

Summary and Evaluation

Robert M. Solow

The group at this conference is fairly uniform. The speakers are all academic economists, especially if you count Geof Moore and Steve McNees as honorary academic economists. A nonprofessional would find this whole meeting very mysterious. The discussion is very abstract; it is full of insiders' language; people break into hysterical laughter for incomprehensible reasons. There are also some people here who are more directly concerned with practical matters. There are even more such people out in the streets of Edgartown, and those are people who could not care less about rational expectations or even about irrational expectations or identifying restrictions, whatever those words mean.

Practical people have been led to believe, first, that economists knew all the answers, and now they seem to believe that economists know absolutely nothing or perhaps even know negative amounts about the determinants of inflation. I guess many practical people would like to know what the truth of the matter is, and whether economics offers any guidance out of what they perceive to be a mess. I would like to assure the practical people in this room and also the ones out in the streets of Edgartown that although the battles that are fought in conferences like this appear to be fought with antique pop guns, the bullets are real and they may soon be fired at you by the Federal Reserve.

I am supposed to give my impression of where this conference leaves us, and Bill Poole will, of course, say exactly the opposite in a few minutes. Naturally I begin with my opinions, and I have to confess that I haven't had any blinding revelations in the last two mornings; but I have learned some useful things.

What really brings us here is Steve McNees' picture of the 1960s and the 1970s. In opening the conference, Frank Morris mentioned his disappointment or disillusionment — which many others share — that the analytical success of the 1960s didn't survive that decade. I think we all knew, even back in the 1960s, that as Geof put it, "inflation doesn't wait for full employment." These days inflation doesn't even seem to care if full employment is going along on the trip. McNees documented the radical break between the 1960s and 1970s. The question is: what are the possible responses that economists and economics can make to those events?

One possible response is that of Professors Lucas and Sargent. They describe what happened in the 1970s in a very strong way with a polemical vocabulary reminiscent of Spiro Agnew. Let me quote some phrases that I culled from their

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paper: "wildly incorrect," "fundamentally flawed," "wreckage," "failure," "fatal," "of no value," "dire implications," "failure on a grand scale," "spectacular recent failure," "no hope." Now if they were doing that just to attract attention, for effect, so that people don't say "yes, dear, yes, dear," then I would really be on their side. Every orthodoxy, including my own, needs to have a kick in the pants frequently, to prevent it from getting self-indulgent, and applying very lax standards to itself. But I think that Professors Lucas and Sargent really seem to be serious in what they say, and in turn they have a proposal for constructive research that I find hard to talk about sympathetically. They call it equilibrium business cycle theory, and they say very firmly that it is based on two terribly important postulates — optimizing behavior and perpetual market clearing. When you read closely, they seem to regard the postulate of optimizing behavior as self-evident and the postulate of market-clearing behavior as essentially meaningless. I think they are too optimistic, since the one that they think is self-evident I regard as meaningless and the one that they think is meaningless, I regard as false. The assumption that everyone optimizes implies only weak and uninteresting consistency conditions on their behavior. Anything useful has to come from knowing what they optimize, and what constraints they perceive. Lucas and Sargent's casual assumptions have no special claim to attention. Even apart from all that, I share Franco Modigliani's view that the alarmism, the very strong language that I read to you, simply doesn't square with what in fact actually happened. If you give grades to all the standard models, some will get a B and some a B minus on occasion, especially for wage equations, but I don't see anything in that record that suggests suicide.

I also think that the Lucas-Sargent judgment is at variance with Geoffrey Moore's findings. I would not regard Geof's findings as very optimistic for received macro theory, either. What he reported is that the rate of inflation appears to accelerate when the unemployment rate falls in the upswings of growth cycles and to decelerate when the unemployment rate is rising. Franco pointed out yesterday, quite correctly, that this almost says that the rate of inflation is high when the unemployment rate is low, and low, when the unemployment rate is high. But it is not quite the same thing. If I can draw a diagram on a nonexistent blackboard for you, you can have a curve which moves through time up to the left, one measurement rising while the other is falling, and then comes back not right down the same curve, but at a slightly higher level, and then goes up again at a still slightly higher level and then comes back down again, once more at a higher level. It will always be true that x is rising while y is falling and y is rising while x is falling, but if you combine all those points you get a scatter that has essentially zero correlation as a whole. Clearly that sort of thing can and does happen. Geof Moore's paper is certainly a problem for Lucas and Sargent but it is not an unmitigated blessing for the traditional macroeconomic view. I say traditional macroeconomic view, not Keynesian view, because if Lucas and Sargent are right, then the St. Louis Fed is as dead as DRI, and you might as well realize that.

A second possibility is not to go so far as Lucas and Sargent in crying catastrophe, but to suppose that the underlying socio-economic structure has

changed. Of course it is always possible, and I believe that this is what Lucas and Sargent would do, to define the structure of the economy as what doesn't change. I think that tactic is futile because it asks more of economics than economics can ever possibly deliver. So I would suggest that another possible response to the events of the 1970s is to suppose that the doctrines of the 1960s were right for the 1960s, and that the situation has changed in the 1970s, so naturally the earlier theories tend to break down. But that would be much too easy, too relaxing. There is a very valuable and important point which is in very large part due to Lucas and Sargent, and one must give them credit for it, that what often looks casually like a change in structure is really the economic system reacting to its own past. It is possible that what happened between the 1960s and the 1970s is a kind of loss of virginity with respect to inflationary expectations. That doesn't mean that it cannot ever be regained. It may be that, if we could only get back to stable conditions for a while, the 1960s might come around again. Needless to say, I am not very confident about that. I also suspect that Lucas and Sargent have a good point about the game between the government and the private sector. You don't have to buy all or most of their whole apparatus to see that monetary policy has become a very peculiar animal when big money supply numbers are regarded by the financial press as deflationary news. And that is indeed a symptom of the game between the private economy and the stabilization effort of the government. In any event, I do want to mention this possible response to McNees' remarks precisely because it is not a popular thing to say; it seems to go against the science of economics. Physicists don't expect the velocity of light to change from one decade to the next, so why should underlying economic structures change from one decade to the next? But the economic world is not exactly like the physical world, and it is not a wholly unreasonable story to tell, that the theories and doctrines of the 1960s were right for the 1960s, only, as in the old television program, they were bound to self-destruct after some interval of time.

There is still another, even less cataclysmic, line of thought that one could take about recent events. Up until very recently, for historical reasons, macroeconomics had devoted almost all of its efforts to refining its understanding of the components of aggregate demand. Consumption functions were a dime a dozen, or ten a minute anyhow, investment demand equations were all over the place, and money demand equations were being estimated daily or hourly. Macroeconomics had utterly neglected to elaborate the supply side of the models. Not surprisingly, then, the sequence of supply shocks in the 1970s from the side of food, oil, nonfuel minerals, and the depreciation of the dollar caught the macroeconomics community by surprise. From this standpoint you would say that those bad points for the 1970s on Steve's graph are simply the track left by a series of supply shocks in a two-dimensional diagram that is ill-equipped to handle them. We know now that it is possible to rebuild the supply side of macro-models so that they do tell a consistent story and can explain the 1970s. That is what Lawrie Klein meant yesterday when he said that if he takes the current Wharton model and imposes a fiscal policy impulse, a demand side shock, he gets a track in the inflation-employment plane that slopes down to the

right, but that if he imposes a food or oil price increase, he gets a path that slopes up to the right. Ray Fair's model that we talked about this morning quite clearly does the same thing. So fast does the economics profession move now that there are already text books that do the supply side quite adequately and have no difficulty in explaining, at least to the intermediate student, how the bad points in Steve's graph can in fact be explained without any revolutionary change in the structure of the model. The fact that you can reconstruct macro-models by paying a little more attention to the supply side and get a reasonable account of the 1970s is certainly better news for macroeconomics than if that could not be done. But I do want to caution you that it is not very good news, because you can almost always patch up a model after the fact. The question really is whether it will hold up into the next decade when the next unexpected event comes along. I think it might, though I would not be inclined to oversell. Helliwell gave the Link models a grade of B. Mom and Dad won't jump for joy, but that is hardly a "spectacular failure."

I rather liked the paper by the Wachters though I also disagreed with a lot of specific things in it. What I mean is that I think they were trying to do the right thing: trying to model the world as we know it with the kinds of institutions that it happens to have. I do think that the paper suffers from a modern disease, which a lot of papers suffer from, a tendency to build too much on a very thin econometric basis. In a complicated nonexperimental statistical situation, there are almost always several hypotheses which fit the data approximately equally well. We have hardly any way of distinguishing confidently among them. Even those horse races that everyone talks about don't really do the discrimination job very well, again because of that ever-present possibility of patching up a model after the fact. The time-honored device in laboratory science for solving this problem is the controlled experiment — the critical experiment. We can't perform experiments; so it is only prudent to be very leery of claims based on one or two t-ratios or on small reductions in standard errors of estimate. The significance tests we use have very little power against the next best competing alternative and I fear we tend often to forget that. I am especially uncomfortable, with long polynomial lags. (I might as well confess this although I'll be probably read out of the Econometric Society.) They usually seem implausibly long to me — not always, but very often, whenever I have some feeling about what the lag ought to be. They seem implausibly long and they also seem too sensitive to minor assumptions to be very reliable. I would not care to be burned at the stake for the Wachter paper's conclusion that lags are shorter at high rates of inflation. I don't find that implausible at all; I just don't think that I would do a Joan of Arc on behalf of that kind of conclusion.

While I am confessing, I also worry a lot about U^* , the natural rate of unemployment. Here I guess I share a lot of Ray Fair's concerns. I even have trouble with the vertical long-run Phillips curve. I see its attractions very clearly, and I saw them at the very beginning. In fact, there is a peculiar inner conflict here. Deep down I really wish I could believe that Lucas and Sargent are right, because the one thing I know how to do well is equilibrium economics. The

trouble is I feel so embarrassed at saying things that I know are not true. The long-run vertical Phillips curve seems so inevitable. On the other hand, nobody believes the deflationary half of the proposition. I don't know anybody who would even lie out in the sun, let alone be burned at the stake, for the belief that if the unemployment rate is U^* plus epsilon and we wait long enough, there would be accelerating deflation. That part no one believes.

I even find it a bit hard to believe in the accelerating-inflation half of the story unless there is a good size zone of $5\frac{1}{2}$ or 6 percent unemployment in which the acceleration is very small and very irregular. Then I can believe it, but then I do not know what implication follows.

What is the value of U^* ? What unemployment rate should policy aim at? Should I believe $5\frac{1}{2}$ percent for now as Wachter tells me I should, or should I believe the 6 percent that Henry Wallich tells me I should, or should I believe the people who tell me that whatever the unemployment rate is today is the natural rate of unemployment for today? I can't believe an answer is to be found in search theory. I regard search notions as simply empirically discredited. People don't do it. Job search is simply not a major occupation of the unemployed.

I concentrate on this point because you have to have a very good reason for believing that the natural unemployment rate is $5\frac{1}{2}$ percent if you want to go out and face all those people who are unemployed. It is no joke. For statisticians it is just numbers, just something that comes out when you set something equal to zero and divide one number by another. But those fellows out there are not working. You ought to be sure of what you are talking about, and that the right figure is $5\frac{1}{2}$ percent and not $3\frac{1}{2}$ or $4\frac{1}{2}$ percent before you pretend that it has some relevance to practical life.

Ray Fair is absolutely right: you can't get a decent estimate of a natural rate of unemployment out of aggregative data, nor is that what Mike Wachter does. Most studies that attempt an estimate of U^* rely on a demographic decomposition of the labor force and of unemployment. I have never been comfortable with that. I can hardly think of any production function that specifies labor input in terms of so many women or so many men, so many 18- to 24-year olds and so many 25- to 29-year olds or anything of that sort. Presumably all this demography is proxying for skills or experience but surely it would be better, if that is what it is after, to demonstrate it directly and to talk about skilled and unskilled, and experienced and inexperienced workers rather than 18- to 24-year old males, 35- to 39-year olds females and so on. There might be something to be said for the demographic origin of U^* in terms of the youth culture, mobility, sampling jobs, frequent voluntary job changes and all that. Although there again I would feel a lot better if someone could demonstrate that those voluntary turnover rates were invariant to the kinds of jobs that people were turning to and from.

One last point on U^* : I want to emphasize that there is, for very good historical reasons, no evidence of the reversibility of the relationship between demography and U^* . We will know soon (eight or ten years) because Mike's data say that U^* will start falling pretty soon as the age structure of the popu-

lation changes. I am going to be very curious to find out if that is so. At the moment we have only seen a movement in one direction, not that anyone can help it, that is just the way the demography has bounced.

It is plain as the nose on my face that the labor market and many markets for produced goods do not clear in any meaningful sense. Professors Lucas and Sargent say after all there is no evidence that labor markets do not clear, just the unemployment survey. That seems to me to be evidence. Suppose an unemployed worker says to you "Yes, I would be glad to take a job like the one I have already proved I can do because I had it six months ago or three or four months ago. And I will be glad to work at exactly the same wage that is being paid to those exactly like myself who used to be working at that job and happen to be lucky enough still to be working at it." Then I'm inclined to label that a case of excess supply of labor and I'm not inclined to make up an elaborate story of search or misinformation or anything of the sort. By the way I find the misinformation story another gross implausibility. I would like to see direct evidence that the unemployed are more misinformed than the employed, as I presume would have to be the case if everybody is on his or her supply curve of employment. Similarly, if the Chrysler Motor Corporation tells me that it would be happy to make and sell 1000 more automobiles this week at the going price if only it could find buyers for them, I am inclined to believe they are telling me that price exceeds marginal cost, or even that marginal revenue exceeds marginal cost, and regard that as a case of excess supply of automobiles. Now you could ask, why do not prices and wages erode and crumble under those circumstances? Why doesn't the unemployed worker who told me "Yes, I would like to work, at the going wage, at the old job that my brother-in-law or my brother-in-law's brother-in-law is still holding", why doesn't that person offer to work at that job for less? Indeed why doesn't the employer try to encourage wage reduction? That doesn't happen either. Why does the Chrysler Corporation not cut the price? Those are questions that I think an adult person might spend a lifetime studying. They are important and serious questions, but the notion that the excess supply is not there strikes me as utterly implausible.

The story that Mike Wachter tells rests a little too much on what he calls "cognitive limitation" or bounded rationality. The fact is true. Even we in this room have cognitive limitations. Ordinary mortals are allowed. But I would not emphasize it so much. Much more important is the rest of the story, especially the bilateral monopoly situation, as I would describe it, which is protected by the value to both parties in the labor market of the continued relationship between them. That bilateral monopoly is not protected by ordinary market imperfections, by the sort of thing that the Sherman Act or the Clayton Act might outlaw, but it is protected by the value to both parties of continuing what they're doing. That relationship opens room for bargaining and simultaneously for a joint need to avoid conflict. Especially because neither party is monolithic. There are different interests on the employer's side and also on the employee's side even within the same trade union, as we know.

Another thing I would have emphasized more in the story that the Wachters tell is the asymmetry between upward and downward flexibility of wages. They

mention it at the end but don't elaborate on it, and I think it is very important. Given that protected bilateral monopoly, given the imperfection of the labor market and its willingness to tolerate nonclearing, partly for noneconomic reasons, there is an asymmetry between upward and downward flexibility in wages and in many other prices as well. Then it almost automatically follows that there is a kind of inflationary bias in the system because the only way the system can generate the relative price changes that it has to bring about is by having the general price level float upward. If, in addition, it should be true as I half think it is, that there are more shocks at high levels of output than at low levels of output, at least more upward shocks, there is already going to be a tendency for prices and wages to rise more rapidly in good years than in bad years. And if there are any long institutionally determined lags in the system, then it is going to be very hard to reverse those movements. I think you can tell a good story that way — especially if it is in fact true, as I casually think, that there is more flexibility on the upside than on the downside for wages and prices. If that story is true, then it suggests two very important roles for public policy. The first is simply to avoid major shocks and to move quickly to temper them when they happen, as they inevitably will, because the more major shocks there are, the faster and longer the price level will tend to rise. The second role for public policy is to remember that shocks can originate on the supply side as well as on the demand side, so supply management of one kind or another could be as important in the future as demand management has been in the past.

Summary and Evaluation

William Poole

I have been puzzling about the title of this conference — “After the Phillips Curve — Persistence of High Inflation and High Unemployment” — and its relationship to the papers we’ve heard. I’ve concluded that we ought to think about substituting a few words in that title.

One thought I had was to change “After” to “Because of.” There certainly is an element of truth in that revised title. Belief in a stable tradeoff between inflation and unemployment has had much to do with the persistence of excessively expansionary policies since 1965. But the most appropriate title is, “The Phillips Curve is Dead — Long Live the Phillips Curve.”

A major theme of the conference papers is the reconstruction of the Phillips curve. As the first element in this reconstruction, everyone decided some time ago that we have to add expectational variables to the Phillips curve. Expectations did not play a prominent role in early discussions of the Phillips curve, but now we know that empirically the Phillips relation must contain distributed lags on past prices or past wages, expectations of future prices, or some similar device. Empirically, you just have to get those past or future price change variables in there somehow. This is the reason that Geof Moore’s paper is written in terms of linking the unemployment rate to *changes* in inflation — the second derivative of the price level — rather than, as Phillips had it, the unemployment rate to the inflation rate, or the first derivative in the price level.

The basic idea behind the Phillips curve is that of a stable supply response traced out by demand shifts. One of the reconstructions that we spent some time on at this conference involves taking account of the fact that the supply side itself is being disturbed and so we have a problem of sorting out the supply disturbances from the demand disturbances.

There are several themes along that line. One, of course, is Moore’s emphasis on employment and not just on unemployment. We have the labor force participation and the demographic effects involved as well. But the emphasis on supply shocks in the way that Larry Klein brought them in — and there was also a simulation discussed by Ray Fair — leaves me quite uneasy. I was taught that prices are endogenous variables, and that we ought not to consider market experiment or to run model simulations based on moving prices exogenously to attempt to trace out supply effects. In a model that has a food supply function, I could understand a simulation experiment in which the supply function was moved to

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the left to represent a harvest failure or similar supply disturbance. But it is not satisfactory to play simulation games where we simply change one component of the total price structure exogenously and trace out the effects.

I was especially interested in a comment that Klein made about his experiment with OPEC price increases. He said these experiments were done on the basis of information as of last fall. Then he pointed out that in fact prices had not gone up and OPEC prices, it is thought, have in fact been shaded a bit. Why is that happening? Well, it is happening in good part because the cartel is finding price shading in its own interest for a variety of reasons and perhaps because the cartel is unable to put through price increases of \$2 a barrel, or \$4 a barrel, or anything else. This experience provides a good example of the invalidity of simulation experiments that move prices around exogenously. In a model properly specified to study this problem, the OPEC supply function would be shifted back and the effect on the average market price would depend on the model's equations for non-OPEC supply responses and on demand responses.

The other attempt to putting the Phillips curve story back together is the new theory of the unemployment-inflation correlation over the business cycle, or what we might as well call the "Lucas curve." I will discuss the Lucas curve later rather than now because it deserves a separate section in my outline.

Another important theme in this conference, which perhaps is not obvious until you look carefully, is that there is a growing interest in careful quantitative assessment of what models mean and what they do. John Helliwell emphasized this point in his discussion of Klein's paper; the model documentation is better and the record-keeping is better. Indeed, the situation is very much improved over the situation we were in five or eight years ago when it was difficult to know what large models meant, to know how to evaluate them, and to know how accurate they were. We have come a long way in this area, because model builders have been paying attention to these very important issues of documentation that physicists and chemists and so forth are brought up on but about which economists are frequently very sloppy. In the same vein, the work by Ray Fair in attempting to formalize the model accuracy question is extremely important for the scientific assessment of what we are doing. Steve McNees' work on forecasting accuracy is also in the same tradition and, if you will, is in fact helping to define the tradition.

If we look at the quantitative assessment of model forecasts, which is extremely important for the policy makers, where do we come out? Consider, for convenience, a forecast horizon of four quarters because that seems to be far enough out to be useful, and yet not beyond the capability of the models. Of course, 8- or 12-quarter forecasts would be very useful but since we do not have much information on the accuracy of such forecasts we can use four-quarter forecasts. As I read the McNees paper I would say that as long as we are talking about policies that are in the ball-park of the experience over which the models were fit, we might reasonably expect a standard error on the unemployment rate of 1 percentage point and a standard error on the inflation rate of about 2 percentage points. This degree of accuracy is a little but not a whole lot better than ARIMA forecasts over the same horizon.

Let me turn now to a general assessment of these Phillips curve fix-ups. Have we learned the right lessons from the experience of the seventies? I think it is fair to say that we can't tell yet. The models have been adjusted; they have gone through several generations. We know that there has been a big difference between the within-sample and post-sample performance of earlier versions of current models. But do we have it right now? I think we have good reason to be skeptical that we have it right now given the record. We certainly don't have any *evidence* that models are now right because we are still talking about incorporating the events of the last few years, and don't have any post-sample observations on which the models can be tested. If we look at this issue from the point of view of skeptical empirical social scientists, then we simply have to say that we don't have the evidence that we have it right now. If you believe that the models are now correct, or will provide forecasting accuracy that it is notably improved from the record of the seventies, then that belief can only be justified on the basis of faith and not evidence. The evidence is not here yet.

I am worried about the problem of the number of observations used to estimate these models. This issue shows up particularly clearly in Moore's paper in that he is looking at the relationships over a data base of only nine growth cycle peaks and nine growth cycle troughs in the postwar period. And the evidence Moore cites for lengthening lags comes from only two observations — the last two.

When we teach our statistics courses, we all warn our students that conclusions based on nine observations are unlikely to be very reliable. Certainly two observations are even less help. The problem of small sample size is not avoided in the large econometric models. Although most models use quarterly data, the bulk of the variance in the data comes from the business cycle fluctuations. The estimated properties of the equations of econometric models primarily reflect the same limited number of cyclical episodes as studied by Moore. In fact, model equations may reflect even fewer than nine business cycles because many models are estimated over shorter time periods than the whole postwar period.

So, on a statistical basis, we really don't have reason for confidence in our empirical work. This is one of the reasons why examination of foreign experience is extremely important. That is really the only way we have to expand our data base other than waiting. Expanding our data base is a very promising thing to do; we ought to be able to extract much more information from the behavior of other economies than we have done so far.

I'll now turn to Sargent-Lucas — the focal point of the controversy at this conference. What about their challenge? One thing that struck me as I was listening to Bob Solow, and some others, is that we have had so many comments to the effect that this work is overstated, exaggerated, and so forth that I suspect that there is a great sense of unease, even among those who are very opposed or very skeptical of the Sargent-Lucas work. All these comments about their work remind me of my freshman course in philosophy. The professor was going through one proof after another of the existence of God showing how all these proofs were flawed. One of the students said, "Well, after going through all this, doesn't the fact that there are so many proofs available show something?" Do all the comments critical of Sargent-Lucas show something? I think so. The

criticisms of Sargent-Lucas reflect model-defenders' efforts to maintain the intellectual case for large macromodels when everyone realizes that the models cannot possibly provide correct predictions of the effects of certain policies.

Let me put the message that I take from the Sargent-Lucas work in this way. First of all, there is no model builder in this room who would expect his model to hold up if we were to consider an experiment, let's say, of 100 percent rate of money growth in the next 12 months. No model builder expects his model to stand up in that kind of an experiment. Clearly the institutional structure in the model, the lag structure, and so forth, simply would fall apart. Now, what about 50 percent money growth? Or 25 percent money growth? As we go down to ranges that are closer to those that we are familiar with, and we have more confidence that we are within the ballpark of the historical range of observation, then we are more confident that the models can tell us something. But that is not the end of the story. In fact, there was a comment — I guess it was by Martin Baily about the Wachters' paper — to the effect that the changes in institutional structure that we have seen such as the growth of escalator clauses seemed rather minor. Indeed, we can mention a long list of apparently minor changes — things like the shorter contract periods we had during the controls period, the more frequent salary adjustments in nonunion situations and so forth. These things all seem relatively small. They don't involve major changes in the institutional structure, and it's hard to see how they make much difference.

But that is not the point, it seems to me. The point is — if you think about a limiting process — whether the changes in institutional structure in response to policy changes are large *compared to* the changes in forecasts of economic variables in response to policy adjustments within a fixed institutional structure. After all, none of us expect very big effects from policy experiments that involve a change in the annual rate of growth of money of 1 percent for six months. If we talk about 2 percentage points for six months, or 3 or 4, as we raise the policy dose, we expect larger policy effects. But, of course, we also expect larger changes in institutional structure.

So, it seems to me useful to think about a limiting process here. Starting from a large policy dose, it is not obvious that in reducing the policy dose the stability of the institutions after a point becomes great enough that we can ignore induced institutional change in forecasting the effects that we are likely to get from policy adjustments.

I was struck by a quote on this matter from the Wachter paper. "For any significant change in the inflation rate, the speed and magnitude of Phillips' relationship, (I think they mean *changes* in the Phillips' relationship) are more important than the short-run movements along the curve." That is a useful way of looking at this issue. Are we, when we change our policy instruments, changing the structure more rapidly than we are producing effects within the given structure, as suggested by the word "more" in the quote?

If we think about the Sargent-Lucas argument in this way, it seems to me that the changing structure point is much more significant than direct examination of institutional changes by themselves would suggest. When we compare

the early 1960s with the 1970s, we are talking about an inflation rate that has risen from about 1 1/2 percent to a trend rate of 6 – 7 percent currently. That is not an enormous change. Indeed, by historical standards in the United States and certainly by standards in other countries, it is not much change at all. Yet this modest acceleration in inflation has caused tremendous problems. I don't think we should be surprised if the institutional changes seem modest, because after all the institutional changes required to adjust to a change in the rate of inflation from 1 1/2 to 6 or 7 percent don't have to be very large. We just don't need a wholesale institutional revolution to cope with a small acceleration in the rate of inflation.

Well, where do I come out on all this? On the basis of forecasting evidence, we need to accept standard errors of perhaps 1 percentage point on the unemployment rate and 2 percentage points on the inflation rate with a four-quarter horizon. Perhaps the models will do better, but we don't have any evidence as yet that that will be the case. We all agree that the Lucas and Sargent criticisms are right in principle — that the world will not stay put under very large fluctuations in policy instruments. We have many disagreements as to how important that point is for policy adjustment in the ballpark of past experience.

We know a lot about the failures of existing models, failures perhaps not as serious as described by Lucas and Sargent who in their paper have used too many fighting words and not enough scientifically neutral words, but failures none the less. Clearly, the model builders have not been totally pleased with the performance of their models over the last five or ten years and are working hard to correct what they admit to be at least some modest failures.

If we are honest about what we do, I think that we have to say that we have amazingly little solidly verified information on which to base an activist stabilization policy. This view is not going to satisfy policy activists because they will rightly point out that even if we have large standard errors added to our forecasting equations — accepting for the sake of this discussion an optimal control framework — the right thing to do is nevertheless to vary the policy instruments in small continuous adjustments in response to changes in point estimates of goal variables. Continuous policy adjustment is still the right thing to do from the point of view of optimal control no matter how large the standard errors are.

But I am convinced that the optimal control framework for policy is fundamentally wrong from a political standpoint. Earlier in the conference, we talked about adjustment costs producing sluggish reaction in investment functions and so forth, and surely the same thing is true in spades in the political process. It does not make sense to emphasize adjustment costs in modeling private behavior and then to ignore the very same consideration in discussing policy. It is not cheap for the President to get small policy adjustments, or any policy adjustments through the Congress. The process is long and it involves threats to the President's credibility if it turns out that before the political process is complete the point estimates swing around and you want the policy instrument to jiggle a bit in the other direction instead. So, from the point of view of the political

process, policy adjustments based on control theory models are out of the question.

I am also concerned that continuous small policy adjustments based on the models we have run a serious problem of discrediting economists. It is difficult to sell the proposition that policy instruments were adjusted to take account of a 1/2 percentage point change in the forecast of unemployment and then we came out with unemployment changing by 1 or 2 percentage points just as a result of the normal standard error we know we have around the forecast. That does not make an economist look good (particularly if unemployment goes in the wrong direction, of course) because it sounds like double-talk justifying a policy failure. Even forgetting about the actual failures, the perceived failures of macro fine-tuning injure the credibility of economists for other matters where we have a lot more to say, such as on matters like the efficiency of the regulatory process. There are a lot of things we have to say on micro-efficiency and government organization and surely we are going to injure our credibility on those issues if we push too hard in the macro area where the evidence suggests that we really do not know all that much.