Macroeconomic Dynamics, **8**, 2004, 395–417. Printed in the United States of America.
DOI:10.1017.S1365100504030202

MD INTERVIEW

AN INTERVIEW WITH ANNA J. SCHWARTZ

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April 21–22, 2003

The *Encyclopedia Americana*, 2002 edition, concludes its entry on Milton Friedman as follows: "His major work, *A Monetary History of the United States, 1867–1960*, was published in 1963." That might be seen as an indirect compliment to Anna J. Schwartz, who coauthored the *Monetary History* with Friedman but is not mentioned in the encyclopedia entry. Similarly, the Nobel Committee neglected to mention Schwartz when, in awarding Friedman his Prize in 1976, it described *A Monetary History* as "[h]is major work [and] . . . one of Friedman's most profound and also most distinguished achievements." Fortunately, the economics profession as a whole has not been so negligent, and the phrase "Friedman and Schwartz" has become second nature to economists when discussing the importance of monetary policy.

Beside her collaborations with Friedman, Anna Schwartz is perhaps best known for her longevity, which is on an epoch-shattering scale. Her career as an economic researcher began a quarter-century before the publication of *A Monetary History*, and has continued in the 40 years since. Schwartz's first journal article was published in May 1940, the month Winston Churchill became Prime Minister of the United Kingdom and over 18 months before the United States entered World War II. She has worked at the National Bureau of Economic Research in New York City continuously since 1941. Monetary economics has been a constant interest for her, and she is the only person to have had items published in the inaugural issues of both the *Journal of Money, Credit, and Banking* (February 1969) and the *Journal of Monetary Economics* (January 1975).[1] The *JME* moved into its fiftieth volume in 2003, and Schwartz became one of only two authors (the other being Thomas Sargent) to have published in both volumes 1 and 50 of the journal. With the NBER branching out in the 1970's into a national network of researchers, Schwartz has been for a quarter century an unmistakable fixture at the NBER monetary economics program's regular meetings at Cambridge, Massachusetts. At the time of the interview, her contributions to the NBER's

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 1365-1005/04 $12.00

Figure 1. Anna J. Schwartz.

various working paper series spanned from NBER Technical Paper No. 4, 1947, to NBER Working Paper No. 9321, November 2002. The latter paper covered the issue of equity price behavior, the same subject of her 1940 publication, implying that Schwartz had entered a remarkable seventh decade of research in that area. Much of Schwartz's early research was on UK economic growth and fluctuations in the nineteenth century, and was reflected in a two-volume study essentially completed in the early 1940's but not published until 1953 [Gayer et al. (1953)]. Robert Lucas, who read the volume as a graduate student in history, describes it as "an amazingly ambitious and exciting mix of history and theory."

Schwartz's collaboration with Milton Friedman on the relationship between the quantity of money and other variables began in 1948. Their early findings on the importance of money were reported by Friedman in a 1952 *American Economic Review* paper, but, by and large, he had problems promoting their work in the 1950's. Friedman's solo work in the fifties on flexible exchange rates, the consumption function, and the limits of stabilization policy would cement his reputation and be cited in his eventual Nobel award, but the monetary policy studies initially made a much more limited impact. All of Friedman's remaining 1950's writings on money were in "in-house" University of Chicago publications or in congressional testimony.[2] Two books by Friedman in the early 1960's previewed some findings from the monetary history project with Schwartz. The first of these, *A Program for Monetary Stability*, is now recognized as a classic, but its muted reception is evidenced by the fact that economists have never been able to agree on which year it was published. The second book, *Capitalism and Freedom* (1962), was intended for a wide audience, but was virtually driven underground when all major U.S. newspapers declined to review it. In 1969, a Federal Reserve Bank of New York official, Richard Davis, gave this perspective on reactions to early monetarist work of the 1950's and early 1960's: "[E]conomists regarded this group—when they regarded it at all—as a mildly amusing, not quite respectable collection of eccentrics .... The fact is that the view held by Friedman and others on the predominant importance of money was just not given serious attention by most economists."

The turning point was in 1963, when Friedman and Schwartz's 15 years of research finally saw print in their *Monetary History* and the article "Money and Business Cycles." The *Monetary History* is justly celebrated, and has remained in print for 40 years.[3] The "Cycles" paper, as Davis acknowledged, provided "[b]y far the largest mass of evidence" on the cyclical relation between money and output. In addition, the paper's "tentative sketch" of the monetary transmission mechanism became a cornerstone of the monetarist literature, and an inspiration to many monetary economists, including the late Rudiger Dornbusch, as they endeavored to escape the "single interest rate channel" view of the transmission mechanism.

Schwartz began the 1970's with another collaboration with Friedman, *Monetary Statistics*, and went on in 1973 to join a number of economists, including fellow monetarists Karl Brunner and Allan Meltzer, in forming the Shadow Open Market Committee (SOMC). The SOMC commented regularly on U.S. economic policy and, in particular, offered monetary policy recommendations to address the severe inflation problem of the 1970's. Schwartz remains on the SOMC 30 years later, the only founding member to serve since the SOMC's inception.

Friedman and Schwartz's final major collaboration, *Monetary Trends in the United States and the United Kingdom*, was published in 1982. Among monetary economists, the response to *Monetary Trends* was mild compared to the reception for the *Monetary History*. Work by Friedman and Schwartz and other monetarists had already changed macroeconomic thinking substantially, and most academic work was now concerned with rigorous modeling of short-run dynamics, rather than the empirical evidence on long-run relations that concerned Friedman and Schwartz in their *Trends* study.

Schwartz's own work over the past 20 years has been prolific, and has often been in collaboration with Michael Bordo, with whom she has written some 25 articles. While her areas of research have included banking regulation and the role of international financial institutions, Bordo's and her mutual interest in monetary economics and economic history has repeatedly been evident in their work. Among the issues that their studies, sometimes with further collaborators, have addressed are the role of monetary targets in Canada and the United Kingdom in the face of money demand instability, whether monetary policy rules could have avoided the Great Contraction, the historical record of alternative monetary policy regimes, and the history of economic thought, including the development of monetarism.

Keywords: Monetary History, NBER

Nelson: You did your undergraduate studies at Barnard College?

Schwartz: Right. Certainly not the most scintillating economics department. This was antediluvian, of course, but essentially it was an economics program that had price theory, although they didn't call it that; a course in money and banking; a course in labor economics; and statistics. That was pretty much the fare. And I majored in economics, as I'd always intended. I certainly tackled lots of other areas as an undergraduate, but they never made me feel I should shift from my original intention.

Nelson: And where did your interest in economics start?

Schwartz: I had a course in economics at high school. It was part of the curriculum, which became unusual in later years. It just seemed to me that the most interesting questions I was confronted with were in the economics course—not in history, not in languages, not in literature.

The economics department at Barnard consisted of three women and one man; the woman who taught statistics I thought was well prepared; the other two women, I don't think had really enlarged their understanding of economics from the time they'd done graduate work. The man came from England, that was Arthur Gayer, and he was probably the most knowledgeable of the group, and I really worked more with him than with the women, so that essentially I was introduced to monetary economics by him. And through him, I continued after I graduated from college on this British study. He had done a study at Oxford on British unemployment. I guess he didn't start as early as 1798, starting sometime in the nineteenth century, and he thought the British economy was a good subject: it could be valuable for his own career, and Wesley Mitchell was interested in a study of British business cycles. And Gayer probably was introduced to him simply because people in Barnard had connections with the graduate school at Columbia. Mitchell supported having this as a research project, and got the Columbia Council for Research in Economics to support him.

Nelson: And you went on to study at Columbia.

Schwartz: Yes. I was in Angell's class from 1934 to 1935. So it was pre-*General Theory*, but we pretty much read whatever Keynes had written from the 1920's on. We spent a lot of time on the *Treatise*. We read Fisher and we might have read Dennis Robertson. There wasn't much structure to the course; it was a course of readings. It was as if there were original books, and this writer talked about that, the next one talked about something else. There was no "end-of-the-year feeling" that I'd really got a solid understanding of monetary theory. The truth of the matter is that I didn't think that my education in economics was really attended to until I started working with Friedman. And it was as if he was my real instructor in economics, because there was a structure to what he was talking about that was absent from Angell's teaching. Although Angell was all for doing research in money, and I think in his own work anticipated some of the findings that Friedman and I really thought were significant when we were writing.

A lot of people think how odd it is that even though I was a graduate student at Columbia in the middle 1930's, and got a Master's there, I didn't get a Ph.D. until 1964. Well, the expectation was that this British study would yield my dissertation.

But Arthur Burns, who was then *the* business-cycle person at Columbia, objected to my using a part of the final manuscript of the British study as my dissertation. He insisted that it had to be an independent study that would not be part of some larger work. I didn't pursue the issue; by the time he'd made this decision, I was already working with Friedman, and I thought to myself, "I'll surely get a dissertation from the work I'll do with Friedman." And I finally did get a Ph.D. on the basis of the joint work with Friedman. Even then, Burns said "Well, are we setting a precedent by permitting someone to offer joint work for a dissertation?" But it *wasn't* a precedent, because this had already happened in the case of Friedman's dissertation, which was based on his work with Kuznets. Although I will say that Friedman had recast the manuscript that he and Kuznets had prepared in such fundamental ways that it was truly more Friedman's dissertation than it was Kuznets', whereas I couldn't claim that *A Monetary History* was basically mine; it wasn't—it was really a shared work. Except that, you know, I had probably done more work on it than anyone does on a dissertation.

Arthur Burns taught the course at Columbia on business cycles after Mitchell stopped teaching it. But I didn't take that course, because by that time I was working on the British business-cycle study, and I would go to the National Bureau, and I learned by doing. There was a business-cycle unit at the National Bureau, and I learned the way the Bureau approached cyclical analysis. It really wasn't theoretical; it was more "this is how you determine turning points in individual series," and then how you slice up any cyclical period into the subdivisions of time that they adopted. And we just approached the British cyclical experience in exactly that manner.

Nelson: You were working on the British study at the NBER?

Schwartz: No, the British study was basically finished by then. Actually, I worked in Washington for half a year. Under the New Deal, the Department of Agriculture got money to do a survey of consumer purchases. And the woman who taught statistics at Barnard was participating in that study. I had finished taking the coursework for my Ph.D. at Columbia. She said to me, "Why don't you come to Washington? I'll recommend you. You'll get some experience in the way a big survey is conducted." And it was a very exciting thing for me—I'd never been anywhere. I was sent mainly to southern cities, where there were local groups that were distributing the survey questionnaires, and I was sent to make sure that they were following the instructions that Washington had sent—I was supposed to examine their schedules and so forth. And that meant I went by train, which was the only way to go, to various southern cities. And I was put up in hotels—I mean, for me, this was all new.

Anyhow, I came back to New York at the time that the funds for the British study were available. And that was the fall of 1936. And that's when I started working on the British study. That study was essentially over by the end of 1940, I guess. The manuscript was five volumes, but the Columbia Council said there wasn't enough money to publish five volumes. We would either have to condense five into two, or just scrap it. Well, then the war was underway and we couldn't find a publisher.

Rostow was really the chief person on the study—Gayer didn't really undertake to do any part of it. Rostow was very eager to get this published; he was just starting out.

Nelson: He was at Columbia?

Schwartz: He had come to Columbia after having finished his dissertation at Yale. It hadn't yet been accepted, he needed a job, and Gayer offered him the chance to work on this study. Rostow also taught at Barnard at some point. During the war he went off to Washington and after the war got a job at MIT. Then in early 1950 I think, he was a professor at Cambridge [UK], and was able to interest Oxford University Press in publishing it, because the Columbia Council agreed to give OUP the money it had set aside for publication.

Nelson: The manuscript was essentially finished since the early forties?

Schwartz: It was just sitting idly; nothing was happening.

Nelson: And by the time of publication, one of the authors had died in an accident.

Schwartz: Yes, Gayer died in 1951 in an automobile accident. There used to be a very circuitous turn on the West Side Highway around 17th Street. He misjudged it and was severely injured, and died a couple of days later.

Nelson: But the project had been finished by then.

Schwartz: Well, not only that, but he didn't really participate.

Nelson: The book uses a lot of NBER techniques.

Schwartz: Yes, the techniques were National Bureau, and the analysis of cyclical behavior was National Bureau. And that's why it didn't get good reviews. R.C.O. Matthews, who wrote one of the major reviews, was just scathing. By that time the National Bureau technique had already been severely harmed by the review of *Measuring Business Cycles*—you know, "Measurement Without Theory." In more recent years, people have looked at the book again and have found something of value, even though the National Bureau method hasn't gotten any more approval.

Nelson: In the introduction of the reprint of the book, in 1975, you repudiated some of the monetary analysis in the book.

Schwartz: Yes, that was clearly the influence that my work with Friedman had had on me. Rostow, on the other hand, was still a confirmed Keynesian. He was very gracious about my saying there was something wrong with our earlier analysis, but he never thought money was a very important variable in explaining how economies worked.

Nelson: What did you work on when you came to the Bureau?

Schwartz: The Bureau in those days had two features. People were collecting time series, and there was great emphasis on making sure the data were accurate. For example, one person might be assembling a time series, and somebody else would be told to collect the same series. And if there was a discrepancy, people had to account for the differences. When I got to the Bureau, the sector that hadn't been assigned to anybody was money. So my first assignment was to see if we could put together a series on U.S. money. And I began with currency series, and then went on to deposit series. Some time after I had been working at the Bureau, Burns said to me, "What would you think if Milton Friedman were to join you on the money project?" I said it would be great.

Nelson: Had you met him?

Schwartz: My recollection is that we met when he was at Columbia during the early part of the war. He and Rose had just had a child, and you couldn't buy a stroller. My older son no longer needed a stroller, and somebody must have said to them, "Why don't you see if you can borrow it from Anna?" So they came to the door of my apartment, Rose and Milton Friedman, and rang the bell and said somebody had suggested I had a stroller. He'd been in New York during the 1930's and 1940's for his dissertation work, but apart from that one meeting I didn't really know him. Then there was a big gap between the time he finished working on his dissertation and the time it was published, because the Directors of the Bureau, who had to approve any publication, made a big fuss because one of the positions of the Friedman-Kuznets manuscript was that the American Medical Association was a monopoly. The Directors thought the National Bureau shouldn't publish a provocative piece like that, but the Bureau eventually did publish it. So they just held up the publication; I don't know how that was finally resolved because the original position of Friedman and Kuznets is certainly still in the published version. And I doubt that Friedman would ever have agreed to take it out.

Anyway, Friedman came and we talked about what we should do, and the agreement was that I would go on putting together a money series.

Nelson: He was not a monetarist then, though he had worked on policy rules.

Schwartz: He had never really done anything with money. He certainly was familiar with Bureau methodology—he had contributed to it. At that point Friedman was regarded as a statistician, and not particularly an up-and-coming economist. If you look at the macro things he had been involved with, his work on inflationary gap and the role of taxation in controlling inflation, there really was no role for money in his analysis.

At the beginning I saw him very rarely. He was in Chicago, and I would get in touch with him very infrequently, only when I had a problem. In those days you didn't pick up the telephone the way you do nowadays—it had to be something very urgent to make a phone call! I would simply write a letter to him, and he would answer it. It took a number of years before we had a final money series. Some of it was based on work done by people in his Money Workshop in Chicago. Friedman would think of dissertation subjects for his students that would be useful for our project.

And it was when we began to chart the semiannual and monthly data, and we began to see what had happened to money over this extended period, that the whole thing seemed to come alive. The first output of the project was really Friedman's paper on the three wartime inflations. And once he had done his restatement of the quantity theory, the theoretical structure for our history came together.

We finished a draft of the *Monetary History* by the late 1950's, but one of the directors' comments on the draft was, "Why didn't you use the Harrison papers that are at the Columbia Rare Book Department?" Well, nobody had ever told me about them. So, essentially, we rewrote the manuscript after I had gone to Columbia and gotten copies of the relevant portion of the Harrison papers—the middle twenties to the end of the thirties. That produced a very substantial revision. Over and above that, with Bureau publications, there were enormous gaps of time between what the author thought was the completion of the manuscript and the final published version, because the Bureau required passing the Reading Committee and the Directors' committee, and you had to respond to their criticisms. It's very different now, with the NBER Working Paper series; you turn in a working paper, and the Bureau puts it out; it's not responsible for the quality of the work.

Nelson: And the original title was something like *The Stock of Money in the United States*?

Schwartz: Yes. The original idea was that ours was one of several parallel NBER studies of aspects of the business cycle, at the end of which you would somehow see how the business cycle operated overall. And the idea was that we would analyze the money series and come up with the kind of stuff that we presented at Carnegie Tech in 1962—"Money and Business Cycles." That's kind of what the National Bureau would have expected. But it became much more historical, and not a conventional Bureau study of the business cycle. We looked at the data and said: "There are so many episodes here; you ought to be able to write a history of money here, based on these fluctuations."

The National Bureau was not sympathetic with the views we eventually expressed in the *History*. They certainly didn't believe that money was the touchstone for what happens cyclically. I think not only Burns but Geoffrey Moore, who was next in command, thought that our use of rates-of-change of money was not really justified. I remember I showed Moore a chart of the rate of change of money, and his comment to me was, "It looks just like wheat production, but don't tell Milton!" They all recognized that Milton knew a lot more than they did, but they didn't really believe the findings that we came up with. And as I say, they weren't sympathetic. And it was only when the book sort of took off on its own, and people started paying attention to it, that whatever misgivings the National Bureau had became subdued. And I guess if there had been somebody less forceful than Milton involved, they probably could have squelched it.

Nelson: How would the quantitative work on money be done in the fifties and sixties? How would you get a regression done?

Schwartz: The computations were done at the Bureau rather than Chicago, and in those days it was all done by punch cards. Even when John Meyer was the Bureau president in the seventies, somebody used to come from Yale, collect the cards for the regressions, bring them to Yale, and come back to the Bureau with the printouts. And there were people at the Bureau who did the punching. We were still punching cards right up to the late seventies. It was only after I did the report for the Gold Commission [1981–1982] that I got a computer. Up to that point I'd typed drafts of the Commission report, and if I corrected a draft, I'd hand it to the Bureau typist to create a new version.

FIGURE 2. Schwartz with Milton Friedman and Marty Feldstein, NBER conference, New York, October 1987.

Nelson: The structure of the Bureau has changed quite a bit in the past 30 years or so; it's more a vehicle for distributing research than organizing research projects.

Schwartz: Yes. The Bureau essentially operated with grants from foundations. Well, in the sixties, the Ford Foundation rejected the application. Partly it was the background that "Measurement Without Theory" had been a very serious assault on the National Bureau's reputation. But what they also said was, "You call yourself 'National' Bureau; you're just a group around New York. Columbia, NYU; where's the 'National'?" They said if we wanted to be supported we had to be a much broader network of people doing research. When Marty Feldstein came aboard in the late seventies it was in light of events like that, and he overhauled the structure of the National Bureau. The broadening of the research program beyond business cycles, which had already begun, became more dramatic, and the membership of the Bureau became one more of academic researchers working independently from one another. Also Feldstein changed the headquarters of the Bureau from New York to Cambridge [Massachusetts]; the unit in New York is now quite small.

Nelson: You worked for Arthur Burns for many years. Was he an easy person to work for?

Schwartz: No. He was a very arrogant man. He was quite different from Wesley Mitchell in personality. Wesley Mitchell was a gentleman; he would never say anything to somebody that wasn't civil. Whereas nothing suited Arthur Burns

https://doi.org/10.1017/S1365100504030202 Published online by Cambridge University Press

better than cutting somebody down. And he did it not just to underlings, but to people whom you would otherwise think of as his peers. So, in many respects, he was feared. He thought he knew more than anybody else. And that's why, in his relations with Congress when he went to Washington as Chairman of the Council and then as Chairman of the Fed, he was looked up to as some kind of God-like creature who understood everything. There was nothing you could ask him that wouldn't get what sounded like an authoritative answer. But I guess the fact that he held these important positions tells you that he was somebody that could get the kind of respect that not many people have. Milton never wanted a policy position, whereas Burns was delighted to have it.

Nelson: His time as Federal Reserve Chairman was not a very happy period.

Schwartz: No, it wasn't. I think he was a disaster, probably because he didn't listen to Milton. There was a real rift between them. When Milton first knew Arthur, Arthur was definitely the senior person, and Milton was very much the up-and-coming young man. But, over the years, Milton became the more influential person. When Nixon closed the gold window and they went for price controls and incomes policy, Milton wrote Arthur a scathing letter saying he never would have believed that Arthur would do something that was so contrary to what he believed were Arthur's principles. That letter apparently is in Arthur's archives at the Gerald Ford Presidential Library, and it won't be available for perusal until 2005. Milton says that somehow he never kept a copy of that letter; he wishes he had.

So, Arthur just didn't stand up for something he believed in, I guess because he wanted to continue in the Fed Chairman position. He was clearly under political pressure to inflate. At the same time, he believed that there was cost-push inflation that incomes policy could suppress. But he certainly didn't end his life in glory.

Nelson: I imagine that, when you began your career in the thirties, the presumption for women, even if they had a degree, often was that their career would come to a grinding halt once they got married or once they had children. That certainly has not been the case for you.

Schwartz: No. I had two children by 1942. Burns had two children too, and said to me: "Now, don't have any more children, because you don't have time for anything else when you have children!" Well, I did have two more children, but as I've told other people, I have always had help at home. A live-in person who was just as much a mother to my children and whom they all loved. I never felt she was preempting my place; I was a mother when I was at home, and when I was in the office I was doing what I wanted to do. And they've grown up and seem to be OK.

When people say to me: "Wasn't it unusual for you, a woman, to be an economist?" I say, "No." There were lots of women at graduate school in economics when I was there. Not all of them went on to get a degree, but there's no reason why they couldn't have gone on. And there were several women at the National Bureau. So I never felt there was something extraordinary about my situation.

Nelson: In your work with Friedman on the *Monetary History*, your main communication was by correspondence? You would not meet in person much?

Schwartz: By correspondence, and we would send manuscripts back and forth. I never took a trip to Chicago that was related to the monetary economics project. He might be here every three or four months; something might bring him to New York. He was in New York as a visiting professor at Columbia in 1964, but that was after the publication of the *History*. I defended the *Monetary History* in December 1964, and it was because of him that I was able to submit a published book as my dissertation, which is quite unusual.

Anyhow, I've always had an amicable relation with him. He could criticize what I'd written, and it wouldn't bother me. I know Allan Meltzer used to say to me, "I can never send anything to Friedman that he doesn't tear apart." Well, to me that has some value; it lets you see what somebody who's familiar with the subject matter of your work thinks you've failed to do, and you can go ahead then and try to repair whatever's been criticized. And, working with Friedman, I could criticize what he did. There was no problem saying to him, "This isn't clear," or "This doesn't follow from what you've written a couple of pages earlier." You couldn't criticize Arthur Burns that way. It was always easy to talk to Friedman and review what we had done and what there was still to do. He's truly an innovative person, and there isn't any issue you can pose to him where he won't come up with something that nobody else has mentioned.

Nelson: I've heard that one thing you don't have in common with Friedman is that he's very noncultural.

Schwartz: Well, early on when I knew Friedman, he was about to go to France. This was around the time he published his case for flexible exchange rates. I asked him whether he would have a chance to go to museums in Paris. He looked at me as if I was crazy. He said, "Why would I spend my time going to museums?" For my part, I'm an opera-goer for many, many years.

Nelson: *Monetary History* has stayed in print for 40 years, but you've told me that you and Friedman don't get royalties.

Schwartz: That's right. At the time of publication the National Bureau's position was that since it was a nonprofit organization, it was wrong for authors to get royalties. Well, Feldstein changed that policy when he became President, but by then it was too late for us. And so, Princeton University Press has made a lot of money, a mint in fact, from the book.

Nelson: Before the *Monetary History* was published, it was the subject of a conference.

Schwartz: That's right. The American Bankers Association used to have conferences of economists, and there were reviews of the book at that conference. The main one was by Tobin, subsequently published in the *American Economic Review*. He and the other conference reviewers were generally favorable. And there were later various book reviews in different journals, on the whole favorable, although Culbertson had some objections.

Nelson: *A Monetary History* was the first of three volumes with Friedman.

Schwartz: Yes. By the time *A Monetary History* was published, we had already begun working on the *Trends* volume. Also, we had a draft of the details of the

https://doi.org/10.1017/S1365100504030202 Published online by Cambridge University Press

compilation of our money series and some background information on money supply data in periods before our series began. At some point the decision was made that we publish that.

By the time we had sent the *Statistics* volume to press, we had a manuscript for *Trends*, and it had been sent to a Reading Committee. And out of that came the big decision in 1966 not to go ahead with publishing the draft that we had, but to expand it to include coverage of data for the U.K. I don't think we realized what extra work we were undertaking then, because there wasn't a body of British data comparable to the series we had for the U.S. It took a lot of time and a lot of interaction with British statisticians, whose work helped us in assembling a series. And then the work had to be done on comparing the U.S. and the U.K., trying to formulate a common money demand function for the two countries.

So this thing just dragged on, and even after we had the British data in shape, revising the original draft was a much more complicated process than we had envisaged. And in the meantime, I think the value of what we had described in the 1966 draft, when it was just a U.S. story, lost some of its freshness. And certainly when it was finally published, *Trends* didn't make the kind of impression that *Monetary History* did.

Nelson: The decision to include the U.K. in the study—was that imposed on you? Did you have any choice on it, or was it something the Reading Committee insisted on?

Schwartz: Well, we didn't oppose it. I mean, in retrospect I think we could have said, "Look, this is going to add *years* to the final version of this manuscript, and we're not convinced that that would make sense." I think there's an awful lot of material in this volume that hasn't been properly digested by the profession; it's basically been ignored. The fact that the initial reviews concentrated on the demand functions for money probably gave the impression that that was what the whole book was about, but that was only one chapter. And when the econometrics was regarded as vulnerable with the Hendry and Ericsson criticism, the profession was less willing to spend time with the book.

Nelson: Partly, the agenda of the profession has changed. As of 1966, there was probably a lot more controversy in the profession about whether certain long-run monetary relations held in the data, than in 1982. The rational-expectations revolution had moved the interest to dynamic adjustment.

Schwartz: Yes. Though, of course, a lot of the development in macro is reflected in this volume. We were aware of what was being published, and there any many comments on those developments. But, as I say, as the climactic volume of I don't know how many decades of work, it just didn't have the kind of influence that I think we had hoped for. In retrospect, in terms of capturing people's attention, we reached a peak with *Monetary History*. The fact that *Trends* didn't really repeat the success of *Monetary History* was a big disappointment. In general, I think Friedman was disillusioned with its lack of an impact on the profession. His mind's perfectly good; he could have gone on to further things in monetary economics and I think he would have, if this had made an impact. He sort of lost

Figure 3. Schwartz with Allan Meltzer, Carnegie-Rochester Conference, Pittsburgh, November 2002.

heart, and chose not to keep in touch with the literature that has been proliferating. Occasionally, he seems to be interested in a paper that someone's written that I've sent to him, but I never had the sense that he was involved in the way he had been earlier. And maybe I'm wrong, but I attribute it to the fact that this volume didn't take off.

We published a few things together, a couple of papers, after *Trends*, but essentially the project had come to an end, and I sort of went on, on my own.

Nelson: You joined the Shadow Open Market Committee at its inception in 1973.

Schwartz: Yes. That was Karl Brunner's innovation. From the mid-sixties on, whenever Karl was in New York, we would meet and talk about economic questions. In the first encounters, the subject was usually a conclusion that *A Monetary History* presented. Once the Shadow was established, the discussion was about current business-cycle issues. The Shadow came about from the feeling that there was just no understanding by journalists of what monetarism was all about. Karl and Allan Meltzer thought that if they assembled a group that was monetarist in leanings, it could talk to the press. Karl was a great fundraiser, and he found money for this project, so that members of the committee could come from various parts of the country for these Shadow meetings.

https://doi.org/10.1017/S1365100504030202 Published online by Cambridge University Press

Nelson: Was that the first time you were involved in policy prescriptions?

Schwartz: Yes, with the exception of my time in the U.K. in the late sixties, when I gave a couple of talks and wrote an article trying to persuade the British that they should look at money. The belief that inflation was a nonmonetary phenomenon permeated British policy for the whole postwar period up to the end of the 1970's.

Once the Shadow began, it branched out from monetarism to a much more generalized discussion of current policy. Essentially, though, we were trying to influence the Fed and the general public through the reports of our discussions in the press. We had a very hostile relationship with the press when we started, because their take on us was we didn't care about people losing jobs. When we said that in order to reduce inflation, you had to have restrictive monetary policy, they would blow up on us. "You don't care about people losing jobs!"—violent sorts of arguments. We'd have to explain that we cared about the unemployed, but that we thought the economy would function much more stably if monetary policy was more stable. And it was quite a while before this hostile attitude changed. I enjoy the SOMC to this day because it keeps me in touch with current policy developments, though our coverage in the press has diminished. The attitude is that we're saying the same thing each time, even when we're not saying the same thing.

Nelson: When you were involved in the Gold Commission, it was a time when the supply-siders with a very nonmonetarist attitude to monetary policy were trying to have influence.

Schwartz: I would go to Washington once a month, when the Commission would meet. Most of the members of the Commission were reasonable, and weren't really interested in reestablishing the Gold Standard. There was one real gold bug on the Commission, though, who was hostile to me. Some of the arguments we got in favor of the Gold Standard were crazy—non-economics.

Nelson: What are your impressions of the last two Federal Reserve Chairmen—Paul Volcker and Alan Greenspan?

Schwartz: My encounters were not friendly with Paul Volcker after 1982, when along with the Shadow I thought he had discontinued disinflation too early. In recent years I've reassessed that position. Of late, I've had entirely friendly meetings with him, including at a New York Fed conference at which I was seated next to Volcker at dinner.

Regarding Greenspan, I was critical in part about his performance, in a book review that I wrote for the *Times Literary Supplement*, but overall he's clearly been one of the best Federal Reserve chairmen. And he has been helpful to me by supporting my efforts to get the New York Fed to give me the data on foreign exchange market intervention by the Fed and the Exchange Stabilization Fund since 1962.

Nelson: In the seventies you started publishing papers with Phillip Cagan.

Schwartz: Well, a number of people in the Money Workshop at Chicago, when they finished their dissertation, came to the Bureau. It was just natural that with Cagan and me working on similar kinds of issues, we would collaborate. We've sort

https://doi.org/10.1017/S1365100504030202 Published online by Cambridge University Press

of remained in touch, although he quit economics and quit teaching long before he should have. The fact that his dissertation work on hyperinflation became his peak achievement bothered him. Princeton University Press turned down the option to publish his study of the *Determinants and Effects of the Stock of Money*, on the grounds that it was a rehash of the *Monetary History*, which was not true at all. At any rate, the Bureau broke its connections with Princeton University Press and published Cagan's book on its own auspices, although Columbia University Press distributed it.

Nelson: It's curious that on the issue of monetary control, Cagan did in some respects fit into a widespread perception of quantity theorists, namely that they want more regulation of the banking system. The SOMC wanted controls on deposit interest rates removed, and opposed the extension of reserve requirements to nonbank financial intermediaries in 1980. Friedman also opposed additional regulation, short of a major institutional overhaul such as 100% reserve banking. Whereas Cagan was in favor of the extension of reserve requirements to nonbanks.

Schwartz: Yes, I thought that was kind of a wishy-washy approach. I think so much of the regulation in this country has been destructive; Regulation Q, the Nixon incomes policy, and so on. Cagan definitely worried that in the absence of binding reserve requirements, central banks would lose control of the money supply. This worry bothered him long before required reserves became insignificant. We now know, though, that his worry was baseless, since central banks exercise control through voluntary clearing accounts that intermediaries maintain with them.

But Cagan is brilliant by my standards. He has a first-rate mind; there's no question about that. And he's a nice guy.

Nelson: How did your collaboration with Michael Bordo begin?

Schwartz: He appeared at the Bureau while he was writing his dissertation at Chicago, and asked if he could be a research assistant during the summer. This was the summer of 1972, something like that, and since then over the years we've written together a fair number of papers. He has a good network that gives him a good feel on what is an up-and-coming topic. He might say: "Do you agree with Eichengreen on this?" and I'd say "No, I don't," and he'd say, "Well, why don't we write a paper on this?"

Nelson: Your position changed, officially at least, to Research Emerita in 1985.

Schwartz: Yes, but it hasn't made any difference. I'm not an Emerita on the list; I'm listed on the NBER Web site as one of the regular researchers. Marty Feldstein and the Directors changed the name of my position, but didn't make any insistence that this was the end of the line. I had no interest in retirement, even less so now. A lot of people say they want to retire and have more time for their grandchildren. Well, I don't know that grandchildren want to have grandparents hanging around them. I keep in touch with them, but I think that having something to think about is a much better life.

https://doi.org/10.1017/S1365100504030202 Published online by Cambridge University Press

NOTES

1. The *JME* piece was a book review, and so does not appear in the bibliography below. Schwartz's numerous book reviews make rewarding reading and include a prediction in *Kyklos* in 1968 of an era when economists would use Internet connections to download macroeconomic databases.

2. A short excerpt from a *Journal of Political Economy* paper by Friedman on money did appear in the 1959 proceedings issue of the *American Economic Review.*

3. Furthermore, of the 93 books listed by Princeton University Press in its *Economics and Finance Catalogue 2003*, the *Monetary History* is the *only* pre-1994 publication included.

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