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The Monetarist Controversy

A Seminar Discussion

Paper by

Franco Modigliani

Discussion by

Milton Friedman

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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 coefficients 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 estimate 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 nonmonetarists, 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.

The Monetarist Controversy

Discussion by Milton Friedman and Franco Modigliani

Michael Keran: The challenge has been made, and the discussant is prepared to respond. Professor Milton Friedman, among his other notable accomplishments, of which I am sure you are all aware, is the Visiting Scholar at our Bank.

We are honored to have him with us, and to have him respond to Professor Modigliani.

Professor Friedman: I certainly agree fully with Franco Modigliani's basic proposition, that the differences that separate so-called monetarists — a term which I try to avoid using myself, because I don't like it — from non-monetarists are entirely empirical rather than theoretical; and I am delighted to have Franco agree with me on this point. I believe that the differences are empirical not merely with respect to our judgments about the size of parameters, but also with respect to our judgment of the way in which policy is formed, operates and develops.

That is not a difference in value judgments. I disagree with Franco on that. I doubt very much that there is any significant difference between him and me, for example, on the value judgments we attach to unemployment and inflation. If there be any difference in value judgments in this respect, I would say that perhaps it is in the discount rate which we use in judging future events relative to current events. Perhaps there is a difference in time perspective.

I have been impressed in the past, that the most consistent difference that I could discern between people who tend to favor fine tuning, and people like myself who tend to favor rules, is in the discount rate that they use — a short versus a long perspective.

I agree, also, with Franco's final point: that steady-state inflation has negligible costs. If you could have inflation at a steady rate, it would not be worth paying much in the form of adjustment costs to move back to a different rate. The fundamental cost, as Franco said, arises from unexpected deviations of inflation from a steady rate. The major reason for favoring zero inflation is that I believe it is almost impossible to have a political set-up which will be consistent with steady-state inflation, unless that steady state is zero, or close to it.

Now, let me go back to Franco's presidential address — both to express agreement and disagreement with it. I may say that I've always thought that Franco, insofar as you use these terms, has always been a monetarist, in very important ways. His famous 1944 paper certainly qualifies as a major element in the so-called monetarist structure. But I must say that in the presidential address he displays a quality that I had never attributed to him: a capacity for understatement. I am referring to his comment, where he is trying to show how little difference there was, theoretically, between the monetarist and non-monetarist view (or the Keynesian and the classical theory), that "fiscal policy was regarded as having *some* advantages — according to the gospel of the General Theory." Or, on the next page, that "there was a *tendency* (among the early Keynesians) to focus on fiscal policy as the main tool to keep the economy at near full employment" (underlining mine). I suggest that if you examine the writings of the people involved in the dispute in the 1950's or early 1960's, the difference was far greater than that. But that is just quibbling.

I can well understand that, while Franco is delighted to agree so fully as he does, he finds it somewhat unseemly to agree completely. Since there is nothing to disagree with on the theoretical level, he does what we all do when we try to differentiate our products; namely, to set up straw men. In his address, Franco sets up four straw men that I might refer to briefly.

The first one is that he attributes to me the view that "wages (are) in reality perfectly flexible," and that the world is "competitive"; and he refers to my "modeling of the commodity market as a perfectly competitive one." I offer a challenge to Franco. I shall pay him a quarter for any statement he can find in any of my published works, in which I make those explicit assumptions. To illustrate the basis for my confidence, let me quote from my presidential address in 1967, in which I said, when describing the natural rate: "Many of the market characteristics that determine its level are man-made and policy-made. In the United States, for example, legal minimum wage rates, the Walsh-Healey and Davis-Bacon Acts, and the strength of labor unions all make the natural rate of unemployment higher than it would otherwise be." That is hardly a statement that is consistent with my assuming perfectly competitive labor markets, or homogeneous labor, or any of the rest.

The second straw man is his assertion that I assume that "expectations must soon catch up with the facts" that what we are dealing with is a "fleeting response to transitory misperceptions." Again, I quote from my earlier paper, "How long . . . is 'temporary' . . . . for unemployment? . . . . I can, at most, venture a personal judgment based on some examination of the historical evidence, that the initial effects of a higher and unanticipated rate of inflation last for something like two to five years; this initial effect then begins to be reversed, and that a full adjustment to the rate of inflation takes about as long for employment as for interest rates, say, a couple of decades." It is hardly accurate to characterize that statement and that explicit discussion of the time period as assuming a very rapid, instantaneous and immediate response. In those two respects, again, I think these are differences that do not exist, and that Franco is closer to me, or I am closer to him, than he suggests.

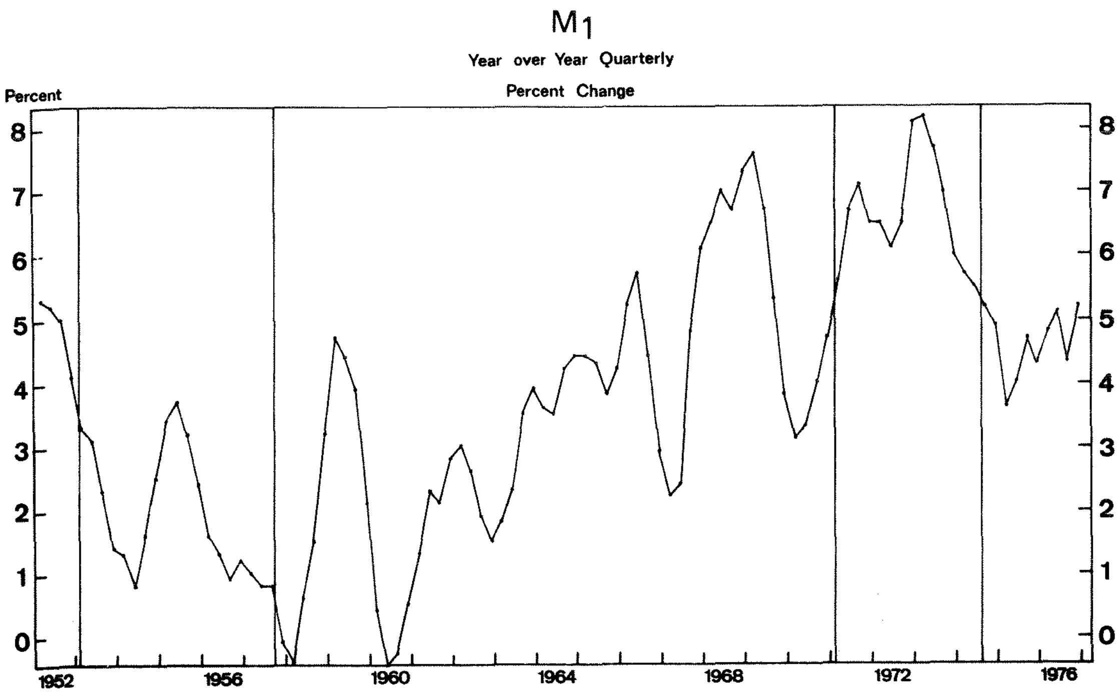
The third straw man, which he emphasized in his verbal statements, is an argument which he described as my attempt to demonstrate logically that monetary policy must be inherently unstable. If that is the interpretation that can be placed on my words, then I expressed myself very poorly. That wasn't the purpose for which I was making the argument. The essence of my argument in that paper was that the monetary authorities had a monetary instrument with which they could ultimately control only monetary variables, such as the price level and nominal income; that it is not possible to use monetary instruments to achieve a real target, whether that real target be the real interest rate or real output or unemployment rate. It was not my purpose to argue that you had dynamic instability in monetary policy in the sense that, if you changed the real target, you were necessarily at a razor's edge in which you were driven one way or the other. The purpose of that argument — and I think it is a valid purpose — was to suggest that monetary policy is an appropriate and proper tool directed at achieving price stability or a desired rate of price change, but is not an appropriate tool for achieving a particular target rate of unemployment. And I think that argument still holds.

The fourth straw man — and here, in a way, I join Franco in attacking what he attacks — is the discussion in his paper about the theory of rational expectations. I'm sure Franco will be pleased to know that in the past several years, in our Workshop on Money and Banking at the University of Chicago, my major problem has been battling with the proponents of Box-Jenkins on the one hand, and rational expectations on the other. (FRANCO: It's the same at MIT.) They are both marvelous ideas and good theories; but you know there is a tendency to carry them much too far and convert them into fads. So I agree with Franco on that subject. Indeed, if he had had a chance to read the later version of some of the material about the natural rate hypothesis, in chapter 12 of my price theory book, he would have discovered that I make some of the same comments about rational expectations that he does. He quotes some users of the theory of rational expectations as asserting that errors can only be short-lived and random; that they must be serially uncorrelated. That is not a valid implication of rational expectations, in my opinion. That is a misinterpretation of the theory of rational expectations.

Let me give you a simple example from recent history. For about five years, the futures price of the Mexican peso was decidedly below the current price. And every year, while the Mexican government maintained the price of the peso at 8¢ a peso, the people operating in the futures market made an error in the same direction. Anybody who sold the peso short was bound to lose money. Did that mean that those expectations were not rational? Not at all. What it meant was that every single year there was one chance in four that the peso was going to go down 50 percent; and that meant that it was appropriate for the future price to be 12½ percent below the current price. And that continued for 4 or 5 years.

The fact that those errors continued year after year was no evidence that the expectations were irrational. It was only evidence that you were dealing with a phenomenon extending through time. I do not believe that rational expectations imply, in any way, that there cannot be significant deviations of expectations from experience for lengthy periods.

But now we get to where we really disagree, and that is on the policy level. Franco referred, in his verbal talk, and has discussed in much more detail in his paper, the evidence which persuades him that monetary policy should have been different than it was in the period after 1974, and that a stable rate of monetary growth is not an appropriate way to conduct monetary policy. And I must say



some of this evidence absolutely baffles me. He says that he has looked back over the past and found that two periods of stable monetary growth, but highly unstable economic activity, were the periods from 1953 to 1957, and 1971 to 1975. He said he formed that judgment on the basis of four quarter changes in the rates of monetary growth. Well, I have prepared some charts, of which we have distributed some copies; and I believe that if you look at M_1 on your graph, you will find it hard to believe that 1953 to 1957, and 1971 to 1975, are periods of especially stable monetary growth. If you will look at the M_2 graph (and I may say, as you know, that I have always tended to have much more confidence in M_2 than in M_1), and if you again check the same periods, from 1953-57 and from 1971-75, they are hardly periods of the most stable monetary growth. I would say that the period which shows the most stable rates of monetary growth, in the sense of deviations from the long upward trend throughout there, is the period from 1961 through 1965 or 1966. If we take that period (1961 to 1965-66), that is the period of relatively stable economic development.

Modigliani: Is that 1961-65 a stable period? Do you realize that M_1 went from 1½ percent up to 7 percent?

Friedman: Of course; but they were proceeding along a steady path.

Modigliani: But what's the difference? You are talking about the second derivative now.

Friedman: Of course I am.

Modigliani: Ah, the second derivative counts. Well, that's a new theory.

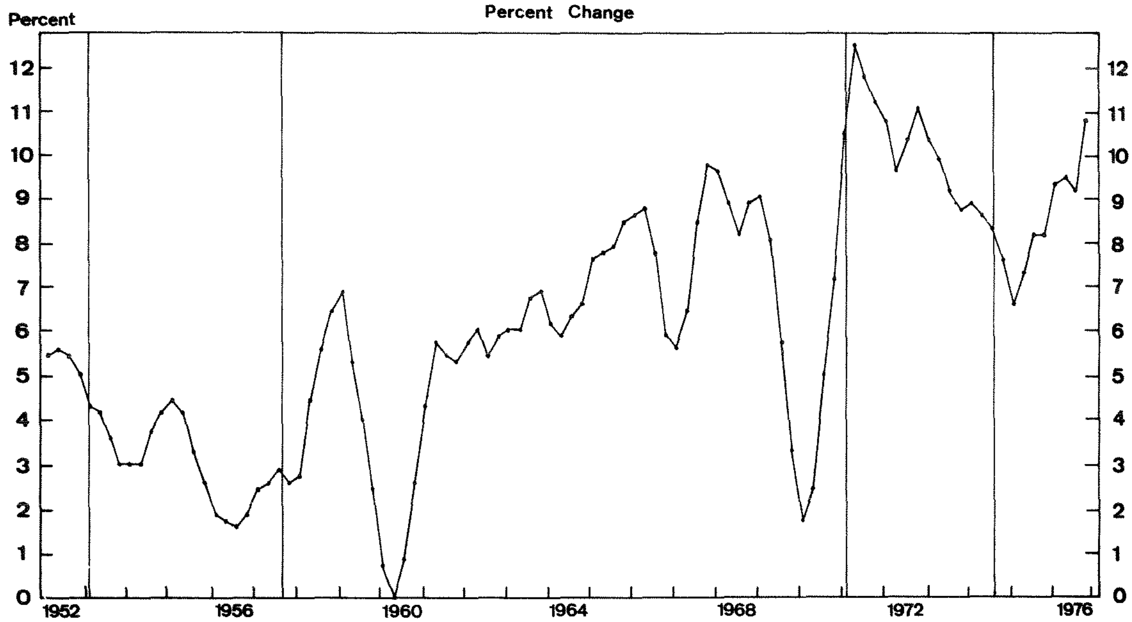
Friedman: Excuse me, it is not a new theory. We both agree that what matters is the difference between anticipations and realizations. And what we are talking about are the deviations from anticipated rates of growth.

Modigliani: And you mean, in this period everybody anticipated that there would be acceleration, acceleration, acceleration?

Friedman: This is off the track; because whether you take it about a trend or whether you take it in absolute terms, in no way are the years from 1953-57 or 1971-74 periods that have a lower standard deviation than the

M_2

Year over Year Quarterly
Percent Change



period from 1962-65 or 1966. Absolute or otherwise.

Modigliani: This is just a factual question; so let's not argue until we have performed the computation.[1]

Friedman: I just want you to do one more thing. Take personal income, which, both in my opinion and Franco's, is a variable that is predominantly moved by the money supply, and ask whether there is a significant difference in the relationship between the movement of personal income and the movement of M_2 in the later part of the period and in the earlier part. The one striking difference is a much deeper recession in 1954 than you would have expected from the monetary change. That is a significant difference.

I have always interpreted that difference (and Anna Schwartz and I did this in our *Monetary History)* as a reflection of the dropping of the bond support program by the Fed. That didn't really become effective until 1953, when, in one fell swoop, it demonetized a large volume of assets, namely government bonds. As a consequence, the effective money supply dropped much more sharply in 1953, than the reported money supply. That is the main discrepancy that I can find in these pictures between the movements in personal income and money. I calculated separate regressions for the earlier and later periods, and again I cannot find any significant difference in the regressions for the two periods.

With respect to the relationship between moving standard deviations of money and

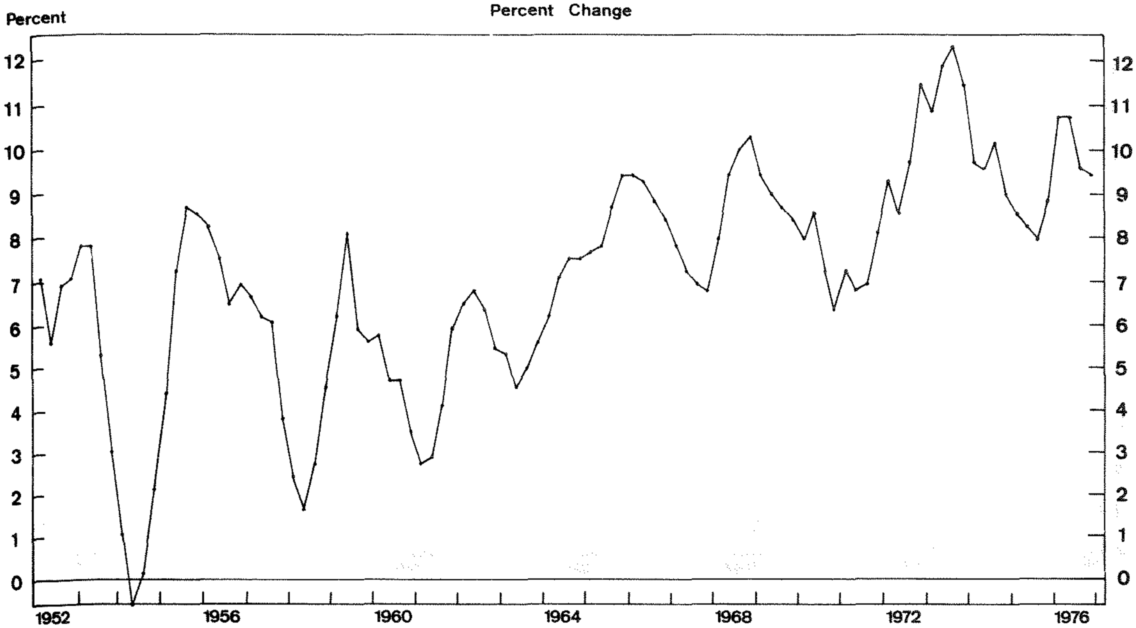
1. Modigliani note (added in galley): The average absolute deviation of the four-quarter change in M_1 is found to be .89 for the period 1962-65 and .95 if one adds 1966, as compared with .72 and .74 respectively for "relatively stable" periods used in my analysis, 1953 to 1957.2 and 1971 to 1974.2. I must add that in the process of checking Prof. Friedman's conjecture I have discovered that there exists another spell in which M_1 was roughly as stable as in the two periods I have singled out, namely the period from the beginning of 1961 to the middle of 1964, for which the average absolute deviation is .72.

Friedman note (added in galley): This is playing games by picking periods. For 1963.3 through 1966, the same length as Franco's second period, the average absolute deviation is .55. For M_2, it is .89 for 1971 to 1974.2 and .72 for 1962 to 1965. And I suspect none of these differences is statistically significant in the light of sampling fluctuations. In any event, little can be learned from such brief periods in analyzing a phenomenon which operates with a long distributed lag.

Personal Income

Year over Year Quarterly

Percent Change



income, I do not know whether Franco referred, in his Boston Fed paper, to the calculations of that kind that Anna Schwartz and I made in our paper in the *Review of Economics and Statistics* in 1963. We made such calculations for a very long period. They show a high positive correlation between the instability of money, on the one hand, and the instability of income, on the other.

Now I come to the next major point that Franco raises about the alleged incompatibility of the monetary movement with the economic movement. He argues that you cannot possibly explain the large price rise up to 1974 as reflecting monetary growth. I don't have the exact words here, but I think that is the essence of Franco's contention.

Well, I made a little calculation. We both agree that the price controls introduced in 1971 altered the time pattern of recorded price change. We would both agree that the "true" inflation in 1972 and early 1973 was larger than is shown by the numbers, and that the "true" inflation in 1973 and 1974 was less than is shown by the numbers, because of the effects of first imposing controls and then removing them. Consequently, I took the whole period from the third quarter of 1971, which was the quarter when price controls were imposed (on August 15), to the fourth quarter of 1974 (the peak of the rate of price inflation). On average, consumer prices over that period rose at a rate of 7.5 percent per year.

In order to see whether monetary growth can account for this, I have to allow for the fact that there is, on the average, a 2-year lag between changes in nominal money and changes in prices. So I took the rate of change of M_2 from the third quarter of 1969 to the fourth quarter of 1972 — that is, I pushed it back two years. The average rate of change over that period was 9.2 percent; i.e., 1.7 percentage points higher than the rate of inflation. On the average for a long period that difference is about 3 percent. On the average over a long period M_2 will grow at about 3 percent more than prices, to allow for real output growth. This time it is 1.7. I believe there are two major factors which contribute to the residual discrepancy of 1.3 percentage points between 1.7 and 3.0. I have no doubt that the oil crisis in late 1973 and early 1974, by making this country poorer, raised the price level. I have variously estimated that as having been about 1½ percent. Spread that over the three years, and you have about ½ a percentage point, per year. The rest of the 1.3-percentage point discrepancy is obviously consonant with our theory. We have always argued that if you shift from one rate of monetary growth to a higher rate, adjustment to that higher rate will involve an increase in velocity, since the higher rate of inflation makes the holding of cash balances more costly and, as a result, it will lead people to try to reduce their cash balances. That shift in velocity is a once-for-all effect. What this implies is that about .8 percent per year over a three-year period, or 2.4 percent in all, was the extent to which the quantity of money demanded fell as a result of the higher interest rates and the higher rates of inflation.

Let me interject that I may be carrying a good thing too far. There is, of course, a good deal of fluff in the relation between monetary change and subsequent price change. My only point is to demonstrate that there is nothing about the price rise from 1971 to 1974 which is not entirely consistent with earlier changes in the quantity of money.

Now we come to the final question of whether you should accommodate, as Franco put it. And here it is hard for me to know how to discuss that issue in brief compass. My major difference of opinion with Franco is in two respects: First, with his assumption that he knows how to accommodate (or that I do, for that matter, or that anybody does); and second, with the assumption that if in fact you adopt a policy of accommodation, Franco Modigliani will be twisting the dials. I have increasingly (and this is a subject on which I must say I've changed my views over the years) become impressed with the need for a positive science of politics, of political science. All of us — Franco, myself, and all the rest of us — have tended to follow the attitude: Well, now, what we need to do is to figure out the right thing. If only we can tell them what the right thing to do is, then there's no reason why able, well-meaning, well-intentioned people should not carry out those ideas. But then we discover, over and over again, that well-intentioned, able people have passed laws, or have established institutions — and lo and behold, they don't work the way able, well-intentioned people expected or believed they would work. And it isn't an accident that that happens. It happens for very systematic, explicit reasons.

Suppose, for a moment, I were to buy all of Franco's arguments. Needless to say, I don't; but suppose I did. I may say I do agree with him completely, at one point, when he says that it would be a miracle if a steady rate of monetary growth would achieve precisely the pattern of inflation and unemployment that he plots. But I believe it would be an even greater miracle that Franco would be in a position to push those dials. That is because, once you adopt a policy of accommodating to changes, there will be all sorts of changes that he and I know should not be accommodated, with respect to which there will be enormous pressure to accommodate. And he and I will not be able to control that. I have increasingly moved to the position that the real argument for a steady rate of monetary growth is at least as much political as it is economic; that it is a way of having a constitutional provision to set monetary policy which is not open to this kind of political objection.

But let me return to the question of whether Franco, or I, knows how to accommodate. He takes the obvious example — 1974. What happened in 1974 was not that the Fed did not accommodate; what happened was that the Fed stepped hard on the brakes, toward the middle of 1974. It is true that the Fed had stepped somewhat on the brakes earlier in 1973, and there was a slow-down in monetary growth in 1973; but there was a much sharper slowdown in 1974. The charts which show annual changes do not bring out these points very clearly. It comes out much more sharply in quarter-to-quarter changes. At the time, I was arguing, along with Franco, against the slowdown in mid-1974. We agreed at that time, not precisely on what the right policy was, but what the right direction of policy was.

I have long been in favor, as you know, of reducing the rate of monetary growth of M_2 to somewhere in the neighborhood of 3 to 5 percent; but I have always been in favor of doing it gradually — precisely for the kinds of reasons that Franco quite properly presents. What happened was that this was not done gradually; it was done very violently. I have no doubt that it reinforced the adverse effect on the economy of the oil crisis, and of the disturbances of that time.

But now, let's go back. Early in 1973, M_2 was going up at roughly 10 percent. Would it have been desirable to continue at 10 percent? Would it have been desirable to accommodate to the oil developments by going to 13, 14 or 15 percent? Or where?

Franco tells us that what was desirable was to increase the money supply enough to get the unemployment rate up to 7 percent. Well, now, I believe that's a terrible prescription. It is a terrible prescription because I do not believe that there is a very close relationship between the unemployment rate and what you do with the monetary growth. In addition, I believe that the unemployment rate is a very undesirable, unreliable criterion of policy. I am persuaded that the ups and downs in recent years have been affected at least as much by changes in the Unemployment Insurance Act, as they have been by the acts of monetary policy. The very sharp rise in unemployment in early 1975, from January to March or April, owed at least as much to the fact that in January of 1975 a new arrangement for unemployment insurance came into effect which doubled the benefit period and widened eligibility, as it did to what was happening to monetary policy.

Let me go beyond the 1974 period. I sat at the Federal Reserve Board with Franco in the summer of 1975, and I remember very well his arguing, at that time, for something over a 10-percent rate of growth in M_1. Would that have been desirable? I don't believe he would think so now. And I didn't think so then; so at least we are in agreement on that — if not at the same time!

My point is that maybe he was right on that particular occasion in 1974; but is there any reason to believe that he or I, or anybody else, can be completely right, year after year, under these circumstances? I don't believe so. And I believe that if you adopt a policy — and this is where I say the political assumptions come in — if you adopt a policy which assigns to individuals the discretionary power to fine-tune, or to gross-tune (I don't care) — but the discretionary power to move things like the rate of growth of the money supply or, for that matter, taxes; then political forces are going to come into play, to see that that power is used for purposes other than those which either Franco or I would approve.

Michael Keran: Professor Modigliani has a couple of comments, before we open this to general discussion.

Modigliani: I would like to react to just a few points, to set the discussion on its right path. First of all, I would like to say that I would disagree completely that the differences between Milton and me, in the evaluation of unemployment and inflation, have anything to do with discounting. As a matter of fact, in the work I'm doing now with Lucas Papademos, we use zero discounting. The whole point of the paper is to take into account all future consequences of an action now. You get some very fascinating results when you take into account all future consequences of an action. For example, even if the Phillips curve is not vertical, the long run, not the short Phillips curve is relevant for policy purposes.

Secondly, he has made a few points that I think are minor, and I want to leave them out; but I do want to plead guilty to two points. Let me make the minor first, and the major next. About rational expectations, I do agree now that I was wrong in saying that rational expectations implies non-serially correlated errors. On the other hand, it is also true that once you allow for serial correlation then generally you also allow for some policy influences, in many cases. One source of serial correlation is long-term contracts; and this is consistent with rational expectations. As soon as you have long-term contracts, you do have room for policy. So, in effect, you can't have things both ways: if you insist there is no room for policy, then you must really rely, to a large extent, on non-serially correlated error.

Now I come to the more serious difference: namely, that I have presented a picture of Milton believing in fast adjustments and in perfect markets. I must confess that here I was confronted with a contradiction between what his words said, at various places, and what conclusions he was drawing from them. I looked at the conclusions, not at the fine words. Now it seems to me that if indeed it takes five years to dispose of unemployment, then it is hard to believe that a policy-maker can be so stupid that one would believe he cannot do something to improve the situation. It seems to me that if you believe the effect of disturbances is fleeting, then you open the room for policy actions even for the fairly stupid policy-maker. I'm not talking about the mean policy-maker; that's a separate issue, to which I want to come later — but stupid, plain stupid. This explains why I attribute to him perfect markets — not perfect labor markets, but perfect commodity markets; because when I try to understand the model that is behind his conclusions, and make it so that the conclusions really follow from it, then I have to go back to marginal cost pricing, and to the sort of situation where workers withdraw from the labor force because they are misled about real wages. And I believe that is the essence of Milton's model, and I think it does require those kinds of assumptions to lead to his conclusions.

Now, being fleeting is different from being perfect, because it has to do with how quickly expectations are corrected. But it seems to me that there are many things I do not understand about Milton's view if he tells me those markets do not adjust quickly, because Albert Ando and I have concluded that monetarism is the non-monetarist world in which lags disappear. Because indeed, if lags disappear, then you do get back to a classical world. All that the classics say is correct. The price mechanism does it all. So, if there are long lags, then I cannot understand why we should disagree about the effect of taxes, or about the effect of many other things.

Now, just a word about the evidence. Money first, and then personal income. I have drawn the period on Milton's M_1 chart which I have used in my paper, and for which I said the money supply was "generally" within one percent of the average. Well, please compute it, and you will see that in the period to which I referred as "stable," the money supply hit a peak of just over 8 and a trough of just below 6.

I've used 3½ years — from the beginning of 1971 to the middle of 1974. Then, when I used output, I used the same period shifted by one year; so I go from 1972 to the middle of 1975. So for those 3½ years, the money supply is indeed quite stable, compared with any other period. I defy you to find any other period in which, for a period of that length, you get that low level of variation[2]. Sure, there is a little peak; but again, are we really worried about the fact that for one quarter the annual growth of the money supply was 8 instead of 7? It seemed to me that you should make it clear, now, whether you really believe in this mechanical view of the money mechanism. After we have agreed about how things work, at the theoretical level, now, all of a sudden, we get these point estimates. It takes two years; a two-year lag and exactly point input, point output. Two years later, boom! I must confess that I lose touch with Milton the scholar, when he comes out with this kind of evidence.

Friedman: You're shifting back from your understatement, to your overstatement. I don't have to believe in a precise point lag to say, as a rough estimate, let's look what happens. If we allow for a two-year lag, of course it's a distributed lag.

Modigliani: If you talk about a couple of quarters, point input, point output may be all right; but with a two-year lag, you just can't do that. You've got to show me what kind of lag you have. I mean, you have to run a regression over a long time, and show me what happens.

Friedman: Well, you go ahead and make that regression for as long a period as you want; but, allowing for averaging out the period from 1971 to 1975, you will find that price reacts to money during that period in the same way as it does in other periods.

Modigliani: Similarly, you will find the same thing to be true for the period I have referred to in 1953-57; and I think you will agree that this period was stable. In contrast, 1961-65 was an unstable period. The money stock was growing faster and faster; so that certainly is not a period of stable growth.

Now, as for the question of personal income which he shows in his charts, the answer is very simple. In the period 1972-75 there was great instability, which is disguised when you plot money income. Prices were rising like mad. Then the whole problem was that the Federal Reserve wasn't providing enough money, and so, naturally, money income didn't change in the face of prices rising by 12 percent. So if, instead of taking money income, you take real income — which is what I measured — you will see the great instability. And I'm surprised that we need to discuss it, because you've all lived it. So do you believe everything was rosy and stable between 1972 and 1975? If you believe it, then I think you'd better go back to school.

Friedman: But one of the things, Franco, that I thought you and I agreed on, and that

2. Friedman note (added in galley): As noted above, 1963.3 through 1966 is such a period.

I have written on extensively, is that we know much more about the nexus between money and money income, or nominal income, than we do about the forces that cause the division between prices and output.

Modigliani: This happens to be a point of complete disagreement. I believe we know equally much about both issues. I think our knowledge of the price mechanism is uncertain at the fringe. For example, if you forever keep unemployment one quarter percent below what it should be, will inflation explode? — or would it stabilize? Those things we don't know. But we do know that wages respond to unemployment, and past inflation, with fair regularity — with the coefficient of past inflation not very far from one. There is an extensive literature that explains why that would be the case. This is perhaps the difference between a monetarist and a non-monetarist, in the sense that, if you start from LM and IS, a non-monetarist will stress real output and will derive money output as a result. Monetarists, instead, tend to go directly to money income — and I think that is misleading. Although in many cases it would be all right, 1973-74 is not one of those periods.

Now, let me finally say that, in this question of looking at inflation as a function of past growth of money, I think there are many things wrong with what Milton said, but I can't immediately tell you what the answers would be. You want to stop in the middle of 1974, because that's the period I'm talking about. The explosion of prices was an explosion, because the rate of inflation of say the CPI went up from something like 7 to something like 13 from the second quarter of '73 to the first of '74. And if anybody believes that an explosion of prices of this sort can be accounted for by these wiggles in the money supply, well, he can believe anything.

I don't know just why you chose a two-year lag. Maybe, if I were to choose three years, I might get another answer. But I think that anybody who looks at the evidence must conclude that what happened in 1974 is primarily an explosion of prices, due to the impact of food and oil; and the price controls had been eliminated before this.

Furthermore, all the evidence I've seen suggests that the price controls and wage controls (and here I believe I agree with Milton) had a very small impact. In fact, the evidence suggests that controls had no effect whatever on wages, and other evidence (some from Bob Gordon, for instance) suggests that it had a small effect on prices, and that it washed out fairly quickly.

Now, I think we come back to the fundamental point. Milton says it is not a question of values. Well, I don't know how you cut this pie. You say that having trust in one's government is a matter of value, or is a matter of technique, or is a matter of empirical estimates. I do not know. I have personally no reason to believe that the United States government (if you were talking about Italy, it might be a different thing) is not able to attract able people who are interested in the common welfare and can do a good job. And I believe that if you look at the quality of the people that have, shall we say, manned the Council of Economic Advisers, I think that suggests the good quality of the advice that is available to the President. If the President wants to use bad advice, I can't really imagine that he will be deterred by some rule that says the money supply should grow at a constant rate; he'll find some way of getting around that. In the final analysis, one has to use one's political activity to make sure that public servants are doing the common good — that their actions are in the public interest.

I complain that the Federal Reserve does not tell us its target. Why do I complain? Because I have no way of telling if it is doing a good job or not. That's why I want them to tell us what their targets are — and not necessarily money targets. I don't care about money targets. They can do anything with money, as long as they tell us what their real targets are — and as long as they take the blame when they do not hit the real targets.

Now, that seems to me to be the fundamental issue.

Do you want to deprive yourself of an important tool to make our life better, because there is some danger that, in fact, the people who are using the tool will be thieves, or inefficient, or under pressures? I cannot imagine the Council really being under pressure of any special interest. I think they would have an interest in the country; and I am quite willing to say that they should be paid a wage which is escalated on real national income. It could go up one point for each one-point rise in real income, and then go down three points each time the inflation goes up. So, you see, I have a very one-sided bias against inflation.

Friedman: Franco, I agree that they should; but tell me, what chance do you think there is that such a method of payment would ever be adopted? And why not? It is such a sensible method; why should we deprive ourselves of such a sensible rule for compensating our servants?

Modigliani: Well, I think that would be a good idea; and, of course, when I look at our record, it seems to me that I can see some errors; and most of those were errors not of the advice, but of not following the advice.

I find relatively few cases of wrong advice; but there are some. I do admit that I would have been wrong in 1968. In 1968, I underestimated the power of inflation — and a number of us did; but I think that is probably the only case where I would really acknowledge that I would possibly have given wrong advice, and that would have been only for a short while — by the way, I think quite short — because I did catch on.

Michael Keran: I think this would be a good place to widen the debate and ask for questions. Who would like to raise the first question?

The Monetarist Controversy

Floor Discussion

Q: *I would like to address a question as follows:* Suppose that from 1946 to 1976, the actual and the expected rate of monetary growth coincided at 5 percent. (M_2) Everyone knew that the money supply was going to grow at 5 percent, and they expected it, and so on. Let me ask each of you: Would the standard deviation of the following variables be greater, or less, than it actually has been — unemployment, real income, and prices.

F: Less, for all three.

M: Well, I have indicated my answer. My answer is that, if we had had everything else the same, including the Korean war and the Vietnam war, then the answer is "More." Distinctly more; especially if you assume constant fiscal policy.

Q: *But all we have been talking about is monetary policy.*

F: The assumption is that you would have had the same set of tax rates, that you would have had the same set of expenditure programs, and that you would have resorted more to borrowing from the market, as opposed to printing money on the average, over that period.

M: Could I make an additional point? I actually have done a simulation of this with a model, and that is exactly what happened: you do get more variability. It all depends on what other things you adjust. If you eliminate all kinds of sources of variation — if you smooth fiscal policy —

F: No, no; don't do that.

M: If you don't do that, then you get more variation.

F: Well, yes; but that shows what's wrong with the model. Because this is the really important insight of the rational expectations approach, in my opinion. However good a model may be, such as Franco describes, for a world in which you do not have agreement between the actual and anticipated rate of money change, that model — the equations of that model — would have been different in a world where you do.

M: Now, let me answer this, because I think it is fundamental. The exact answer is that in my view — not only the model's view, since sometimes I disagree with the model; we fight, and I say you're stupid, you know that isn't right — but it is my view that the *expected* rate of money growth, when you're talking about 5 percent, is of absolutely no consequence. Nobody paid any attention until Milton told them that they should look it up. Nobody would ever dream of looking at the money supply; and they don't dream of doing it in other countries, except after Milton goes there. Obviously, if you told me that you announce a 20-percent rate of growth of money, well, then, people who are moderately smart will react to this. But if you are saying that the money supply is behaving reasonably, then what is expected or unexpected makes absolutely no difference to anyone.

Who looks at the money supply? What merchant, what industry, looks at the money supply?

F: Nobody. Who cares whether they look at it?

M: Okay; but then what does the expected money supply matter? If I don't look at it, how can I expect it?

Q: *It seems to me that there is one difference between the monetarists and the non-monetarists, which in a way is a value* *judgment — but perhaps not in the usual sense of the term. And that is: that the monetarists are more averse to risks than non-monetarists. That monetarism is playing a minimum risk role, while non-monetarists like Franco are willing to take risks.*

M: I hesitate to accept this proposition, because one of the reasons that makes me very opposed to the money supply is that I can conceive of situations where you would get tragedies; and 1974 was one of them. In another context, stubbornly insisting on stable money growth is wrong when we have just observed a great decline in the demand for money.

F: Oh, no, we haven't.

M: That is another discussion that we can go into later.

F: Currently?

M: Yes; over the last two years. Not M_2, but M_1.

F: Neither one. There is only a breakdown in the bad demand functions that people fit.

M: Well, what stability means is, of course, one's judgment; but from any point I've seen, M_2 is quite stable, so we agree on that. But M_1 is quite unstable. In a situation like this, if you continue on the stable money supply, you could get bad effects. That, by the way, applies equally — and I didn't make this point before, but let me make it now — when I said that I used unemployment as a target and said it should have been 7 percent, obviously using one target is a convenient simplification. You may have to use something a little more complex than that, in any policy decisions.

Secondly, the unemployment rate target was not very good after 1975, because the policy of that Administration was to create a lot of unemployment and then make it easy to be unemployed (doubling the period during which people would be eligible for unemployment insurance). A completely nonsensical, contradictory policy. If you want to end inflation, you want unemployment to be hard. And if you want to make it easy, then don't use it to end inflation, so that we have in the end the worst of all worlds, in which we essentially end up by wasting resources. By making it easy to be unemployed, we didn't accomplish what we wanted to accomplish.

One other thing which bears on the effectiveness of stabilization policy: Before the Second World War, unemployment fluctuated more than it did after the war.

F: Of course, that's true. But again, that is really evidence in favor of stable monetary growth; because, before World War II, you had of course the most extraordinary instability in monetary growth, with the quantity of money declining by a third, from 1929 to 1933.

M: No, no, no — leaving out the Great Depression.

F: But the money supply was more variable before 1929, than it was after World War II.

M: Which means that the stabilization policy has stabilized the money supply, and that is great! It is true, by the way; and it is exactly what you would expect.

F: Now, be careful. You're going to go back on your initial statement that we agree in theory; because I believe that is a statement which it will be very hard to defend on monetarist theory — or on any theory.

M: The statement is: If I can use fiscal policy, and it has an effect, then I can stabilize the economy with less variation in the money supply.

Q: *I think my question follows the statement you have just made. In the past, monetary and fiscal policies, at times, have been found to be at cross purposes with each other. Suppose we do accept Professor Friedman's proposition that the rate of growth of money supply would be made a part of the Constitution, and is known in advance to be 4 percent, or 5 percent — you can argue about the numbers. Then it* *follows that all the economic policy and the stabilization programs would be addressed by fiscal policy; and, in fact, money supply then would become a known constant. What objection would you have? You would have less to work with, in terms of variability of money supply. But you would have more discretion in doing what you want to do with the budget, or the taxes, to stabilize the economy.*

Do you think it will yield finer tuning than we have had in the past?

M: I think the answer to that is very similar to saying that if you are very careful and distribute the weight properly, you can drive a car with three wheels. Does it follow, however, that I should advise you to do it?

It is exactly the same story. You are removing one useful tool which permits a blend. For instance, if there is a decline in money demand (as might happen, for instance, if we start paying interest on demand deposits), in those circumstances, if I am forced by your 4-percent rule, I'll find myself having to cut off all investment, possibly; or, alternatively, having to cut off consumption.

If for some reason the demand for money rises, I have to respond by policy which forces me to reduce investment and increase consumption. And why should I do that? Why should I cut myself off? Exactly like the guy on three wheels.

Q: *Is this the same story that used to be used to justify foreign exchange intervention? The central bank knows better how to equate the demand and supply for foreign exchange, and therefore they have to intervene and offset the unwarranted gyrations in the private sector. If you are willing to argue that it takes the central bank to stabilize the demand for money in the monetary sector, you should argue for fixed exchange rates.*

M: First of all, I do argue for managed exchange rates; but that's a different story. I can't understand the twisting of certain things. Look, suppose the demand for money declines by 10 percent; how do you think the private economy adjusts? How does it adjust?

The demand for money shifts, so that, at a given interest rate and given income, the demand for money is 10 percent less.

Q: *In the short run, the interest rate is up.*

M: And what else?

Q: *Interest rates would adjust, in the short run.*

M: In a short while, prices would rise 10 percent.

Q: *They would?*

M: There is no other adjustment.

Q: *All the redundant money would be converted into commodities instead of bonds?*

M: Given the circumstances, it is clear what must happen in a short time is a rise in the price level.

F: Well, let's not say in a short time; but sooner or later.

M: In short order, in short order. Do you like that?

F: Excuse me — both of you; but I think we are begging the real question. You are begging the fundamental question of how do you know there has been a decline in money demand, and thus whether you're going to do the right thing?

M: I abolish interest on demand deposits. Okay?

F: First of all, as you know, that is one of the reasons I've never wanted to use M_1. I want to use M_2.

Let's go back for a moment, because you made a statement earlier that I think is not right, and you won't want to stick with it. I would warn all of you that, whenever anybody resorts to analogies, there is something wrong with the logic. If you've got a good logical argument, you don't need a three-wheeled car.

You will agree with the following proposition: that a policy of discretionary movement in an instrument can lead to worse results than stability in it, if there is enough lack of correlation between the actions taken and the actions that should be taken, even though, on the average, those actions are in the right direction. Therefore, to get your three-wheeled car analogy really going, you have to demonstrate that we know enough to be able to take those discretionary actions which, on the whole, are stabilizing, as opposed to the errors which are destabilizing. The basic empirical judgment on this score which leads us to stable monetary growth is the belief that we do not know enough to do that. And that is true, even if you leave aside for a moment the political constraints. If you know there is a 10-percent decline in the quantity of money, you can offset it — of course! But what you really have to demonstrate is that, over time, you will in fact know enough about such changes and will be able to identify them soon enough, so that you can make adjustments which, on the average, will do more good than harm.

I have observed over a long period of time that whenever anything goes wrong with monetary policy, the favorite excuse of the monetary authorities is that there has been an exogenous shift in the demand for money. But that is only an excuse. They don't know it in advance. If they did, they wouldn't have to bring in that excuse after the event. So what evidence do you really offer that we know enough so that we know how to handle that fourth wheel to be more stable, rather than less stable?

M: Well, this would get us pretty much astray; but let me just give an indication. First of all, I agree with you that, in principle, fewer instruments could be working better. In principle, anything can happen. And the actual question is: Are these coordinated so that in fact they don't work independently of each other? In other words, does it happen that when the Federal Reserve decides that you need an expansion, the fiscal policy authority also decides to have an expansion, and we have a positive correlated error, when it wasn't needed. If they are coordinated, this is highly unlikely to occur. The other point, of course, is that this is precisely why the targets need to be coordinated; that is precisely why I am against the independence of the Federal Reserve, if defined as independence of targets. I am completely for independence of the Federal Reserve, if understood as independence of tools. Given the target, given the fiscal policy which is now fairly clearly stated by the Congressional and Administrative policy, through the budget process — given those targets, the Federal Reserve ought to have the same target, not a different one; or disagree with the target, if it wants to, and have it changed. But once the target is there, it ought to work for the same target.

Michael Keran: There's an old Jewish proverb which says that "When scholars disagree, the public and the truth will both benefit." I think this has been an excellent example of that. I, for one, will look upon this as one of the most enlightening seminars I have attended. I hope you feel the same. And I want to thank our very distinguished speaker and discussant for a fascinating afternoon. Thank you very much.

The Monetarist Controversy Or, Should We Forsake Stabilization Policies?

American Economic Association Presidential Address by Franco Modigliani*

In recent years and especially since the onset of the current depression, the economics profession and the lay public have heard a great deal about the sharp conflict between "Monetarists and Keynesians" or between "Monetarists and Fiscalists." The difference between the two "schools" is generally held to center on whether the money supply or fiscal variables are the major determinants of aggregate economic activity, and hence the most appropriate tool of stabilization policies.

My central theme is that this view is quite far from the truth, and that the issues involved are of far greater practical import. There are in reality no serious analytical disagreements between leading Monetarists and leading non-Monetarists. Milton Friedman was once quoted as saying, "We are all Keynesians, now," and I am quite prepared to reciprocate that "we are all Monetarists" — if by monetarism is meant assigning to the stock of money a major role in determining output and prices. Indeed, the list of those who have long been Monetarists in this sense is quite extensive, including among others John Maynard Keynes as well as myself, as is attested by my 1944 and 1963 articles.

In reality the distinguishing feature of the Monetarist school and the real issues of disagreement with non-Monetarists is not monetarism but rather the role that should probably be assigned to stabilization policies. Non-Monetarists accept what I regard to be the fundamental practical message of the *General Theory:* that a private enterprise economy using an intangible money *needs* to be stabilized, *can* be stabilized, and therefore *should* be stabilized by appropriate monetary and fiscal policies. Monetarists by contrast take the view that there is no serious need to stabilize the economy; that even if there were a need, it could not be done, for stabilization policies would be more likely to increase than to decrease instability; and, at least some Monetarists would, I believe, go so far as to hold that, even in the unlikely event that stabilization policies could on balance prove beneficial, the government should not be trusted with the necessary power.

What has led me to address this controversy is the recent spread of Monetarism, both in a simplistic, superficial form and in the form of growing influence on the practical conduct of economic policy, which influence, I shall argue presently, has played at least some role in the economic upheavals of the last three years.

In what follows then, I propose first to review the main arguments bearing on the *need* for stabilization policies, that is, on the likely extent of instability in the absence of such policies, and then to examine the issue of the supposed destabilizing effect of pursuing stabilization policies. My main concern will be with instability generated by the traditional type of disturbances — demand shocks. But before I am through, I will give some consideration to the difficult problems raised by the newer type of disturbance — supply shocks.

Presidential Address delivered at the eighty-ninth meeting of the American Economic Association, Atlantic City, New Jersey. Reprinted by permission from the* **American Economic Review, *March 1977.*

I. The Keynesian Case for Stabilization Policies

A. The General Theory

Keynes' novel conclusion about the need for stabilization policies, as was brought out by the early interpreters of the *General Theory* (e.g., John R. Hicks; Modigliani, 1944), resulted from the interaction of a basic contribution to traditional monetary theory — liquidity preference — and an unorthodox hypothesis about the working of the labor market — complete downward rigidity of wages.

Because of liquidity preference, a change in aggregate demand, which may be broadly defined as any event that results in a change in the market clearing or equilibrium rate of interest, will produce a corresponding change in the real demand for money or velocity of circulation, and hence in the real stock of money needed at full employment. As long as wages are perfectly flexible, even with a constant nominal supply, full employment could and would be maintained by a change of wages and prices as needed to produce the required change in the real money supply — though even in this case, stability of the price level would require a countercyclical monetary policy. But, under the Keynesian wage assumption the classical adjustment through prices can occur only in the case of an increased demand. In the case of a decline, instead, wage rigidity prevents the necessary increase in the real money supply and the concomitant required fall in interest rates. Hence, if the nominal money supply is constant, the initial equilibrium must give way to a new, stable one characterized by lower output and by an involuntary reduction in employment, so labelled because it does not result from a shift in nominal demand and supply schedules in terms of real wages, but only from an insufficient real money supply. The nature of this equilibrium is elegantly captured by the Hicksian IS-LM paradigm, which to our generation of economists has become almost as familiar as the demand-supply paradigm was to earlier ones.

This analysis implied that a fixed money supply far from insuring approximate stability of prices and output, as held by the traditional view, would result in a rather unstable economy, alternating between periods of protracted unemployment and stagnation, and bursts of inflation. The extent of downward instability would depend in part on the size of the exogenous shocks to demand and in part on the strength of what may be called the Hicksian mechanism. By this I mean the extent to which a shift in IS, through its interaction with LM, results in some decline in interest rates and thus in a change in income which is smaller than the original shift. The stabilizing power of this mechanism is controlled by various parameters of the system. In particular, the economy will be more unstable the greater the interest elasticity of demand for money, and the smaller the interest responsiveness of aggregate demand. Finally, a large multiplier is also destabilizing in that it implies a larger shift in IS for a given shock.

However, the instability could be readily counteracted by appropriate stabilization policies. Monetary policy could change the nominal supply of money so as to *accommodate* the change in real demand resulting from shocks in aggregate demand. Fiscal policy, through expenditure and taxes, could *offset* these shocks, making full employment consistent with the initial nominal money stock. In general, both monetary and fiscal policies could be used in combination. But because of a perceived uncertainty in the response of demand to changes in interest rates, and because changes in interest rates through monetary policy could meet difficulties and substantial delays related to expectations (so-called liquidity traps), fiscal policy was regarded as having some advantages.

B. The Early Keynesians

The early disciples of the new Keynesian gospel, still haunted by memories of the Great Depression, frequently tended to outdo Keynes' pessimism about potential instability. Concern with liquidity traps fostered the view that the demand for money was highly interest elastic; failure to distinguish between the short- and long-run marginal propensity to save led to overestimating the long-run saving rate, thereby fostering concern with stagnation, and to underestimating the short-run propensity, thereby exaggerating the short-run multiplier. Interest rates were supposed to affect, at best, the demand for long-lived fixed investments, and the interest elasticity was deemed to be low. Thus, shocks were believed to produce a large response. Finally, investment demand was seen as capriciously controlled by "animal spirits," thus providing an important source of shocks. All this justified calling for very active stabilization policies. Furthermore, since the very circumstances which produce a large response to demand shocks also produce a large response to *fiscal* and a small response to *monetary* actions, there was a tendency to focus on fiscal policy as the main tool to keep the economy at near full employment.

C. The Phillips Curve

In the two decades following the *General Theory,* there were a number of developments of the Keynesian system including dynamization of the model, the stress on taxes versus expenditures and the balanced budget multiplier, and the first attempts at estimating the critical parameters through econometric techniques and models. But for present purposes, the most important one was the uncovering of a "stable" statistical relation between the rate of change of wages and the rate of unemployment, which has since come to be known as the Phillips curve. This relation, and its generalization by Richard G. Lipsey to allow for the effect of recent inflation, won wide acceptance even before an analytical underpinning could be provided for it, in part because it could account for the "puzzling" experience of 1954 and 1958, when wages kept rising despite the substantial rise in unemployment. It also served to dispose of the rather sterile "cost push"-"demand pull" controversy.

In the following years, a good deal of attention went into developing theoretical foundations for the Phillips curve, in particular along the lines of search models (e.g. Edmund Phelps et al, 1970). This approach served to shed a new light on the nature of unemployment by tracing it, in the first place, to labor turnover and search time rather than to lack of jobs as such: in a sense unemployment is all frictional — at least in developed countries. At the same time it clarified how the availability of more jobs tends to reduce unemployment by increasing vacancies and thus reducing search time.

Acceptance of the Phillips curve relation implied some significant changes in the Keynesian framework which partly escaped notice until the subsequent Monetarists' attacks. Since the rate of change of wages decreased smoothly with the rate of unemployment, there was no longer a unique Full Employment but rather a whole family of possible equilibrium rates, each associated with a different rate of inflation (and requiring, presumably, a different long run growth of money). It also impaired the notion of a stable underemployment equilibrium. A fall in demand could still cause an initial rise in unemployment but this rise, by reducing the growth of wages, would eventually raise the real money supply, tending to return unemployment to the equilibrium rate consistent with the given long run growth of money.

But at the practical level it did not lessen the case for counteracting lasting demand disturbances through stabilization policies rather than by relying on the slow process of wage adjustment to do the job, at the cost of protracted unemployment and instability of price. Indeed, the realm of stabilization policies appeared to expand in the sense that the stabilization authority had the power of choosing the unemployment rate around which employment was to be stabilized, though it then had to accept the associated inflation. Finally, the dependence of wage changes also on past inflation forced recognition of a distinction between the short- and the long-run Phillips curve, the latter exhibiting the long-run equilibrium rate of inflation implied by a *maintained* unemployment rate. The fact that the long-run tradeoff between unemployment and inflation was necessarily less favorable than the short-run one, opened up new vistas of "enjoy-it-now, pay-later" policies, and even resulted in an entertaining literature on the political business cycle and how to stay in the saddle by riding the Phillips curve (see, e.g. Ray Fair, William Nordhaus).

II. The Monetarists' Attack

A. The Stabilizing Power of the Hicksian Mechanism

The Monetarists' attack on Keynesianism was directed from the very beginning not at the Keynesian framework as such, but at whether it really implied a need for stabilization. It rested on a radically different empirical assessment of the value of the parameters controlling the stabilizing power of the Hicksian mechanism and of the magnitude and duration of response to shocks, given a stable money supply. And this different assessment in turn was felt to justify a radical downgrading of the *practical relevance* of the Keynesian framework as distinguished from its *analytical validity.*

Liquidity preference was a fine contribution to monetary theory but in practice the responsiveness of the demand for money, and hence of velocity, to interest rates, far from being unmanageably large, was so small that according to a well known paper by Milton Friedman (1969), it could not even be detected empirically. On the other hand, the effect of interest rates on aggregate demand was large and by no means limited to the traditional fixed investments but quite pervasive. The difficulty of detecting it empirically resulted from focusing on a narrow range of measured market rates and from the fact that while the aggregate could be counted on to respond, the response of individual components might not be stable. Finally, Friedman's celebrated contribution to the theory of the consumption function (1957) (and my own work on the life cycle hypothesis with Brumberg and others, reviewed in Modigliani, 1975) implied a very high short-run marginal propensity to save in response to transient disturbances to income and hence a small short-run multiplier.

All this justified the conclusion that i) though demand shocks might qualitatively work along the lines described by Keynes, quantitatively the Hicks mechanism is so strong that their impact would be *small* and *transient,* provided the stock of money was kept on a steady growth path; ii) fiscal policy actions, like other demand shocks, would have *minor* and *transitory* effects on demand, while changes in money would produce *large* and *permanent* effects on money income; and, therefore, iii) the observed instability of the economy, which was anyway proving moderate as the postwar period unfolded, was most likely the result of the unstable growth of money, be it due to misguided endeavors to stabilize income or to the pursuit of other targets, which were either irrelevant or, in the case of balance of payments goals, should have been made irrelevant by abandoning fixed exchanges.

B. The Demise of Wage Rigidity and the Vertical Phillips Curve

But the most serious challenge came in Friedman's 1968 Presidential Address, building on ideas independently put forth also by Phelps (1968). Its basic message was that, despite appearances, wages were in reality perfectly flexible and there was accordingly *no* involuntary unemployment. The evidence to the contrary, including the Phillips curve, was but a statistical illusion resulting from failure to differentiate between price changes and *unexpected* price changes.

Friedman starts out by reviving the Keynesian notion that, at any point of time, there exists a unique full employment rate which he labels the "natural rate." An unanticipated fall in demand, in Friedman's competitive world, leads firms to reduce prices and also output and employment along the short-run marginal cost curve — unless the nominal wage declines together with prices. But workers, failing to judge correctly the current and prospective fall in prices, misinterpret the reduction of nominal wages as a cut in *real* wages. Hence, assuming a positively sloped supply function, they reduce the supply of labor. As a result, the effective real wage rises to the point where the resulting decline in the demand for labor matches the reduced supply. Thus, output falls not because of the decline in demand, but because of the entirely voluntary reduction in the supply of labor, in response to erroneous perceptions. Furthermore, the fall in employment can only be temporary, as expectations must soon catch up with the facts, at least in the absence of new shocks. The very same mechanism works in the case of an increase in demand, so that the responsiveness of wages and prices is the same on either side of the natural rate.

The upshot is that Friedman's model also implies a Phillips-type relation between inflation, employment or unemployment, and past inflation — provided the latter variable is interpreted as a reasonable proxy for expected inflation. But it turns the standard explanation on its head: instead of (excess) employment causing inflation, it is (the unexpected component of) the rate of inflation that causes excess employment.

One very basic implication of Friedman's model is that the coefficient of price expectations should be precisely unity. This specification implies that whatever the shape of the short-run Phillips curve — a shape determined by the relation between expected and actual price changes, and by the elasticity of labor supply with respect to the perceived real wage — the long-run curve *must be vertical.*

Friedman's novel twist provided a fresh prop for the claim that stabilization policies are not really needed, for, with wages flexible, except possibly for transient distortions, the Hicksian mechanism receives powerful reinforcement from changes in the real money supply. Similarly, the fact that Full Employment was a razor edge provided new support for the claim that stabilization policies were bound to prove destabilizing.

C. The Macro Rational Expectations Revolution

But the death blow to the already badly battered Keynesian position was to come only shortly thereafter by incorporating into Friedman's model the so-called rational expectation hypothesis, or REH. Put very roughly, this hypothesis, originally due to John Muth, states that rational economic agents will endeavor to form expectations of relevant future variables by making the most efficient use of all information provided by past history. It is a fundamental and fruitful contribution that has already found many important applications, e.g., in connection with speculative markets, and as a basis for some thoughtful criticism by Robert Lucas (1976) of certain features of econometric models. What I am concerned with here is only its application to macro-economics, or MREH, associated with such authors as Lucas (1972), Thomas Sargent (1976), and Sargent and Neil Wallace (1976).

The basic ingredient of MREH is the postulate that the workers of Friedman's model hold rational expectations, which turns out to have a number of remarkable implications: i) errors of price expectations, which are the only source of departure from the natural rate, cannot be avoided but they can only be short lived and random. In particular, there cannot be persistent unemployment above the natural rate for this would imply high serial correlation between the successive errors of expectation, which is inconsistent with rational expectations; ii) any attempts to stabilize the economy by means of stated monetary or fiscal rules are bound to be totally ineffective because their effect will be fully discounted in rational expectations; iii) nor can the government successfully pursue ad hoc measures to offset shocks. The private sector is already taking care of any anticipated shock; therefore government policy could conceivably help only if the government information was better than that of the public, which is impossible, by the very definition of rational expectations. Under these conditions, ad hoc stabilization policies are most likely to produce instead further destabilizing shocks.

These are clearly remarkable conclusions, and a major *re*discovery — for it had all been said 40 years ago by Keynes in a well-known passage of the *General Theory:*

"If, indeed, labour were always in a position to take action (and were to do so), whenever there was less than full employment, to reduce its money demands by concerted action to whatever point was required to make money so abundant relatively to the wage-unit that the rate of interest would fall to a level compatible with full employment, we should, in effect, have monetary management by the Trade Unions, aimed at full employment, instead of by the banking system." [267] The only novelty is that MREH replaces Keynes' opening "If" with a "Since."

If one accepts this little amendment, the case against stabilization policies is complete. The economy is inherently pretty stable — except possibly for the effect of government messing around. And to the extent that there is a small residual instability, it is beyond the power of human beings, let alone the government, to alleviate it.

III. How Valid Is the Monetarist Case?

A. The Monetarist Model of Wage Price Behavior

In setting out the counterattack it is convenient to start with the Monetarists' model of price and wage behavior. Here one must distinguish between the model as such and a specific implication of that model, namely that the long-run Phillips curve is vertical, or, in substance, that, in the long run, money is neutral. That conclusion, by now, does not meet serious objection from non-Monetarists, at least as a first approximation.

But the proposition that other things equal, and given time enough, the economy will eventually adjust to any indefinitely maintained stock of money, or nth derivative thereof, can be derived from a variety of models and, in any event, is of very little practical relevance, as I will argue below. What is unacceptable, because inconsistent with both micro and macro evidence, is the specific Monetarist model set out above and its implication that all unemployment is a voluntary, fleeting response to transitory misperceptions.

One may usefully begin with a criticism of the Macro-Rational Expectations model and why Keynes' "if" should not be replaced by "since." At the logical level, Benjamin Friedman has called attention to the omission from MREH of an explicit learning model, and has suggested that, as a result, it can only be interpreted as a description not of short-run but of long-run equilibrium in which no agent would wish to recontract. But then the implications of MREH are clearly far from startling, and their policy relevance is almost nill. At the institutional level, Stanley Fischer has shown that the mere recognition of long term contracts is sufficient to generate wage rigidity and a substantial scope for stabilization policies. But the most glaring flaw of MREH is its inconsistency with the evidence: if it were valid, deviations of unemployment from the natural rate would be small and transitory — in which case the *General Theory* would not have been written and neither would this paper. Sargent (1976) has attempted to remedy this fatal flaw by hypothesizing that the persistent and large fluctuations in unemployment reflect merely corresponding swings in the natural rate itself. In other words, what happened to the

U.S. in the 1930's was a severe attack of contagious laziness! I can only say that, despite Sargent's ingenuity, neither I nor, I expect most others at least of the non-Monetarist persuasion, are quite ready yet to turn over the field of economic fluctuations to the social psychologist!

Equally serious objections apply to Friedman's modeling of the commodity market as a perfectly competitive one — so that the real wage rate is continuously equated to the *short-run* marginal product of labor — and to his treatment of labor as a homogenous commodity traded in an auction market, so that, at the going wage, there never is any excess demand by firms or excess supply by workers.

The inadequacies of this model as a useful formalization of present day Western economies are so numerous that only a few of the major ones can be mentioned here. Friedman's view of unemployment as a voluntary reduction in labor supply could at best provide an explanation of variations in labor force — and then only under the questionable assumption that the supply function has a significantly positive slope — but cannot readily account for changes in unemployment. Furthermore, it cannot be reconciled with the well known fact that *rising* unemployment is accompanied by a fall, not by a *rise* in quits, nor with the role played by temporary lay-offs to which Feldstein has recently called attention. Again, his competitive model of the commodity market, accepted also in the *General Theory,* implies that changes in real wages, adjusted for long-run productivity trend, should be significantly negatively correlated with cyclical changes in employment and output and with changes in money wages. But as early as 1938, John T. Dunlop showed that this conclusion was rejected by some eighty years of British experience and his results have received some support in more recent tests of Ronald Bodkin for the U.S. and Canada. Similar tests of my own, using quarterly data, provide striking confirmation that for the last two decades from the end of the Korean war until 1973, the association of trend adjusted real compensations of the private nonfarm sector with either employment or the change in nominal compensation is prevailingly positive and very significantly so.[1]

This evidence can, instead, be accounted for by the oligopolistic pricing model — according to which price is determined by *long-run* minimum average cost up to a mark-up reflecting entry preventing considerations (cf. Modigliani, 1958) — coupled with some lags in the adjustment of prices to costs. This model implies that firms respond to a change in demand by endeavoring to adjust output and employment, without significant changes in prices relative to wages; and the resulting changes in available jobs have their initial impact not on wages but rather on unemployment by way of lay-offs and recalls and through changes in the level of vacancies, and hence on the length of average search time.

If, in the process, vacancies rise above a critical level, or "natural rate," firms will endeavor to reduce them by outbidding each other, thereby raising the rate of change of wages. Thus, as long as jobs and vacancies remain above, and unemployment remains below, some critical level which might be labeled the "non-inflationary rate" (Modigliani and Lucas Papademos, 1975), wages and prices will tend to accelerate. If, on the other hand, jobs fall below, and unemployment rises above, the non-inflationary rate, firms finding that vacancies are less than optimal — in the limit the unemployed queuing outside the gate will fill them instantly — will have an incentive to reduce their relative wage offer. But in this case, in which too much labor is looking for too few jobs, the trend toward a sustained decline in the rate of growth of wages is likely to be even weaker than the corresponding acceleration when too many jobs are bidding for too few people. The main reason is the non-homogeneity of labor. By far the largest and more valuable source of labor supply to a firm consists of those already employed who are not readily interchangeable with the unemployed, and in contrast with them, are concerned with protecting their earnings and not with reestablishing full employment. For these reasons, and because the first to quit are likely to be the best workers, a reduction of the labor force can, within limits, be accomplished more economically, not by reducing wages to generate enough quits, but by firing or, when possible, by lay-offs which insure access to a trained labor force when demand recovers. More generally, the inducement to reduce relative wages to eliminate the excess supply is moderated by the effect that such a reduction would have on quits and costly turnover, even when the resulting vacancies can be readily filled from the ranks of the unemployed. Equally relevant are the consequences in terms of loss of morale and good will, in part for reasons which have been elaborated by the literature on implicit contracts (cf. Robert Gordon). Thus, while there will be some tendency for the rate of change of wages to fall, the more so the larger the unemployment — at least in an economy like the U.S. where there are no overpowering centralized unions — that tendency is severely damped.

And whether, given an unemployment rate significantly and persistently above the non-inflationary level, the rate of change of wages would, eventually, tend to turn negative and decline without bound or whether it would tend to an asymptote is a question that I doubt the empirical evidence will ever answer. The one experiment we have had — the Great Depression — suggests the answer is negative, and while I admit that, for a variety of reasons, that evidence is muddied, I hope that we will never have the opportunity for a second, clean experiment.

In any event, what is really important for practical purposes is not the long-run equilibrium relation as such, but the speed with which it is approached. Both the model sketched out and the empirical evidence suggest that the process of acceleration or deceleration of wages when unemployment differs from the non-inflationary rate will have more nearly the character of a crawl than of a gallop. It will suffice to recall in this connection that there was excess demand pressure in the U.S. at least from 1965 to mid 1970, and during that period the growth of inflation was from some 1.5 to only about 5.5 percent per year. And the response to the excess supply pressure from mid 1970 to early 1973, or from late 1974 to date was equally sluggish.

B. The Power of Self-Stabilizing Mechanisms: The Evidence from Econometric Models

There remains to consider the Monetarists' initial criticism of Keynesianism, to wit, that even without high wage flexibility, the system's response to demand shocks is small and short lived, thanks to the power of the Hicksian mechanism. Here it must be acknowledged that every one of the Monetarists' criticisms of early, simple-minded Keynesianism has proved in considerable measure correct.

With regard to the interest elasticity of demand for money, post Keynesian developments in the theory of money, and in particular, the theoretical contributions of William Baumol, James Tobin, Merton Miller and Daniel Orr, point to a modest value of around one-half to one-third, and empirical studies (e.g., Stephen M. Goldfeld) are largely consistent with this prediction (at least until 1975!). Similarly, the dependence of consumption on long run, or life cycle, income and on wealth, together with the high marginal tax rates of the postwar period, especially the corporate tax, and leakages through imports, lead to a rather low estimate of the multiplier.

Last but not least, both theoretical and empirical work, reflected in part in econometric models, have largely vindicated the Monetarist contention that interest effects on demand are pervasive and substantial. Thus, in the construction and estimation of the MIT-Penn-Social Science Research Council (MPS) econometric model of the U.S., we found evidence of effects, at least modest, on nearly every component of aggregate demand. One response to money supply changes that is especially important in the MPS, if somewhat controversial, is via interest rates on the market value of all assets and thus on consumption.

There is, therefore, substantial agreement that, in the U.S., the Hicksian mechanism is fairly effective in limiting the effect of shocks, and that the response of wages and prices to excess demand or supply will also work *gradually* toward eliminating largely, if not totally, any effect on employment. But in the view of non-Monetarists, the evidence overwhelmingly supports the conclusion that the *interim* response is still of significant magnitude and of considerable duration, basically because the wheels of the offsetting mechanism grind slowly. To be sure, the first link of the mechanism, the rise in short term rates, gets promptly into play and heftily, given the low money demand elasticity; but most expenditures depend on long-term rates, which generally respond but gradually, and the demand response is generally also gradual. Furthermore, while this response is building up, multiplier and accelerator mechanisms work toward amplifying the shock. Finally, the classical mechanism — the change in real money supply through prices — has an even longer lag because of the sluggish response of wages to excess demand.

These inferences are supported by simulations with econometric models like the MPS. Isolating, first, the working of the Hicksian mechanism by holding prices constant, we find that a one-percent demand shock, say a rise in real exports, produces an impact effect on aggregate output which is barely more than one percent, rises to a peak of only about two percent a year later, and then declines slowly toward a level somewhat over one-and-one-half percent.

Taking into account the wage-price mechanism hardly changes the picture for the first year because of its inertia. Thereafter, however, it becomes increasingly effective so that a year later the real response is back at the impact level, and by the end of the third year the shock has been fully offset (thereafter output oscillates around zero in a damped fashion). Money income, on the other hand, reaches a peak of over two-and-one-half, and then only by the middle of the second year. It declines thereafter, and tends eventually to oscillate around a *positive* value because normally a demand shock requires eventually a change in interest rates and hence in velocity and money income.

These results, which are broadly confirmed by other econometric models, certainly do not support the view of a highly unstable economy in which fiscal policy has powerful and everlasting effects. But neither do they support the Monetarist view of a highly stable economy in which shocks hardly make a ripple and the effects of fiscal policy are puny and fast vanishing.

C. The Monetarist Evidence and the St. Louis Quandary

Monetarists, however, have generally been inclined to question this evidence. They countered at first with tests bearing on the stability of velocity and the insignificance of the multiplier, which, however, as indicated in my criticism with Albert Ando (1965), must be regarded as close to worthless. More recently, several authors at the Federal Reserve Bank of St. Louis (Leonall C. Andersen, Keith M. Carlson, Jerry Lee Jordan), have suggested that instead of deriving multipliers from the analytical or numerical solution of an econometric model involving a large number of equations, any one of which may be questioned, they should be estimated directly through "reduced form" equations by relating the change in income to current and lagged changes in some appropriate measure of the money supply and of fiscal impulses.

The results of the original test, using the current and but four lagged values of M_1 and of high-employment federal expenditure as measures of monetary and fiscal impulses, turned out to be such as to fill a Monetarist's heart with joy. The contribution of money, not only current but also lagged, was large and the coefficients implied a not unreasonable effect of the order of magnitude of the velocity of circulation, though somewhat higher. On the other hand, the estimated coefficients of the fiscal variables seemed to support fully the Monetarists' claim that their impact was both small and fleeting: the effect peaked in but two quarters and was only around one, and disappeared totally by the fourth quarter following the change.

These results were immediately attacked on the ground that the authors had used the wrong measure of monetary and fiscal actions, and it was shown that the outcome was somewhat sensitive to alternative measures; however, the basic nature of the results did not change, at least qualitatively. In particular, the outcome does not differ materially, at least for the original period up to 1969, if one replaces high-employment outlays with a variable that might be deemed more suitable, like government expenditure on goods and services, plus exports.

These results must be acknowledged as disturbing for non-Monetarists, for there is little question that movements in government purchases and exports are a major source of demand disturbances; if econometric model estimates of the response to demand disturbances are roughly valid, how can they be so grossly inconsistent with the reduced form estimates?

Attempts at reconciling the two have taken several directions, which are reviewed in an article with Ando (1976). Our main conclusion, based on simulation techniques, is that when income is subject to substantial shocks from many sources other than monetary and fiscal, so that these variables account for only a moderate portion of the variations in income (in the U.S., it has been of the order of one-half to two-thirds), then the St. Louis reduced form method yields highly unstable and unreliable estimates of the true structure of the system generating the data.

The crucial role of unreliability and instability has since been confirmed in more recent work of Daniel O'Neill in a forthcoming MIT thesis. He shows, in the first place, that different methods of estimation yield widely different estimates, including many which clearly overstate the expenditure, and understate the money multipliers. He further points out that, given the unreliability of the estimates resulting from multicollinearity and large residual variance, the relevant question to ask is not whether these estimates differ from those obtained by structural estimation, but whether the *difference is statistically significant*, that is, larger than could be reasonably accounted for by sampling fluctuations.

I have carried out this standard statistical test using as true response coefficients those generated by the MPS model quoted earlier.[2] I find that, at least when the test is based on the largest possible sample — the entire post-Korean period up to the last two very disturbed years — the difference is totally insignificant when estimation is in level form (F is less than one) and is still not significant at the five-percent level, when in first differences.

This test resolves the puzzle by showing that there really is no puzzle: the two alternative estimates of the expenditure multipliers are not inconsistent, given the margin of error of the estimates. It implies that one should accept whichever of the two estimates is produced by a more reliable and stable method, and is generally more sensible. To me, those criteria call, without question, for adopting the econometric model estimates. But should there be still some lingering doubt about this choice, I am happy to be able to report the results of one final test which I believe should dispose of the reduced form estimates — at least for a while. Suppose the St. Louis estimates of the expenditure multiplier are closer to God's truth than the estimates derived through econometric models. Then it should be the case that if one uses their coefficients to forecast income beyond the period of fit, these forecasts should be appreciably better than those obtained from a forecasting equation in which the coefficients of the expenditure variable are set equal to those obtained from econometric models.

I have carried out this test, comparing a reduced form equation fitted to the period originally used at St. Louis, terminating in 1969 (but reestimated with the latest revised data) with an equation in which the coefficients of government expenditure plus exports were constrained to be those estimated from the MPS, used in the above F test. The results are clear cut: the errors using the reduced form coefficient are not smaller but on the average substantially *larger* than those using MPS multipliers. For the first four years, terminating at the end of 1973, the St. Louis equation produces errors which are distinctly larger in eight quarters, and smaller in but three, and its squared error is one-third larger. For the last two years of turmoil, both equations perform miserably, though even here the MPS coefficients perform just a bit better. I have repeated this test with equations estimated through the first half of the postwar period, and the results are, if anything, even more one-sided.

The moral of the story is pretty clear. First, reduced form equations relying on just two exogenous variables are very unreliable for the purpose of estimating structure, nor are they particularly accurate for forecasting, though per dollar of research expenditure they are surprisingly good. Second, if the St. Louis people want to go on using this method and wish to secure the best possible forecast, then they should ask the MPS or any other large econometric model what coefficients they should use for government expenditure, rather than trying to estimate them by their unreliable method.

From the theory and evidence reviewed, we must then conclude that opting for a constant rate of growth of the nominal money supply can result in a stable economy only in the absence of significant exogenous shocks. But obviously the economy has been and will continue to be exposed to many significant shocks, coming from such things as war and peace, and other large changes in government expenditure, foreign trade, agriculture, technological progress, population shifts and what not. The clearest evidence on the importance of such shocks is provided by our postwar record with its six recessions.

IV. The Record of Stabilization Policies: Stabilizing or Destabilizing

A. Was Postwar Instability Due to Unstable Money Growth?

At this point, of course, Monetarists will object that, over the postwar period, we have *not* had a constant money growth policy and will hint that the observed instability can largely be traced to the instability of money. The only way of meeting this objection squarely would be, of course, to rerun history with a good computer capable of calculating three percent at the helm of the Fed.

A more feasible, if less conclusive approach might be to look for some extended periods in which the money supply grew fairly smoothly and see how the economy fared. Combing through our post Korean war history, I have been able to find just two stretches of several years in which the growth of the money stock was relatively stable, whether one chooses to measure stability in terms of percentage deviations from a constant growth or of dispersion of four-quarter changes. It may surprise some that one such stretch occurred quite recently and consists of the period of nearly four years beginning in the first quarter of 1971 (cf. Modigliani and Papademos, 1976). During this period, the average growth was quite large, some seven percent, but it was relatively smooth, generally well within the six-to eight-percent band. The average deviation from the mean is about .75 percent. The other such period lasted from the beginning of 1953 to the first half of 1957, again a stretch of roughly four years. In sharp contrast to the most recent period, the average growth here is quite modest, only about two percent; but again, most four-quarter changes fell well within a band of two percentage points, and the average deviation is again .7. By contrast, during the remaining 13-year stretch from mid-1957 to the end of 1970, the variability of money growth was roughly twice as large if measured by the average deviation of four-quarter changes, and some five times larger if measured by the percentage deviation of the money stock from a constant growth trend.

How did the economy fare in the two periods of relatively stable money growth? It is common knowledge that the period from 1971 to 1974, or from 1972 to 1975 if we want to allow a one-year lag for money to do its trick, was distinctly the most unstable in our recent history, marked by sharp fluctuations in output and wild gyrations of the rate of change of prices. As a result, the average deviation of the four-quarter changes in output was 3.3 percent, more than twice as large as in the period of less stable money growth. But the first stretch was also marked by well above average instability, with the contraction of 1954, the sharp recovery of 1955, and the new contraction of 1958, the sharpest in postwar history except for the present one. The variability of output is again 50 percent larger than in the middle period.

To be sure, in the recent episode serious exogenous shocks played a major role in the development of prices and possibly output, although the same is not so readily apparent for the period 1953 to 1958. But, in any event, such extenuating circumstances are quite irrelevant to my point; for I am not suggesting that the stability of money was the major cause of economic instability — or at any rate, not yet!! All I am arguing is that i) there is no basis for the Monetarists' suggestion that our postwar instability can be traced to monetary instability — our most unstable periods have coincided with periods of relative monetary stability; and ii) stability of the money supply is not enough to give us a stable economy, precisely because there are exogenous disturbances.

Finally, let me mention that I have actually made an attempt at rerunning history to see whether a stable money supply would stabilize the economy, though in a way that I readily acknowledge is much inferior to the real thing, namely through a simulation with the MPS. The experiment, carried out in cooperation with Papademos, covered the relatively quiet period from the beginning of 1959 to the introduction of price-wage controls in the middle of 1971. If one eliminates all major sources of shocks, e.g., by smoothing Federal government expenditures, we found, as did Otto Eckstein in an earlier experiment, that a stable money growth of three percent per year does stabilize the economy, as expected. But when we allowed for all the historical shocks, the result was that with a constant money growth the economy was far from stable — in fact, it was distinctly less stable than actual experience, by a factor of 50 percent.

B. The Overall Effectiveness of Postwar Stabilization Policies

But even granted that a smooth money supply will not produce a very stable world and that there is, therefore, room for stabilization policies, Monetarists will still argue that we should nonetheless eschew such policies. They claim, first, that allowing for unpredictably variable lags and unforseeable future shocks, we do not know enough to successfully design stabilization policies, and second, that the government would surely be incapable of choosing the appropriate policies or be politically willing to provide timely enforcement. Thus, in practice, stabilization policies will result in destabilizing the economy much of the time.

This view is supported by two arguments, one logical and one empirical. The logical argument is the one developed in Friedman's Presidential address (1968). An attempt at stabilizing the economy at full employment is bound to be destabilizing because the full employment or natural rate is not known with certainty and is subject to shifts in time; and if we aim for the incorrect rate, the result must perforce be explosive inflation or deflation. By contrast, with a constant money supply policy, the economy will automatically hunt for, and eventually discover, that shifty natural rate wherever it may be hiding.

This argument, I submit, is nothing but a debating ploy. It rests on the preposterous assumption that the only alternative to a constant money growth is the pursuit of a very precise unemployment target which will be adhered to indefinitely no matter what, and that if the target is off in the second decimal place, galloping inflation is around the corner. In reality, all that is necessary to pursue stabilization policies is a rough target range that includes the warranted rate, itself a range and not a razor edge; and, of course, responsible supporters of stabilization policies have long been aware of the fact that the target range needs to be adjusted in time on the basis of forseeable shifts in the warranted range, as well as in the light of emerging evidence that the current target is not consistent with price stability. It is precisely for this reason that I, as well as many other non-Monetarists, would side with Monetarists in strenuous opposition to recent proposals for a target unemployment rate rigidly fixed by statute (although there is nothing wrong with Congress committing itself and the country to work toward the eventual achievement of some target unemployment rate through *structural* changes rather than aggregate demand policies).

Clearly, even the continuous updating of targets cannot guarantee that errors can be avoided altogether or even that they will be promptly recognized; and while errors persist, they will result in some inflationary (or deflationary) pressures. But the growing inflation to which Friedman refers is, to repeat, a crawl not a gallop. One may usefully recall in this connection the experience of 1965-70 referred to earlier, with the further remark that the existence of excess employment was quite generally recognized at the time, and failure to eliminate it resulted overwhelmingly from political considerations and not from a wrong diagnosis.[3]

There remains then only the empirical issue: have stabilization policies worked in the past and will they work in the future? Monetarists think the answer is negative and suggest, as we have seen, that misguided attempts at stabilization, especially through monetary policies, are responsible for much of the observed instability. The main piece of evidence in support of this contention is the Great Depression, an episode well documented through the painstaking work of Friedman and Anna Schwartz, although still the object of dispute (see, e.g. Peter Temin). But in any event, that episode while it may attest to the power of money, is irrelevant for present purposes since the contraction of the money supply was certainly not part of a comprehensive stabilization program in the post Keynesian sense.

When we come to the relevant postwar period, the problem of establishing the success or failure of stabilization policies is an extremely taxing one. Many attempts have been made at developing precise objective tests, but in my view, none of these is of much value, even though I am guilty of having contributed to them in one of my worst papers (1964). Even the most ingenious test, that suggested by Victor Argy, and relying on a comparison of the variability of income with that of the velocity of circulation, turns out to be valid only under highly unrealistic restrictive assumptions.

Dennis Starleaf and Richard Floyd have proposed testing the effectiveness of stabilization by comparing the stability of money growth with that of income growth, much as I have done above for the U.S., except that they apply their test to a cross section of industrialized countries. They found that for a sample of 13 countries, the association was distinctly positive. But this test is again of little value. For while a negative association for a given country, such as suggested by my U.S. test, does provide some weak indication that monetary activism helped rather than hindered, the finding of a positive association across countries proves absolutely nothing. It can be readily shown, in fact, that, to the extent that differential variability of income reflects differences in the character of the shocks — a most likely circumstance for their sample — successful stabilization also implies a positive correlation between the variability of income and that of money.

But though the search for unambiguous quantitative tests has so far yielded a meager crop, there exists a different kind of evidence in favor of Keynesian stabilization policies which is impressive, even if hard to quantify. To quote one of the founding fathers of business cycle analysis, Arthur Burns, writing in 1959, "Since 1937 we have had five recessions, the longest of which lasted only 13 months. There is no parallel for such a sequence of mild — or such a sequence of brief — contractions, at least during the past hundred years in our country." [p. 2]. By now we can add to that list the recessions of 1961 and 1970.

There is, furthermore, evidence that very similar conclusions hold for other industrialized countries which have made use of stabilization policies; at any rate that was the prevailing view among participants to an international conference held in 1967 on the subject, "Is the business cycle obsolete?" (see Martin Bronfenbrenner, editor). No one seemed to question the greater postwar stability of all Western economies — nor is this surprising when one recalls that around that time business cycle specialists felt so threatened by the new-found stability that they were arguing for redefining business cycles as fluctuations in the *rate of growth* rather than in the *level* of output.

It was recognized that the reduced severity of fluctuations might in part reflect structural changes in the economy and the effect of stronger built-in stabilizers, inspired, of course, by the Keynesian analysis. Furthermore, the greater stability in the U.S., and in other industrialized countries, are obviously not independent events. Still, at least as of the time of that conference, there seemed to be little question and some evidence that part of the credit for the greater stability should go to the conscious and on balance, successful endeavor at stabilizing the economy.

V. The Case of Supply Shocks and the 1974-76 Episode

A. Was the 1974 Depression Due to Errors of Commission or Omission?

In pointing out our relative postwar stability and the qualified success of stabilization policies, I have carefully defined the postwar period as ending somewhere in 1973. What has happened since that has so tarnished the reputation of economists? In facing this problem, the first question that needs to be raised is whether the recent combination of unprecedented rates of inflation as well as unemployment must be traced to crimes of commission or omission. Did our monetary and fiscal stabilization policies misfire, or did we instead fail to use them?

We may begin by establishing one point that has been blurred by Monetarists' blanket indictments of recent monetary policy: the virulent explosion that raised the four-quarter rate of inflation from about 4 percent in 1972 to 6½ by the third quarter of 1973, to 11½ in 1974, with a peak quarterly rate of 13½, can in no way be traced to an excessive, or to a disorderly, growth of the money supply. As already mentioned, the average rate of money growth from the beginning of 1970 to the second half of 1974 was close to 7 percent. To be sure, this was a high rate and could be expected sooner or later to generate an undesirably high inflation — but how high? Under any reasonable assumption one cannot arrive at a figure much above 6 percent. This might explain what happened up to the fall of 1973, but not from the third quarter of 1973 to the end of 1974, which is the really troublesome period. Similarly, as was indicated above, the growth of money was reasonably smooth over this period, smoother than at any other time in the postwar period, staying within a two-percent band. Hence, the debacle of 1974 can just not be traced to an erratic behavior of money resulting from a misguided attempt at stabilization.

Should one then conclude that the catastrophe resulted from too slavish an adherence to a stable growth rate, forsaking the opportunity to use monetary policy to stabilize the economy? In one sense, the answer to this question must, in my view, be in the affirmative. There is ample ground for holding that the rapid contraction that set in toward the end of 1974, on the heels of a slow decline in the previous three quarters, and which drove unemployment to its 9-percent peak, was largely the result of the astronomic rise in interest rates around the middle of the year. That rise in turn was the unavoidable result of the Fed's stubborn refusal to accommodate, to an adequate extent, the exogenous inflationary shock due to oil, by letting the money supply growth exceed the 6-percent rate announced at the beginning of the year. And this despite repeated warnings about that unavoidable result (e.g., Modigliani, 1974).

Monetarists have suggested that the sharp recession was not the result of too slow a monetary growth throughout the year, but instead of the deceleration that took place in the last half of 1974 and early 1975. But this explanation just does not stand up to the facts. The fall in the quarterly growth of money in the third and fourth quarters was puny, especially on the basis of revised figures now available: from 5.7 percent in the second to 4.3 and 4.1 — hardly much larger than the error of estimate for quarterly rates! To be sure, in the first quarter the growth fell to .6 percent. But, by then, the violent contraction was well on its way — between September 1974 and February 1975, industrial production fell at an annual rate of 25 percent. Furthermore, by the next quarter, monetary growth had resumed heftily. There is thus no way the Monetarist proposition can square with these facts unless their long and variable lags are so variable that they sometimes turn into substantial leads. But even then, by anybody's model, a one-quarter dip in the growth of money could not have had a perceptible effect.

B. What Macro Stabilization Policies Can Accomplish, and How

But recognizing that the adherence to a stable money growth path through much of 1974 bears a major responsibility for the sharp contraction does not per se establish that the policy was mistaken. The reason is that the shock that hit the system in 1973-74 was not the usual type of demand shock which we have gradually learned to cope with, more or less adequately. It was, instead, a supply or price shock, coming from a cumulation of causes, largely external. This poses an altogether different stabilization problem. In particular, in the case of demand shocks, there exists, in principle, an ideal policy which avoids all social costs, namely to offset completely the shock thus, at the same time, stabilizing employment and the price level. There may be disagreement as to whether this target can be achieved and how, but not about the target itself.

But in the case of supply shocks, there is no miracle cure — there is no macro policy which can both maintain a stable price level and keep employment at its natural rate. To maintain stable prices in the face of the exogenous price shock, say a rise in import prices, would require a fall in all domestic output prices; but we know of no macro policy by which domestic prices can be made to fall except by creating enough slack, thus putting downward pressure on wages. And the amount of slack would have to be substantial in view of the sluggishness of wages in the face of unemployment. If we do not offset the exogenous shock completely, then the initial burst, even if activated by an entirely transient rise in some prices, such as a once and for all deterioration in the terms of trade, will give rise to further increases, as nominal wages rise in a vain attempt at preserving real wages; this secondary reaction too can only be cut short by creating slack. In short, once a price shock hits, there is no way of returning to the initial equilibrium except after a painful period of both above equilibrium employment and inflation.

There are, of course, in principle, policies other than aggregate demand management to which we might turn, and which are enticing in view of the unpleasant alternatives offered by demand management. But so far such policies, at least those of the wage-price control variety, have proved disappointing. The design of better alternatives is probably the greatest challenge presently confronting those interested in stabilization. However, these policies fall outside my present concern. Within the realm of aggregate demand management, the only choice open to society is the cruel one between alternative feasible paths of inflation and associated paths of unemployment, and the best the macroeconomist can offer is policies designed to approximate the chosen path.

In light of the above, we may ask: is it conceivable that a constant rate of growth of the money supply will provide a satisfactory response to price shocks in the sense of giving rise to an unemployment-inflation path to which the country would object least?

C. The Monetarist Prescription: Or Constant Money Growth Once More

The Monetarists are inclined to answer this question affirmatively, if not in terms of the country's preferences, at least in terms of the preferences they think it should have. This is evidenced by their staunch support of a continuation of the six-percent or so rate of growth through 1974, 1975 and 1976.

Their reasoning seems to go along the following lines. The natural rate hypothesis implies that the rate of inflation can change only when employment deviates from the natural rate. Now suppose we start from the natural rate and some corresponding steady rate of inflation, which without loss of generality can be assumed as zero. Let there be an exogenous shock which initially lifts the rate of inflation, say, to 10 percent. If the Central Bank, by accommodating this price rise, keeps employment at the natural rate, the new rate of 10 percent will also be maintained and will in fact continue forever, as long as the money supply accommodates it. The only way to eliminate inflation is to increase unemployment enough, above the natural rate and for a long enough time, so that the cumulated reduction of inflation takes us back to zero. There will of course be many possible unemployment paths that will accomplish this. So the next question is: Which is the least undesirable?

The Monetarist answer seems to be — and here I confess that attribution becomes difficult — that it does not make much difference because, to a first approximation, the cumulated amount of unemployment needed to unwind inflation is independent of the path. If we take more unemployment early, we need to take less later, and conversely. But then it follows immediately that the specific path of unemployment that would be generated by a constant money growth is, if not better, at least as good as any other. Corollary: a constant growth of money is a satisfactory answer to supply shocks just as it is to demand shocks — as well as, one may suspect, to any other conceivable illness, indisposition or disorder.

D. Why Constant Money Growth Cannot Be the Answer

This reasoning is admirably simple and elegant, but it suffers from several flaws. The first one is a confusion between the price level and its rate of change. With an unchanged constant growth of the nominal money stock, the system will settle back into equilibrium not when the rate of inflation is back to zero but only when, in addition, the price level itself is back to its initial level. This means that when inflation has finally returned back to the desired original rate, unemployment cannot also be back to the original level but will instead remain above it as long as is necessary to generate enough deflation to offset the earlier cumulated inflation. I doubt that this solution would find many supporters and for a good reason; it amounts to requiring that none of the burden of the price shock should fall on the holder of long-term money fixed contracts — such as debts — and that all other sectors of society should shoulder entirely whatever cost is necessary to insure this result. But if, as seems to be fairly universally agreed, the social target is instead to return the system to the original rate of inflation — zero in our example — then the growth of the money supply cannot be kept constant. Between the time the shock hits and the time inflation has returned to the long-run level, there must be an additional increase in money supply by as much as the price level or by the cumulant of inflation over the path.

A second problem with the Monetarists' argument is that it implies a rather special preference function that depends only on cumulated unemployment. And, last but not least, it requires the heroic assumption that the Phillips curve be not only vertical in the long run but also linear in the short run, an assumption that does not seem consistent with empirically estimated curves. Dropping this last assumption has the effect that, for any given social preference, there will be, in general, a unique optimal path. Clearly, for this path to be precisely that generated by a constant money growth, would require a miracle — or some sleight of the invisible hand!

Actually, there is ground for holding that the unemployment path generated by a constant money growth, even if temporarily raised to take care of the first flaw, could not possibly be close to an optimal. This conclusion is based on an analysis of optimal paths, relying on the type of linear welfare function that appears to underlie the Monetarists' argument, and which is also a straightforward generalization of Okun's famous "economic discomfort index." That index (which according to Michael Lovell appears to have some empirical support) is the sum of unemployment and inflation. The index used in my analysis is a weighted average of the cumulated unemployment and cumulated inflation over the path. The weights express the relative social concern for inflation versus unemployment.

Using this index, it has been shown in a forthcoming thesis of Papademos that, in general, the optimum policy calls for raising unemployment at once to a certain critical level and keeping it there until inflation has substantially abated. The critical level depends on the nature of the Phillips curve and the relative weights, but does not depend significantly on the initial shock — as long as it is appreciable. To provide an idea of the order of magnitudes involved, if one relies on the estimate of the Phillips curve reported in a joint paper with Papademos (1975), which is fairly close to vertical, and uses Okun's weights, one finds that i) at the present time, the non-inflationary rate of unemployment corresponding to a 2-percent rate of inflation can be estimated at 5.6 percent, and ii) the optimal response to a large exogenous price shock consists in increasing unemployment from 5.6 to only about 7 percent. That level is to be maintained until inflation falls somewhat below 4 percent; it should then be reduced slowly until inflation gets to 2½ (which is estimated to take a couple of years), and rapidly thereafter. If, on the other hand, society were to rate inflation twice as costly as unemployment, the initial unemployment rate becomes just over 8, though the path to final equilibrium is then shorter. These results seem intuitively sensible and quantitatively reasonable, providing further justification for the assumed welfare function, with its appealing property of summarizing preferences into a single readily understandable number.

One important implication of the nature of the optimum path described above is that a constant money growth could not possibly be optimal while inflation is being squeezed out of the system, regardless of the relative weights attached to unemployment and inflation. It would tend to be prevailingly too small for some initial period and too large thereafter.

One must thus conclude that the case for a constant money growth is no more tenable in the case of supply shocks than it is in the case of demand shocks.

VI. Conclusion

To summarize, the Monetarists have made a valid and most valuable contribution in establishing that our economy is far less unstable than the early Keynesians pictured it and in rehabilitating the role of money as a determinant of aggregate demand. They are wrong, however, in going as far as asserting that the economy is sufficiently shock-proof that stabilization policies are not needed. They have also made an important contribution in pointing out that such policies might in fact prove destabilizing. This criticism has had a salutary effect on reassessing what stabilization policies can and should do, and on trimming down fine-tuning ambitions. But their contention that postwar fluctuations resulted from an unstable money growth or that stabilization policies decreased rather than increased stability just does not stand up to an impartial examination of the postwar record of the U.S. and other industrialized countries. Up to 1974, these policies have helped to keep the economy reasonably stable by historical standards, even though one can certainly point to some occasional failures.

The serious deterioration in economic stability since 1973 must be attributed in the first place to the novel nature of the shocks that hit us, namely supply shocks. Even the best possible aggregate demand management cannot offset such shocks without a lot of unemployment together with a lot of inflation. But, in addition, demand management was far from the best. This failure must be attributed in good measure to the fact that we had little experience or even an adequate conceptual framework to deal with such shocks; but at least from my reading of the record, it was also the result of failure to use stabilization policies, including too slavish adherence to the Monetarists' constant money growth prescription.

We must, therefore, categorically reject the Monetarist appeal to turn back the clock 40 years by discarding the basic message of the *General Theory*. We should instead concentrate our efforts in an endeavor to make stabilization policies even more effective in the future than they have been in the past.

Footnotes

[1]Thus, in a logarithmic regression of private nonfarm hourly compensation deflated by the private nonfarm deflator on output per man-hour, time, and private nonfarm employment, after correcting for first-order serial correlation, the latter variable has a coefficient of .17 and a *t*-ratio of 5. Similar though less significant results were found for manufacturing. If employment is replaced by the change in nominal compensation, its coefficient is .40 with a *t*-ratio of 6.5. Finally, if the change in compensation is replaced by the change in price, despite the negative bias from error of measurement of price, the coefficient of this variable is only -.09 with an entirely insignificant *t*-ratio of .7. The period after 1973 has been omitted from the tests as irrelevant for our purposes, since the inflation was driven primarily by an exogenous price shock rather than by excess demand. As a result of the shock, prices, and to some extent wages, rose rapidly while employment and real wages fell. Thus, the addition of the last two years tends to increase spuriously the positive association between real wages and employment, and to decrease that between real wages and the change in nominal wages or prices.

[2]For the purpose of the test, coefficients were scaled down by one-third to allow for certain major biases in measured government expenditure for present purposes (mainly the treatment of military procurement on a delivery rather than work progress basis, and the inclusion of direct military expenditure abroad).

[3]Friedman's logical argument against stabilization policies and in favor of a constant money growth rule is, I submit, much like arguing to a man from St. Paul wishing to go to New Orleans on important business that he would be a fool to drive and should instead get himself a tub and drift down the Mississippi: that way he can be pretty sure that the current will eventually get him to his destination; whereas, if he drives, he might make a wrong turn and, before he notices he will be going further and further away from his destination and pretty soon he may end up in Alaska, where he will surely catch pneumonia and he may never get to New Orleans!

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