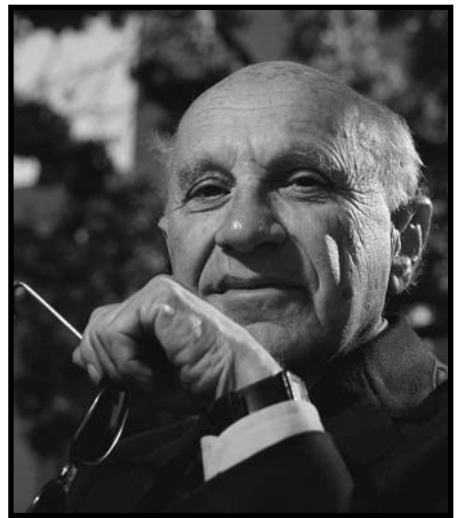


Inside the Economist's Mind

by john b. taylor

Milton Friedman, you probably remember, died last Nov. 16 at the age of 94. What you may not remember is that long before he became a libertarian icon and polemicist on behalf of free-market capitalism, he was an incredibly prolific economic theorist with few peers in his era. ¶ This in-depth interview of Friedman, which took place on May 2, 2000, is excerpted from the recently published collection, *Inside the Economist's Mind* (edited by Paul Samuelson and William Barnett).* It is all the more interesting because the interviewer is John B. Taylor, a former undersecretary of the Treasury (now a professor at Stanford) who is one of pre-eminent macroeconomists of his generation. Taylor's occasional notes are bracketed; my own are in italics.



— Peter Passell

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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 20th century.” These words in praise of Milton Friedman are from economist and [former] Federal Reserve Chairman Alan Greenspan. They were 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 somewhere in 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 [of unemployment] theory
- the case for floating exchange rates
- money-growth rules
- the optimal quantity of money
- the monetary history of the United States, especially the problematic role of the Federal Reserve in the Great Depression.

And, of course, his achievements outside macroeconomics are almost as remarkable – 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 illicit drugs.

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 policymaking 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 his 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 and then went to study economics at the University of Chicago, where he met fellow graduate student Rose Director, whom he later married.



Rose and Milton

For nearly 10 years after he left Chicago, he worked at government agencies and research institutes (he spent one year visiting at the University of Wisconsin and one 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 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 get pleasure from 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 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 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. Those charts – which he had recently prepared from data he had downloaded from the Internet – raised questions about some remarks that I had given several weeks before at a conference, which he had read about on the Internet.

As we began talking about them, 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.

Soon we segued into the series of questions that I had planned to ask (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 broad topic areas.

on money growth, thermostats and alan greenspan

Milton Friedman: I thought that you'd be interested in this chart [referring to the figure below]. Don't you think it's as if the Fed has installed a new and improved thermostatic controller in the 1990s!

John B. Taylor: I can see that there is a change in the relationship between money growth and real GDP, and that the sizes of fluctuations in the economy have diminished greatly. There is much greater stability starting in the early 1980s. 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?

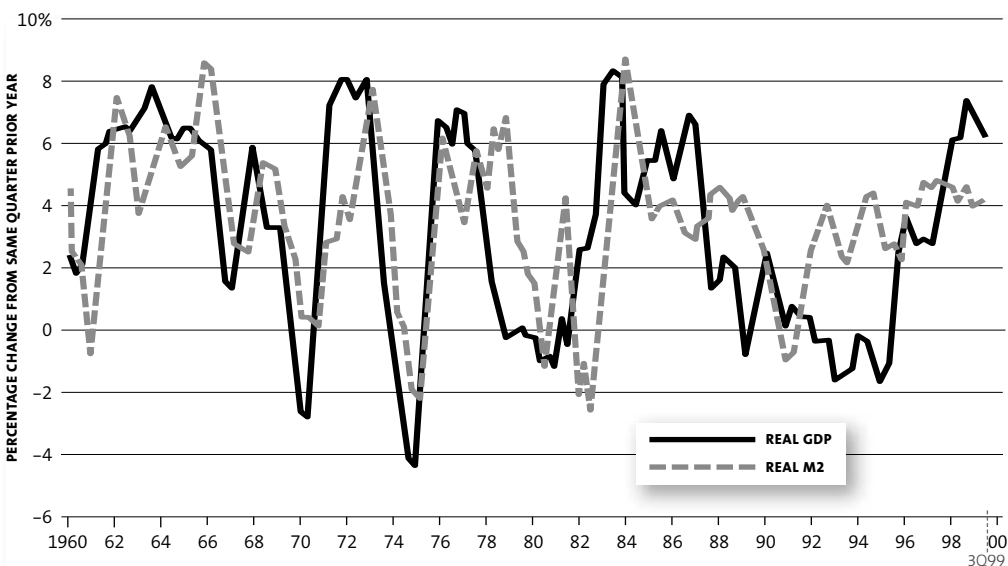
MF: I'm baffled. I find it hard to believe. They haven't learned anything they didn't know before. There's no additional knowledge. Literally, I'm baffled.

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

MF: I believe 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 1980s. The other is the breakdown in the relation between money supply and GDP. That came in the early 1990s, 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 affect it that other people don't have?

ANNUAL CHANGE IN U.S. REAL MONEY SUPPLY AND REAL GDP



SOURCE: Economic Report of the President, 2001

JBT: Well, it's possible.

MF: Another explanation is that the information revolution has enabled enterprises to manage inventories so much better. But inventories can't be the answer because the same thing has happened to non-inventories.

JBT: 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.

MF: It may be a statistical artifact. They [the Fed] may have somehow changed their methods. There have been significant changes in estimation.

JBT: 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? [*The rate money turns over in transactions. Formally, the ratio of the money supply to GDP.*]

MF: But then again, if you look at these shifts in velocity, they don't come until 1992. It looks as if somehow in 1991-92 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.

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 (a thermostat), there will tend to be a positive correlation between the quantity of money (the fuel) and GDP (the temperature), as there was 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 after 1992.

Money may still vary widely, but GDP will vary little, as after 1992.

on causes of the great inflation and its end

JBT: 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 1970s and what caused it to end.

MF: 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 non-inflationary economic conditions he inherited to "get the economy moving again." With zero inflationary expectations, monetary and fiscal expansions primarily affected output. The delayed effect on prices came only in the mid-1960s 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 it. In the 1970s, 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 percent 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.

I had a session with Nixon sometime in 1970, it 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 re-elected." And he said, "Well, we'll worry about that after we get re-elected." Typical. So there's no doubt what Nixon's pleasure was.

JBT: 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 [*the alleged tradeoff between inflation and unemployment rates*]?

MF: 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.

JBT: What about the end of the Great Inflation? It lasted beyond Burns' time. We had G. William Miller and then Paul Volcker [*as chairmen of the Fed*].

MF: 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.

They did step on the brake, and in addition, sometime in February 1980, President 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, money supply growth slowed down. If Carter had been elected, I don't know what would have happened. However, Reagan was elected, and

Nixon had a higher IQ than Reagan, but he was far less principled.

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 increase the money supply and at that point the recession came to an end and the economy started expanding.

JBT: Our 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?

MF: I may be overemphasizing Burns' 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 that you could not slow down the inflation without having a recession.

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

MF: 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 Reagan and Nixon would have acted as they did if they had never seen me or heard from me.

on graduate school and early “on the job” training

JBT: 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?

MF: 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 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.

JBT: 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.

MF: 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, 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.

JBT: 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 such an article?

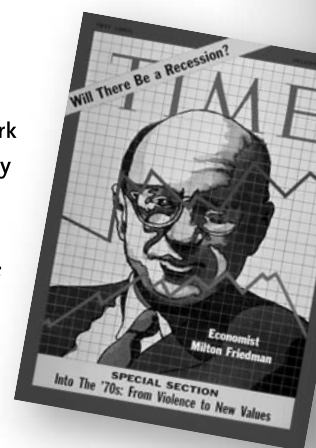
MF: Schultz's book that I was working on was on the theory and measurement of demand. The Pigou article was on the elasticity of demand, so it came right out of what I was doing with Schultz. He probably suggested that I publish it; I don't remember.

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

MF: 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 Frank Taussig [a great economist at Harvard] 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 *QJE* because it was refereed by Vassily Leontief. Then Pigou submitted a criticism of it to the *QJE* and Taussig wrote to me and sent me a copy of the criticism. The *QJE* then published both Pigou's criticism and my response.

JBT: Did that experience whet your appetite for controversy?

MF: I really can't say; it was 65 years ago.



JBT: 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!

MF: 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.

JBT: 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?

MF: 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 every one of the current 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 Harriss and Bill Vickrey [*the latter much later to win a Nobel Prize*].

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

MF: 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.

JBT: 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?

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

JBT: 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?

MF: No economics at all. I shouldn’t say *no* economics. 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.

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

MF: They 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.

I hate those words, micro and macro.

I think it's price theory and it's monetary theory.

Why the hell do we have to use these Greek words?

.....

JBT: What kind of problems did you work on?

MF: We were primarily concerned with such problems as: You've got an anti-aircraft 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 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.

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

MF: I mean it in a broader sense. What we discovered 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 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.

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

MF: 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 [*a Hungarian-American econometrician*], 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."

JBT: 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?

MF: 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 anti-aircraft 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.

JBT: That sounds like an amazingly complex problem. Did you write up papers or reports?

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

I've never been willing to publish something under my name written by somebody else.

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

MF: 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. 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.

on the return of monetary economics

JBT: When did your interest in monetary economics begin, exactly?

MF: 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 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

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.

on fiscal and monetary policy rules

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

MF: 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 decrease when there was an expansion. Rules for taxes and spending that would give budget balance on average. But budget 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.

JBT: 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?

MF: 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.

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

MF: I don't think so.

JBT: 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 reduce money growth in a boom because of the surplus, that seems to me to be a short-run consideration.

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

JBT: 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?

MF: Oh yes. 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.

JBT: In the early 1950s you were one of the very few people who were talking about money. But real controversy developed until later, perhaps the 1960s.

MF: 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.

JBT: 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?

MF: 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.

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

MF: Perhaps I was a better publicist.

JBT: 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.

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

There was no controversy in the sense that I was simply way out in left field.

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JBT: Agrees with you?

MF: 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.

on the use of models in monetary economics

JBT: 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?

MF: I believe that one reason the work had whatever effect it has had is because it did have an empirical base. 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 a much larger role, that would have shown up in the data.

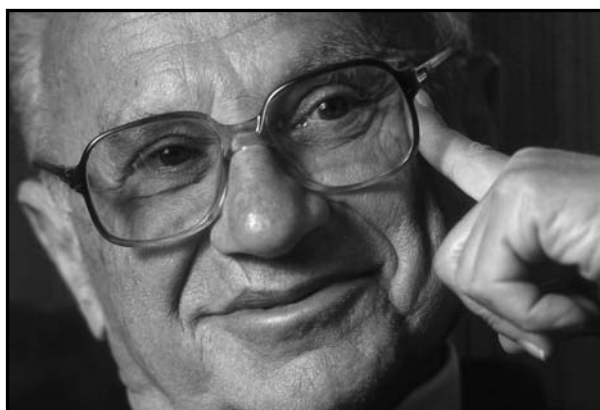
JBT: In your work in consumption theory, for example, there is a more explicit model than in your work in the money area. Is that because you feel it's just too difficult to use models in the latter? Why do you think there is that difference?

MF: 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.

I go back to what Alfred Marshall said about economics: Translate your results into English and then burn the mathematics.

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

MF: 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 it's up to you to test. I think the person who produces the model has some obligation to state what evidence would contradict it.



JBT: 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?

MF: I don't want to say you shouldn't use models. Somebody will come up with one that will prove me wrong. But I think 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.

on the natural rate hypothesis

JBT: 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?

MF: 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

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.

JBT: Do you think that the depreciation of the euro is bad sign? [*It was worth about \$0.90 at that time.*]

MF: 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. [*He was right; the euro was worth more than \$1.30 at the end of 2006.*]

JBT: 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?

MF: That article originated from three 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.

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

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

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

MF: 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 world-wide conventional wisdom. [*Taylor's widely used prescription for setting the Fed's "overnight" interest rate as a weighted average of the inflation rate and the gap between actual and potential GDP*]

JBT: 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.

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

