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The Monetarist Controversy

A Seminar Discussion

**Paper by
Franco Modigliani**

**Discussion by
Milton Friedman**

THE MONETARIST CONTROVERSY

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EDITORIAL NOTE

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

The Monetarist Controversy

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

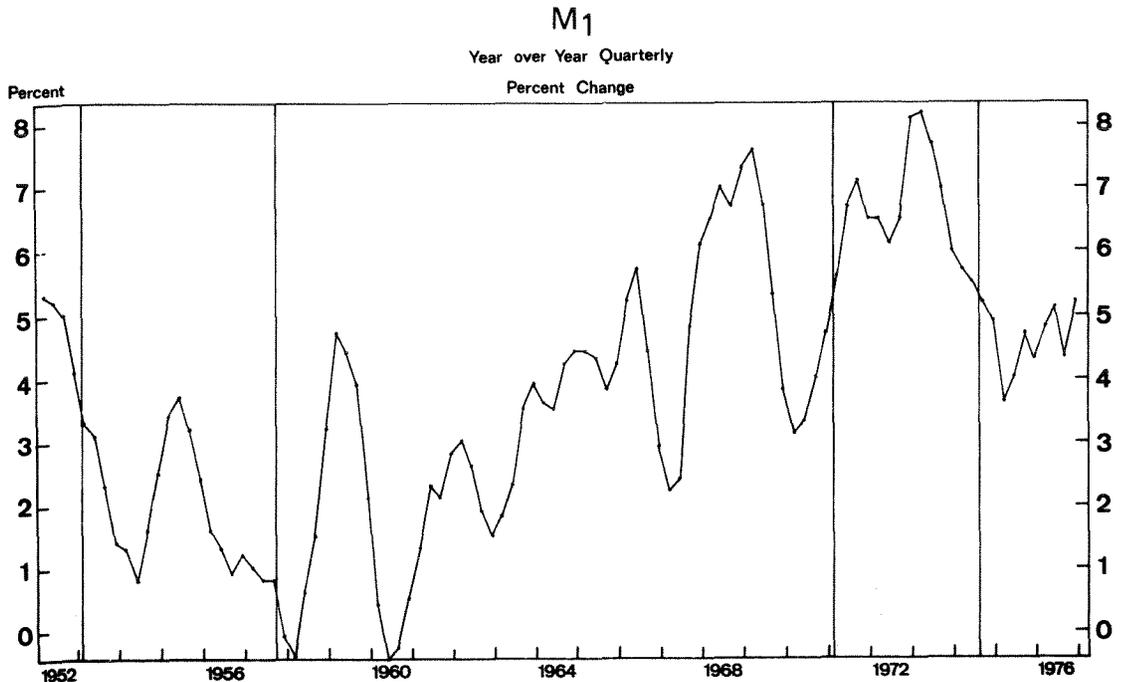
theories; but you know there is a tendency to carry them much too far and convert them into fads. So I agree with Franco on that subject. Indeed, if he had had a chance to read the later version of some of the material about the natural rate hypothesis, in chapter 12 of my price theory book, he would 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.

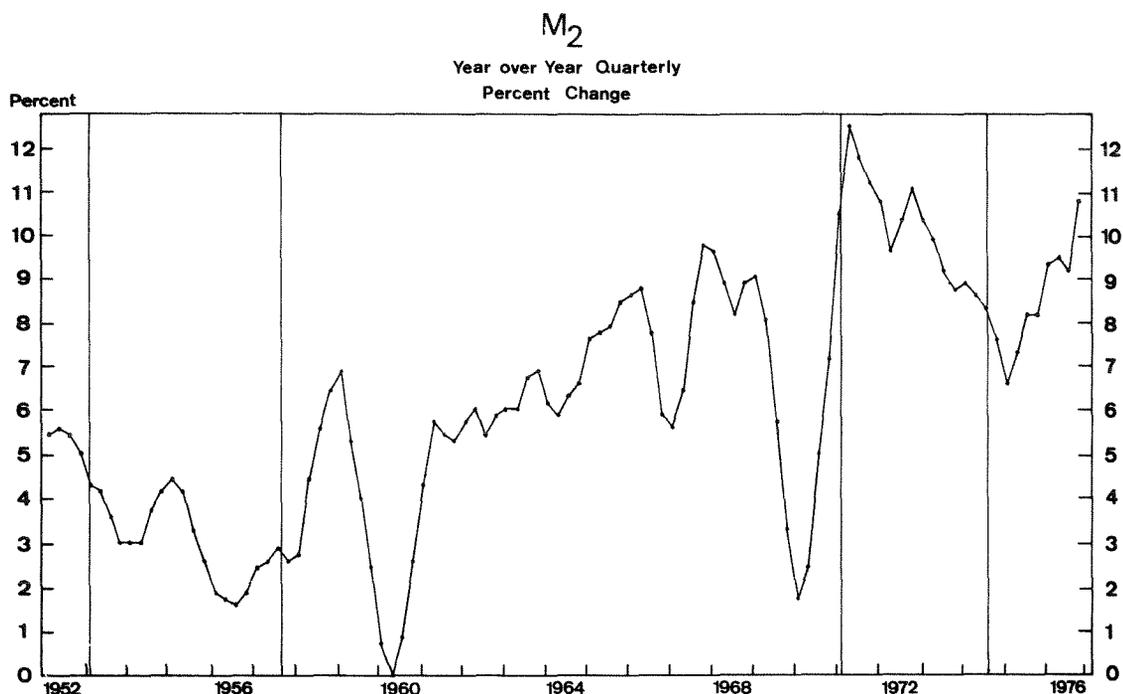
Friedman: Of course I am.

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



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 differ-

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.

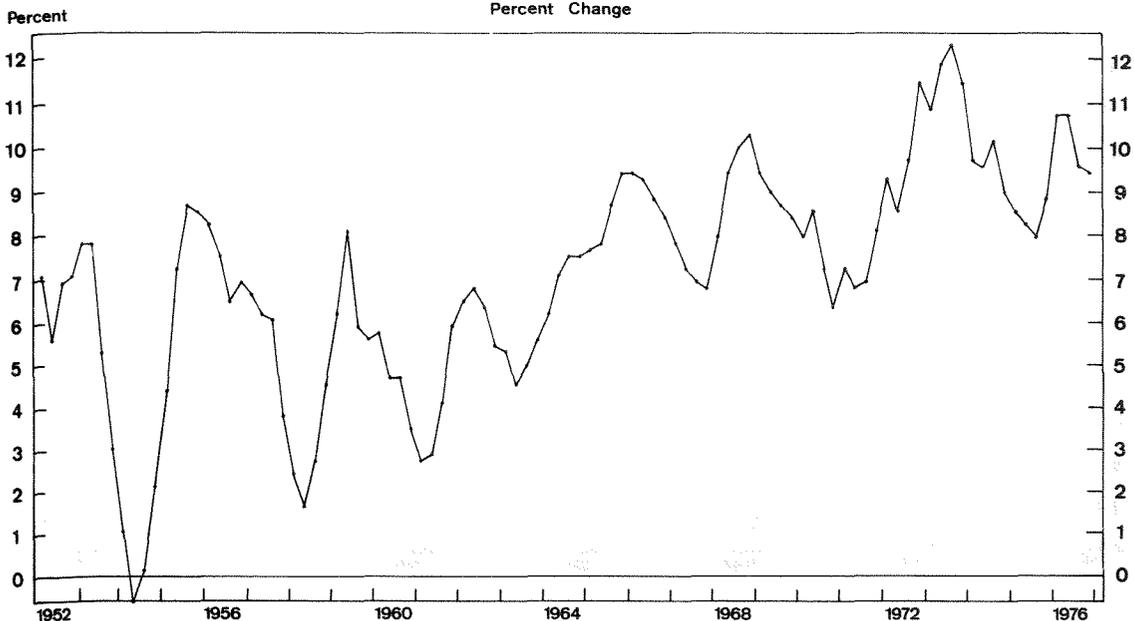
ence in the relationship between the movement of personal income and the movement of M_2 in the later part of the period and in the earlier part. The one striking difference is a much deeper recession in 1954 than you would have expected from the monetary change. That is a significant difference.

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

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 $3\frac{1}{2}$ 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 $3\frac{1}{2}$ 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 variation². 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.

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

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

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 pri-

marily 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 our-

selves 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 expecta-

tions 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.