

MD INTERVIEW

AN INTERVIEW WITH ALLAN MELTZER

*Interviewed by Bennett T. McCallum
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Allan Meltzer's career in economics has featured outstanding contributions in an astonishingly wide range of activities. As the basis of all of these, of course, lies his work in economic research. Perhaps most well known is Allan's long line of papers in monetary economics, many written together with Karl Brunner, which helped to establish the broad and widely accepted approach once known as monetarism. But several other areas have, at different times, attracted his main research efforts; among these are business-cycle analysis, financial intermediation, analytical political economy, and the history of economic thought. Recently, he has become deeply immersed in a major historical project—the writing of an extensive history of the Federal Reserve System and its monetary policymaking. A second type of outstanding accomplishment has been Allan Meltzer's work as a conference-series creator and organizer. In the 1970's, he and Karl Brunner founded the Carnegie-Rochester Conference Series on Public Policy, which has been unusually fruitful as an incubator of new ideas and talent. Together, Brunner and Meltzer also founded the Interlaken Seminar on Analysis and Ideology, which for many years brought together economists, political philosophers, and other social scientists. Allan was a major contributor to Brunner's organization of the Konstanz Seminar on Monetary Theory and Policy—still a creative force in European economics—and with colleagues he created and ran the Carnegie Mellon Conference on Political Economy from 1979 to 1990.

As if all this were not enough for three or four normal beings, Allan and Karl created the Shadow Open Market Committee. At its inception this was a unique institution, but it has since served as a model for other groups designed to provide policy analysis for a wider public audience. In terms of that latter objective, Allan has been and continues to be one of the economists most frequently sought out and quoted in the national and international press. He maintains an amazingly fresh and extensive store of knowledge about economic and social affairs the world over, one that he shares generously with other scholars.

Allan Meltzer has not spent much time in full-time governmental positions, but has served extensively as a consultant or advisor to the U.S. Treasury and the

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Council of Economic Advisors, as well as official agencies in several other nations, including most notably the Bank of Japan. Also he has for several years spent a good bit of time at the American Enterprise Institute. For over 40 years, however, his principal professional home has been the Graduate School of Industrial Administration at Carnegie Mellon University.

From the foregoing account, it will be obvious that Allan Meltzer is equipped with an enormous supply of energy and enthusiasm, as well as analytical ability. A closely related characteristic, familiar to all those who are lucky enough to spend time with him, is an unfailing attitude of optimism and cheerfulness.

My interview with Allan took place on May 14, 1997, in his office, with its pleasant corner location in the new wing of GSIA's building. We talked in the afternoon and continued somewhat longer than intended because there was so much of interest to discuss. Even after 16 years of having nearby offices and multiple conversations—on days when we both are in Pittsburgh—I found it instructive and enjoyable to learn more about Allan Meltzer's remarkable career. The interview was taped, transcribed, and edited lightly.

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McCallum: Let's start by my asking you how you became interested in economics as the area in which you were going to make your career.

Meltzer: I grew up in the 1930's, a time when economic questions were paramount in society. I was active in politics and became interested in economics from the political and other activity that I engaged in as a youngster. I thought of economics as a way to solve some of the problems that society faced and individuals in society faced. When I took the first course in economics as an undergraduate I found it fascinating as a subject. It seemed to offer both a lot of application to real problems in the world and at the same time to have an interesting intellectual attraction—problem solving attraction—so both of those things got me interested. I had gone to college with the idea that I would probably become a lawyer. After I took a first course in economics I changed to an economics major and continued along those lines.

McCallum: You had some inclination in that way even before you went to college, then?

Meltzer: Yes, I had. I wasn't exactly sure what economics was until I took the course. They didn't teach economics in high school in those days, but I knew it had something to do with the way in which society gets organized and manages to solve some of its problems. That much I knew, but not very much more than that. I had been very active for my time in politics, so I thought I knew something about the kinds of problems that economics dealt with, certainly at the macro level. I'd been a delegate, even though I wasn't old enough to vote, at the Henry Wallace convention in Philadelphia, and worked hard for him in North Carolina as



FIGURE 1. Allan Meltzer.

a student. I'd done other things of that kind, so I was pretty much up on issues of the day and sort of saw economics as leading into those issues. I'm not sure that from a mature perspective I would make the same judgment, but from an immature perspective it certainly seemed like that was a way to learn more about and be able to do something about some of these problems.

McCallum: So this was sort of left-wing political activity?

Meltzer: Oh, very much so at that time. I was very much a left-wing political activist as a student.

McCallum: Yes, as people—as young intellectuals—tended to be in those days, I suppose.

Meltzer: Especially in the thirties. There really was, at that time, a sense that somehow the system we had, had not worked very well. I think it makes a great deal of difference whether you were born in the late 1920's as I was and grew up through the thirties or whether you were born in the postwar, as to how well you think society operates when you reach college.

McCallum: Okay, well the next question that I planned to ask was whether you feel that there have been any distinct changes in direction or emphasis during your career but, given the response you just gave me, there certainly was.

Meltzer: Well, there certainly has been a major shift in my orientation and that came along—perhaps when I was in my twenties—which would be in the late 1940's and early 1950's. Some of that had to do with my graduate training and learning and having teachers like Brunner and Alchian. They had a big influence on me at that time. I had already begun to change by the time I went to graduate school. I was 20 at the time I finished undergraduate school and started to go to graduate school at Harvard. I never went to a class because I changed direction and decided to go to work. I worked for a while and went back to graduate school at UCLA where I was fortunate enough to have two really great teachers in Karl Brunner and Armen Alchian and very small classes. I had some advantages in going there. The profession has changed enormously. It has become so much more rigorous, it's hard to believe. One big change was in calculation. When I wrote my thesis, I had to do most of the regressions on a hand calculator and do check sums and that sort of thing. I don't think people would be doing nearly the amount of empirical work that they now do and certainly doing it as casually, if they had to run multiple regressions on a hand calculator the way I did. That's certainly one big change. Of course, the whole emphasis on modeling, on explicit modeling, explicit introduction of the reasons for errors in equations is new. So is the emphasis now on dynamic models—the effort to bring together micro and macro theory—all these things were talked about I suppose in the 1940's and the 1950's, but they really had not been implemented to a very great extent. Changes have not always been better for economics. Maybe over the longer term some of these changes will prove to be more valuable than they have so far proved to be. What I mean is that to make this technical leap, we've had to strip away a lot of the complexity of the models. While there are gains from the improvement and rigor, there are big losses in terms of what we're forced to work with and on.

McCallum: But once you started teaching—once you became an assistant professor—have you felt that you personally changed directions or changed emphasis in the kind of issues that you were concerned with and interested in?

Meltzer: It started out with a sort of vague idea that what an assistant professor ought to do is find out what you can do with the tools you learned. So I worked with those tools, trying different kinds of studies, and eventually got interested or

reinterested in money. I'd written my thesis—an empirical thesis—on the money supply in France during a period of inflation. As a good student of Karl Brunner, I had been working on the money supply. I spent a year in France. Then I went to the University of Pennsylvania and finished my thesis. I came to Carnegie, worked on different things including some of the projects that were going on in the school at that time. I did a computer simulation of somebody making investment decisions, just exploring different aspects or opportunities. Then I came back to doing empirical work, this time on the demand for money. I think in the first stage of most people's career, you try several different topics. Then you settle on one thing that seems interesting and work on that for a while. I worked on the demand for money. I did a number of different studies of the demand for money, using cross sections and time series. I then began to put that together with Brunner's work on supply. Then together we broadened that, to look at a general equilibrium model of the financial system. Then a general equilibrium model with the financial system tied to the real economy.

I'd been doing other things at the same time, many of them policy related. Slowly I became more and more interested in the question about why what seemed so obvious to me was so hard to get across to other people, particularly central bankers. From that and other things that were going on at that time. I became interested in political economy. I saw that as a way to model how the political process and the economic process interact. I got interested in political economy to a very large extent because Karl Brunner had started the Interlaken Seminars in Switzerland. There were a lot of people there who were trained in philosophy or sociology. Since my training was in economics, I thought we ought to try to model some of these problems. I began to read Hayek at that point, particularly the *Constitution of Liberty*. It is a fascinating book, dealing with all the problems of how institutions fit together. I was very fortunate to work with Scott Richard, and later with Alex Cukierman, on trying to model some of these processes. We tried to develop simple models in which there was a government that did something more than provide a public good, or act as if it had solved the planner's problem. One of the works that influenced me at that time, as I said, was Hayek. Hayek would certainly not try to solve the planner's problem. He saw life as being a great deal more uncertain and government as being very unlike an optimizing planner.

Where classical and neoclassical economics emphasized the certainty of life, Hayek and the Austrians emphasized the uncertainty of life. That aspect of the Hayekian program had great appeal for me. Karl and I had earlier worked on information and uncertainty as a reason for using money—that is, using an institution such as the medium of exchange. Why not other institutions?

One source of uncertainty is uncertainty about accruing information. These interests led Brunner and me to work on permanent and transitory influences. I did some work using filtering—trying to sort out the nature of these different shocks based on some work by Eduard Bomhoff and his student, Clemens Kool.

My current interest is to write the history of the Federal Reserve. I have done quite a bit of work on that for the past couple of years and that work continues.

There's been a lot of evolution. My thinking has changed a great deal; I suppose you are the major consumer of your own research, so you have to be influenced by it. If you're not influenced by it, it is unlikely anyone else will be.

McCallum: Your collaboration with Karl Brunner went on for a long time and produced an enormous amount of published output. It was one of the most remarkable and productive collaborations that I can think of in economics. Tell us a little bit about how it got started and then how it progressed—how it went on, how you worked together, and things like that.

Meltzer: Well, Karl of course had been my teacher and—

McCallum: You wrote your thesis with him?

Meltzer: I wrote my thesis with him. He was my thesis advisor. I had only been a graduate student for a little over two years when I decided I would take my examinations and write my thesis. I was fortunate enough to get support from a Fulbright and the Social Science Research Council to write a thesis in France. I left UCLA at that time and never really returned for more than a few months after that. When I left France I went to the University of Pennsylvania, finished my thesis and then began, as I said earlier, to search around for interesting topics. I would try to use what tools I had. In 1957, I moved to Carnegie. I was doing various studies when Karl came to visit about 1960 or 1961. He suggested we write a textbook on money and banking. I said, "I really am not interested in writing a textbook." I couldn't see how you could write a textbook that was different from all the other textbooks unless you did some research to put into the book. We set out to do that. In fact we signed a contract with a publisher to write a treatise and then a textbook based upon the treatise.

That's how our collaboration got started. Originally he was going to work on the money supply and I was going to work on the demand for money. I may have started by that time. If so, I continued to work on the demand for money. Then, at some point, we would put our work together and see what we could do jointly. That was how we started about 1961. In 1963, we wrote two papers, the first two papers we wrote together. One was called, "Predicting Velocity." We took all the money-demand equations that were common or around at that time. We used rolling regressions to predict the next observation. Then we redid the estimation and predicted again. We had a series of 39 predictions, if I remember correctly. We looked at how these different models did over different time periods.

We found a very interesting result which apparently has not attracted the attention which I think it deserves. All the models that use current income did substantially worse than any of the models that used permanent income or wealth. That fit well with some of the work I had been doing before on time series. It really seemed a clear-cut and unambiguous test. The data nicely showed that the errors for any different time period were substantially larger in one case than they were in the other. I still think that's an important observation.

The more I've done empirical work in economics, the more I have become convinced that robustness is very important. You can often find a sample which

will confirm almost any hypothesis. In economics it's finding evidence over and over again that becomes important for changing ideas and beliefs.

McCallum: Let me go back to clarify something about when you left UCLA and went to France. Were you just starting your thesis?

Meltzer: Yes, I hadn't started it. In those days the common path was to take a bunch of courses. Then you searched around for a thesis topic. It wasn't until I came to Carnegie that I learned there were better ways to do things, like get students to start writing papers so they would think about possible topics early. Usually, people would thrash around for six months or a year and not do very much. I was married at the time and had one child. I wasn't very interested in thrashing around. I went off to France with the idea that I was going to write a thesis and I took some statistics books and some macroeconomics books with me and some papers that Karl Brunner had done at that time on the money supply. I started to look around for data.

McCallum: And your thesis was on?

Meltzer: The French money supply.

McCallum: And did you choose that topic because you had a fellowship to go to France?

Meltzer: No, I chose the topic because I wanted to go to France. It combined two things. One was Brunner's interest in money supply. I didn't have any strong interest one way or the other in the topic. I had a strong interest in going to France. We put the two together. Since he was working on the US money supply, it didn't make a lot of sense for me to do that. That's how that came about.

McCallum: What I don't have a very clear picture of is how close you and Karl were at that time.

Meltzer: Well, I'd taken several courses with him. He was a wonderfully open man. Graduate students had their offices together, and Karl often came down to go out for coffee with us. While I was only at UCLA for a little over two-and-a-half years altogether, he was the professor that I knew best. I had taken a reading course with him, a one-on-one reading course, from which I learned a great deal. I knew him well, and I had been to his house, but we were certainly not close friends at that point. Then I wrote my thesis with him. He was very helpful in commenting on it and keeping me from going astray. That was our relationship, at the time. We were not close friends. We became extremely close friends afterward. I think, in a eulogy, I once said that, very few men of my generation really had close friends. I felt myself extremely fortunate to have a friend who was as close to me as Karl was in so many ways. It was remarkable in that respect, because a collaboration that goes on for as many years as ours did has many aspects of a marriage. There are ups and downs and disagreements. There are good days and bad days. You get along and you feel unhappy. Certainly our relationship went through those things, but we always talked things out. We would have a phone call, at least once a week. The telephone conversation often lasted for several hours. We would go over the papers we were writing and would talk about the ideas that we had. We talked about lots of other things not directly related to immediate concerns.

We had quite an active relationship. I was interested in some things Karl wasn't interested in. He was always interested in the philosophy of science and methods. I learned from him a great deal about that just in these conversations. He would be reading Carnap or some other philosopher, and he would be excited about it. He would make me get interested in Carnap as well. I learned quite a bit about the philosophy of science, logic, all of which Karl liked very much. Similarly, I was getting interested in political economy at one time and so I would talk to him about Hayek, Popper, and about the Austrians. Later we talked about Keynes when I worked on my Keynes book.

McCallum: Had he read Hayek at that time or did you get him to do so?

Meltzer: He had read it when he was much younger. I possibly had read it also when I was much younger. But I think it has always been my view that certain books strike you very differently depending upon the stage where you are and how receptive you are to the ideas. Probably when I read Hayek as a young man with a very strong leftist leaning I did not get interested in the *Road to Serfdom* or the *Constitution of Liberty*. I believed that society could reinvent itself in the short term if necessary. The view that I took from Hayek is the importance of uncertainty, tradition, and institutions. Hayek and Popper, who also are very much in that tradition, had a great influence on me sometime in my thirties. I had not read Popper earlier. I felt very differently about Hayek when I read it as a 30-year old than when I read it as an 18- or 20-year old.

McCallum: I read those in my thirties too and really liked both of them.

Meltzer: I should go back to say that, in 1963, Karl and I also gave a paper at a session of the American Economic Association on financial intermediaries. That paper was the blueprint or outline for the work that we did together for the next 15 or more years on general equilibrium models of intermediaries, money, and macroeconomics.

McCallum: This was in the early 1960's?

Meltzer: In the early 1960's. It was in the 1963 *American Economic Review*. We called it "The Place of Financial Intermediaries in Monetary Analysis," or something like that. I'm not sure I can remember the exact title at the moment. What we did was lay out the problem of what would now be called multiple markets for assets and their relationship to output. In place of the ISLM idea that there is a single rate of interest, and that single rate of interest is *the* rate of interest, we laid out a relative price framework in which asset markets and output markets interact. It was a verbal sketch of work that we were going to do. It was at the same session that Tobin and Brainard laid out their ideas, in many ways similar, but also different in ways, about building a general equilibrium model in which asset prices and output prices and output interact. So that was laid out in 1963 and that was really the basis for much of our collaboration.

The empirical paper that we did at the time, testing these various money-demand equations, brought to an end a line of work. We never did more empirical work on the demand for money. The paper on financial intermediaries was the beginnings of the next stage of our collaboration.

McCallum: Now it was about that time that you and Karl started doing some work for the House Committee on Banking and Currency?

Meltzer: Yes, I had done—all this was going on about the same time—I had done some work earlier with Gert von der Linde for the House Committee and Congressman Patman, called “A Study of the Dealer Market for U.S. Government Securities.” I had written a description—as far as I know the first description—of that market. I’d not been able to find any discussion of what the market did and how it operated. It’s of course the place where open-market operations occur. The dealers are the people who buy and sell the government securities on the other side from the Federal Reserve. There were issues about how the dealers are financed and how they decide what they want to do. How much inventory do the dealers hold, how fast is the turnover, things of that kind. Congressman Patman was interested in how much profit they make. I had done that largely descriptive study with Gert von der Linde who was at Carnegie at the time.

A few years after Congress published that study, Congressman Patman evidently remembered that I had done the study. He called me. I can’t remember what he asked me to do, but it had something to do with further work on the dealer market. I went down and I met with him and explained that the problems were not in dealer markets. They were really problems of the Federal Reserve. If we were going to pursue this, we had to look at what the Federal Reserve did and how they did it.

He agreed to that and appointed me to the committee staff to do the study. I asked Karl to join me in that activity. We tried to elicit from the Federal Reserve what their conception was. Since we worked for the House Banking Committee, we could send them a questionnaire and expect them to answer it. We could arrange to have interviews.

I’ll never forget one of those interviews. I went to the New York Federal Reserve Bank with David Meiselman who was working for the Banking Committee in some other capacity. We interviewed Alfred Hayes, president of the New York Federal Reserve Bank at the time, and the man who ran the open-market desk, Robert Rouse. They were very pleasant and polite. They kept talking about credit. We asked them what they meant. Was credit a stock or a flow? We were interested in how they thought open-market operations worked. They would answer that they were trying to control credit. The answer would come in the form of pictorial representations. They were trying to keep the credit flow within the banks of a river or some such thing as that. There was very little analytic understanding of the process. Very little had been written.

It is remarkable how much has changed since then. The level of sophistication has gone way up and also the amount of information available. I had been reading about monetary policy at that time for five or six years. There were no descriptions or discussions of what was done by the central bank. It was all very secret.

One of the purposes of the Patman Committee study was to try and put some light where, before, there had been very little illumination. After we had asked these questions about whether credit was a stock or a flow, President Hayes said, “I think we need an economist.” They sent off to get Peter Sternlight, who was

brought up to the meeting to answer questions. It was striking how, when you asked them simple basic questions like what is it you're trying to control and how does it influence the market, there were no clear answers. The answers we would get were of the kind I described before. I think that Chairman Martin described credit as flowing through a river. You didn't want to get the river over the banks but you also didn't want to get it too low. You wanted to keep it just at the right level. Then we went to talk to the Vice Chairman. At that time, Canby Balderston was Vice Chairman of the Board of Governors. He said that credit was like a river. I thought, "well, we heard this before." But I was wrong. To him the problem was to get the river out over the banks so it would irrigate the fields—irrigate them but not flood them. They had these pictorial representations. That was as deep as they would go. It was really shocking to learn how little they had thought about what they did and how little lay behind it.

They concentrated on a variable called free reserves at the time and didn't go much beyond that. They thought if they got the level of free reserves right, other things would take care of themselves. There were no models really, no thought process even about how this related to income and prices. One of the recommendations we made was that they begin to think much more seriously about the process.

McCallum: And what's the definition of free reserves?

Meltzer: Free reserves are member-bank excess reserves minus borrowing.

McCallum: Minus borrowing—yes.

Meltzer: I never did understand why there was so little thinking about this process. Now that I'm doing the history of the Federal Reserve and have gone back into the 1920's, it turns out that ideas about free reserves were based on the work done then. The most influential people at the Federal Reserve Board—as it was called in the 1920's—believed that if the Fed provided reserves in relation to output, the price level would be stable. They followed the real-bills doctrine. They didn't believe that there was any way to learn about the transmission process. To the question, "do you affect the price level?" people from the Board, including the then Governor of the Board, Mr. Young, or Miller, who was the most influential economist on the Board, would say "No, we don't have any direct effect on the price level." We affect credit by influencing the volume of member-bank borrowing. By the 1950's—or what I now understand about the 1950's—there were not many changes in operations or analysis from the 1920's. They were still back where they had been in the 1920's.

McCallum: And they were using free reserves as a signal or a measure as to whether they were being tight or loose?

Meltzer: Originally, they thought that borrowing—member-bank borrowing—was the key to everything. Then, in the 1930's, excess reserves began to appear in the banking system. In the 1920's, there weren't any excess reserves beyond the minimum. Excess reserves were ignored. In the 1930's, of course, excess reserves appeared and borrowing disappeared. What was to take the place of borrowing? Negative borrowing, that is, excess reserves. The important indicator became the difference between excess reserves and borrowing and that was free reserves.

During the 1930's, the Federal Reserve did very little. During the 1940's it did almost nothing because it was, of course, under the thumb of the wartime Treasury. Interest rates were fixed. When the Fed went back in 1951 to try to reconstruct a monetary policy of sorts, it went back to pick up the thread that it had left in the 1920's. Many of the same people—Riefler, Thomas, Goldenweiser—were still there. They just picked up where they left off. They continued to think and apply the same ideas. In the sixties—1963 and 1964—when we were doing interviews, Goldenweiser and Riefler were gone. We were getting back mainly their ideas about how to run policy.

McCallum: They would look at free reserves to determine whether their policy stance was being expansionary or restrictive?

Meltzer: Right, right. There's a quote that is repeated many, many times from the 1920's, a quote from Benjamin Strong that says, when the system has 500 million dollars worth of borrowing, then the system is about normal. When it's more than that, it's tight, and less than that, it's easy. He had some other yardsticks for the New York banks, but that basically was the idea. That was what they had gone back to, except they used free reserves in place of borrowing. They had some numerical value.

One of the things that struck Karl and me when we looked at it was that they were guilty of the fallacy of composition. If a bank borrowed, there were more reserves in the system. Why did more borrowing lead to tighter conditions? One would think that more borrowing would mean more reserves and therefore the monetary base would be higher. Consequently, the money stock would increase. How did they explain the opposite? They said banks were reluctant to borrow. When they borrow, they want to repay because they are uncomfortable. They contract, so it's a restrictive policy.

Karl and I were convinced this was the fallacy of composition. An individual bank might want to do that but, when they repaid, some other bank would borrow. Looking at free reserves was bound to be misleading and had been misleading. That was a large part of our report to Congress. The Fed really didn't have a valid idea about how to control the money stock. There wasn't much relationship between free reserves and the money stock, and there wasn't much relationship between free reserves and bank credit. This led to research by us on targets and indicators of monetary policy.

McCallum: And were they using that measure of monetary or credit conditions to try and achieve certain well-defined macroeconomic aims?

Meltzer: Well, not if you put in the words "well-defined." They were trying to achieve some macroeconomic aims. Like all central banks, they were concerned with price stability and trying to maintain full employment. The full-employment act had been passed but, even in the 1920's, the Fed had been interested in maintaining high production or whatever word they used to describe full employment at that time.

We didn't differ as to what they should be doing. We differed about whether the procedures that they used had any relationship to the goals that they named. The

questions we asked, such as “How does what you do relate to the ultimate goals you want to serve?” would get answers about rivers being too big, too high, or too low, and so on. There was really no analytic connection as far as we could see. It just didn’t seem to be a very well-developed framework for thinking about monetary policy. We said that, and of course that turned out to be a rather inflammatory set of comments and reports, especially since it was written in a congressional hearing or congressional document. But, as I said, it led us to research the issue of optimal targets and indicators.

McCallum: Didn’t you have an argument to the effect that they were behaving in a way that was procyclical? That they were being expansionary during the time in which you would want to tighten?

Meltzer: Yes, indeed. Thank you. To go more deeply into the study, we did argue that this policy had proved to be procyclical. We developed a scaling. The minutes weren’t available to us, so we had to use something called the Record of Policy Action. The Record of Policy Action was a statement they wrote about what they had done at each meeting. Karl and I sat independently and separately, and we scaled each one of the decisions on an arbitrary scale, between minus one being tight and plus one being easy. We went through each report looking at what they had decided. They used vague words such as we want “somewhat less ease,” or “gradually move toward tightness,” and so on. When we looked at the scales, I think there was only one or two meetings where we differed.

McCallum: The difference between your reading and his reading was small—you agreed very closely?

Meltzer: Which meant that there really wasn’t a lot of subjectivity in the scaling. The scaling is arbitrary as to its origin. It is a relative, not an absolute, scale. Nevertheless, we could agree on what was tighter and what was easier, how that related to the movements of free reserves. Of course it moved very, very closely with free reserves. That suggested to us that they really were doing pretty much what the record said they were doing.

One of the interesting findings at that time was the relation to discussions about the lags in monetary policy. It turned out that they got the turning points very well when the economy moved up or down. They recognized turns very quickly, sometimes right on; sometimes it would take them a week or three weeks, but there was no problem in getting the timing right. Where the problems came was in the relationship between what they did and the broader goals that they were trying to achieve. The point that you reminded me of is that policy was procyclical, when judged by the growth of money or the monetary base. They would allow the money stock to rise during periods of expansion relative to trend, and in recessions the money stock would fall relative to trend. That’s what we meant by a procyclical monetary policy. Using money, they were making the recession deeper and the inflations longer. That continued, by the way, until very recently. The 1980’s were probably the first period in which there isn’t a pretty strong procyclical movement between money growth and the growth of nominal GNP.

McCallum: How was your work, your joint work with Brunner, during the 1960's and 1970's related to that of Milton Friedman? How was it related to the work of other monetarists, or other monetary economists, but especially Friedman?

Meltzer: Well, we were certainly aware of what Friedman was doing and, I would say, at different times in different ways. For example, my early work on the demand for money was very much influenced by his paper in the *Journal of Political Economy* in 1959, claiming that interest rates didn't enter the demand function for money. Initially, I wanted to see whether we couldn't do better with a more standard demand-for-money function. In that sense, I was very much influenced by him. Of course, when the monetary history came out, I reviewed it. But my review is a comment pretty much on the way we related to Friedman. We thought he had never developed a model. It was clear he's a brilliant man and had ideas that influenced not just me but many other people, but you never could find or write down the model. In my review of his book with Anna, called *Monetary Theory and Monetary History*, I tried to write down what I thought was the model they had in mind. A main difference between us is that Karl and I worked to develop a general equilibrium model of money involving debt, output, and prices. As I said, we had started on that path in 1963. We were very much influenced by Friedman and what he did, as were many other people in the profession, both positively and negatively, I suppose. We were influenced positively by what he was doing, but we differed from him in our effort to build what would be called a general equilibrium model relating money to economic activity and prices, and incorporating the role of intermediaries and debt.

McCallum: Well, you and Karl on the one hand and Friedman himself and his work with Anna Schwartz are thought of as two of the main sources of the doctrine known as monetarism. What do you think monetarism is, what's the essence of that point of view, or do you think there is any such thing?

Meltzer: Many people tried to define the term. Brunner probably created the name in a paper that he wrote for the Federal Reserve Bank of Saint Louis. He outlined some principles. My own description of monetarism is a theory that relates money to prices and economic activity. It's just part of the classical model. Karl and I said that, or something very much like it, at the Brown University conference. Jerome Stein brought together Tobin, Friedman, Modigliani, Karl, and me and himself to talk about these issues at a conference and to see where the differences were ("An Aggregative Theory for a Closed Economy," in J. Stein (ed.) *Monetarism* 1963). Many people got very upset because we said monetarism was a stock-flow model in which money was the most important stock determining interest rates and asset prices.

People thought that we were stealing something that was general property. I always thought of monetarism as being general property. The thing that was special was the early Keynesian view which denied any relationship of money to economic activity. This view is most clearly represented perhaps in the early Keynesian work, but later in the work of the Radcliffe Commission in England or of Nicky Kaldor and Joan Robinson. When Karl and I wrote "Predicting Velocity," which I mentioned

earlier, one main point was to show that demand functions were reasonably stable at the annual frequency. We also did a considerable amount of work on “reverse causation” to show that the impulse from money to prices did not simply reflect the effect of prices on money. That work came later, after the profession began to accept that the demand for money was not a will of the wisp.

McCallum: OK, now besides Karl Brunner, are there other economists who have had a really major influence on your thinking?

Meltzer: Well I mentioned Hayek. There are two ways. One is because of my interest in political economy. The other way is that Hayek was a pioneer in the use of information in economics. One of the papers that Karl and I wrote together that I continue to like was a paper called “The Uses of Money.” In that paper, we tried to incorporate information and the cost of information to explain why people use money. One of Hayek’s most basic ideas is that institutions are a way of reducing uncertainty. Man struggles to find institutional arrangements which, on average, make life a bit more predictable. Our “Uses of Money” is not so much about money as we conventionally think about it; it’s about the idea of a medium of exchange, the function of an institution called the medium of exchange, and how the medium of exchange as an institution resolves a part of people’s uncertainty about the future.

Another influence was Jim Buchanan at the Interlaken Seminars that Karl ran. I was very intrigued with the kinds of things that Buchanan was doing at that time. The effort to think of the government not as a government of good will that carries out whatever social purposes people want but, like any other institution, full of people who are trying to achieve both social ends and private ends at the same time. Buchanan had a big influence on me in trying to think about those questions. The conference would meet in the morning and we would all assemble for dinner in the evening. The papers were in the morning. We could climb the mountains if we wanted to in the afternoon. I learned a lot from Jim Buchanan both at the conference and at the sessions outside and that influenced me a lot in doing the work on political economy that I later did with Alex Cukierman and Scott Richard.

I had two influences that were of a different kind. When I came to Carnegie Tech, at the time, it was really a very exciting and challenging academic atmosphere. There was an awful lot going on here that was different. I learned from Herb Simon and Dick Cyert, perhaps most of all, that there were a lot of interesting and legitimate problems that didn’t happen to appeal to the current fashion or taste of academics. The message was: Pursue the things that interest you and that are real problems. I worked on problems that interested me. When we started political economy here, for example, not many economists were interested. Now political economy is very popular. I’m glad to see that happen. When I started the program here, many economists didn’t think it was very interesting or didn’t want to know that something like that could be “legitimate activity.”

McCallum: How about Bob Lucas?

Meltzer: Bob Lucas was, of course, a wonderful colleague, and I certainly learned things from him. We often walked home together, and I learned a lot from talking to him and from reading his papers. The direction that he took and the

direction that I was taking at that time were not very similar, so I can't really say. Of course, we all are influenced by great people like Bob Lucas and great ideas like rational expectations.

Bob is very good, and both Karl and I were very interested in what he was doing. We tried to use the early days of the Carnegie-Rochester Conference to encourage him and others to pursue that kind of work because we thought that, from a policy standpoint, it was very important. But it didn't influence what I did or what Karl did very directly. As we said in our Mattioli lectures, we accept the importance of information and efficient use of information. We think rational-expectations models ignore costs of acquiring information.

McCallum: I guess what I had thought was that all of your analytic work from a certain point on and from a very early point in time on was really rational-expectations analysis. Even your political economy work typically used rational expectations and the interactions among the private agents and what was going on in the economy.

Meltzer: That was as much or more the influence of Jack Muth who was here in my early days at Carnegie. I learned the idea of rational expectations when Muth was working it out in the late 1950's. What Lucas did, and what I associate most with him, was not to develop rational expectations because Muth had done that. What he did was to make the general equilibrium model, the Walrasian model, into an operational model as opposed to simply a set of identities. He gave the general equilibrium model some empirical content and gave a tremendous impetus to dynamic economics. I never did a lot of work in dynamic economics. I certainly use rational expectations, but everybody now uses rational expectations. I should add that he was a great colleague and a wonderful person to talk to. He could summarize ideas succinctly. I was teaching Ph.D. students at the time. They would come to me and get a paragraph and they would go to Bob Lucas and get a sentence that made the point.

McCallum: Well, we've briefly mentioned some of the conference activities that you have been involved with, but let's back up and talk about them. In particular, you and Karl have contributed a lot to the profession in the Carnegie-Rochester Conference Series and other conferences that you or he or the two of you organized and ran and I think we should talk a little bit about those.

Meltzer: Well let's sort them out. There are a lot of them. There was Konstanz, Interlaken, Carnegie-Rochester. I'm very pleased, and I'm sure Karl would be very pleased, to know that two of the three of them continue to run. Interlaken has terminated but, I think, appropriately.

McCallum: And there was also the Carnegie Political Economy Conference. So there were really four.

Meltzer: Yes, but that one was the one I did alone, that is, not alone, but with Peter Ordeshook, originally, then with Keith Poole and Thomas Romer. But let me talk about each one of them briefly. The Carnegie-Rochester is one that I perhaps have had the most input into. I edited the papers for 23 years or something like that. Karl would run conferences sporadically. Beginning back in the 1960's, he ran a

conference on econometrics. He had various conferences on different topics going all the way back to his time at UCLA. In the 1970's, we decided to regularize the conference and publish the papers. We called it the Carnegie-Rochester Conference Series on Public Policy.

The Public Policy part of it was very important to both of us. The reason for starting was that much of the policy analysis at that time seemed very ad hoc to us, as in the Brookings Papers at the time. We didn't think that that was the right way to go about it. The way to go about policy analysis is to think about policy within the framework of an economic model and to test hypotheses. The idea of the Carnegie-Rochester Conference when it began was to try to use tested current economic theory to apply to the problems of the day. That was the idea.

Then, of course, we were lucky, surely the only way to describe it was lucky, because in the first conference one of the players, Bob Lucas, hit a home run. He wrote his econometric policy paper.

I will always remember that conference. I was the chairman of that session. The formal discussant was Bob Gordon. After Lucas sat down, nobody said a word. There wasn't a question. Nobody said anything. I tried to stimulate a little bit of discussion. People, I think, were stunned.

Nowadays, we look at that paper as a classic. Everyone knows it. The paper showed that simulation of econometric models would not give a correct estimate for the effect of a change in policy. Of course, a similar point had been by Marschak much earlier, but not as powerfully. I don't know if everybody agreed that he was right, but there wasn't very much to say other than I agree with you or I disagree with you. Lucas, of course, is not one who is given to verbosity, so nothing was said and that was the end of that.

That put us on a very good track, and we made a deliberate effort to follow up. We thought, that paper was both the right kind of answer and the right kind of question to focus on.

McCallum: Then, to some extent in the early years, you promoted work that was in some ways following Lucas's line in macroeconomics and monetary economics.

Meltzer: Very much so. We encouraged people to do that. It took a while to get the first volume out, so that paper, I think, was actually presented at a conference in 1973 and it didn't appear until sometime in 1976.

McCallum: But it was the first paper.

Meltzer: Right, and then we deliberately determined to push that line and we initially decided that each conference would have a focus. We kept that up, Karl and I, doing this together. That became increasingly hard. Also, we wanted to anchor the conference firmly in the economics profession, so we invited in the first of many advisory boards to help us plan these conferences. And I must say I'm very pleased that, some 25 years later, the conference is still running. It's had support from the NSF through almost all the years since 1973 and I think it has produced some really good papers, several of yours. You've been a major contributor.

Then there's Konstanz. Karl went to be professor at the University of Konstanz, I think, in the summer of 1968.

McCallum: This would be as a part-time professor there?

Meltzer: Yes. That brought him back close to Switzerland, which he liked. He was very interested in seeing what was going on in Germany and in trying to help young German and Swiss economists. I was in Vienna at the Institute for Higher Studies, giving lectures in March 1968. I got a call from him asking me to go to Konstanz and talk to some of his assistants. He had them planning a conference and he wanted to get my ideas about where it should be, and what it should be, and so on. That was the beginning of the conference. The next year, he hired Manfred Neumann as his top assistant at Konstanz, and he and Manfred did most of the work. They eventually set up an advisory board, so I had a formal relationship.

Karl and I always had such an informal relationship that we would talk about everything. It didn't matter to me if he ran it with Manfred, and it didn't matter to him if he ran it with Manfred or me. Whatever we were doing, we were usually doing together. The aim of the conference was to try to bridge the gap he had observed between the quality of economics as it was taught in Europe and the U.S. at that time.

The best European scholars had emigrated, so there really weren't good teachers to teach the young people, to build the intellectual capital that would produce quality economics there. The aim of that conference was to try to do some of that. I think the conferences contributed, and it's certainly true that the gap between the United States and Europe is narrower now than it was in 1969. I think it's been very useful and I'm glad it also continues. Then there is Interlaken.

McCallum: Konstanz was a conference on monetary economics?

Meltzer: On monetary economics and monetary policy, right; it still is, so it's really a monetary and macro conference. We were very fortunate that the Bundesbank was very supportive. They would send top people. Dr. Schlesinger came many times, and other officials came to the conference. They gave us some financial support. The more important thing they gave us was the support of their top officials who would come down to the conference, deliver a lecture, and participate in policy discussions with the young economists. It was a very important part of their training.

Karl thought that you could criticize policymakers, and that they could be asked to answer. That went against what he thought of as the German tradition up to that time: that officials would be responsible and academics would have some opportunity to question them about what they did and why they did it.

In a polite, well not always polite, but usually polite, way we would have a discussion. Then, we would all sit down and have dinner or a beer together and still be friends even though we might have different points of view. He thought that idea in itself was important to get across. The Bundesbank officials, especially Dr. Schlesinger, were very helpful to us in getting it across. Dr. Irmeler, who was a director of the Bundesbank, came to the first or second Konstanz conference, and Dr. Schlesinger came many times. Other Central Bank Governors from Switzerland, the U.S., and England came to the conference. I think that helped a lot to get across that idea of policymakers exchanging ideas with academics.

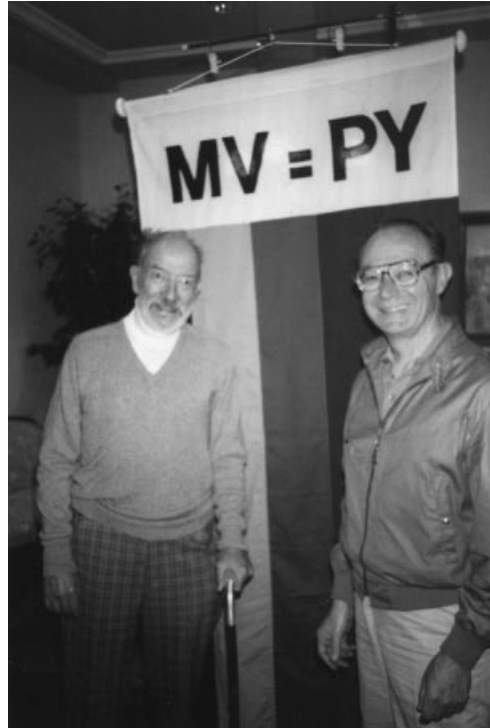


FIGURE 2. Karl Brunner and Allen Meltzer in Konstanz, Spring 1987.

The other thing that the conference did was bring American scholars to Europe. Many of them, including Rudy Dornbusch, Michael Mussa, Harry Johnson, Michael Parkin, Robert Barro, Bob King, Charles Plosser, and you, came and in a sense were part of the transmission belt for new ideas. Of course, Karl himself was also involved in trying to bring the best ideas.

That led to a major project on inflation which Karl and I did, with Michele Fratianni, Manfred Neumann, Peter Korteweg, and Andre Fourcans. The study was presented and published as a collection of Carnegie-Rochester papers. We tried to look at the influences on price movements and inflation. I would say the interesting part of that study was that the authors tried to sort out the monetary influences on inflation from the one-time tax effects and exchange rate depreciation on consumer prices. It is still the best effort I know to separate one-time influences on price levels from the permanent long-term influences of money growth. How much of the price movement was inflation would be another way of describing the content. Although we hadn't quite gotten in the 1960's to thinking that way, it was the start of our interest in separating permanent and temporary effects.

To work on the study, we would spend a couple of days after the Konstanz conference going through the papers that we were writing.

Then there was Interlaken. Karl got very concerned when he first went to Germany and later Switzerland, but even more in Germany, about the growing Marxist, or Neomarxist, influence in the universities. What Interlaken hoped to do was to bring together people from different backgrounds and to demonstrate through the papers that many of the problems that people talked about, the broader problems, not the direct allocation problems, but many of the other problems, could be analyzed using the tools of economics. It wasn't just a matter of a breakdown of the system or a failure of the market. Many problems arose because of restrictions which were placed on the market, so that it wasn't allowed to operate. Also, the kind of behavior that individuals showed in the marketplace was, broadly speaking, applicable to nonmarket situations.

We brought in some sociologists and philosophers, even some Marxists. I remember vividly, we had some well-known Marxist, I'm sorry I can't remember his name, who gave a paper. At the end of the paper, we had a discussion period. One of the questions was: What would falsify your paper, your model, the Marxist model? What would you regard as compelling evidence to make you think that this model was wrong? All he could say was that he thought that was a hostile question.

We had fun at those conferences, and I think I learned a lot. After 10 or 15 years, I thought we had accomplished most of what it was that we were going to get. We kept it going for a couple of years after Karl died, then I brought it to an end. I really thought it had accomplished most of what it would accomplish, and from my own point of view, I had completed the work I was going to do in political economy and was in the process of publishing a book of collected papers.

Finally, the Carnegie Political Economy Conference was the last of these. I got interested in political economy out of the Interlaken Seminar. I wanted to do it in a more formally analytic way than they were doing it at Interlaken. There were several people here at the time: Ordeshook, Rosenthal, Romer. Keith Poole was one of the first fellows of the political economy program. We hired him at Carnegie. We had funding from several foundations. We brought in some very good students and some very good postdoctoral students. The conference also helped to carry out the educational mission of our program by bringing in scholars who presented their work.

After a while, the interest in political economy that I hope we helped to nurture developed in the profession. It didn't seem that our activity needed to continue, so we ended the political economy conference. We continued the program at Carnegie for several years after that.

McCallum: Running all of those, organizing and editing all those conferences, is an awful lot of work. How did you find the stamina to do all of that?

Meltzer: I enjoyed most of it. I always think that one of the great advantages of being an academic is, if you don't like what you're doing, you just decide to change it. We have more freedom to do that than almost anybody else in society. I liked what I was doing. I was interested in all of those activities. Of course, reading and editing the papers for the political economy and the Carnegie-Rochester Conferences was

something of a chore. At the same time, it was a learning experience for me and one of the ways to keep me actively up on what was going on in the profession.

As you know, editing a paper and reading a paper are two different things. Editing a paper means you have to read it very carefully and figure out whether the author has pulled the wool over your eyes or somebody else's eyes. And that forced me to learn a lot of things that I wouldn't otherwise have learned, extended the length of my professional career, and taught me new techniques. I had to learn new things to find whether the papers were good or bad or foolish. That probably prolonged my intellectual life by forcing me to do things that I wouldn't otherwise have wanted to take the time to do. One of the reasons I kept the editorship of Carnegie-Rochester for as long as I did was that I was perhaps the most careful reader—I hoped—of many of those papers and learned a great deal from what the younger people did.

Now that I've moved off to do economic history and am really pressed to read Federal Reserve minutes and related materials, I really don't have the time to keep up on that anymore. I regret it in a way, but I'm in a different stage now. I think that the conference is better off if someone who is much more up on what's going on, like you, takes responsibility. I'm very glad it continues in such good hands.

McCallum: Well, all of those conferences that you and Karl did over the years made a very big contribution to the profession.

Meltzer: Thank you. I should add of course, none of this would have been possible if it weren't for my Administrative Assistant and Secretary, Jean Patterson and Alberta Ragan, who are terrific at keeping things running. I could tell Jean that we were going to have a conference with 50 people and not have to worry very much about the arrangements. The two of them took charge of all the arrangements.

McCallum: Yeah, Jean's been handling these for a long time, right from the start?

Meltzer: Yes, with Alberta's help.

McCallum: Now, another activity of yours and Karl's is the Shadow Open Market Committee?

Meltzer: Yes.

McCallum: Tell us a little bit about that.

Meltzer: Well, you can tell from earlier comments, a lot of these things start small and grow. In this case, the frustration was caused by the Nixon price and wage controls. In 1971, when President Nixon put on price and wage controls, we thought that was a terrible thing to do, a pandering or surrender to the very worst nonanalytic approaches to economic policy. We are going to solve inflation by controlling relative prices. I talked to Karl. We decided to put out a statement and say that it was a bad idea and why. To get other signatures, we called a lot of people. That turned out to be very time-consuming. We did get the statement out, and we did publish it in *The Wall Street Journal* and elsewhere. It was time-consuming because every time you changed something to satisfy one person you had to call all the other people and tell them about the change. We didn't have fax machines at that time, so we had to do all of this by phone. It was a terrible chore.

The end result was that I was glad it was done, and there would never be another one like that. But we believed that well-trained economists should comment on public policy issues, so we had to find another way.

Another frustration at the time, that to some extent is still true, was with the way public debate proceeded. We would read publications like the *New York Times*, or *The Wall Street Journal* or *Time* or *Business Week*. They often discuss issues by taking two extreme, but opposite, views. This is a disservice to the public.

Meltzer: They rarely ask: What is the cognitive issue between you? We thought of this as the journalist's fallacy: If you take the two extreme points of view, people would find the truth somewhere in between. We wanted to bring together a group of competent economists who shared certain general perspectives about major issues to talk about policy issues.

McCallum: Monetary policy issues?

Meltzer: Monetary policy issues initially. Then we went into fiscal policy, trade policy, and other macro issues. We still do, of course, but we always talk about monetary policy issues.

That's how the Shadow Committee got started. Karl and I thought that this would be a five-year venture, that we would put a zipper on it at the end of four or five years. But now, it has been going for something like 24 or 25 years. I suspect it's coming near the end of its useful life. The inflation rates are down. People have learned the principal lessons of inflation. The one remaining thing, which is a continuing problem, is that, while the inflation record in most central banks is much better, here in the United States we are still subject to what I would describe, and have described, as rampant eclecticism. The fact that policy action is much better is almost unrelated to theoretical work. It has more to do with better judgments by the people who happen to be on the Board and the committee. And, of course, they now know that central banks bear some of the costs of disinflation, so they are more cautious. The way in which they make judgments, however, is far from systematic rule-based policy.

McCallum: You are emphasizing the way in which the Fed does its analysis. I thought that when you started off you were going to say something about the targets, about the Fed's objectives, about the lack of any clear-cut hierarchy among its objective variables.

Meltzer: I think that we've come a long way on that. If we go back to the days when the Shadow started, central banks were concerned with the housing market and income distribution. They were concerned with all sorts of problems that no central bank these days would today think of as in its domain, but, it is true, as you point out that, in the U.S., we have not set a determinate goal of keeping the inflation rate or the expected inflation rate close to zero. I prefer the expected inflation rate to the actual inflation rate or the price level. One of those three should be the main objective of the Federal Reserve. But the process by which it gets to its objective is also a serious problem.

McCallum: So it's both of those things that you're concerned with.

Meltzer: And we really need to improve. We need to institute some kind of rule-like behavior, or quasi rule, or general operating procedure, or whatever name one wants to give it. We're a long way from doing that. So, while the performance is much better all over the world, here the process and the explicit choice of objective has not improved as much as they need to. To the extent that the Shadow has any influence, we'll try to talk about those issues and try to educate the press.

McCallum: Well, the SOMC is a little bit hard to figure out. This is just a group of you who got together and called press conferences and, by the sheer force of your arguments and ability to be persuasive, you managed to turn yourselves into an institution that the press takes very seriously and reports.

Meltzer: It is an amazing thing in a way in that testifies—

McCallum: How did you get that started?

Meltzer: Karl and I and two other people were the ones who got it started. One other person who had a large role was a man named Bill Wolman, currently the editor of *Business Week*. At that time he was working on Wall Street. He knew a lot about the press and we knew him. We had talked to him over the phone and I think had met him—he had been at some of our conferences—so we began to talk to him about how this should be done. I think it was he and I together that came up with this name. Open-Market Committee and Shadow, you know, putting together the idea that the British government has a shadow cabinet with the Open-Market Committee. That was a way of attracting attention. Then, together, we got up a list of people from the press, with the help of Lindley Clark who was at *The Wall Street Journal* at that time. He gave us attention. The first meeting—

McCallum: Where was it?

Meltzer: In New York.

McCallum: So you and Karl organized it and you had other people on the committee at the time.

Meltzer: We decided originally to have 12 people because the Open-Market Committee had 12 people. Wolman was, of course, one of them; Jim Meigs was another. We invited Anna Schwartz. Of the people who are there now, only two other than me survived from the very beginning: Anna Schwartz, who has always been a terrific member, and Bob Rasche.

McCallum: Bob was there at this time?

Meltzer: Bob was there then and has been ever since. Over the years, various people have been there. Jerry Jordan was there when he was not working at the Fed. Bill Poole was a member, until he became president of the St. Louis Reserve Bank. Jan Tumlir was there. He would come over from Geneva to talk about trade policy. He was the chief economist at GATT. He did a very good job, not a very forceful man, but a very good job of trying to get the press in the 1970's interested in trade and protectionist issues. Rudy Penner was there for a while before he went to CBO. As a result of being on our committee, he almost didn't get confirmed as the head of CBO. We had Wilson Schmidt who went on to become the U.S. Executive Director of the World Bank, and Beryl Sprinkel. Beryl was an original member who remained until he went to be Undersecretary in 1981.

All these members shared many of our general views about the role of government and about monetary policy without agreeing on all the details. The discussions would often be very lively. The press liked that. I can remember meeting in New York on beautiful football Sundays in September and being amazed that 20 people would come instead of watching the football game or going to the park. They came to hear us talk about monetary policy.

McCallum: Twenty members of the press?

Meltzer: Yes. They would come and participate and we still—we are in Washington now—we still get on a Sunday, 10 or 12 people. Some people come down from New York, from the financial community, to listen to our discussion, and some members of the press have been there almost from the beginning. John Berry from the *Washington Post* has been at almost every meeting of the committee. He used to come to New York. He usually asks a lot of good questions at the meetings. He usually has a lot of up-to-date information that he offers from the floor.

McCallum: At the very first meeting, you had then 12 of you who were the committee members, and how many journalists or press people came?

Meltzer: Well, the first meeting, we didn't do a press conference. We had our meeting. It was open. I can't recall how many reporters came, but there were a number. Then, at the end of the day, we issued a statement. We had to get them to come back to pick up the statement. The surprising thing was that many of them did. However, we never did that again. I learned, I think we all learned, out of that experience that we needed to prepare a little bit better for the meeting, then rewrite the statement. Now what we do is meet on Sunday; we discuss several issues. We go over the statement. We invite any of the journalists or others who want to come to the meeting and hear the discussion. We prepare papers including the statement before we meet. We revise the statement and, the next day, we hold a press conference and issue the statement. Many more people come to the press conference than to the meeting. In recent years we've had a lot of television, as well as newspaper, coverage. We still get the Japanese, the Germans, the Swiss, some of the Canadian press to our press conference. We still get a reasonable amount of press attention from the wire services and others.

McCallum: I see.

Meltzer: When we did this the first time we didn't have any idea as to how it would be received or what would be received. One of Karl's favorite expressions was "Well, let's give it a try." So we gave it a try, and a lot of journalists came and they seemed very interested in having this kind of discussion and criticism of policy. And most of them wrote articles that appeared in prominent places in their own papers, for example, the front page of the business section of the *New York Times*, *The Wall Street Journal*, and the *Washington Post*. We were very critical of what the Federal Reserve was doing at that time, as well we should have been. We pointed out that they had not learned how to control inflation and the way in which they were conducting policy was a mistake. The fact was that the inflation rate was rising rather than falling and the unemployment rate was rising.

McCallum: What year was this?

Meltzer: 1973.

McCallum: 1973?

Meltzer: Right, and things got worse after that. At that time the inflation rate was around 4 or 5%. Burns was chairman. Now one of the interesting things was that Burns regarded us—although we were critical of him—regarded us as a good thing. There were so many people on the other side talking about how we needed to have more expansive policies that he was happy to have a group saying we need less expansive policies to control inflation. Our committee has never favored reducing the inflation rate at any cost. Our aim has always been to reduce the inflation rate and try to find the minimum cost.

McCallum: Yeah, sure.

Meltzer: Let me put a zipper on this by saying that in thinking about how I ran my center here at Carnegie, the Center for the Study of Public Policy, I always thought there were three things to do: You have to do the research; you have to bring the policymakers and the academics together so that they can learn from each other, and of course a lot of these conferences had that intention. Bring the policymakers to Konstanz. Bring the academics to Konstanz. Let the academics talk about the problem. Let the policymaker say, “Well, that’s not really the problem.” We tried to get them to understand each other a little bit better. We always did that at Carnegie-Rochester and Konstanz. The third thing is to get that message out to a broader audience, to influence the way in which the debate takes place.

Karl and I engaged in research. That was our contribution to trying to solve some of these problems. We organized these conferences to try to get people together and discuss the issues in what we thought were useful ways. Then, the Shadow was one of the efforts to get these ideas out to a broader public.

That goes back to where we began these tapes. I came into economics interested in public policy and how society could work better. Certainly, I never dreamed in the 1940’s and 1950’s how this would all work out. Very much in the spirit of Hayek, you develop structures, institutions take form, and you find yourself doing things later that you would never have anticipated earlier. That’s essentially the way all this evolved.

McCallum: There’s one more thing I forgot to ask you about your relationship with Karl. How did the two of you write things? Would you each draft part of your paper and then exchange them, or would one person do the draft? How did it work?

Meltzer: Usually we would talk a lot on the phone over several weeks, maybe even send notes back and forth to each other on parts of the paper, working out some of the analytic parts. Then at some point somebody would say okay, I’ll write this up or you write this up. Depending upon time schedules and the kind of paper it was, and relative inclinations, one would write a draft. We would exchange drafts, and someone else would take the draft, throw away part of it, expand part of it, change part of it, add to it, and send it back. We kept doing this until we finished.

McCallum: So whoever drafted something first, it would get revised by the other one a couple of times. I'm asking because you can't look at your writings and see, well Karl wrote this part and Allan wrote that part.

Meltzer: The only time that that was ever really true was in writing the Mattioli Lectures. We did actually break out chapters and, because it was a longer study, said you write this chapter of the first draft and I'll write that chapter of the first draft. Even then we exchanged them and rewrote them, added to them, and so on. Usually one person wrote the paper and the other person revised it. We early developed a relationship in which if you didn't think that you liked something, you didn't, as Karl would say, do an egg dance. That's a Swiss or German expression. It is descriptive enough. We didn't do an egg dance. We said, look I don't like the way this thing starts or I don't like the way this thing is going. I don't think that's capturing the main idea we want to emphasize, so I'm going to change it. That was fine. We had a good relationship in that respect.

McCallum: Typically, for a given article, one person would draft the whole thing?

Meltzer: Yes, almost always, in fact always. I can't remember that we ever said you write half and I'll write half and put it together—until the Mattioli lectures. We never chose that way. One person always undertook to draft the paper. My experience has been that I learn a lot in the process of writing a paper. Things don't work out quite the way you think they are going to when you start out.

McCallum: Another interesting and striking aspect of your career was that you spent a remarkably long time in one place. You've been 40 years on the faculty at Carnegie Mellon and so I would like to know how you happened to come to Carnegie Mellon and how you happened to stay for such a long period.

Meltzer: Well, I came in a most unusual way. I was teaching at the University of Pennsylvania. One of my teachers, Armen Alchian, had told me that, if you're really not happy in a place, go to another place because, if you're unhappy, that will cut down on your productivity. You will spend too much of your time worrying about the fact that you're unhappy. And I have to say that my first year at Pennsylvania, partly due to the fact that I was writing or finishing my thesis, was a miserable experience. I thought the department was not an interesting place at that time. I wasn't very happy there. As I saw that my thesis was going to get finished that year, or looked as if it was going to be done that year, I started to look for jobs.

The way I looked for them was the only way I had available at that time. Karl Brunner was, after all, an assistant professor. He wasn't in a very strong position to help me. I just wrote letters to various places and tried to interest them in me. And a couple of them responded.

Although it was an unusual way to look for a job, that was the way I found this one. And I found it because Carnegie Tech was starting a program—the Indian Steel training program. The U.S. brought a group of East Indians in 1957 to MIT, Carnegie Tech, and other American engineering schools to teach them how to make steel. They came here, so we had to teach them a little bit of economics. A couple of our faculty were assigned to teaching in the Indian steel training program. Lee

Bach hired me to teach in the regular program, because he needed more manpower. That's how I got here.

McCallum: So the University of Pennsylvania was your first teaching job? And you got that job while you were in France?

Meltzer: I got that job while I was in France.

McCallum: The real question is how did things work out here at GSIA?

Meltzer: It was a great place when I came here. It was exciting. Simon and Modigliani were here. Muth was figuring out rational expectations. Miller and Modigliani were working on the cost of capital. Modigliani, Muth, Holt, and Simon were working on planning production and inventories. It was a very exciting environment. Ed Mansfield was working on innovation. In the course of the years, I visited other places. I had offers at other places, but I always liked it here. I liked it as a place to live. My family liked it as a place to live. The University seemed an interesting place to stay, and they treated me very nicely, so I didn't have any reason to leave. One of the great things about Pittsburgh that I learned when I visited at Harvard and at Chicago was that there are fewer visitors. Here you were really free to do your work. There wasn't a lot of interruption. I like that aspect of living in Pittsburgh and being at Carnegie Mellon. I always got a lot of support, so there really wasn't very much reason to leave, nothing more complicated than that.

McCallum: Do you think of yourself as a conservative?

Meltzer: No, as a libertarian, not as a conservative. Conservatives look to the past and want to restore it. They have a rigid dogma as well. It just happens to be a different rigid dogma from socialists. I'm a libertarian, that is, an old-style liberal.

McCallum: Do you have any big projects under way now?

Meltzer: Yes indeed. I am writing the history of the Federal Reserve. There are thousands of boxes of letters and minutes. It's an absolutely fascinating experience.

It has a brief history. It started with the report that Karl Brunner and I wrote for the House Banking Committee back in 1964. We wanted to turn that into a book. That required some editing and some work and I thought perhaps I would work on it for a year or two, then Karl would work on it, and we would have it finished as a book. And I actually did work on it for about a year on leave in 1966 I think. Following our procedure I worked on that. Karl was doing something else, and we would exchange drafts. He never worked on this. He always expressed great interest in it and enthusiasm for it, but Karl was a man who was going in many directions at a time. I often think of myself as having a fairly busy and active schedule, but his was much busier and much more active than mine. He went in many different directions. He never got around to doing anything on the history. I continued to work on it. I put it aside and came back to it in 1969 or 1970. I know that because I look at the old drafts as I start to write the book now and I see dates like 1966 and 1970 and 1971 on them.

I decided a few years ago that I really wanted to finish it, and I'm very glad that I waited all these years. It has worked out well. I have a different perspective. The records of the Federal Reserve are more available to me. I've had a lot more experience advising central banks in various countries and talking to policymakers,

so I write it from a somewhat different perspective than I would have written it in 1966. I have more sympathy for the problem of the central bankers, and the need to solve their problems.

I was always convinced that central bankers didn't deliberately cause the great depression in the sense that they said, "Gee it would be a good thing if we depressed the economy." Nor did they deliberately cause the inflation in the sense that they said, "Well, let's have an inflation." So there is a question: Why did they do it? What were they thinking about at the time that caused them to do it? What was the analytic framework—the framework that led them to believe that what they were doing was the right thing to do at the time?

With hindsight we see that it worked out very badly. That is a main theme of the history that I am writing. I am probably better equipped to write it than I was 30 years ago, when I started.

McCallum: Were there any particular things that you did that refocused your attention on that, such as the paper that you wrote for Saint Louis?

Meltzer: The 75th anniversary of the Fed came along. I was toying with the idea of doing the history. I was thinking about different projects that I wanted to take up after I finished the book on Keynes. I had put together the papers on political economy and published a book. I had given the Mattioli Lectures with Karl. In a sense, I had kind of put a finish on things I had worked on for many years. The question was, what do I want to do next? I was thinking about different things. Then the invitation came to give a paper for the 75th anniversary, so I thought I would look at the international side of the Fed and the breakdown of Bretton Woods and all that and see if I wanted to get into that set of issues as a way of going back to my book and writing it.

Then I had to give a lecture, the Homer Jones Lecture, in Saint Louis. I wrote another paper which began to bring the material I had stopped working on in the 1960's up to date. I used that occasion to do some of those things. Then, in 1994, I made the decision that I really wanted to write this book. I knew it would take me somewhere between perhaps 5 and 10 years to go through all the material. I decided to make the commitment, and that's where I am now. I've got 3 years in, and I've got about a quarter of the book drafted.

McCallum: For the 75th Anniversary of the founding of the Fed, you wrote a paper for a conference?

Meltzer: Yes, the Saint Louis Fed Conference.

McCallum: So there were really two papers for St. Louis that you wrote at different times that got you back into this historical work.

Meltzer: Right. Then I just began work and slowly plodded through the minutes with the help of some able assistants who are reading some of the voluminous papers. I get excellent comments from people who are interested in this subject. Oftentimes, you send out a paper and if you get back one or two comments that's a lot. I get back many really quite good comments from people who are interested in economic history and monetary policy. It's going to be a very big book, the chapters so far are running 150 to 180 pages. I'm trying to be comprehensive.

McCallum: Sounds like a project that probably already has attracted a lot of attention.

Meltzer: I hope so.

McCallum: That's about it for my questions. Are there any things you think we have left off that you would like to talk about?

Meltzer: About the only thing left in my academic career which I think we didn't mention was how I came to write a book on Keynes.

McCallum: That's a good topic.

Meltzer: Well, it is really the story of how a simple step leads you farther and farther along a path. Research is very much like that. Mark Perlman had asked me to write a paper for the first issue of the *Journal of Economic Literature* that he had started. I had written a paper for the first issue on money, financial intermediaries, and economic growth. He asked me to write a paper for the last issue that he was going to edit. We had been talking about Keynes and I had been reading some things that Don Patinkin had written about Keynes. I thought he had it wrong. He saw the General Theory as a theory of the depression and a case of fiscal policy.

My recollection of Keynes was that the fiscal policy or budget policy practically doesn't enter the General Theory. I thought there was a different view of Keynes and I had been very much impressed with the importance of this distinction between permanent and transitory changes. In rereading parts of the General Theory, lo and behold there it was—rational expectations in the short run. He uses some words, I can't remember them exactly, but the idea is that we might just as well assume that the actual equilibrium is the same as the expected equilibrium in the short run. That's what he says. But, he says, over the longer term we can't use that expectation. He asks: What is the probability that capitalism will survive into the 1970's? Or what is the probability that there will be a European war? About such things, Keynes says, we have no prior information.

Keynes, of course, was a probabilist. Since I had just started thinking seriously and was at that time working with Karl and later Alex Cukierman on the idea of building permanent and transitory changes into a general equilibrium model, that really struck me as something that I thought was a good way to think about the problem. I wrote a paper for Mark. I got part of the idea down.

Research takes on a life of its own. Soon it was the 100th anniversary of Keynes's birth. They invited me to Cambridge to give a paper. They didn't like the paper at all. I think if I ever bombed at a conference, it was that conference.

I didn't change my mind. Maybe it would be impossible to explain it to them. In any case, I thought that it was a good and correct idea. Then some other group asked me to give a paper on some aspect of the General Theory.

Colin Day of Cambridge University Press asked me if I wanted to expand the first paper into a book. He was making a practice of taking some of the interesting papers out of the *Journal of Economic Literature* and expanding them as books for Cambridge. He asked me if I wanted to do that. I said no, I didn't. After I had written these three or four other papers, I thought, now I will put these together. Around that time, the 30 volumes of Keynes papers appeared, so I thought, I shouldn't do

it in a slipshod, haphazard way. I might as well do it right. So I'll read the better part of these 30 volumes to see what I find.

What might have been a six-month or a one-year project of putting together the four papers became a several-year study in which I read most of the Keynes papers, and comments and letters and other things. Keynes was a prodigious writer. You come away with a great appreciation for his ability to get right to the core of a problem in one sentence.

That's how that got started. Eventually I wrote a book which I am happy to say is still in print and selling. Then, Jim Dorn did me a great favor. He organized a conference to discuss my book before it was published. I don't know if that often happens. He invited a lot of the people who had worked on Keynes, including Milton Friedman, and we went to a conference. Everybody read at least one of my chapters. They were all written at that point except for the introduction and conclusion. They read it and commented on it. I tape recorded the whole thing and then went back and spent another year trying to answer their questions and incorporate them into the text. The marginal product was certainly positive. I don't know if it was equal to the time cost or not. Who knows?

McCallum: Jim Dorn works at Cato. Was this a Cato-sponsored thing?

Meltzer: Cato arranged the conference, and Jim Dorn and Bill Niskanen came to the conference, but it didn't appear as a Cato publication. It just appeared as my book. It was just a nice thing that he did. He thought it would be an interesting thing to do. They did all the work of getting it organized and getting the people to come.

McCallum: Without taking any major credit or anything.

Meltzer: Yes, I thanked them of course in the preface, and I'm thanking them now. That was really a great service to me. It's painful to go back over all these manuscripts and rewrite them, but it made the book, I think, much clearer and better than it was because it answered some of the questions.

Despite some discussion after the book appeared, I remain convinced that I have the right idea.

I can't leave this topic without telling an anecdote about one of the things that happened at the conference. Milton Friedman was there and we also had Donald Moggridge, who edited Keynes's papers. Milton read from the copy of the *General Theory* which he had there. And Moggridge said, "where did you get that quotation?" Milton said "it's right here in the *General Theory*." Moggridge asked him which edition he had. Milton said, "of course the edition I got in 1936." Oh, said Moggridge, Keynes revised that particular paragraph and it's not in the edition that most of us use.

McCallum: And that had been revised?

Meltzer: Evidently.

McCallum: Not a new edition?

Meltzer: To correct the printing.

McCallum: It must have been a very touchy point for Moggridge to—

Meltzer: Or it was just a mistake perhaps. I don't remember what the point was, I just remember the story.

McCallum: We haven't talked much about your work in government. What are some of your experiences?

Meltzer: As I said at the outset, I have a long-standing interest in economic policy and a more recent interest in the policymaking process. These led me to welcome the opportunities that I had early in my career to work in government and to observe some parts of the political process close up. Later, I became less interested because my early observations caused me to wonder whether the influence of ideas is not more powerful than the influence of administrators. I guess I've come to a conclusion that the chance of making a decisive impact is small either way, but smaller inside than outside. But much depends on what one wants. There is fun and excitement in being involved in the discussions leading up to a decision, but there is a lot of frustration also.

Looking back, I think my friends and I on the Shadow Open-Market Committee probably had a useful influence on central-bank policy by emphasizing monetary control as a necessary condition for control of inflation. By repeating our message frequently and criticizing policies and procedures, we established a record. When events confirmed much of what we said would happen, central bankers began to change. Of course, they haven't adopted monetary control, but they haven't let the money growth explode. I believe they give it more emphasis, certainly in Germany, but even here, some FOMC members pay attention to money growth.

I don't know how much to attribute to our efforts. I did learn that it is difficult to produce policy changes even when there is substantial agreement on the analysis. Much harder is getting a change when the academic specialists are divided. Some examples are indexed bonds or sugar quotas. Most academic economists who took up these issues in government came to similar conclusions. Nothing was done until recently on indexed bonds. I give credit to the Clinton Treasury for getting some indexed bonds. I could not get that done when the Treasury was selling 14% long-term bonds.

Those are small issues. Big issues are much, much harder. The big changes come usually after there is a crisis, or perceived failure, of the old policy that can no longer be denied. Fluctuating exchange rates, the draft, disinflation, and now educational reform are examples.

McCallum: You mentioned that you worked in government early in your career: When and how did you get started?

Meltzer: My first experience was in 1960. The Joint Economic Committee commissioned a study called Employment, Growth, and Price Levels. Congressman Patman was interested in the dealer market for government securities, so they collected some data from the dealers as part of the study. Eli Shapiro, then at MIT and later at Harvard, was involved in the study. I think Otto Eckstein was one of the principals. The people who worked on the study did not want to do anything about the dealer market. I can't recall how Eli knew me, but he recommended me. I didn't know much about the market then, so I invited Gert von der Linde, a colleague in finance, to work with me. Together we published a study of the dealer market. It was probably the first glimpse people had into how the market was organized.

The study had a lot of description. The main conclusions did not support Congressman Patman's view that the market was monopolized. The interviews we did and the data we analyzed didn't give much support to that view, and we said so. That pleased the dealers who were wary of us because we worked for Patman. It also pleased people at the Fed. Some of them told us that they found the study informative.

Two things happened as a result. Bob Roosa invited me to go to work in the Treasury as an assistant shortly after his appointment as Undersecretary at the start of the Kennedy administration. About two years later, Congressman Patman asked me to do some more work on the dealer market for government securities.

I went to the Treasury for two years but stayed much less. Roosa didn't seem willing to keep his agreement to let me observe and be part of the policy decision process in the Treasury. Also, I thought his policy of keeping interest rates high for balance-of-payments reason made little sense. If gold flowed out, it would return when we recovered from the 1960–61 recession. Why delay the recovery? I resigned but remained as a consultant. The main things that came out of that experience were a better understanding of what it was like to work in government and a book on the taxation of municipal-bond interest that I co-authored with the late David Ott. I started the work at the Treasury. It became part of a very large study of taxation run by Joe Pechman at Brookings. Hard to believe that I worked part-time at Brookings in the summer of 1962 probably.

The other opportunity was much more fruitful. I convinced Congressman Patman to focus on the Fed, not the dealers. It wasn't too hard. Karl Brunner joined me in the study. I talked about this work earlier and how it leads into the work I'm doing now on the Fed history. We learned a great deal from that study.

Over the years, I have done some work for foreign governments, sometimes by invitation of the government or central bank, sometimes sponsored by the World Bank. For example, I spent four summers in Brazil during the late 1970's giving a course at the Vargas Institute in Rio and acting as a consultant to the central bank. I particularly enjoyed those years. I left because I became an adjunct professor in London and worked on some of the early programs to help the new Thatcher government.

I liked working in South America. The variations are much greater, so it is often easier to see what is wrong. And the harm done by some of the policies is great, so a change for the better, if it is carried through, has a big effect on people's incomes and their lives.

My experience in the Kennedy administration was reinforced by being around policymakers. I had little desire to serve on a regular basis. I finally agreed to go to the Council of Economic Advisers at the very end of the Reagan administration. I knew that I would only be there for 4 or 4½ months. I think I surprised Beryl Sprinkel, the Chairman, by saying yes. I had previously always said no.

Although the administration was winding down, I had the experience of participating in discussions leading up to the Canadian-U.S. free trade agreement, supervising the last economic report of the Reagan administration, making the

forecast on which the government's budget is based, and being involved in the usual work of the CEA. Beryl was a great person to work with. He involved the staff and the other members in most of what he was doing. I was glad to have the experience and just as glad that I was only there for a few months. I believe I got the benefits and learned a lot without paying much cost.

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