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MD INTERVIEW

AN INTERVIEW WITH MILTON FRIEDMAN

Interviewed by John B. Taylor
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"His views have had as much, if not more, impact on the way we think about monetary policy and many other important economic issues as those of any person in the last half of the twentieth century." These words in praise of Milton Friedman are from economist and Federal Reserve Chair Alan Greenspan. They are spoken from a vantage point of experience and knowledge of what really matters for policy decisions in the real world. And they are no exaggeration. Many would say they do not go far enough.

It is a rare monetary policy conference today in which Milton Friedman's ideas do not come up. It is a rare paper in macroeconomics in which some economic, mathematical, or statistical idea cannot be traced to Milton Friedman's early work. It is a rare student of macroeconomics who has not been impressed by reading Milton Friedman's crystal-clear expositions. It is a rare democrat from a formerly communist country who was not inspired by Milton Friedman's defense of a market economy written in the heydays of central planning. And it is a rare day that some popular newspaper or magazine around the world does not mention Milton Friedman as the originator of a seminal idea or point of view.

Any one of his many contributions to macroeconomics (or rather to monetary theory, for he detests the term macroeconomics) would be an extraordinary achievement. Taken together they are daunting:

- permanent income theory;
- natural rate theory;
- the case for floating exchange rates;
- money growth rules;
- the optimal quantity of money;
- the monetary history of the United States, especially the Fed in the Great Depression, not to mention contributions to mathematical statistics on rank-order tests, sequential sampling, and risk aversion, and a host of novel government reform proposals from the negative income tax, to school vouchers, to the flat-rate tax, to the legalization of drugs.

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Milton Friedman is an economist's economist who laid out a specific methodology of positive economic research. Economic experts know that many current ideas and policies—from monetary policy rules to the earned-income tax credit—can be traced to his original proposals. He won the Nobel Prize in economics in 1976 for "his achievements in the field of consumption analysis, monetary history and theory and for his demonstration of the complexity of stabilization policy." Preferring to stay away from formal policy-making jobs, he has been asked for his advice by presidents, prime ministers, and top economic officials for many years. It is in the nature of Milton Friedman's unequivocally stated views that many disagree with at least some of them, and he has engaged in heated debates since graduate school days at the University of Chicago. He is an awesome debater. He is also gracious and friendly.

Born in 1912, he grew up in Rahway, New Jersey, where he attended local public schools. He graduated from Rutgers University in the midst of the Great Depression in 1932. He then went to study economics at the University of Chicago, where he met fellow graduate student Rose Director whom he later married. For nearly 10 years after he left Chicago, he worked at government agencies and research institutes (with one year visiting at the University of Wisconsin and one year at the University of Minnesota) before taking a faculty position at the University of Chicago in 1946. He remained at Chicago until he retired in 1977 at the age of 65, and he then moved to the Hoover Institution at Stanford University. I have always found Milton and Rose to be gregarious, energetic people, who genuinely enjoy interacting with others, and who enjoy life in all its dimensions, from walks near the Pacific Ocean to surfs on the World Wide Web. The day of this interview was no exception. It took place on May 2, 2000, in Milton's office in their San Francisco apartment. The interview lasted for two-and-a-half hours. A tape recorder and some economic charts were on the desk between us. Behind Milton was a floor-to-ceiling picture window with beautiful panoramic views of the San Francisco hills and skyline. Behind me were his bookcases stuffed with his books, papers, and mementos.

The interview began in a rather unplanned way. When we walked into his office Milton started talking enthusiastically about the charts that were on his desk. The charts—which he had recently prepared from data he had downloaded from the Internet—raised questions about some remarks that I had given at a conference several weeks before—which he had read about on the Internet. As we began talking about the charts, I asked if I could turn on the tape recorder, since one of the topics for the interview was to be about how he formulated his ideas—and a conversation about the ideas he was formulating right then and there seemed like an excellent way to begin the interview. So I turned on the tape recorder, and the interview began. Soon we segued into the series of questions that I had planned in advance (but had not shown Milton in advance). We took one break for a very pleasant lunch and (unrecorded) conversation with his wife Rose before going back to "work." After the interview, the tapes were transcribed and the transcript was edited by me and Milton. The questions and answers were rearranged slightly to fit into the following broad topic areas:

- money growth, thermostats, and Alan Greenspan;
- causes of the great inflation and its end;

- early interest in economics;
- graduate school and early "on-the-job" training;
- permanent income theory;
- return of monetary economics;
- fiscal and monetary policy rules;
- use of models in monetary economics;
- use of time-series methods;
- real business-cycle models, calibration, and detrending;
- natural rate hypothesis;
- role of debates in monetary economics;
- capitalism and freedom today;
- monetary unions and flexible exchange rates.

Keywords: Permanent Income Theory, Natural Rate Hypothesis, Floating Exchange Rates, Monetary Policy Rules, Money Growth

MONEY GROWTH, THERMOSTATS, AND ALAN GREENSPAN

Friedman: [Referring to the charts in Figures 1 and 2] I thought that you'd be interested in these charts. Don't you think it's as if the Fed has installed a new and improved thermostatic controller in the 1990s![1]

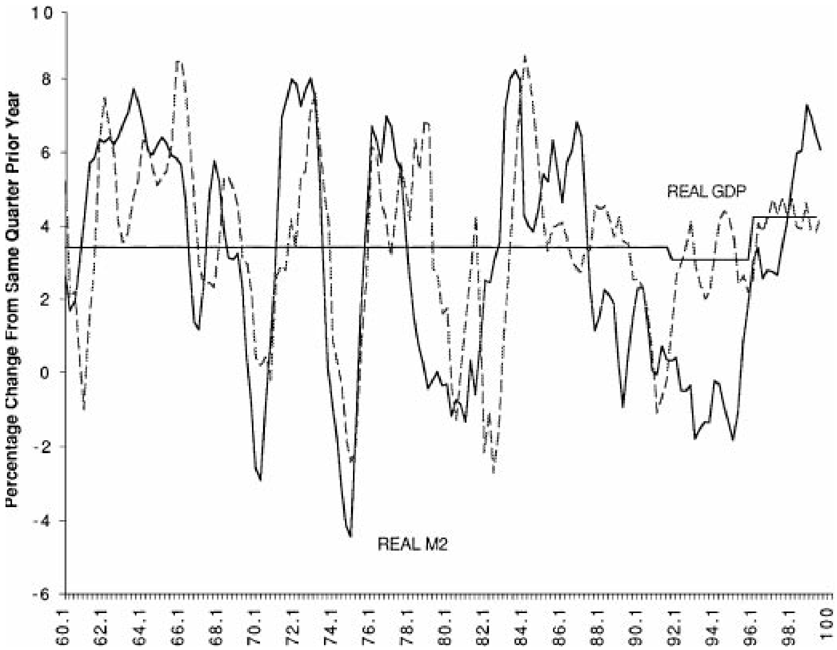

FIGURE 1. Year-to-year change in U.S. real M2 and real GDP, 1960.1–1999.3. (Source: Milton Friedman, February 20, 2000.)

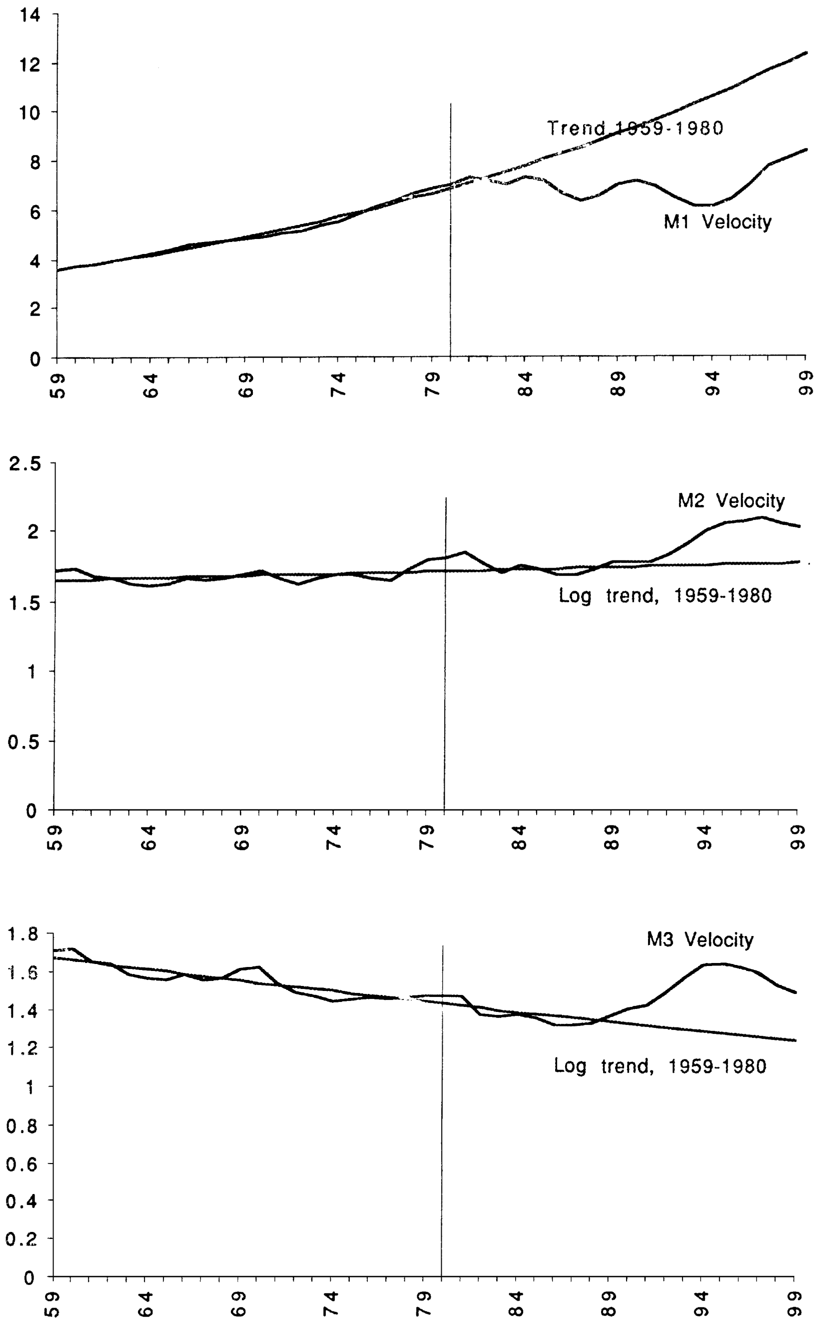

Figure 2. Velocity of M1, M2, M3, and log trends based on data from 1959–1980, annual data, 1959–1999. (Source: Milton Friedman, April 30, 2000.)

Taylor: I can see that there is a change in the relationship between money growth and real GDP and that the size of fluctuations in the economy has diminished greatly. There is much greater stability starting in the early 1980's.

Friedman: The change in stability really comes in 1992.

Taylor: Isn't 1982 the best break point?

Friedman: I think 1992 is the break. [Referring to the charts in Figure 2] Here are the charts that show the velocity of M1, M2, and M3 against the logarithmic trend.

Taylor: One reason to focus on 1982 is that it was the beginning of an expansion. There are also statistical tests that several people have done to test when the size of the fluctuations changed. Most say that it is in the early 1980's. Since then, the fluctuations in real GDP seem smaller. There is only one recession in 1991 and that is pretty small.

Friedman: [Pointing to the dip in real GDP growth in 1990–1991] But this looks like a pretty big recession.

Taylor: Well, whatever the break point is, why do you think things have changed? Why, as you put it, does the Fed seem to be operating the monetary-policy thermostatic regulator so much better now? What do you think the reason is?

Friedman: I'm baffled. I find it hard to believe. They haven't learned anything they didn't know before. There's no additional knowledge. Literally, I'm baffled.

Taylor: What about the idea that they have learned that inflation was really much worse than they thought in the late 1970's, and they therefore put in place an interest-rate policy that kept inflation in check and reduced the boom/bust cycle?

Friedman: I believe that there are two different changes. One is a change in the relative value put on inflation control and economic stability and that did come in the eighties. The other is the breakdown in the relation between money and GDP. That came in the early nineties, when there was a dramatic reduction in the variability of GDP. What I'm puzzled about is whether, and if so how, they suddenly learned how to regulate the economy. Does Alan Greenspan have an insight into the movements in the economy and the shocks that other people don't have?

Taylor: Well, it's possible.

Friedman: Another explanation is that the information revolution has enabled enterprises to manage inventories so much better, as you pointed out in your recent discussion. But inventories can't be the answer because the same thing has happened to noninventories.

Taylor: I agree with that. If you look at final sales, you see the same change in stability, unless you really want to focus on very-short-term wiggles, such as the quarterly rates of change in real GDP during an expansion.

Friedman: And it may get big again. It may be a statistical artifact. They may have somehow changed their methods. There have been significant changes in estimation.

Taylor: Yes, but going back to the possibility that the Fed has more knowledge, do you think that they have learned more about controlling liquidity or money while at the same time recognizing the fact that there are these shifts in velocity?

Friedman: But then again, if you look at these shifts in velocity, they don't come until 1992.

Taylor: Well, what about this one?

Friedman: That's M1, but, all along, M2 has been the preferred aggregate exactly because of this change, which was the result of eliminating the prohibition on paying interest on demand deposits. So I don't think you can explain it through velocity. It looks as if somehow in 1992—1991–1992— they were able to install a good thermostat instead of a bad one. Now, is Alan Greenspan a good thermostat compared to other Fed chairmen? That's hard to believe.

CAUSES OF THE GREAT INFLATION AND ITS END

Taylor: Hard to believe, yeah. Well, let's go back to an earlier period when things did not look so good. In recent years, there has been a lot of interest in what caused the Great Inflation of the 1970's and what caused its end. Why did inflation start to rise in the late 1960's and 1970's in the United States?

Friedman: Yes, the Great Inflation. The explanation for that is fundamentally political, not economic. It really had its origin in Kennedy's election in 1960. He was able to take advantage of the noninflationary economic conditions he inherited to "get the economy moving again." With zero inflationary expectations, monetary and fiscal expansions affected primarily output. The delayed effect on prices came only in the mid-sixties and built up gradually. Already by then, Darryl Francis of the St. Louis Fed was complaining about excessive monetary growth. Inflation was slowed by a mini-recession but then took off again when the Fed overreacted to the mini-recession. In the seventies, though I hate to say this, I believe that Arthur Burns deserves a lot of blame, and he deserves the blame because he knew better. He testified before Congress that, if the money supply grew by more than 6 or 7% per year, we'd have inflation, and during his regime it grew by more than that. He believed in the quantity theory of money but he wasn't a strict monetarist at any time. He trusted his own political instincts to a great degree, and he trusted his own judgment. In 1960, when he was advising Nixon, he argued that we were heading for a recession and that it was going to hurt Nixon very badly in the election, which is what did happen. And Nixon as a result had a great deal of confidence in him.

From the moment Burns got into the Fed, I think politics played a great role in what happened. So far as Nixon was concerned, there is no doubt, as I know from personal experience. I had a session with Nixon sometime in 1970, I think it was 1970, might have been 1971, in which he wanted me to urge Arthur to increase the money supply more rapidly (laughter) and I said to the president, "Do you really want to do that? The only effect of that will be to leave you with a larger inflation if you do get reelected." And he said, "Well, we'll worry about that after we get reelected." Typical. So there's no doubt what Nixon's pleasure was.

Taylor: Do you think Burns was part of the culture of the times in that he put less emphasis on inflation, or that he was willing to risk some inflation to keep unemployment low, based on the Philips curve?

Friedman: Not at all. You read all of Arthur's writings up to that point and one of his strongest points was the avoidance of inflation. He was not part of that Keynesian group at all. In fact, he wrote against the Keynesian view. However, it did affect the climate of opinion in Washington, it did affect what activities of the Fed were viewed favorably and unfavorably, and therefore it did affect it that way, but not through his own beliefs of the desirability of inflation.

Taylor: Another thing that people say now is that Burns was as confused as other people were about potential GDP, and that he thought the economy was either below capacity or that it was capable of growing more rapidly than it was. Do you think that was much of a factor?

Friedman: I don't think that was a major factor. I think it may have been a factor.

Taylor: Mainly political?

Friedman: Yeah.

Taylor: What about the end of the Great Inflation? It lasted beyond Burns's time. We had G. William Miller and then Paul Volcker.

Friedman: Well, there's no doubt what ended it. What ended it was Ronald Reagan. If you recall the details, the election was in 1980. In October of 1979, Paul Volcker came back from a meeting in Belgrade, in which the United States had been criticized, and he announced that the Fed would shift from using interest rates as its operating instrument to using bank reserves or base money. Nonetheless, the period following that was one of very extreme fluctuations in the quantity of money. The purpose of the announcement about paying attention to the monetary aggregates was to give Volcker a shield behind which he could let interest rates go.

[Pointing to Figure 1] That's the period, here . . . ups and downs. (The picture of the nominal money supply is very much the same as for the real money supply.) They did step on the brake, and in addition, sometime in February 1980, Carter imposed controls on consumer credit. When the economy went into a stall as we were approaching the election, the Fed stepped on the gas. In the five months before the election, the money supply went up very rapidly. Paul Volcker was political, too. The month after the election, the money supply slowed down. If Carter had been elected, I don't know what would have happened. However, Reagan was elected, and Reagan was determined to stop the inflation and willing to take risks. In 1981, we got into a severe recession. Reagan's public-opinion ratings went down, way down. I believe no other president in the postwar period would have accepted that without bringing pressure on the Fed to reverse course. That's the one key step: Reagan did not. The recession went on in 1981 and 1982. In 1982, finally Volcker turned around and started to raise the money supply and at that point the recession came to an end and the economy started expanding.

Taylor: Your explanations of both the start and end of the Great Inflation are very much related to changes in people in leadership positions, as distinct from changes in ideas. What you seem to be saying is that it was mostly Burns, Nixon, Reagan. Could you comment on that a little bit?

Friedman: I may be overemphasizing Burns's role. I certainly am not overemphasizing Reagan's. And again, in both cases I feel I have personal evidence. I was one of the people who talked to Reagan and there's no question that Reagan understood the relation between the quantity of money and inflation. It was very clear, and he was willing to take the heat. He understood on his own accord, but he also had been told so, that you could not slow down the inflation without having a recession.

Taylor: In the first case, a president didn't take your advice, and in the second case, a president did take your advice.

Friedman: Correlation without causation. They were different characters and persons. Nixon had a higher IQ than Reagan, but he was far less principled; he was political to an extreme degree. Reagan had a respectable IQ, though he wasn't in Nixon's class. But he had solid principles and he was willing to stick up for them and to pay a price for them. Both of them would have acted as they did if they had never seen me or heard from me.

EARLY INTEREST IN ECONOMICS

Taylor: I'd like to change the topic from politics to your work in economics. I hope you can share some personal recollections about your remarkable contributions to economics, especially to macroeconomics. How did you get the ideas? Who influenced you? Which parts of your background, education, or work experience were most important? I know it's a long time ago . . .

Friedman: It is a long time ago! But sure, you go right ahead, but I don't trust my memory that far back.

Taylor: Just to get started, let's go back to when you went to college at Rutgers. At first you were interested in mathematics, but then you got interested in economics. Is that correct?

Friedman: I graduated with essentially a double major of mathematics and economics.

Taylor: You got interested in economics in college though?

Friedman: Yeah.

Taylor: And the two people who you say influenced you early on were two economists: Arthur Burns and Homer Jones. Could you share a little bit about how that occurred? Was Burns teaching you microeconomics, or was he more influential on the macroeconomic side of things?

Friedman: It was much more micro than macro. We had a seminar with Burns in which we went over the draft he had written of his book on production trends in the United States. As we went over his manuscript with him, it was one of the best educational experiences I've ever had, because it gave me a feeling for how to do research. It demonstrated a willingness on his part to accept criticism from people who were not in a way his peers, and so it was a very educational experience.

FIGURE 3. In own living room.

So far as Homer was concerned, Homer taught a course on statistics and one on insurance. He was a novice himself; he was just keeping one lesson ahead of his students. He clearly was a disciple of Frank Knight of Chicago. He was a member of the Chicago school of economics as it was then. And Homer had a very great influence on me both through his teaching and by getting me to Chicago!

Taylor: He taught you statistics mainly?

Friedman: That, plus the course on insurance, which dealt with economic issues.

Taylor: So you didn't really study macroeconomics or monetary theory much then?

Friedman: I'm sure I had a course in money and banking. It was a standard undergraduate course, no real macro. I didn't get any real training in economics until I went for graduate work in Chicago.

Taylor: It is remarkable that Burns would be working with undergraduates at that level on his own research, that level of detail.

Friedman: Burns at that time was finishing his Ph.D. dissertation. He was a young man; he was not what you think of usually. He had just gotten married and was living in Greenwich Village. He had long hair, long fingernails. You know, he was a different character than he was later on. But he was always an enormously able person intellectually and very dedicated to the research he was doing, to getting it right. And somehow, I'm not sure where, Marshall came in. He was a great student of Marshall and a great admirer of Marshall.

Taylor: So he introduced you to Marshall?

Friedman: Yeah.

Taylor: What about the idea that the free-market system is a good way to organize a society? Was that part of the microeconomics you were learning?

Friedman: Remember, I'm talking about 1928–1932; that was before the real change in public opinion, and that really wasn't the kind of issue then that it was scheduled to become. There was, of course, discussion about the breakdown of the economic system, but I graduated in June of 1932 and most of my years there, 1928, 1929, people didn't teach "if markets work well"; they just taught markets. You took it for granted in a sense. Of course, there was a strong intellectual movement toward socialism but it wasn't of the kind that later developed. Norman Thomas was at that time the leading socialist; he was enormously respected, and he got more votes as candidate for president in 1928 than any socialist ever did before or since. The intellectual community in general was socialist, but so far as the department of economics was concerned, I don't think there was much of that.

Taylor: So you wouldn't even have given it a thought?

Friedman: No, I never got involved in politics. I probably would have described myself as a socialist, who knows. When I graduated from college, I wrote myself an essay about what I believed at the time, and I left it in my mother's apartment where I grew up; my father had died when I was in high school. When I went back years later and tried to find it, I never could find it, and I've regretted that very much. That would be a nice document for this purpose.

Taylor: You can't even guess what you wrote?

Friedman: I'm pretty sure I did not have the views I later developed. I probably had the standard views that we needed to do something, but I have no idea what they were.

Taylor: So economics was more technical—supply-and-demand curves, this is how a market works—rather than philosophical?

Friedman: My impression is that it was much less philosophical.

Taylor: So how did Homer Jones encourage you to go to Chicago?

Friedman: He not only encouraged me to go, he made it possible for me to go. People now don't recognize what the situation was then. There were very few scholarships, almost no fellowships of the sort we now take for granted. When I graduated from Rutgers, I applied for graduate work to a number of places, and I received two offers, one from Brown University in applied mathematics, and one from Chicago, thanks to Homer, in economics. Both of them were tuition scholarships, no money beyond free tuition. That was the standard practice at that time. Graduate students mostly paid their own way.

Taylor: Did you have an idea of what you wanted to work on as an economist then?

Friedman: None whatsoever. When I originally entered college, I thought I was going to be an actuary and I took actuarial exams because that was the only way that I knew of that a person could make a living using mathematics. And it is, it's a very skilled job. Only after I got into college and started taking economics courses as well as mathematics courses did I discover that there were alternatives. Of course, the fact that we were in a depression at that time made economics a very important subject.

GRADUATE SCHOOL AND EARLY "ON-THE-JOB" TRAINING

Taylor: You were at Chicago for graduate school for a year and then you went to Columbia for a year, and then you went back to Chicago. My understanding is that during this time you developed an interest in mathematical statistics and working with data, with Henry Schultz at Chicago and with Harold Hotelling and Wesley Mitchell at Columbia. And right after graduate school you took a job in Washington working on a new consumer spending survey and then you moved to New York to work on income survey data with Simon Kuznets. Did working with data and using mathematical statistics interest you a lot?

Friedman: Yes, it did. First of all at Chicago I took Schultz's course in statistics, and when I came back to Chicago after a year at Columbia, I came back as a research assistant to Schultz. Let me go back, and really trace this to Rutgers, to Arthur Burns, because the book that we reviewed, *Production Trends in the United States*, which was his doctoral dissertation, was essentially data analysis. The thesis of the book is that retardation in the growth of each industry separately does not imply retardation in the economy as a whole.

Taylor: My impression is that, at least in your early work with survey data, you put less emphasis on economic models, or formal theories, and more on describing the facts and using mathematical statistics?

Friedman: No, I don't think so. I was trying to explain the data, but not through models, not through multi-equation models, but through more informal stories—basically trying to appeal to microeconomic interactions.

My first year in Chicago really gave me an understanding of economics as a theoretical discipline. In my first year at Chicago, Jacob Viner, Frank Knight, and Lloyd Mints were my main teachers. Both of what's now called micro and macro. I hate those words, I think it's price theory and it's monetary theory. Why the hell do we have to use these Greek words?

Anyway, it seemed to me at that time, spending a year at Chicago first and then a year at Columbia was the ideal combination. Chicago gave you the theoretical basis with which you can interpret the data. Also, there was an empirical slant at Chicago compared with an institutional slant at Columbia. When I went to Washington to work at the National Resources Council in 1935, my work was almost entirely statistical, very little economic theory.

Taylor: Before you went to Washington, you wrote your first published paper, an article criticizing a method proposed by the famous Professor Pigou of Cambridge University. It was published in 1935 in the *Quarterly Journal of Economics*; it must have been written in your first or second year in graduate school. What motivated you to write and publish such an article?

Friedman: Schultz's book that I was working on was on the theory and measurement of demand, the Pigou article was on the elasticity of demand, so it came right out of what I was doing with Schultz. He probably suggested that I publish it, I don't remember.

Taylor: Pigou took the article as a very strong criticism and there was a debate. Did you enjoy that aspect of it?

Friedman: What really happened is this: I sent the article to the *Economic Journal,* where the editor was John Maynard Keynes. Keynes rejected the article on the grounds that Pigou didn't think it was right. I then sent the article to the *Quarterly Journal of Economics,* where Taussig was editor. Fortunately, in submitting it to the *Quarterly Journal of Economics*, I said that I had earlier submitted it to the *Economic Journal* and gave the reason why it was rejected and why I didn't think that was right. I guess it was published in the *Quarterly Journal of Economics* because it was refereed by Leontief. Then Pigou submitted a criticism of it to the *Quarterly Journal of Economics* and Taussig wrote to me and sent me a copy of the criticism. The *Quarterly Journal of Economics* then published both Pigou's criticism and my response.

Taylor: Did that experience whet you appetite for controversy?

Friedman: I really can't say. That's now what, 1935; it's 65 years ago.

Taylor: That story reminds me of referee work you once did for me when I was an editor at the *American Economic Review*. You signed your "anonymous" referee report!

Friedman: I always believed I should be responsible for what I write. I didn't want to go under an anonymous name. And I've never been willing to publish something under my name written by somebody else. You know, I've frequently been asked to, somebody wants propaganda for something or other, but I don't believe that's the appropriate thing to do.

Taylor: I want to ask you about your work at the Statistical Research Group at Columbia University during World War II, but what other experiences were important around that time in your career?

Friedman: So far as your questions about economics versus statistics is concerned, you should note that, for the two years before I went to the Statistical Research Group, I was at the U.S. Treasury Department where it was entirely economics and negligible statistics. We were designing the wartime tax program. Unfortunately, a large part of the income tax today derives from what happened during the war. That was when withholding was introduced, that was when rates were really hiked way up and they were made more progressive, so everyone of the present disputes existed then, even the marriage penalty. In the proposal we made at the Treasury, we eliminated the marriage penalty but our solution wasn't politically feasible. There was a very good group of economists at the Treasury including Lowell Harris and Bill Vickrey.

Taylor: So that was also part of the war effort?

Friedman: Sure. I went there in 1941 just before we got into the war and the big issue during that period was the argument between the price control people and the people who wanted to hold down inflation through taxation. In the summer of 1941, I participated in a research project with Carl Shoup and we wrote a book, *Taxing to Prevent Inflation*. It's not something I'm very proud of now. It was in the style of a model and it had to do with how much taxation was required to prevent inflation, which I now believe was the wrong issue.

Taylor: You published a paper in the *American Economic Review* in 1942 on the inflationary gap. I want to come back to that, but was it also part of your work at Treasury?

Friedman: Oh yes, it was while I was at the Treasury.

Taylor: Let's discuss your work at the Statistical Research Group in New York during the war. It was heavily statistically oriented, but was there much economics?

Friedman: Oh, entirely statistically oriented; no economics at all. I shouldn't say no economics at all. One of the things that was found out during the war was that social scientists are more effective than natural scientists in dealing with many wartime data problems because social scientists are accustomed to dealing with bad data and natural scientists are accustomed to dealing with good data. And here you have all sorts of problems that arose involving the analysis of data.

Taylor: Do you think that social scientists have a better sense of approximation? What is their advantage?

Friedman: Social scientists have ways of trying to judge the quality of data, to find proxies, to find substitutes, to find ways of evaluating it. Now, in what we did at the statistical research group, that wasn't so evident most of the time.

Taylor: What kind of problems did you work on?

Friedman: We were primarily concerned with such problems as: You've got an antiaircraft missile. It's possible to produce it in such a way that you can control how many pieces it breaks into when it explodes. Should you have a lot of little pieces, so there's a high probability of hitting, but it won't be as harmful to the object hit? Or, should you have a few big pieces, each of which will destroy the plane you're shooting at if it hits it, but the probability of hitting it is less? One of the jobs I worked on was to write a paper on the optimum number of pieces into which to break up a shell. We had data from various test firings on what would be the effect if a fragment of a certain size hit a certain place on a plane, and so on. It was that kind of a problem. Now that's an economic problem.

Taylor: Could you elaborate on that? Why is it an economic problem?

Friedman: I mean it in a broader sense. What we discovered on that project is what you always discover in economics. If you ask people what are the biggest industries in the United States, they'll give you the wrong answer every time. They'll say steel or automobiles. More people are employed in domestic service than in either steel or automobiles and many more still in wholesale or retail trade. That is because those industries consist of a large number of small enterprises. So in this shell project, the naval experts and the military people all came down for a fairly small number of large fragments, so if you hit, you really do damage. Our calculation came out with something different. We showed that there should be a large number of small fragments because the probability of hitting is so much higher than with the large pieces. And that's why I say that's an economic problem—maximization subject to restraints. Again, it always comes down to, should you have one big aircraft carrier or two small ones?

Taylor: Maybe you could say a little about your work on sequential testing. How did you get the idea?

Friedman: Well, Allen Wallis tells the story in an article in the *Journal of the American Statistical Association*. Allen came back to the office one day saying that he had just been with a navy captain who had been observing tests of artillery. The captain said, "You know these statisticians always have to make so many shots, but I know long before the test is done which is the right one." And so Allen came back and said, "you know there's some sense in that." We agreed and we thought about it and I fixed up an example in which I was able to demonstrate that by having a good stopping rule, you could achieve the same probability of error with a much smaller sample on average.

We knew we didn't have the mathematical competence and could not afford the time to do this ourselves, so we shopped around. But we stated the problem in such a way that statisticians found it difficult to accept. We said, "we know how to construct a test that's more powerful than the uniformly most powerful test." They said, "That's mathematically impossible, you can't do that, we've proved that this is the most powerful test." And so statisticians wouldn't have anything to do with it. Then, we talked to Abraham Wald, and he initially had the same reaction. But then he went home and a day later he called and said, "you are right and I know how to do it and I know what the answer is."

Taylor: A lot of things followed from that important discovery. And you had worked out a little numerical example to show that it would work, at least in some cases?

Friedman: A very simple case, I've forgotten what it was. And then later, one of the jobs we had was to advise the Navy on sampling inspection. So we got up a whole series of sampling inspection programs including sequential analysis using those findings.

One of the other problems, probably the most important one I worked on, had to do with proximity fuses, which are used when firing an antiaircraft gun at an incoming bomber or fighter. A proximity fuse is designed to eliminate the error in timing by being so adjusted that it would go off when it was near the target. The fuse sends out a radio signal that would bounce back from the target; if the target was close enough, the fuse would go off. The radio signal sent out could be adjusted to different angles and different intensities. What was the optimum design of the proximity fuse to maximize the chance of hitting the object? A very interesting problem, and one that we spent a lot of effort on.

Taylor: That sounds like an amazingly complex problem to be working on. Did you write up papers or reports?

Friedman: Oh, sure. I have those reports somewhere.

Taylor: How did you feel about writing important papers that you wouldn't be able to publish, to show to the world?

Friedman: You can't conceive of what the situation was at the time. The war was the most important thing going on and everybody, not me particularly, but everybody was putting aside almost all other considerations to contribute what they could to help in the war. I don't think there was any feeling on the part of any of us that we were concerned about what would happen to our research. In any event, this was in an area that was not of much long-term interest for me.

Taylor: What about the methodology of optimization that you used at the Statistical Research Group. Is that something that you have used later in economic research, perhaps in your research on monetary policy rules?

Friedman: I think it comes the other way. The economic view of seeking an optimum subject to constraints was a way to approach these military problems, rather than the other way around. But I will say that that was very interesting because it was so different from anything we had been exposed to before.

Taylor: Is there anything else that you would like to add?

Friedman: No, I really don't think there is. The Statistical Research Group got me involved with a group of people that I wouldn't otherwise have been involved with. For example, it was the way I got to know Jimmy Savage. He and I wrote a number of papers later together.

Taylor: Do you remember how you happened to write the paper with Savage on utility functions, which gave risk preference at low incomes?

Friedman: I don't know. I honestly don't know. Somehow Jimmy and I must have been talking about it, but I cannot reproduce it. Jimmy Savage was a real genius, there's no question that he was a remarkable character.

Taylor: How did you come to collaborate with him?

Friedman: We got to know one another at the Statistical Research Group. What happened was that at the time he didn't know how to write and I was forced to rewrite some of his papers. He later developed into an excellent writer. You know, he was almost blind, he could only see out of one corner of his eye. He was trained as a mathematician, he had a Ph.D. in mathematics, and then he went on to statistics and really revolutionized statistics. How we got into the risk paper, I no longer have the slightest recollection.

PERMANENT INCOME THEORY

Taylor: Now let's go on to your research. Let's start with your research on the consumption function. I understand that you think that this is your best purely scientific contribution.

Friedman: I think it is.

Taylor: Could you say a little more about it? Relating to our earlier discussion, did your early work with data and mathematical statistics help you develop the idea?

Friedman: Aside from the work I did on the consumer spending survey in Washington during the 1930's, I also spent several years at the National Bureau of Economic Research working with Simon Kuznets. That ended up in the book, *Income from Independent Professional Practice*. It served as my Ph.D. dissertation. It was largely statistical and empirical, dealing with a whole bunch of questionnaires Kuznets had sent out while he was working at the Department of Commerce. But it also involved the application of economic theory dealing with the explanation for differences of income in different professions. An early venture in the analysis of human capital.

The book on the consumption function was a combination of ideas from the professional income study, from the consumers' spending study, and the work I was doing on methodology (which ultimately appeared in the article I wrote on methodology). What I like about the consumption function book is that it is the best example I know, in my own work, of the methodological principles that are laid out in my essay on methodology. You start with a hypothesis. It has implications. You test whether those implications are correct or not. If the implications are not correct, you try to adjust your hypothesis and readjust.

In this case I started out with a hypothesis that is similar to that which underlies the distinction between real and nominal interest rates. How do people adjust their expectations? How do they decide what fraction of their income to spend? I developed the hypothesis along these lines. I put it in a form in which it could be tested and I derived its implications. I tested those implications and, on the whole, they tended to confirm the hypothesis. I suggested additional tests that should be made to test the hypothesis. So it was, in this way, methodologically pure.

In addition, it produced a hypothesis that seemed to explain the data. As you know, the original pressure for the analysis was the apparent inconsistency between two bodies of data: long time-series data and cross-sectional budget data on consumption and income. The question was: "How could you reconcile those two apparently contradictory bodies of data?" A lot of hypotheses had been offered to reconcile them. The hypothesis I offered, the permanent income hypothesis, seemed to me a much more elegant way to rationalize that difference. And it had, as special cases, almost all of the alternative hypotheses, so it was a consolidation of a lot of empirical evidence as well as theoretical analysis.

Taylor: It seems to me that your signal extraction characterization of the problem, as we call it these days, was quite revolutionary at the time.

Figure 4. March 1992.

Friedman: That really came out of the work with Kuznets' data on incomes from professional practice. In that earlier work, I introduced the concepts of permanent income and transitory income in a simplified form, and I just carried that right over. In the professional income data research, I had three categories: permanent, quasi permanent (that's what I called the intermediate one), and transitory. Later I got it down to two.

Taylor: Where did you get the idea to use such statistical decomposition theories in economics?

Friedman: Just from the fact that I was simultaneously becoming an expert in statistical analysis.

Taylor: I guess it is an example of the benefits of a little cross-fertilization. Your work on the consumption function got characterized sometimes as kind of an attack on the Keynesian consumption function. Did that motivate you at all?

Friedman: I don't think so, and it isn't an attack, it's just a demonstration that the Keynesian consumption function is not a long-run function; it's a transitory function as he defined it.

Taylor: Did you argue that your theory would imply that a Keynesian model wouldn't be very stable?

Friedman: I think I did argue that in the conclusion.

RETURN OF MONETARY ECONOMICS

Taylor: When did your interests in monetary economics begin, exactly?

Friedman: It really began I guess when I was serving in the Treasury Department from 1941 to 1943, because the crucial question was, "what are we going to do to keep down inflation?" Everybody was aware that, during the first world war, taxes had paid for a very small fraction of the war and, during the second world war, they were determined to raise the fraction paid for by taxes. At the same time, they also had the problem of predicting inflation and that's how I got involved. I was at the Treasury, Division of Tax Research, and our job was to prepare tax proposals for Congress.

The problem—it was interesting from a political point of view and from a scientific point of view—was that a group in the administration who were trying to get a price control statute didn't want us to come up with a tax proposal because they were afraid we would say, "we can stop inflation through taxes, we don't need price controls." They wanted price controls.

We were making estimates of the amount of taxes you would need to stop inflation. Our estimates of how much taxes you would need were much higher than comparable estimates made by those favoring price controls. A month after the price control law was passed, their estimates were much higher than ours. *Now* they wanted all the help they could get from the tax system.

Taylor: Why didn't people mention money through all of this talk about inflation? Was it discussed at all?

Friedman: Hardly. As a result of the Keynesian revolution, money had almost dropped out of the picture. I look back at that and say, how the hell could I have done that? I had good training in monetary theory at Chicago and yet, once the Keynesian revolution came along, everything was on taxes and spending, everything was on fiscal policy, and that's why I was trying to answer the question about the level of taxation needed to stem inflation. With a sufficiently expansive monetary policy, no amount of taxes could do it. It was the wrong question. The right question was, "What monetary policy do we need?" That was the result of the mindset we had.

Taylor: So that's when your 1942 *American Economic Review* article on the inflationary gap was written. When did you go back to basic monetary theory you had learned at Chicago?

Friedman: All I know is from the record. When I republished that article in *Essays in Positive Economics* [published in 1953], I added sections about money and I had a footnote saying that the original article was deficient in this respect. It must have been only a few years before, somewhere in between, that I suddenly realized, or somebody made me realize that money mattered. I no longer remember now.

FISCAL AND MONETARY POLICY RULES

Taylor: Of your two early articles on stabilization policy, the first one is on fiscal policy rules, which had implications for money, of course, and the second one focused more on money growth rules. Could you talk a little about that?

Friedman: Sure. In the earlier paper, I was at the point where I would say money is important but the quantity of money should vary countercyclically—increase when there was a recession and, the opposite, decrease when there was an expansion. Rules for taxes and spending that would give budget balance on average but have deficits and surpluses over the cycle could automatically impart the right movement to the quantity of money.

Then I got involved in the statistical analysis of the role of money, and the relation between money and money income. I came to the conclusion that this policy rule was more complicated than necessary and that you really didn't need to worry too much about what was happening on the fiscal end, that you should concentrate on just keeping the money supply rising at a constant rate. That conclusion was, I'm sure, the result of the empirical evidence.

Taylor: Was part of the reason for the change that the link from deficits and surpluses to changes in money growth were not so tight with changes in the money multiplier?

Friedman: Partly it was that, and partly it was that the link from fiscal policy to the economy was of no use.

Taylor: I remember Bob Lucas saying, in reference to your constant money growth proposals, that they were designed to work in the long run, but that, when you thought about it, they worked well in the short run too. Were you thinking more of the long run? How did you think about the short run?

Friedman: I'm sure I was thinking more of the long run. I've always had the view that you ought to try to design policies for the long run. Given the view that you want the role of government to be stable, that immediately imposes on you a long-run point of view.

Taylor: Did you have a sense that they would work well in the short run?

Friedman: I don't think so.

Taylor: But didn't your first proposal have some of that? If you increase money growth in a recession because of the deficit, and if you retract money growth in a boom because of the surplus, that seems to me to be a short-run consideration.

Friedman: That was short run. That was still the relic of the Keynesian thinking. It was really a waste, I think, trying to reconcile the Keynesian thinking with the monetarist thinking.

Taylor: Was there any relationship between your thinking about these monetary control issues, and your work in statistical analysis? Did you think about these policy problems as regulator problems, thermostats, in any way?

Friedman: Oh yes, I'm sure I did. Thermostatic analysis goes back decades. There were several articles by Levis Kochin, at the University of Washington, on thermostatic analysis of the relation between the quantity of money and the economy.

Taylor: Continuing on the issues of money and monetary policy, in the early 1950s you were one of the very few people who were talking about money, but real controversy developed later, perhaps not until the 1960s.

Friedman: There was no controversy in the sense that I was simply way out in left field. In the 1950s, Chicago and UCLA, maybe, were the only places where anybody was talking about money.

Taylor: Did you think your proposal for a fixed money growth rule or your empirical work on the importance of money in the economy was more responsible for setting off the debate?

Friedman: I'm not sure what you're asking. For the fixed-growth rule to make sense you had to have an empirically supported theory with money in the model. The fixed-growth rule was not original with me; it's a rule that was recommended repeatedly decades ago by different economists.

Taylor: You certainly get the credit for most of it and you deserve it.

Friedman: Perhaps I was a better publicist.

Taylor: But if you explain things more clearly and explicitly than others, you put yourself out further on a limb and therefore you deserve more of the credit when you are right.

Friedman: Certainly the argument that money plays an important role in the economy has been settled. That was the result of the so-called radio AM/FM debates [Ando and Modigliani versus Friedman and Meiselman].

Taylor: Yes, that debate is not going on much anymore.

Friedman: It's over, everybody agrees fundamentally.

Taylor: Agrees with you?

Friedman: In large part, but not wholly. I still have more extreme views about the unimportance of fiscal policy for the aggregate economy than the profession does.

USE OF MODELS IN MONETARY ECONOMICS

Taylor: In looking back at these monetary versus fiscal debates it seems that most of your articles are empirical rather than theoretical. Macroeconomic models appear sometimes, but they are not the main focus. Would you agree with that?

Friedman: I believe that one reason the work had whatever effect it has had is because it did have an empirical base. I believe that I can honestly say that I never reached a judgment about monetary or fiscal policy because of my beliefs in free markets. I believe that the empirical work is independent and honest in that sense.

If fiscal policy had deserved to play a much larger role, that would have shown up in the data.

Taylor: In your work in consumption theory, for example, there is a more explicit model than in your work in the money area. Is that because you feel it's just too difficult to use models in the latter. Is macro a much more difficult area? Why do you think there is that difference?

Friedman: I really don't know. I think it's partly to do with the use of mathematics in economics in general, and I go back to what Alfred Marshall said about economics: Translate your results into English and then burn the mathematics. I think there's too much emphasis on mathematics as such and not on mathematics as a tool in understanding economic relationships. I don't believe anybody can really understand a 40-equation model. Nobody knows what's going on and I don't believe it's a very reliable way to get results.

Taylor: Didn't the work you did during the war involve complex mathematical models?

Friedman: They very seldom had models of that kind. The one place where you seem to be having that kind of modeling now is in the debate about global warming. And those models seem to be very unreliable and inaccurate. But if you think of physics, they usually have models with only a few equations. In any event, if you have a lot of equations, you ought to be able to draw implications from them that are capable of being understood. You should not present the model and say, now its up to you to test. I think the person who produces the model has some obligation to state what evidence would contradict it.

Taylor: I know that many people who follow the overall economy worry about using models for the reasons you're saying. But do you think the models can be helpful just to keep track of the many relationships?

Friedman: I don't want to say you shouldn't use models. Somebody will come up with one that will prove me wrong. People should do what they want to do. But I think, on the record, you've got to ask yourself whether large-scale modeling is going to continue to exist? You can't do without models—don't misunderstand me. You always have to have some kind of theoretical construct in your mind and that's a model. I think the large models are conceptually different from those with a few equations.

USE OF TIME-SERIES METHODS

Taylor: In recent years, you have had some debates with David Hendry about statistical issues relating to your empirical work on money. And that's related to the use of modern methods of statistics and time series. Could you describe your views about various approaches to time-series analysis? Where do you see some advantages and disadvantages?

Friedman: I think the major issue is how broad the evidence is on which you rest your case. Some of the modern approaches involve mining and exploring a single body of evidence all within itself. When you try to apply statistical tests of significance, you never know how many degrees of freedom you have because you're taking the best out of many tries. I believe that you have a more secure basis if, instead of relying on extremely sophisticated analysis of a small fixed body of data, you rely on cruder analysis of a much broader and wider body of data, which will include widely different circumstances. The natural experiments that come up over a wide range provide a source of evidence that is stronger and more reliable than any single very limited body of data.

Let me put it another way. I don't believe that we can possibly understand enough about the economy as a whole to be able to predict or interpret small changes. The best we can hope for is to be able to understand significant larger changes. And, for that, you want a wide body of data and not a narrow body of data. If you have a complex model and then try to extrapolate outside of that model, it will not be very reliable.

I learned that lesson very well while I was at the Statistical Research Group, going back to that. One of the problems I worked on was a metallurgy problem with an application to jet engines. There was a big project during the war of trying to determine the alloy that would have the greatest strength under high temperatures. We were called in as statistical consultants to the various groups working on the problem. I had a lot of data from all their experiments. I computed a multiple regression using these data—data that had been derived by hanging a weight on an experimental turbine blade to see how long it took for the blade to rupture at a given temperature. I regressed the length of time to rupture on the chemical composition and various other variables based on the best metallurgical theory I could find. I got an excellent correlation. So I used my regression to predict what new alloys would have a longer time before rupture. I got wonderful results even though I insisted on restricting every variable separately to the range of values that had been used in the experiment. My equation predicted something like 200 hours until rupture for my constructed alloy. That would have been an enormous success compared to the existing alloys.

Unlike in economics, we could put the prediction to a test. I called some people up at MIT and they constructed this alloy and tested it. And it took an hour, or maybe 2 hours, to break. It was an utter failure! That taught me that you could not depend on a narrow range of evidence using a lot of variables. I think I had a half-dozen or more variables.

By the way, at that time we did not have our present high-speed computers. So on that occasion I had to use the Mark I or some big machine up at Harvard, which was a collection of IBM sorting equipment. With the desk calculators we had, it would have taken 3 months to compute the regression. It took 40 hours up at Harvard. That was an enormous achievement. Now it would take 5 seconds on my Mac.

Taylor: So, did you have to have more discipline in trying out different regressions then?

Friedman: Boy, you sure did! Improvements in computing capacity have made this problem much more serious. It is so easy to fish around for high correlations.

I don't have any confidence in a correlation obtained that way. People today pay all too little attention to the quality of data they're analyzing as opposed to the sophistication of the methods they use.

Taylor: As you described earlier, your first few jobs were very data-intensive. Do you think that kind of work is rewarded very much today?

Friedman: No, it isn't rewarded today.

Taylor: And many young economists do not seem to find it as enjoyable as more theoretical work. Did you find it enjoyable?

Friedman: Well, yes. I did and I do. It's kind of fun trying to figure out what's wrong with the data, like these charts we were looking at. Why is this damn thing happening? Is this is a pure data issue? Then we can think of all these great theories we love to try to explain the data, and that's where the fun comes in.

REAL-BUSINESS-CYCLE MODELS, CALIBRATION, AND DETRENDING

Taylor: A related question on statistical analysis, and on time series in particular, concerns the trend in the economy, whether you come back to a deterministic trend or not. Some real-business-cycle work was generated by the notion that real GDP does not come back to trend. What do you think about the real-business-cycle view?

Friedman: Well, I've always been rather skeptical about the real business cycle, primarily on the grounds of its empirical methodology, which is not to try to fit the data, but rather to calibrate. I think that's not a reliable way to get good results. I think Slutsky proved that years and years ago.

Taylor: Can you elaborate a little bit on that? Why don't you think that's a legitimate way to proceed?

Friedman: It's a perfectly legitimate way to derive hypotheses, but it doesn't test them. If I show you that with this calibration I get results that look like the observed data, okay, that's interesting. But why don't you go test it and use your analysis to see if you can reproduce real data that way and predict it for a period for which you did not have the data when you formulated your hypothesis. Either backward, or for another country, or something.

Taylor: So, just the fact that it looks like a business cycle is not enough?

Friedman: That's what I say. Slutsky proves that with an accumulation of random shocks. Maybe Slutsky's series are right there [pointing to Figure 1], I do believe that short-run fluctuations in the economy are simply the accumulation of random shocks. I don't believe there is such a thing as a business cycle. I think there are fluctuations and there are reaction mechanisms. Various parts of the economy react systematically to shocks to the system, but in the sense of regularly recurring cycles, the kind of thing that Mitchell was trying to describe, I don't think they exist.

Taylor: What about the notion that the economy returns to a trend after a recession?

Friedman: Well, I don't know what the opposite view is.

Taylor: The opposite view is that if you are at the bottom of a recession, then your best guess is that you're going to have only trend growth from that point onward.

Friedman: Oh, I see what you're saying. Oh, no, no, I think that there is a basic equilibrium position and the economy as a whole will tend to return to it. But that trend may change sometimes. Surely if something has been going on for 100 years, you've got to be a little skeptical in saying it's not going to go on again!

THE NATURAL RATE HYPOTHESIS

Taylor: Let's talk about a concept of equilibrium that you have made famous—the natural rate of unemployment. Your presidential address to the American Economic Association in December 1967 was on the Phillips curve and the natural rate hypothesis. It must have been quite an event. Could you talk a little about how that happened?

Friedman: The basic ideas in my presidential address were already present in a comment that I made at a conference on guidelines, the proceedings of which were published in a 1966 book edited by George Shultz and Robert Aliber.[2]

I'm sure the basic idea grew out of the discussions about guidelines and, in particular, out of the Samuelson and Solow paper on the Phillips curve. I can't say exactly where my ideas originated; all I know is by the time I gave the presidential address in 1967, there was nothing new in that compared to what I had earlier published. Arthur Burns was in the chair when I gave the presidential address, and he had gone over the address earlier. Arthur always went over my papers.

Taylor: You're kidding. He would read all your papers?

Friedman: Sure, and I went over his. Despite what I said about his chairmanship of the Fed, Arthur was a first-rate economist. He had a feeling for the English language and an ability to use it, which was unusual. He was always one of the most valuable critics of anything I wrote. He didn't always agree with what I wrote, don't misunderstand me, and I'm not sure on this occasion that he agreed with me, but he was one of the people who had commented on early drafts of the paper. At the time, I never had any expectation that it would have the impact it did. It only had that impact because of the accidental factor that you had a test right after.

Taylor: Yes, very impressive.

Friedman: This was one of the few occasions when something was predicted in advance and confirmed later.

Taylor: Did you think much in advance about whether this would be a good topic for the presidential address?

Friedman: You want to talk on what you are working on, and the major focus of my work at that time was monetary policy, so I talked about the role of monetary policy.

Taylor: That work has, of course, generated much work by others. One could argue that the whole rational expectations revolution came out of that research because you focused on expectations.

Figure 5. At 80th birthday party, 1992, given by Frazer Institute in Vancouver.

Friedman: I think the focus on expectations was important. But as for rational expectations, I think you have to give Bob Lucas a lot of credit for that.

RATIONAL EXPECTATIONS

Taylor: That brings me to the question about what causes the short-run impact of money. Do you feel that it's mainly unfulfilled expectations or do you think that sticky prices and wages play a role?

Friedman: You've mentioned both the things that are no doubt the legitimate causes. After all, a wage agreement is not for a day, it's for a year, 2 years, 3 years. It's costly to change prices and so on, but I think the most important single thing is the tendency for expectations to be backward looking and to be adjusted slowly so that it takes time before any expectation is altered by the impact of an event.

Taylor: Does that mean you disagree with rational expectations?

Friedman: I have no basic disagreement with rational expectations. The question is, "how do you form your rational expectations." Let me start over. You are talking about what's going to happen tomorrow. The price is either going to go up or it isn't. If it goes up, the probability that it went up is one; the probability that it went down is zero.

What you are doing with rational expectations is to ask yourself, what is the probability that the movement tomorrow will be up or the movement tomorrow will be down. And now the thing that you have to ask yourself is, "I have an expectation. How do I know after the event whether that expectation was fulfilled or not? I said the probability that the price was going to go up was 60%; now, it actually went up. Does that confirm it? I can't tell. I have to have a lot of similar cases." And so, the notion of "correct rational expectations" is a notion I find very hard to give much content to.

If the idea is that people try to predict what is going to happen tomorrow, then rational expectations, in that sense, certainly makes sense, but on what do they base their rational expectations? They base it on past experiences; there is always going to be a lag in expectations catching up.

ROLE OF DEBATES IN MONETARY ECONOMICS

Taylor: In my view the debates in macroeconomics have helped get people interested, and this has motivated more research. Was there some strategy behind your role in generating debate?

Friedman: I don't think so. It just happened. I think most of the things that just happen are likely to be more valuable and interesting than those you plan!

Taylor: How did you get to be such a good debater? Did that just happen too?

Friedman: That just happened, too.

Taylor: You weren't a debater in college?

Friedman: I may have been involved, but that was not a major activity of mine. I just like to talk, that's all! And I like to argue. I enjoy the stimulus of arguments back and forth, but I never did anything special to improve my skill as a debater.

Taylor: Well, I do think it's an effective way to get people interested.

Friedman: It is, I agree with you. What people like is that a person is willing to take positions. He's not hedging all the time. The idea of the one-armed economist, one-handed, I guess.

Taylor: I always have to watch when I say "on the other hand."

Friedman: Right!

Taylor: Is hedging your views something that you strive not to do?

Friedman: No. It's the way I am. You know, somehow or other, people have a tendency to attribute to me a long-term plan; they think I must have planned this campaign. I did no planning whatsoever. These things just happened in the order in which they happened to happen. And luck plays a very large role, a very large role indeed. Take the effect of presidential elections.

CAPITALISM AND FREEDOM TODAY

Taylor: Let me ask about your work on capitalism and freedom. *Capitalism and Freedom* was published in 1962 and has influenced people all over the world, but you did not do a second edition. Is there a reason?

Friedman: I think *Free to Choose* is, in a sense, another edition, from a different perspective. But since my main activity was science and economics, this is essentially a secondary activity.

Taylor: You mean to say that *Capitalism and Freedom* was secondary?

Friedman: Oh, sure. It was a series of lectures I gave at Wabash College in 1956 at a summer conference for assistant professors. The organizers wanted me to talk about free markets and those lectures were really the basis for *Capitalism and Freedom*. It was not a book that was conceived from the outset as a book.

Taylor: Did you take much time to write them?

Friedman: I had to spend time preparing the initial lectures and I also spent a lot of time editing the volume, but it was an avocation rather than a vocation. My wife did most of the work of turning the transcripts of the lectures into publishable prose.

Taylor: As your public policy work is in general?

Friedman: It's always been an avocation. I've often had students come up to me and say that they want to promote free markets or they want to get involved in politics and the advice I uniformly gave is, don't do that as a profession. Get yourself established in something you believe in and can work in and which has no necessary ideological component so you have a little nest. Then go on and get involved in public policy, otherwise the public policy will impose itself on you and will affect what you believe rather than your beliefs affecting it. That's why I think that people stay in Washington too long.

Taylor: I remember one time when I was working in Washington, as a member of the Council of Economic Advisers, you said as much to me. I called to get your support on an important policy issue, and your first answer was, "why don't you just come back to Stanford. You have been there too long." But how did you manage to have so much impact?

Friedman: I stayed away from Washington.

Taylor: Would you like to see a new *Capitalism and Freedom*, one that would be oriented to where we are now? In many respects the world has moved in the direction that you advocated. Do we need another book? Do you think we have moved?

Friedman: We need another one, but I can't write it. In many ways we are worse off. Government spending as a fraction of income is higher now than when *Capitalism and Freedom* was published. A good deal higher. Unless I'm mistaken, I think it was 30% then and 40% now.

Taylor: That is for the United States?

Friedman: Yeah, just for the United States. And also worldwide; I once got together a list of 10 to 12 countries and how much they were spending as a fraction of income, and in every single country the fraction of income spent by government had gone up. We're much better off in the realm of ideas. The intellectual climate of opinion is more favorable to a free-market society, but the practical world is less favorable. Just look at the regulations we've got now that we didn't have then.

Taylor: That's true, there is more social regulation, but millions of people around the world have been freed from communism.

Friedman: That's true. In the former communist countries, there's no doubt. In a country like Britain, France, or Germany, I'm sure there are more regulations now than there were 30 to 40 years ago, so that, far from having moved in the right direction, in practice it's moved in the wrong direction. And that's why, going back to your comment, that's why we need another *Capitalism and Freedom* to start from where we are now.

MONETARY UNIONS AND FLEXIBLE EXCHANGE RATES

Taylor: Let me ask a question about monetary issues that relates to the global economy. You have Europe's new single currency, and you have Bob Mundell arguing that we should have one world currency. You also have talk about dollarization in Argentina and a greater commitment to floating in Brazil. Where is this all going?

Friedman: From the scientific point of view, the Euro is the most interesting thing. I think it will be a miracle—well, a miracle is a little strong. I think it's highly unlikely that it's going to be a great success. It would be very desirable and I would like to see it a success from a policy point of view, but as an economist, I think there are real problems, arising in a small way now when you see the difference between Ireland and Italy. You need different monetary policies for those two countries, but you can't have it with a single currency. Yet they are independent countries; you are not going to have many Italians moving to Ireland or vice versa. So I do not share Bob Mundell's unlimited enthusiasm for the Euro. But it's going to be very interesting to see how it works. For example, I saw a study in which somebody tried to ask the question, "What is the effect of having a common currency on the volume of intercountry trade?" And the result was surprising. It was that having a common currency had a surprisingly large effect, about four times the effect of geographical proximity or of flexible exchange rates. Now that was just a small sample.

Taylor: And beware of multiple regressions!

Friedman: Right! At any rate, one thing that I could be leaving out in my evaluation of the dangers of the Euro is the effect of a common currency on the volume of trade between the countries. If it has a major effect on trade, it may enable trade to substitute for the mobility of people.

Taylor: Do you think that the depreciation of the Euro is bad sign [it was about $0.90 at that time]?

Friedman: No, not for a second. At the moment the situation is very clear. The Euro is undervalued; the U.S. dollar is overvalued. As a result of the undervaluation of the Euro, the producing enterprises in Europe are doing very well, the consumers in Europe are suffering, the consumers in the United States are getting a good deal, and the opposite is true for the producers in the United States. And there's very little doubt that within the next few years that's going to come together. Relative to the dollar, the Euro will appreciate and the dollar will depreciate.

Taylor: One of your most famous articles is the one advocating flexible exchange rates, though you stressed microeconomic speculation more than macroeconomic issues in that article. Do you want to say something about how that article came about?

Friedman: That article originated from 3 months I spent in France as a consultant to the Marshall Plan agency in 1950. At the time, the German mark was having balance-of-payments problems and I was asked to analyze proposed solutions. I concluded that the best solution would be to float the exchange rate, but that was so far out of sync with the attitudes of the time that it was summarily rejected.

Taylor: That article, like many others of yours, has been tremendously influential.

Friedman: Yes, I think it has been very influential.

Taylor: Does it surprise you sometimes, the things that are more influential than others?

Friedman: I think it's almost impossible to predict what will be influential. You know that from your own work. You never dreamed when you presented the Taylor Rule that it was going to become worldwide conventional wisdom.

Taylor: I think that's true.

Friedman: It's an accident what happens to get picked up and what doesn't. It depends on the circumstances that develop afterward.

Taylor: Well, that's sounds like a good place to end, but maybe I should just ask one more question: Is there anything else you want to say?

Friedman: I don't want to say anything else. I've already said too much.

Taylor: Thank you. I have enjoyed this interview greatly.

NOTES

1. On editing the transcript of our conversations, Milton Friedman added the following explanation of his reference to "thermostatic control":

The temperature in a room without a thermostat but with a heating system will be positively correlated with the amount of fuel fed into the heating system and may vary widely. With a thermostat set at a fixed temperature, there will be zero correlation between the intake of fuel and the room temperature, a negative correlation between the intake of fuel and external temperature. Also, the room temperature will vary little.

By analogy, without a successful monetary policy to stabilize the economy (thermostat), there will tend to be a positive correlation between the quantity of money (the fuel) and GDP (the temperature), as there is in Figure 1 before 1992, and both may vary widely. With a successful monetary policy, there will be a zero correlation between the quantity of money and GDP, as there is in Figure 1 after 1992. Money may still vary widely, but GDP will vary little, as in Figure 1 after 1992.

2. Solow, R.M. (1966) "Comments on 'The Case against the Case against the Guideposts."' In G.P. Shultz & R.Z. Aliber (eds.), *Guidelines, Informal Controls, and the Market Place*, pp. 55–61. Chicago: University of Chicago Press.

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