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Interview of Milton Friedman

Responding to Questions Posed in Writing by Robert L. Hetzel

January 5, 1997

Milton Friedman: Bob, this is a continuation of my answers to the points you raised in your series of questions and points attached to you letter of July 18, 1996.

I have already answered for you, in the tape I sent you earlier, the questions on the first 20 pages. I start on page 21, the natural rate hypothesis, re Roman III, IV, and V.

Important to distinguish between the sense in which people know something and the sense in which it really becomes part of their working apparatus or vocabulary, as it were. The distinction between anticipated and unanticipated changes is very old in economics. You can find statements in Schumpeter way back, I'm sure in Marshall and elsewhere, on the difference between the two. Certainly Irving Fisher, back in the 1890s or whenever it was that he wrote on the interest rate, was very explicit about the difference between anticipated and unanticipated changes in inflation.

However, what had happened, and then a Keynesian simple apparatus was extremely attractive to economists. It tended to neglect the difference between anticipated and unanticipated. If you had asked any Keynesian would it make any difference, he would immediately have said yes, but he did not build it into the model with which he was working. It was not part of his day-to-day apparatus.

The sense in which the profession was influenced was in being forced, as it were, to abiding factual evidence more than by anything else, to [skipped] the crucial detection between anticipated and unanticipated into their day-to-day working model. I've always argued that the role of economists in public policy is not to persuade people or anything, but to keep open alternatives when a crisis arises; and when an alternative has to be selected, have one available.

My favorite example has always been floating exchange rates. We never persuaded anybody of floating exchange rates, but a crisis arose where you had to have floating exch-, well, you had to do something. And because the theory of floating exchange rates had been discussed, elaborated, worked out, it was something to do.

This is the same kind of thing. It was not the arguments, in my opinion. It was the factual evidence that the complete breakdown of the predictions of the Keynesian model that then forced the profession to change its working apparatus. The role that Phelps and I played was simply in making it clear that there was a way to account for that phenomenon.

As to dating, it's hard to know when anything develops. There's no question that the notion of that was expressed in my Presidential address and that I had been expressing for years before that was not a new idea. The explicit formulation of [the writing of it down to the? 00:04:24] in the more acceptable and persuasive fashion does date from then, but my own adoption of the idea surely goes much earlier.

Re Roman IV, item C, that is a very strange paper issue and I wish you would look at it. The empirical evidence that they present in that paper is simply very unpersuasive, and must have been to them as well as to anybody else. They never had data from which they could derive the very simple curve that they presented as a Phillips curve that you would count on for policy purposes.

[00:05:19]

Robert Leeson, L-E-E-S-O-N, from Australia, who has been devoting his research largely to Bill Phillips's work and to the Phillips curve, in some papers he has written—I'm not sure whether these are published or these are drafts he sent to me—has raised the question whether that paper, presented as it was in 1960, in the year in which Kennedy was elected to the Presidency, was not affected by the role that both Samuelson and Solow played as advisors to Kennedy.

You will recall that Kennedy's slogan was that he was going to get the country moving again. And the way in which he proposed to get the country moving again, as suggested to him by Samuelson and Solow, was by exploiting a Phillips curve. Did they really believe there was a long-term Phillips curve? I have too much respect for their intellectual ability to believe that they really did. But it was an extremely useful bit of apparatus for a purpose.

Walter Heller was a different problem. He wasn't in the same class intellectually as either Samuelson or Solow, and he became a really true believer in a sense which I'm not sure either of them ever was. However, I am now getting into psychoanalysis rather than economic analysis, and that's something that you and I should both stay away from.

I will say, however, that after Leeson called and emphasized this point and asked whether I thought that was a possible explanation. And I went back and reread the paper, and I had a great inability in rejecting interpretation. The first part of the paper is very good. They state all of the limitations and qualifications. But then they come out, as I recall it, without qualifications, on a very well-defined curve.

It may be worth noting that that was not a paper submitted to a journal and refereed. It was a paper given at a meeting of the American Economic Association, and as a result,

published in the proceedings without refereeing. I am saying that from memory, but I believe it is right.

Re your point Roman V b, I question the first reason you mention. I believe that the economists at that time, or at least a great many of them, attributed the broader economic stability to the passage of the Full Employment Act and the establishment of the Council of Economic Advisors. That is, they took it for granted that the role of economic stability reflected better economic policy. And, in a sense, it did. Monetary policy was much stabler during that period. It was better. But, of course, the dominant Keynesian view was that fiscal policy was playing the role, and that it was responsible.

I think the second reason is correct.

Re your V c, and indeed the whole general approach here, I believe you must go beyond the natural rate idea and approach and the new classical approach to something very different, which was the accumulating evidence that undesirable behavior occurs because of government interference; the government is cause and not a solution to the problem.

[00:10:12]

The wider acceptance of that, I think, owes a great deal to our own monetary history, published in 1963, and the work that George Stigler was doing on regulation; his successful studies demonstrating that the regulators were serving to influence the people they were supposed to regulate rather than that of the public, and that regulation did not, in fact, lead to the results that it was intended to lead to.

I believe there was a good deal of additional empirical study and material coming out along these lines. And I believe all of that played a very important role in starting to change the attitudes of the economic profession about the role that government could or should play.

Re VI a, I never did come up with a model that satisfied me as explaining the determination of a short-run division of a change in nominal income between prices and output. Empirical puzzles abound. One of the more recent is the experience in Japan in the ‘80s and ‘90s. After the Louvre Agreement, when Japan tried to peg the yen-dollar exchange rate, the quantity of money started going up at a very much more rapid rate.

I predicted, some of the key Japanese monetary economists, such as Yoshio Suzuki, S-U-Z-U-K-I, predicted that this was going to lead to inflation and a typical boom. What happened was that there was very little inflation, except in land and stock prices. Now, why is it that on this occasion, the expansionary monetary influence seemed to be concentrated in those areas, and had relatively—it had some effect, but relatively little effect on either real income, or the level of prices of consumer goods?

When in the 1980s the Japanese Central Bank finally put its foot down and started reducing the rate of growth and the quantity of money very sharply indeed, the bubble collapsed; in stock markets, the bubble collapsed in prices. It was an economic recession.

But there was nothing as severe as when one would have expected from the change in monetary growth rates alone. I do not believe—perhaps I am wrong, I haven't followed the literature recently, but I do not believe we have a satisfactory explanation of that episode.

Re b, there is no doubt that I thought, and the evidence strongly indicates, that expectations adapt much more quickly the higher the rate of inflation, or for that matter, deflation. The lag between monetary growth and nominal income growth is unquestionably much shorter; more volatile is monetary growth. And that is indeed why I was interested in working with Maurice Allais, which gave you a time scale that would reflect, that would rationalize, this phenomenon.

Re VI c, I do not believe that my critical reaction of rational expectations is for the reason that you mentioned. I rather think it is for the reasons that I explained on pages 556 and 557 and 630 of *Monetary Trends*. Mainly, the difficulty of attaching meaning to quote "correctness" unquote of a personal probability judgment period.

[00:15:36]

Like the idea of the difference between action and anticipate, the basic idea of rational expectations is very old. It is the old Lincoln quote about you can't fool all the people all the time. Moreover, I remember once finding in Schumpeter a very clear statement as applied to economic issues. The contribution that Lucas made was to make it explicit and to put it in a form in which it could be used in empirical analysis.

That's the end of your numbered pages. And I go on to the three pages that follow on paraphrases.

I have not found fault with any of the paraphrases on the final question about George Stigler, which you say understates the closeness. We were students together at Chicago, and thereafter, he, for a while, went to Iowa State to teach. We corresponded regularly from that time on, exchanged papers we wrote, and criticized one another's writings.

During the war, he and I were together at the statistical research group for several years in the war when he became part of that group. He also spent some time with the National Bureau. He was responsible, in fact, for my getting a job in Minnesota after the end of the war, and we shared an office for a year in Minnesota.

I'm sure you know the story, but he was Chicago's first choice to replace Viner [00:17:45], but was turned down by Caldwell, the provost while Hutchins was out of town, on the ground that he was too empirical. George tells that story in his intellectual memoirs.

He and I were again together at the Center for Advanced Studies in the Behavioral Sciences at Stanford in—what was that?—'58 or '59, I'm not absolutely sure of the exact year. And then, of course, he came to Chicago. Allen Wallis [00:18:23] at the time was dean of the School of Business at Chicago and played a critical role in persuading George to come back to Chicago after George had turned down several earlier attempts to get him back there.

I have no doubt whatsoever that George had a great influence on what I did, and that I had a great influence on what he did.

In re a much earlier comment having to do with the acceptance, with the spread of opinion that there was greater stability in the economy in the 1960s and '70s, in looking up some of Burns's writing in connection with your very interesting piece on Burns—which I am even more derelict in replying to, which is dated May 29, 1996—I found on page 56 of his thin book on memorial lectures that he gave called "The Management of Prosperity: 1965 Benjamin Fairless Memorial Lectures," the following statement quote: "In short, several major developments—the more active role in government in promoting stable prosperity, more efficient controls, control of inventory by the business community, and structural changes in the economy—have combined to moderate the business cycle." Unquote.

[00:20:06]

That just adds to what I said earlier. It has some relevance, even though it's not clear what, to your question about Burns's views of the quantity of money. He goes on to say that quote: "Certainly no administration nowadays would tolerate destruction of one-third of the nation's money supply during a period of declining economic activity." Unquote.

There is one statement in this lecture that is relevant to the role Burns later played in wage and price control. Quote: "Whatever may have been the case a year or two ago, it should be clear, from the more recent behavior of wages and prices that the Federal government cannot continue pursuing policies that release powerful expansionist forces, and yet trust that its guideposts, prices and wages, will prevent inflation. Continuance of governmental reliance on the guideposts merely postpones collective measures. Worst still, it may excite public clamor for direct price and wage controls—a grim expedient that would indeed suppress inflation for a time, but at the cost of impairing efficiency as well as crushing economic freedom. The wise course is to check gradually the rise of expansionist credit, and to check more sharply, in view of rapidly rising military costs, the growth of Federal spending on civilian programs." End quote.

It is certainly clear from these protections, as well as from what my own knowledge of Burns's views, that prior to 1970, when he became chairman of the Fed, Arthur had always been a strong opponent of price and wage controls and of guideposts. I do not have ready at hand the proceedings in the [unintelligible 00:22:30] guidelines, etc., that had been during George Shultz may have been the author, but I believe he did have a contribution in that.

As to the role of money and inflation, that is a less clear picture. He clearly thought money played an important role, as indicated by his statements as quoted, and by his testimony after he did become Federal Reserve chairman.

On the other hand, he was not a monetarist. He, like his mentor, Mitchell, put a great deal of emphasis on the state of business confidence, and was inclined to look at many things—again, following Mitchell—rather than to concentrate on any one thing. His special forte was bringing together a variety of observations, data, information on a large number of

different things, and drawing a conclusion from them, and stating the conclusion in a way that seemed very reasonable; indeed, inevitable. In this respect, he and Alan Greenspan were very similar, in my opinion, although Alan has moved much closer to being a monetarist than Arthur ever was.

As I am sure I have told you, during the summer of 1968, I had many long discussions with Arthur in which I tried to persuade him of the desirability of closing the gold window and floating the exchange rate. I was unable to persuade him, which is, I believe, the reason why Nixon did not follow that course on first coming into office.

At the same time, I do not have a recollection that at any time during that period, he in any way hinted at the use of wage and price controls or guideposts or anything like that as an alternative to a flexible exchange rate, or as a way to avoid inflation.

[00:25:18]

Similarly, during the year he was at the White House, and the next year, 1970, at the Federal Reserve Board, I corresponded with him frequently and saw him frequently, and had no reason whatsoever to expect that he would be in favor of price and wage controls.

On the monetary side, I recall one meeting with Nixon, which I think was in summer of 1970 or fall of 1970, though I would not guarantee you that date, in which he tried to urge me, he tried to get me to urge Arthur to expand the money supply more rapidly. Conceivably, that could have been early in '71.

I objected, saying that that would simply produce inflation, and in my mind, that would create problems that he might not want to face if he got re-elected. His reply was, "Let's get re-elected first and then worry about it."

At any rate, at the time, he apparently felt that Burns was dragging his feet about monetary expansion, which is consistent with the story you tell about Herb Stein's attitude and approach, and belief that the way to get Burns to expand money was to impose some kind of a price and wage control structure.

I found the material you enclosed absolutely fascinating. Much of it, I had no idea of at all. In particular I was astounded at the positions that McCracken and Stein took, as well as some of the things that Arthur said in his letter to Nixon.

As to Arthur's role in initiating wage and price control, the first occasion on which I was struck by it, of Arthur having any role at all in recommending wage and price control, was when I heard, I believe on a radio or TV report or something like that, a report of a talk which Arthur had given in which he had recommended voluntary price restraint on the part of price and wage constraint, in a sense as the equivalent of guideposts.

My faulty memory suggests to me that that was in the spring of 1971, yet you refer to his remarks before the Monetary Conference on May 18, 1970. That may have been the one I

heard, but somehow or other, early 1970 seems too early. But perhaps I don't have any confidence in my memory at all. At any rate, I have a vague impression that somehow, the talk he gave was broadcast—either it was somehow broadcast over the radio that I heard it, or that I read a detailed report on what he had said, perhaps in the *Wall Street Journal*.

On all that, I don't remember. What I do remember is that I was enormously upset about it, and that I wrote him a letter, which terminated our relationship for a time. In that letter, I said that I felt quote "betrayed" unquote. That was the word that really got to him. I do not have a copy of the letter. I wrote the letter in longhand. I did not make a copy. I do not know whether the letter still is in Arthur's files, or whether it has completely disappeared.

At any rate, that was the first time I had been aware that he was in favor of any kind of wage and price controls, in view of his earlier strong opposition to them, as per my earlier quotes.

[00:30:16]

We were not really reconciled until years later when his son, Joe, who was then a student of mine, went out of his way to try to reconcile us. But this was years later, after Reagan was in the White House.

On one point there are no doubts. Arthur disappointed very much the expectations I had when he was appointed chairman of the Federal Reserve Board. I believe it can be said that the period of his chairmanship displayed the worst monetary policy since the early 1930s. The mistakes in the early 1930s led to a major depression. The mistakes in the 1970s led to substantial inflation. The inflation did less harm, by far, than the depression did. However, from the point of view of monetary policy

[END OF TAPE 57, SIDE A]

[BEGINNING OF TAPE 57, SIDE B]

However, from the point of view of monetary policy, the errors in the 1970s were as egregious as the errors in the 1930s.

I do not have any good explanation of what it was that induced Arthur to behave the way he did. I have always argued, or I've always suggested, that a major role is played by Arthur's great confidence in his political judgment. He seemed to have more confidence in his judgment of what was both critically feasible and desirable than he did in his economic analysis.

At any rate, I can only repeat what I have said before. He did not behave as I anticipated he would, which means that any explanation I have is purely ex post. I am not inclined to speculate on that. Your own speculations are highly plausible and persuasive, but I would not be prepared to issue any kind of a final judgment on the question.

This is the end of my comments on both the questions you sent me and the material that accompanied your letter of May 29, 1996.

[END OF RECORDING]