never got anywhere—until the events that I'm sure we'll come to in a minute.

MR. SMALL. It was designed wrong because you had to submit one job in punch cards and then get back at the end of the queue and put in another stack—or, what way was it?

MR. ENZLER. It wasn't the punch cards. The punch cards were eventually replaced by time-sharing terminals. But the problem remained. I mean, you would sit there at the computer terminal, and you would type up something, and you would hit the enter key, and then you waited. And you waited. And you waited. And maybe 45 seconds later, something would happen. Then you push in another line, and you waited. And you waited. And you waited.

So while a problem seemed solved in principle, it wasn't really solved in practice. We had people just sitting at computer terminals for hours and hours and hours. And it slowed down everything we did to just a crawl. Very frustrating system. And it was that sort of system, of course, that we later tried to combat.

MR. DRZYZGULA. I recall listening to you talk about your frustrations, largely rooted in the planning regime they had for the mainframe, and the budgeting system, with it being below the line and making it difficult for the Data Processing Division to accurately project demand for the equipment. And, as a result, it was underconfigured.

MR. ENZLER. Yes. I've got a quite a number of things to say about that.

MS. SHACK-MARQUEZ. Well, why don't you go for it?

MR. ENZLER. Let me describe to you how they did it. And, yes, the billing system was a big part of it. The system worked a bit like Medicare does these days. Under Medicare, neither a doctor nor patient cares how much something costs. And with that, demand explodes.

Many unnecessary medical procedures get done. And that's what kept happening to our mainframe computer.

Our attempted use of the thing kept growing and growing. There was nothing to hold the section chiefs to consider [the] computer costs of what they were doing. And, as a result, demand grew without limit. And one result of that was that about every so often, the Board would say, "We're not going to pay for any of this anymore." And then we would get this situation of gridlock in which everybody is trying to get on the machine, and nobody can get there, and you just can't get anything done.

And, yes, it was partially the way in which the Division of Data Processing looked at the problem, and it was partially that we were doing resource allocation systems that could have [been] thought up by Nikita Khrushchev. They just didn't match, and you got the result that you would have expected. I wanted the section chiefs in R&S to be responsible for financing the computer capacity for the following year by having it in their budget and reviewed by the division management.

The only way I could see that you could solve that problem would be if every section had its own computer(s). Then that allocation can be done pretty easily at the section level, and you would always have enough capacity, and so this gridlock would never develop. But, of course, the Division of Data Processing was entirely against any such thing as that. I just wanted to separate the research computing and get it distributed and leave the nonresearch data processing business centralized down in data processing. It was a long, hard slog.

MS. SHACK-MARQUEZ. So when did that battle begin? I know there must have been, like, a decade of whining, which I would assume wasn't part of the battle.

MR. ENZLER. I can't say that the battle really began until quite late, but I will tell you a couple of things that happened earlier on, when Doug Battenberg was working with us. Well, one day—and I would guess maybe it was about 1979 or so—we got wind of a Hewlett-Packard computer. It was a microcomputer, and it was off-site somewhere. And we went over and had a look at it. And it was just a black box, like a little tiny beer refrigerator or something, small enough [that] you could sit it on your desk. But it did FORTRAN, and we realized that we could run the model on it. And if we had one of those in our office, we could do all the model work on that thing. And then we would compete against absolutely nobody. And, furthermore, we were spending thousands and thousands of these phony dollars on computer time. And yet, if we had this one maybe $10,000 machine, we could run that thing day and night. It wouldn't cost anything. And we'd get our work done much faster.

I started talking about that. I intended to personally purchase one machine to prove the concept. But I got called in by the Data Processing Division director who told me I could not bring any machine in the building that's not IBM and running MVS.

MR. DRZYZGULA. Was that Chuck Hampton by that time?

MR. ENZLER. Yes, that's right. And so I had to back off. But it was that episode that gave me the idea: "Yes, I think I see how to solve this problem, but I don't have any power to solve it."

MS. SHACK-MARQUEZ. So what changed?

MR. ENZLER. So what changed? Well, for one thing, the word processors began to come into use, and we developed an office automation committee. It was a Boardwide office automation committee. And I was the division's representative to it, and we were looking at

Boardwide solutions. Now, when the office automation machines came in, I can't remember what the name of them was anymore—

MR. DRZYZGULA. The Lexitrons.

MR. ENZLER. They were just little computers, and they just ran a word processing program and put the results on a floppy disk. Well, the minute the first one of those came in the section where I had my office, I noticed that instead of the secretaries running the thing, all [of] the economists were taking turns sitting there at that word processor—because before that, you always had to write your papers out longhand. You gave them to the secretary, and two days later you got it back, and you revised it, [and it] went back. But now, if you could sit at the word processing machine, you could do the whole thing in one crack.

So I could see that that was the solution to the computer congestion. And what I wanted was a computer system that would both be able to do that word processing and to be able to do real computing. And I said I wanted to locate the computer right in the sections themselves, although at that point, I didn't really know how.

And then here came the main thing. Along about 1981 or 1982, we got into one of these gridlock situations—I don't know exactly the date, but I do remember the episode—and everything came to a stop. And everybody's tearing their hair out. Nobody knows what to do. And I wrote a memorandum to my division director, Jim Kichline, saying, "You know, it would be possible for us to have a mini- or perhaps microcomputer system of our own, different from the data processing system, that would work much better and be much cheaper. I just give you this idea for your consideration." And that was the worst mistake of my life, because he wrote back a memorandum saying, "That sounds like a good idea. You're in charge." Oh, my God! And that's when the war started.

MS. SHACK-MARQUEZ. Ah, but he gave you the opportunity to change the world, right?

MR. ENZLER. Yes. But I wanted somebody else to do it, not me!

MS. SHACK-MARQUEZ. When did the word processing machines show up? Do you remember?

MR. ENZLER. I can't tell [you]. What I can tell you is, it was after Special Studies and the Econometrics and Computer Applications (E&CA) sections moved to the Martin Building, and I think that was about 1980. So it happened somewhere in the early 1980s.

MS. SHACK-MARQUEZ. Okay.

MR. DRZYZGULA. They were Z80-based systems, so it would [have] to be after that.

MS. SHACK-MARQUEZ. Oh, okay. Bob is Googling to figure out when they could have existed.

So I'm just very curious. I started out in Economic Activity as a labor economist and was one of the judgmental forecasters, but my first management job was as the section chief of Automation and Research Computing (ARC), which I took over in 1994. So I've spent a good part of my career fighting the ongoing historical battle here and defending our computer system repeatedly about why it's the right thing to have. So I'm interested a lot in the story of how you grudgingly convinced others that we should do this and what the implications, in your mind, of that were for how we were able to get our work done in the division, in terms of our economic analysis.

MR. ENZLER. Well, okay. I think I'll just pick up the story where I left it, right after Jim Kichline put me in charge. At that point, I began to assemble a group of people: Mark Greene and Bob Drzyzgula were part of it, [as well as] Steve Schwartz, Jim Berry, Carl

Malamud, Louise Roberts, and a couple [of] others. We formed a group and started looking at various solutions that might occur. Let me say, the aforementioned are the real heroes of this story. I was just the cranky executive who got it started.

One possibility was to either convince the Division of Data Processing to buy a VM machine that ran the VM operating system, which helped a lot with the research but wouldn't help at all with the office automation, but that was one possibility. The other possibilities were to go and do some sort of minicomputer system like Digital Equipment or Data General or one of those. And the final one was to develop some sort of a microsystem, but at that point, those were not at all very well developed. They were more gleams in the eye that people were making them than anything else. And we ran around looking at them, and we found several potential microcomputer solutions and several potential minicomputer solutions.

And then one day we went up to Boston in order to attend a Digital Equipment conference of some sort. It was an exhibition, and several of us went up there. And while we were there, somebody told us that an outfit called Sun Microsystems had an office on the edge of town, and that they had some computers out there we might want to look at. So we drove out, and we took a look at them. And then I took one look at that first Sun machine, and the future flashed before my eyes. I mean, I could see something that had the answer to everything.

First, it had graphical user interface, which we had never seen before, and you ran it with a mouse. I don't have to explain to you what we saw. And, in addition, it was running a program called Interleaf. Now, Interleaf, we never in the end, I think, actually used, but—

MS. SHACK-MARQUEZ. Oh yes, we did.

MR. DRZYZGULA. We did, yes.

MS. SHACK-MARQUEZ. We used it a lot.

MR. ENZLER. I remember getting reports from the ARC staff that Interleaf was hard to manage. But it was wonderful to use. You could handle mathematical symbols and equations with it.

Furthermore, the Sun computers were very powerful, and they could easily run something like the model with no problem at all. And they were very programmable. They could be connected to each other by Ethernet. You could run servers to do various things. In the course of an hour, the whole world opened up. I could see what I thought the right decision would be, although it would not necessarily be from that particular company.

From that point on, we mostly focused on those kind of solutions. And after a while, we had to decide which one of the groups we were going to buy from, and we decided we would buy from a group that was named Apollo. But no sooner did we do that than one of the competing companies complained that we hadn't gone through the usual procurement procedures that governments are supposed to go through when you're purchasing stuff. And so we had to run a formal procurement thing, and we did. This time, Sun won, and it won it quite easily. And so that's where the Sun machines came from.

So I had in mind, of course, not that Sun was just going to be an office automation thing, I had intended absolutely for it to be the solution to the computer problem as well. And so, then, one might ask, why did we purchase a VM machine at the same time? And the answer to that question is one of pure politics. I mean, there were a lot of people who saw microcomputers as just toys, and they didn't see them as being able to do anything useful. They thought you couldn't do real computing on a microcomputer. Well, I knew better. But there was a sizable contingent of those people. Furthermore, there was the Division of Data Processing that you had to take care of, because they, too, were very skeptical of microcomputer solutions. And the third

thing is that the VM machine was a fallback position. If we did both the Sun machines and the VM machine, and the Sun machines didn't work—or, actually, my problem was not that the Sun machines were not capable, but that we might not have the capacity within the research division to successfully run them. We had no experience with that. But I thought we could. Bob Drzyzgula and his people were pretty good, and I thought they could do it. But I went for the VM machine as a fallback position—if the Sun thing failed, at least we had our computing problem more or less solved.

But my expectation was that the VM machine would just die out, that once people got started working on the Sun machines, they would just gobble up everything. I don't know how long it took for that to happen.

MR. DRZYZGULA. It did. Well, actually, I wanted to say that, to my recollection, one of the other big values provided by the VM machine was that it ran some software that just wasn't ready for the Suns yet—in particular, FAME.

MR. ENZLER. Exactly.

MR. DRZYZGULA. The FAME database was on the VM system for quite a while before it was ready for the Suns.

MR. ENZLER. Yes. I agree with that. Well, I thought we could probably find something like that for the Suns. And I'm aware of FAME, of course, and I was aware that we were gearing up to run that. But I'm just telling you that, in my own mind, I did not think the VM thing would last very long. I don't know whether it lasted—you can tell me.

MR. DRZYZGULA. A few years.

MS. SHACK-MARQUEZ. So, Jerry, when did you leave the Board?

MR. ENZLER. I left the Board in September 1987. I had a health problem, the same thing that's causing me to have the hearing problems that you're aware of now. That happened in 1986. And so I immediately knew I was going to leave.

MS. SHACK-MARQUEZ. So you left right at the inflection point, because early in 1989 was when—by that point, we were using word processing stuff—I worked with a group of people to migrate all the judgmental forecast stuff off of the mainframe and onto the Suns.

MR. ENZLER. Right.

MS. SHACK-MARQUEZ. So that by early 1989, we were basically done with the data processing group, except for the big data collection. So that's interesting.

MR. ENZLER. Yes. Well, this discussion is exactly the thing I worry about when you're recording something. I always hoped that they would invite me back to pull the plug on that mainframe.

MS. SHACK-MARQUEZ. Well, if it makes you feel any better, and we can redact this part from the transcript later, but here it is, 2013, and we're still having lively conversations with our colleagues about their billing system and the perverse incentives that the billing system creates.

MR. DRZYZGULA. For the past quarter century, we've had to relitigate this again and again and again. It doesn't go away.

MR. ENZLER. Before I leave this particular subject, since I've thrown a lot of rocks at the Data Processing Division, I want to be careful about one thing. During the later stages of this conflict, the person they had assigned to liaise with us was Steve Malphrus, who later became the division director down there. And Steve was wonderful. He was open minded about everything.

He assisted us in every conceivable way possible. He's just absolutely great. And I'm sure he must have made a terrific director of that division when he became head of it.

MS. SHACK-MARQUEZ. He had a lot of vision, that's for sure.

MR. DRZYZGULA. I remember he actually came along with us on many of those visits to the computer companies.

MR. ENZLER. Yes, he did, yes. Steve was wonderful.

The Relationship between Computing and Research

MS. SHACK-MARQUEZ. So I'd love to go back to the 1960s and ask you how, in your mind, the computing capacity intersects with what we're really able to do on the analytical front and how those two things are working together.

MR. ENZLER. Wow. That's a pretty broad question.

MS. SHACK-MARQUEZ. Yes, I know.

MR. ENZLER. When I went to graduate school, of course, we didn't have computers, and so we were doing all our work on those desktop machines which were electricity powered, but that's all that was electrical about them. Of course, in that world, there are just so many things you can't do.

And, immediately, when those early IBM machines—they were VM machines—came into the universities, it just changed everything dramatically. Within a year or so, we'd switched over to doing all [of] our work on the computer. I learned to be a FORTRAN programmer. Everyone was learning to be a FORTRAN programmer. We could do all kinds of statistical work that was previously just inconceivable.

And my only problem is, it took a long time for the computer systems to be tailored very well for the research work. They were tailored more for production work. Even in most

businesses, they did production work. They did accounts receivable, they did payroll—they did things like that that you do over and over again. It took a long while for people to get off the mindset that all you were doing there is just automating away statistical clerks of some kind. Actually, when you get to something like the Board or a university, the real value of those things is that you can do things that you only do once, but you can do them quickly. They have to be very flexible—very flexible, easy to program—and they have to be very accessible. What researchers need is speed, speed, and more speed. And that's what developed—that's what makes researchers productive, because one piece of research depends upon previous pieces of research, so the progress you make depends upon how many of them you can get done. Under the older, bigger computer systems, we weren't getting much of that, but under the newer ones, we got an awful lot of it. And it just transformed research immensely.

MS. SHACK-MARQUEZ. Were there shifts that you were able to see here in the work that you were doing at the Board, in terms of—were there leaps in the analytical capacity that you were able to see as a result of changes on the automation front, or was it a more gradual thing?

MR. ENZLER. Of course, I didn't really live through the revolution at the Board, which I started, so I never saw what happened here, but I did live through the introduction of the computers at my university, and I did live through the introduction of the microcomputers in the Monetary Authority of Singapore where I went to work. I saw it in both cases.

The evolution seemed to me to be very gradual. And the reason is that when you bring in a new technology, people always have an idea of what they're going to do with the technology—we're going to do this, we're going to do that, and so on. But you bring it in, and what you find

is that instead you do something else, something that you never thought of doing before. But it reorganizes your workload. It just reorganizes everything.

And I saw that both at Penn and again in Singapore. We would bring in the machines to do one thing. Three years later, we were doing something totally different not only with the machines, but we had reorganized the way in which we looked at the world. And so I don't believe that you generally get great leaps in it.

There's a quote by Bob Solow, way back, and he said something about how we can see computers in the statistics everywhere except in productivity. Well, he was right. I mean, we brought in computers, and people were using them, but they were just using them in the old way —to do the same old things that they'd always done. The productivity comes when you find out that you can do things that you've never done before. But that takes a long time, and it comes piecemeal. You know, sometimes somebody changes something, and that allows somebody else to change something, and so on.

So I think that computing power should not be expected to make dramatic leaps in things like productivity, but they will cause an awful lot of it if you just wait a while and watch closely.

Monetary Policy during the Volcker Era

MR. SMALL. So we talked about monetary policy up through the mid-to-late 1970s, and I think we've covered a lot of the computing stuff up through about that time. So, returning to monetary policy, we talked about [that] inflation was increasing, and you were aware of that, and defense spending. But then things get out of control, and then Volcker comes on board and really switches completely the way policy is run. What do you remember about that period?

MR. ENZLER. Well, first, Volcker did what absolutely had to be done at the time. You may recall that there was a period there where Volcker appeared, on the surface, to be a kind of

monetarist, because he started setting money growth targets and insisted that we keep below them. But Volcker, in my opinion, was never anything like a monetarist. But what he did understand was that he had to get the unemployment rate high, and he was going to have to hold it there for a very long time. And all he was doing was doing whatever it takes to do that. And he did.

And his view of inflation was what I would call a much more modern view. He recognized that he had to do that, and he couldn't get it down in any other way. But it wasn't really a money growth targeting regime. It was an output and unemployment targeting regime where he intended to keep output low and unemployment high for whatever length of time it took to grind down the inflation [rate]. Yes, it was an absolute sea change. We'd never had the courage to do that before.

MR. SMALL. Were the models of much use? I mean, did you calculate what we now call "sacrifice ratios," like 10 unemployment point years, to bring inflation down? Or did you see 20 percent interest rates?

MR. ENZLER. Yes, yes. We were never asked for a sacrifice ratio, but our forecasts implied them. And we had this interesting meeting—I referenced it once before—where Volcker accused the staff of having a monolithic view of inflation. Basically, the staff view of inflation we had by that time was essentially the same as it [was] modeled.

Dave Stockton presented the staff view, and the Board seemed to buy it—in fact, to such an extent that they shocked me by two or three of the Board members saying, "I think we're going to have to keep the unemployment up there even longer than you say." They weren't correct. As near as I can tell, we did just fine through that period, and the inflation rate backed off at about the rate we thought it was going to do so.

But before that meeting, I'd never heard most of the Governors say anything like that.

MR. SMALL. Were you forecasting longer-term interest rates, or were you taking reads off longer-term interest rates when inflation expectations came down and credibility was earned?

MR. ENZLER. Well, no. Your question is a "rational expectations" question. As I say, we never used that.

On the other hand, our term structure equations rarely ever gave us much of problem, at least during the period when I ran the model. You might ask Flint Brayton and Eileen Mauskopf, who worked on the model, about later periods. The term structure equations just mean that the longer-term rates depend upon a weighted average of past short-term rates, but there's some other little embellishments in there that might pick up some of the rational expectations forces.

But my memory of that is that we never really had very much trouble with those term structure equations. I think they worked fairly well through that period, but I haven't looked at the data now for many years, and so I don't remember. My only assertion is that—very rarely that we had any problem with those term structure equations.

MR. SMALL. So you ran a Phillips curve with the weight summing to 1 and targeting M1?

MR. ENZLER. That's the way we did it, yes.

MR. SMALL. And it worked reasonably well?

MR. ENZLER. Yes. We were certainly not having much trouble with the price and wage equations. I can remember once back in the 1970s—1972 or 1973—when the price controls came off, and I attended a Federal Reserve conference in Boston. And the title of the

conference was "After the Phillips Curve." And the premise was that the Phillips curve was destroyed.

But, no. What you had to have was a fairly sluggish price determination or inflation expectation term in the wage equation with the coefficient summing to 1, and then you can create equations that work tolerably well all through that period. So anything except during the period of wage controls or the few quarters right after worked reasonably well.

MR. SMALL. At that time, what other big macromodeling groups did you compare notes with, either domestically or at other central banks? Was there developing a community of macromodel builders?

MR. ENZLER. No, we didn't. I can't really think of a single example where we did that. We were pretty isolated except from people in the academic community that we talked with a bit, but not other model builders.

MR. SMALL. Did the microcomputers allow people to build their own micromodels? You know, like four-equation macromodels? Did you get a diversity of other simple models or policy rules?

MR. ENZLER. Are you asking did we do that within the Board? The answer to the question is, basically, "no," at least not that I'm aware of. I mean, sure, there were people who would develop them for a particular research project, but, no, we did not.

Budgeting for Research

MR. SMALL. You had, I presume, [a] financial annual budget process. What was the process for people determining what research projects they would work on, what were high priority for the section? Was that top-down? Was that bottom-up? Did you have an annual debate about who would do what research? Was there free time and mandatory time?

MR. ENZLER. That's an interesting question. We did go through this annual budgeting process. And we went in there and had discussions with the senior staff about what things we were going to do, what things we proposed, who we might have to bring in to help us, and that [sort of] thing. It always seemed like a sensible budgeting process, provided you ignored the computers.

But, as a general rule, at least from my two sections—I only ran two until near the very end—they gave us a great deal of freedom as to what it is we wanted to work on. And while we tried to keep the stuff relevant to the Board, we had a lot of freedom in deciding what it is we were going to do. We would get a few assigned things, but that was pretty rare. You would get assigned things during the year when various crises would come up. But, as a general rule, during the budgeting process, they treated my sections more like you might treat a university research department. They let us decide what we thought was important, because they learned something from that, too, as to what we thought was important, maybe they ought to be thinking about, so that's the way it went. But I couldn't tell you what happened with the other sections.

MS. SHACK-MARQUEZ. So you had the Special Studies and E&CA sections?

MR. ENZLER. That's right. I had those two up until ARC was formed, and then I had ARC as well.

MR. SMALL. So one name that has not come up yet that you might comment on is Peter Tinsley.

MR. ENZLER. Peter was one of the charter members of the Special Studies Section, way back in 1968. And he was still at it when I left. And I don't know when he left the Board, but I think he worked there quite a while after I left. Peter was one really smart guy. You could

consult him on anything and get real help. He could see amazingly deeply into any economic subject.

MS. SHACK-MARQUEZ. Yes. So one of the things that you said—like, right at the beginning, when we started this conversation—was that you came in the fall of 1966, and that Frank de Leeuw was the only modern economist in R&S at that time.

MR. ENZLER. I shouldn't have said it like that. There were others, but it took me some time to learn of their existence.

MS. SHACK-MARQUEZ. I'm not saying that you are accusing somebody else of not being modern, but I'm just curious—I realize that you would be a really new person in the division, but what was your understanding of how we started building up modern Ph.D. economists on the staff?

MR. ENZLER. I can't give you much insight into that, but what I can tell you is that it happened just as I got there. When I say that Frank was the only modern guy around, I mean he was the only one I knew. Ned Gramlich was hired in 1965. Ralph Bryant had been in the International Finance Division even before. Peter Tinsley was there before Special Studies was formed. But the buildup of intellectual capital that started late in the William McChesney Martin era, before Arthur Burns got there, was really spectacular. At that point, we didn't hire anything except budding Ph.D.'s from major universities. But that hadn't been what happened before.

MS. SHACK-MARQUEZ. Got it.

MR. ENZLER. And I don't know what caused it. I wasn't in on it. But what I can say is, it was spectacular. I mean, within four or five years, between the Division of Research and Statistics and the Division of International Finance, we had a major department there.

MS. SHACK-MARQUEZ. Yes. So that was when Dan Brill was the division director?

MR. ENZLER. Dan Brill was the division director. And to tell you the truth, I think Dan Brill had a lot to do with that sudden buildup in human capital. And I don't remember who followed Brill. I think it was Chuck Partee, I'm not sure. And then Lyle Gramley after that, and then, I think, Kichline.

MS. SHACK-MARQUEZ. Oh, and then Kichline. Okay. As you recall that really early cohort, you said there was Ralph Bryant and Gramlich and you. And I'm just curious who else you can recall from that very early crowd.

MR. ENZLER. Well, you know, depending upon where I put down the timeline, first there was nobody, and then all kinds of people appeared. All I can tell you is that 10, 15 of them just magically appeared out of the sky very quickly.

MS. SHACK-MARQUEZ. That's a lovely image, isn't it? Ph.D. economists falling from the sky?

MR. ENZLER. It just rained on us there for a while. Boy, it was a great relief, I'll tell you.

MS. SHACK-MARQUEZ. Okay. So I think we're winding down on questions. Jaime, I don't know—if you're still on the phone—if you have anything else?

The Phillips Curve

MR. MARQUEZ. No, I don't have anything else. There's one logical item that I was curious [about]. What do you mean by the "vanishing Phillips curve"?

MR. ENZLER. Sure. When the Phillips curves were originally estimated by Phillips himself, they were just one equation that related the rate of wage inflation to the rate of unemployment. But if you combine that with the price equation—that's something of the sort that says prices are a markup over unit labor costs, or something of that sort—you get a two-

equation system. And they put expected inflation as one of the terms in the wage inflation. So, in other words, wage inflation becomes the function both of the unemployment rate and of the expected rate of change of prices. Now, if the sum of the distributed coefficients on past inflation are less than unity, you get a long-run Phillips curve that is not quite vertical. And that's what I meant by the nonvanishing Phillips curve. When the coefficients sum to 1, the long-run Phillips curve is vertical. It just defines an unemployment rate (the NAIRU) above which inflation rises forever and below which inflation keeps falling forever.

MR. MARQUEZ. I got it.

MR. ENZLER. Even the long run, if you plug in an unemployment rate of 7 percent and you want to know how things will come out, those two equations will generate some particular inflation rate if the price inflation coefficients sum to less than unity. But if the price coefficients sum to 1, then it turns out that it's the change in the wage rate that's a function of the unemployment rate, so if the unemployment rate is below or above the natural rate, the rate of inflation increases or decreases forever. That's what I mean by a vanishing Phillips curve. And that is due to Friedman and Phelps, who developed that thing sometime in the 1960s.

And so, yes, at first we did have nonvanishing Phillips curves in there. Before long, we had vanishing ones. So we had a system that had a natural unemployment rate in it.

Life after the Board

MR. MARQUEZ. One quick question that I have. What do you miss the most about the Board?

MR. ENZLER. Well, I miss the Board. It was absolutely a great place to work. It had very nice people, very intelligent people. We worked where you felt that what you were doing was something that mattered to the world. That was one of the things that attracted me [to it] in

the first place. I loved the place. You know, if you ask me this day who I am, I will tell you I'm a former employee of the Federal Reserve System. That's my identity. And so, yes, I miss the whole place.

But I think it was the power of the place that attracted me, and then it turned out that it just was an immensely interesting place to work, always dealing with very interesting questions.

MR. MARQUEZ. Perfect.

MS. SHACK-MARQUEZ. That's awesome. I think all of us are here nodding our heads in agreement. That's exactly right. So I don't think that part has changed at all.

MR. MARQUEZ. Well, thank you very much.

MR. ENZLER. Thank you.

MS. SHACK-MARQUEZ. Jerry, this has been just incredibly helpful. We are grateful for your taking so much time to share some of your recollections with us. I think, for Bob and me in particular, it's really fun to hear the ancient-history piece of it, because it helps us, you know, [see] how it all fits together. Jaime is working on this history of the Division of Research and Statistics for us, and he and I are having a heck of a good time digging around in some of this ancient history, and it's all coming together. So I think you've helped us plug some big holes that we had in our stories. And that's been wonderful.