# The Monetarist Controversy

## A Seminar Discussion

Paper by

Franco Modigliani

Discussion by

Milton Friedman

# THE MONETARIST CONTROVERSY

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## EDITORIAL NOTE

At the January 1977 meeting of its monthly Economic Seminar series, the Federal Reserve Bank of San Francisco was honored to present Prof. Franco Modigliani, Immediate Past President of the American Economic Association. In his paper, Prof. Modigliani developed some of the themes which he had first covered last September in his AEA Presidential Address, "The Monetarist Controversy — Or, Should We Forsake Stabilization Policies?" The Bank was doubly fortunate to obtain, as seminar discussant, Nobel Laureate Milton Friedman, who was serving as Visiting Scholar at this institution during the winter term. This supplement to the Bank's **Economic Review** contains Prof. Modigliani's lecture, Prof. Friedman's reply, the discussion between the two and a floor discussion — plus, as an appendix, Prof. Modigliani's AEA Presidential Address. The seminar was chaired by Dr. Michael W. Keran, Vice President and Director of Research for the Federal Reserve Bank of San Francisco.

# The Monetarist Controversy
## Presentation by
## Franco Modigliani

*Michael Keran:* On behalf of the Federal Reserve Bank of San Francisco, I'd like to welcome you all to our monthly seminar series.

This month represents the beginning of the fourth year of this series. The purpose of these seminars is to bring together professionals in the Bay Area business and financial community with active research workers who are largely but not entirely from the academic community, to talk about issues that are of common concern in areas of public policy and to which people bring different perspectives.

Today represents the high water mark in our monthly seminar series because of the distinguished character of both the discussion leader, Franco Modigliani, and his discussant, Milton Friedman.

We will start off with the discussion leader, Professor Modigliani, spending 30 or 40 minutes laying out the issues. This will then be followed by a comment by Professor Friedman on the issues. Then we will open it to a general exchange of views from the audience.

Franco Modigliani is a professor of economics and finance at the Sloan School of Management at the Massachusetts Institute of Technology, and is the immediate past president of the American Economic Association. He was born in Rome, studied at the University of Rome, and received his Ph.D. from the New School of Social Research in New York. He has taught at the University of Illinois, the Carnegie Institute of Technology, and Northwestern University, before he came to MIT in 1962.

Professor Modigliani is well known for his research in a wide range of areas. In finance, he has made state of the art contributions in the area of cost of capital and the theory of

investment. In consumer behavior, his name is associated with the life cycle hypothesis, which helps to explain personal savings rates. He is here with us today to talk about one of his major areas of research interest: monetary theory and policy. His interest in this topic goes back to his Ph.D. dissertation published in 1944, which I suspect all of us in this room who were ever graduate students in economics have had to read for macro theory courses. Since that time, he has not only been a leading figure in monetary theory, he has also been an active participant in and an advisor on monetary policy.

The Federal Reserve has had very close working relationships with Professor Modigliani, particularly because of his role in designing the financial sector of the large econometric model that the Federal Reserve System uses for its monetary policy simulations.

The topic of Professor Modigliani's paper today is *The Monetarist Controversy,* subtitled "Should We Forsake Stabilization Policies?"

*Professor Modigliani:* It is a pleasure to be here today, to enjoy the invigorating air of the Bay Area, and to enjoy invigorating intellectual exchanges with Professor Friedman. It is indeed refreshing to realize how much we still have to argue about. At MIT, everybody agrees. The differences between us are so puny that we can understand each other very quickly.

Mr. Keran has mentioned that my initial work in economics, at least the initial significant work, was in the area of understanding the relation between Keynes and the classics. And apparently I am destined to end my career by being concerned with the relation between Mr. Keynes and Monetarism; and that is really the subject of my conversation today. I have been attracted back to this area

during the 1974 period, because it seemed to me at that time that everybody who had any common sense would agree on certain basic rules of the game, including the fact that when you get an outside price shock of the magnitude experienced in that year, you do need to relax your money supply rules and allow for a more rapid growth of the money supply. I had a hard time understanding why my monetarist friends did not agree. I knew, of course, that it would take quite a bit of imagination, after having spent their life saying money supply must always grow at the same rate, to make a change; but I thought that, given the circumstances, they would. Monetarists have changed in one very minute way: they would now target a 5- or 6-percent increase, while five years ago it would have been somewhat less. Still they have a very rigid formulation.

I began to spend time trying to understand how intelligent people dealing with the same world, and having presumably similar analytical tools, should come to such different conclusions. I have written a couple of papers on this topic. One is my presidential address; another is a paper that I prepared for the Federal Reserve Bank of Boston, entitled "Monetary Policy for the Coming Quarters: The Conflicting Views." Since you have seen my presidential address, I shall only summarize some key issues that I think will be useful for discussion.

I started out, under the inspiration of Professor Friedman, to distinguish the sources of differences: (1) differences in analysis; (2) differences in empirical assessment of parameters; and (3) differences in value judgments. The latter may get imported into our policy prescription without carefully saying "I'm advising you to do this because I think you *should* have low unemployment." And somebody else, who thinks low unemployment is not as important, doesn't understand how you arrived at your conclusions.

Well, let me first say that the conclusion of my work is very clear: namely, that there really are no significant differences of analysis

between able, intelligent, open-minded monetarists and non-monetarists. This is true despite the fact that very frequently a monetarist and a non-monetarist will start out approaching the same problem with somewhat different models. What I maintain is that, in those cases, if a monetarist wants to, he will be able to recast his analysis into a non-monetarist language; and the non-monetarist, if he will try, can recast his analysis into a monetarist language. And that it will always turn out that one is consistent with the other, except possibly in the sense that one is a limiting case of the other — for instance, a limiting case in which a particular parameter is zero. Assigning zero value to the parameter is a particular value; so in this sense, there are no major differences.

To give an illustration: When a monetarist describes the relation between money M and income Y, he'll start by writing $Y = aM$; and the non-monetarist will start by writing $M = aY$. The monetarist will think that his equation is a statement about income, and the non-monetarist will think that his equation is the demand for money. Yet you can write the equation either way, and you can also interpret it either way, if you are careful enough. In particular, a non-monetarist should see — and I have now learned to do that, so my blood no longer rises when I sit down to read the monetarist literature — that the proposition $Y = aM$ is perfectly consistent with the Keynesian framework under a number of conditions. The most trivial one is that "a" is definitional; that is, there exists, at every point of time, an "a" such that $Y = aM$. So, if a monetarist writes in this sense, and sometimes he does, there is nothing to get your blood pressure up.

Secondly, you can write $Y = aM$ in a more general sense, as a reduced form of the Hicks IS-LM curve, provided you are careful to note that, in that case, "a" is either a function of interest rates — or, if you don't want to use interest rates, then "a" is a function of output. Of course, recognizing that makes a lot of difference, because then the derivative

of "Y" with respect to "M" does depend on how the effect of "M" is distributed between price P and output X. It makes a lot of difference because then "a" is not a constant, but depends on M in ways which the equation Y = aM cannot account for.

Finally, suppose that, sticking to this last case, you happen to believe that what I've called the Hicksian mechanism is very powerful. Then to a good approximation "a" is a constant — or, at any rate, is a random variable. Its value may move a little bit, but not as a function of what you are doing. Even if it does, it is a secondary effect; so, for many purposes, you may want to take it as a constant.

I maintain that, by the same token, a monetarist should be willing to take the traditional two equations of the non-monetarist Hicksian framework (which sometimes becomes two thousand equations), and see that the Hicksian framework is consistent with his model.

So it seems to me that the framework is the same, and that the real issue has to do, on the one hand, with assessment of the value of the parameters; and, on the other hand, on assessment of the crucial simplifications that are appropriate at different times. With respect to the latter, I think any intelligent non-monetarist will agree that if you are dealing with the post-World War I German inflation (or I would even say with the British or Italian inflation of this time), the Hicksian mechanism is a refinement that we can forget about. However, I wish the monetarist would understand that if you are dealing with the Great Depression, then the constancy of "a" is a luxury that you cannot use. It is a convenient approximation which is no longer useful. For that reason, when either a monetarist or a non-monetarist deals with extreme situations, he should have no difficulty coming to similar conclusions.

I have just been working on a paper dealing with the problem of the Italian inflation, and I'm sure that Milton would be willing to sign his name to it. I conclude that you can only maintain a certain rate of inflation if the money supply is growing at the appropriate rate. If you stop the money supply from growing, you cannot for long have any significant inflation as employment tends to shrink to some critical level consistent with price stability.

Note that Italy has a fairly stable Phillips curve, because the economy is indexed 100 percent, and contracts are highly centralized. In this context, expectations have no role. Thus it turns out that you can keep employment above the critical level; but, because, at the higher level of employment prices that firms charge are not consistent with the wage demanded, you get a higher rate of inflation. The larger the level of output the greater the discrepancy between wages and prices, and the higher the inflation. However, even this Phillips curve is unstable in the longer run, because it relies entirely on the lag in the escalator clause of wage adjustment behind prices. So high employment is made possible because the real wage is reduced through the inflation. But workers must at some point catch on, and must either agree to a lower real wage, or must try to shorten the escalator lag — in which case, inflation would grow; and so you do have an explosive inflation.

So you see that when we are dealing with concrete problems, non-monetarists can come to quite monetarist conclusions.

Then the question is: Where is the main source of difference? Is it in empirical assessment? I have indicated that essentially, at the start, there was a great difference of empirical assessments, with the monetarist thinking that the Hicksian mechanism is very powerful, while the early Keynesians thought it was rather powerless. I guess I've always been between the two positions, although I would say that I have moved toward the view that the power of the Hicksian mechanism is sizable, unless you get into a really deep depression.

The assessments that come out of the MPS model and many other models agree, at least

in terms of order of magnitude, that the double mechanism, the Hicksian mechanism and the price flexibility mechanism, put together, really does reduce considerably the impact of outside disturbances.

Just to remind you of what happens in terms of the MPS model, we find that if you have an exogenous disturbance to demand, the multiplier far from being a gigantic number is reasonably small; it is not quite one, to begin with; then it gets to a maximum of two, and then stays there. But that assumes perfect price rigidity. Once you allow for the fact that prices gradually respond to unemployment, then you do find that the maximum impact is reached in about a year; it is not much over one, and then it dies out. After a couple of years, the net effect is zero.

On the other hand, the effect in terms of money income must be appreciatively larger if prices are responding. It is at this point that there has been some difficulty with some monetarists, because of what I call the St. Louis quandary. According to the St. Louis equation, which is estimated in terms of *money income,* the fiscal effect would last two quarters and disappear after that. I have always felt that result was inconsistent with any sensible monetarist point of view. I am pleased to say that I've never heard Milton quite endorse that result — particularly since he is known to emphasize the fact that the responsiveness of prices is slow. In other words, with his distribution of the change in income between output and prices, the price component is sluggish. If that is the case, how could you not get a fairly long diffused effect?

In my presidential address, I have mentioned a number of tests which resolve the issue to my satisfaction. I think the St. Louis results are not inconsistent with what a non-monetarist would expect, *a priori*. The apparent difference is due, first of all, to the fact that the St. Louis approach is unreliable. You can get almost any answer, if you play around with that equation. That's understandable, given the many variables you are leaving out. The St. Louis equation attempts to explain income with just two variables, fiscal policy and money, when in fact there are two thousand other things that are whipping it around. So to pretend that you can explain fiscal influences with simple short lags, and particularly ignoring the distinction between real income and money, is just to ask too much. You get a very poor fit, and you get poor estimates — estimates that are highly unreliable. I have shown this to be the case, by two approaches. One, suggested by a student of mine, Dan O'Neill, is an F test, which shows that the difference between the St. Louis coefficient and the coefficient which you estimate from a model like the MPS is not significant, most of the time, for most of the tests I have made.

Secondly, and perhaps more interestingly, by showing that the fiscal multipliers estimated from the MPS model are more reliable than the St. Louis estimate. To this end I took the St. Louis equation, estimated the co-efficients of the fiscal variables through 1969 and extrapolated. I got worse results than if I extrapolated a St. Louis equation in which the fiscal coefficients were set equal to the MPS coefficients, which have never changed in time. I get a better fit outside the sample period, which simply means that their estimate is not very reliable. In other words, the St. Louis coefficients give a closer fit during the period of estimation, but when you extrapolate — they go to pieces.

The reason, by the way, why the extrapolation is so poor, is the following: When you refit the St. Louis equation for a longer and longer period, the estimates tend to get closer and closer to those implied by the MPS and other econometric models. Indeed, Ben Friedman has just finished and submitted a paper in which he shows that if you go through 1976, and particularly if you go from 1960 to 1976, the coefficients of the government expenditure variable in the St. Louis equation are essentially the dream of a non-monetarist. They are essentially two, and they never become negative. They start positive and they always remain positive. This may actually be too good to be true, because my own esti-

mate is that they ought to be not quite as high as that; so even these are not very reliable. But what this evidence really shows is the unreliability of a method whose coefficients keep changing as you move along.

But while there is by now relatively little theoretical disagreement, I suspect that when we apply our conclusions to the real world perhaps each side tends to forget that he has admitted his earlier faults of exaggerating in one direction or another. For instance, I heard Milton say again, just before this seminar, that a tax cut which is not accompanied by a change in money has "almost" no effect. I think I know what "almost" means. It means that in the first year it has an effect of something like half. But that does not sound to me like "almost" nothing. Or perhaps he is somehow disagreeing with what I thought he agreed to in theory.

Of course, there are situations where Milton and I would agree that the fiscal effects are almost zero; namely, a transient tax cut which is stated to be transient, and which everybody knows is transient. I think there is strong reason to believe that it will produce no effect; and I have been fascinated by studying the evidence that comes out of the latest experience we have had in 1975. That is a fascinating experience; because that tax rebate was as transient as you can get. In one quarter we reduced taxes at the annual rate of some $32 billion. The next quarter, we raised taxes at the same annual rate. Will people behave as if they had lost $30 billion of income permanently in the first quarter, and gained $30 billion the next quarter? The evidence seems to me strikingly against it. The amount of savings went up by $37 billion in the first quarter, and went down by $24 billion the next quarter. A close examination of this episode led me to the conclusion that people treated the rebate as a one-time gift, and spent it as dribble. You don't see much evidence of an effect either in the same quarter or in the immediately following ones.

Let me indicate, since I mentioned this, that of course I am now touching on one point in which Milton and I see eye-to-eye, and that is the theory of consumption. This is an area in which our work complements each other beautifully. It seems to me that his work had a great impact beyond mine, at the methodological level, by showing the relationship between theory and tests, and how you define under what conditions a theory would be rejected. This has a lot of carry-over to problems other than consumption. On the other hand, my model doesn't have much carry-over, because it is specific to the life cycle. It is like Milton's permanent income hypothesis; but life is finite. There are all kinds of fascinating consequences that come out of my approach which you could not conveniently derive from his model. And so it really pushes in one direction — in that particular direction I think quite far.

Let me now return to the question: If there is no difference in analysis, how can we disagree? Well, before we come to value judgments, there is still a difference, an empirical difference, which Milton has stressed in the past. And, by the way, I must give credit to Milton for having said many times that the differences are empirical. The empirical difference essentially is this: the monetarists' belief that, whatever the coefficients are on the average, they are very unstable; and that since you are dealing with a dynamic system you don't really have enough knowledge to stabilize the economy.

Actually, at this level, Milton has in my view made a grave mistake; he has tried to establish as a logical proposition that you cannot stabilize the economy. I say that is a mistake because as he has stressed, these are not logical differences. You know his logical argument — he stated in his presidential address that the Phillips curve is vertical at the natural rate of unemployment, and we don't know exactly where that is located.

If we are anywhere on one side or the other of the natural rate, we have a cumulative process of either inflation or deflation. Therefore to try to stabilize unemployment can only result in extreme instability.

I am very proud of my analogy, which some of you may have seen in the footnote, that says this sounds just like advising a man in Minneapolis, who wants to go down to New Orleans, along the following lines: "Look here, there are two ways to go. I know you are trying to go by car; but there is only one way to go that is sure. You should put yourself in a tub and drift down the river. Because we know that the Mississippi River has a current, you can't fail to get there eventually. Whereas, if you take the car, you might make a wrong turn, and you might end up in Alaska. You might catch pneumonia. You might never get there."

It seems to me that it is exactly the same argument. Let me make it specific. There are circumstances in which taking the tub down the Mississippi is better than taking an automobile; suppose for instance, the automobile is a wagon, and there are lots of Indians in the way, whereas the Mississippi is secured by your friendly troops. Well, in that case, I would say the tub is a good idea.

Or let me take another example, which I think is pertinent. Suppose you don't want to go to New Orleans, but just wish to visit somebody a few miles down river, and you don't exactly know the road and haven't got a map. In that case, you might find that it is more reliable to take the tub. The moral: a) you should not try to use stabilization policies for fine tuning; but b) it is a different matter when you have a long trip; and I submit that our knowledge of the economy is sufficient to make the situation far closer to the automobile in a friendly country than to Milton's wagon in Indian country.

In any event, what I am really saying is that this is an empirical question. Of course, it may be that if you have nothing but a constant money supply in the longest run you may eventually get there; but it doesn't mean that on the average you will not be far away from target. That is just an empirical matter of how well your stabilization works, and how serious is the risk of major errors.

Now, I have tried to face the problem as an empirical one, and I have tried, in my paper, to provide a good deal of evidence that suggests, on the one hand, that a constant money supply does not work well; and, on the other hand, that stabilization policies have worked well. I'm sure there will be some room for discussion on this point.

I do have something to say about value judgments. Just a few words. I think there is no question but that value judgments play a major role in the differences between economists. And I think it is unfortunate, but true, that value judgments end up by playing a role in your assessment of parameters and of the evidence we consider. And here, let me remind you of one very important development of recent years: We have all learned about Bayesian statistics and Bayesian inference rules. Now, in one sense, Bayesian statistics and inference is a very good approach to problems; but it has its drawbacks. And there is no question that Milton and I, looking at the same evidence, may reach different conclusions as to what it means. Because, to him, it is so clear that government intervention is bad that there cannot be an occasion where it was good! Whereas, to me, government discretion can be good or bad. I'm quite open-minded about that, and am therefore willing to take the point estimate. He will not take the point estimate; it will have to be a very biased estimate, before he will accept it.

It is very important to understand the sources of differences; there is no question that, in the advice we have been giving at different times, we value differently the cost of unemployment versus, let's say, the cost of inflation. I must say that I, among non-monetarists, am particularly sensitive to the cost of inflation; and, in fact, the last part of my paper deals precisely with the question of how to respond to inflation when you think it is costly. The literature on the cost of inflation has been waylaid by trivia about the little triangle due to the fact that, when interest rates rise, you economize on the use of money. I think that is trivia since, with respect to most of the money (namely, demand

deposits), we could eliminate that loss immediately by just letting interest rates be paid on it. Once you have done that, the only thing left is currency; and that is a small quantity, and we can probably invent ways of saving on that, too — credit cards, and what not. So I think that is really trivial.

The real costs of inflation are, I think, related to unexpected changes. I believe that steady inflation (and I think Milton would not disagree with this) has almost zero cost. There is to be sure a very small welfare triangle; but it's pretty trivial; and almost any other cost you can mention, I think even that can be taken care of. If we lived in a world of steady state inflation, I'm sure we could find ways of making its cost pretty negligible. So I think that what is really costly about inflation is unexpected deviations of inflation from the anticipated steady state path. This is the problem that I have tried to address. If you find yourself off the long-run target on the Phillips curve, because of unexpected events, such as the oil crisis, or because of errors in policy such as the Vietnam War and the way it was financed, how do you return to the long-run path? That, I think, is a very important problem; and I wish that monetarists and non-monetarists could join forces in the interesting task of estimating what are the costs of being off the long-run path. What are the costs of taking longer to get there? Is it the change in the price level that matters? Or is it the rate of inflation, per se? I think these are fascinating questions, which should provide a common ground for monetarists and non-monetarists alike.

Thank you.