Commentary

Alan Blinder

I would like to begin by quoting Ben McCallum's words at an earlier conference:

> "My reaction to the paper. . . is one of great enthusiasm. What a discussant wants most in a paper, after all, is something with which he can wholeheartedly disagree. And for me the . . . paper is unusually rich in such items."
>
> —B.T. McCallum, March 1984

Actually, I don't disagree with everything in this paper. For example, neither Ben nor I like the gold standard. But, of the four main points I find in Ben's paper, I disagree with all.

They are:

- We should not be convinced by evidence showing that the recent disinflation is more or less in line with earlier Phillips curve estimates.
- Central banks lack credibility because, in their effort to cause unanticipated inflation, they wind up causing excessive anticipated inflation—for reasons outlined by Barro and Gordon.
- The Federal Reserve System does not want its policies to be credible.
- The Fed should get around these time-inconsistency/credibility problems by adopting and adhering to a fixed rule.

The evidence from the recent disinflation

By now, quite a few people have noticed that, given the unemployment we experienced, the recent disinflation in the U.S. was more or less in line with what earlier econometric estimates of Phillips curves suggested—in apparent contradiction to the credibility hypothesis.

To the studies cited by Ben, I would add a fascinating paper by Ando and Kennickell (1983) that shows not only that the equation in the current MPS model (which is little different from the equation estimated in 1973) tracks the last decade very well, but that even the version estimated by DeMenil and Enzler back in 1970 does not do all that badly.

I can also add the following personal observation.

For some years now, I have been using the simple rule of thumb that each point of unemployment, henceforth U, above 5.8 percent (my estimate of the natural rate) reduces p by 0.5 points. (This corresponds to "a sacrifice ratio" of 5-6.) Periodically during the disinflation of the past three years, I have checked the accuracy of this rule, and been constantly amazed by its accuracy.[1]

Using the four years from 1980 to 1983 as the disinflation period, the rule of thumb says that inflation should have fallen by 5.4 points between early 1980 and early 1984. No matter what price index you use, this is not far from the actual decline. If you then factor in the amazing climb of the dollar, it seems surprising that inflation has not declined further.

Yet somehow McCallum claims that "the evidence purporting to contradict . . . the credibility hypothesis . . . is unconvincing at best." Why? Because he estimates an old-fashioned Phillips curve—with no supply shock variables—over 1954-1982, and finds that the coefficients on lagged inflation are higher post-1966 than pre-1966.

I find McCallum's alleged evidence on credibility rather incredible.

The credibility hypothesis is a very *specific* application of the Lucas critique, which says that you will get more disinflationary bang for your unemployment buck if you pursue a tough anti-inflation policy. In terms of a theoretical expectations augmented Phillips curve,

$$\dot{p} = \text{const.} + E(\dot{p}) - aU,$$

it says that a gets bigger.

But Ben simply identifies the credibility hypothesis with the ***general*** Lucas critique and looks for any parameter shifts. Now, the one parameter shift that we all know took place—thanks largely to the annual Phillips curves estimated by Bob Gordon—is that the coefficients on lagged inflation (interpreted as a proxy for expected inflation) rise as you extend the sample beyond the late 1960s into the early 1970s, and then stop rising.

1. **A published example appeared in the Boston Globe on Feb. 9, 1982, under the title "Unemployment up means inflation down."**

McCallum finds this. But so what? He is testing for a shift of the wrong parameters in the wrong time period. He should be looking for changes in the U coefficients during the disinflation of the early 1980s.

What would such a test show? To find out, I ran some regressions of my own.

- First, I (approximately) replicated his equation 6 and then extended the sample one year—to 1983:IV. The differences were trivial.
- Then, following McCallum's procedure, I tested for shifts in the unemployment coefficients—starting the dummy in 1980:III, roughly when disinflation began.
- Results: The two dummy variables got roughly equal and opposite coefficients, each with a t statistic about 0.5 in absolute value. The F-statistic for the joint hypothesis that both were zero was $F = 0.16$.
- If we accept the point estimates at face value, the U coefficients in my version of McCallum's equation are

$$-.0006U(t) - .0011\Delta U(t)$$

until 1980:II and

$$-.0006U(t) - .0002\Delta U(t)$$

after. So the point estimates say that there was no change in the level effect and a large reduction in the ephemeral effect of rising unemployment.

- Next, I ran the equation only through 1980:II and looked at post-sample prediction errors.

Looking first at onequarter-ahead residuals, 9 of the 14 are negative (as the credibility hypothesis suggests). But that's not much more than 50-50, and none of them are larger than one standard error. The only large residuals are positive, making the average prediction error slightly positive.

Similarly, a 14-quarter dynamic simulation of the model leaves the price level only 0.9 percent too high by 1983:IV.

- Conclusion: If the right questions are asked, McCallum's specification gives the same answer as the others: The disinflation was just about what should have been expected, given the behavior of U.

This means either that credibility is not very important for the slope of the Phillips curve, or that the Fed did not gain credibility despite the deep recession.

But there is one pretty glaring fact that argues against the second interpretation.

Starting in October 1979, Chairman Volcker publicly and repeatedly identified inflation-fighting with money targeting. He then put us all through a small depression to lower inflation, all the while stressing the importance of controlling M growth. Then, in October 1982, he suddenly abandoned money targeting and let the M's soar, while pledging that this policy change did not mark abandonment of the battle against inflation.

If he lacked credibility, long-term nominal rates would have shot up. Instead, they fell, suggesting that Paul Volcker has both chutzpah and credibility.

Thus the evidence strongly suggests that the credibility hypothesis, sensible as it is, is not of great empirical importance.

Why central banks lack credibility

In the next section, Ben is very happy with the Barro-Gordon explanation for high inflation and low credibility. I am not. One set of objections is practical, the other theoretical.

On the practical level, I think we must seriously entertain the notion that many of the surprises in M are just as surprising to the Fed as they are to us, i.e., that they are not deliberate policy moves.[2] Short-run M surprises may be of little importance anyway. Mishkin's (1982) results suggest that they mean nothing special for output—and hence fail to reap the benefits assumed in the Barro-Gordon analysis. Furthermore, since we all know that M affects P with a long and variable lag, short-run money surprises mean virtually nothing for inflation.

If the Fed's actions are not the source of unanticipated inflation, maybe not even of unanticipated M, and if unanticipated M is not very important anyway, then the Barro-Gordon analysis may not be a good guide for practical policymaking.

On the theoretical level, the way Barro-Gordon handles reputation and credibility is—as they themselves admit—ad hoc. It is only one of many possibilities.

2. This idea rings true, and is similar to that of the Cuckierman and Meltzer paper that McCallum cites.

Davis Backus and John Driffill (1984) have ingeniously applied the theory of reputation due to Kreps and Wilson (1982) to the Barro-Gordon model, and reached rather different conclusions.

According to Backus and Driffill, lack of credibility stems from the fact that the public is not sure about how serious the government is about fighting inflation. The government tries to build an anti-inflation reputation by being tight-fisted, while the public learns in a Bayesian manner. (Does this sound familiar?)

As a result, they show, the government may well stick to a tough anti-inflation policy for many periods—especially early in its term.

Thus, even within the Barro-Gordon framework, the government may—for a long time—opt for zero inflation, not for the high inflation posited by Barro-Gordon.

Does the Fed want credibility? How can it get it?

Ben then constructs a revealed preference argument that the Fed does not wish to be credible.

His evidence is that the Fed:

- refuses to announce clear and explicit target paths for ultimate goal variables like p and $\dot{y}$.
- equivocates on how important control of M growth really is, and permits base drift when it redefines its 'cones."

I agree with Ben that the Fed's pronouncements do not "engender belief that the Fed is frankly conveying a clear notion of its goal and intentions." But I don't think this is because the Fed loves inflation or wishes to be disbelieved.

First of all, if velocity follows a random walk, then allowing long-run base drift is perfectly consistent with a long-run P level target. On the contrary, rigid adherence to a predetermined path for M would make P drift away from its target path.

More importantly, however, it seems to me that the reason the Fed refuses to announce its goals for $\dot{y}$ and p is because these goals place far more weight on low inflation, and far less weight on high employment, than the goals of the body politic. Since it is impolitic to fess up, the Fed sets up smokescreens—just as its professed conversion to monetarism in 1979 was a smokescreen for pushing interest rates up.

Notice that this interpretation of the Fed's fondness for baloney is the absolute opposite of McCallum's. In his view, the Fed dissembles because it is surreptitiously promoting inflation. In my view, it dissembles because it is surreptitiously promoting unemployment.

Should the Fed commit itself to a rule?

In his concluding section, Ben takes the optimality of a fixed rule for granted and suggests using a feedback rule for manipulating the monetary base as a way to keep nominal GNP on a preassigned path.

I'm not convinced—for several reasons.

- While a $\dot{Y}$ rule is no doubt better than an M rule, holding to a predetermined path for Y is a very unforgiving policy when there are supply shocks. If $\dot{Y}$ is fixed, then $\dot{y}$ must fall by as much as p rises. This seems suboptimal to me.
- Ben's main argument for preferring a rule to discretion amounts to a preference for far-sighted over short-sighted policies.

No doubt, far-sighted policies are better than short-sighted policies, and discretionary policy is sometimes myopic. But I don't think this is inevitable. For example, discretionary policy, not constitutional rules, has kept commercial development to a minimum in the Grand Tetons. And the same can be said for environmental policy in general.

Besides, given limited knowledge about how the economy works, I doubt that we can design a rule that we'll be happy to live with for a long time. So when to change the rule will always be a discretionary decision.

- This brings me to my last point.

Policy rules with feedback, computed in the Tinbergen-Theil framework, used to be thought of allegorically—as approximate descriptions of reasonable behavior, around which there would always be deviations. An optimal rule was not meant to be written into law and followed religiously; it was meant to give guidance to policymakers. Thus I always thought of a feedback rule as a stylized representation of discretionary policy.

The time-inconsistency literature has changed this perspective. Suggested feedback rules are now meant to be taken *literally*—as formulas that obviate the need for human intervention. McCallum clearly advocates a rule as a way to tie policymakers to the mast so that they cannot exercise discretion.

While I recognize that time inconsistency is a problem, and realize that to err is human, I am troubled by this new perspective. For I think it loses touch with reality, and thereby contributes to the growing irrelevance of economic research to economic policy.

As Jim Tobin (1982) put it on this platform two years ago: "Policy rules are a myth of economic theorists' simplified models. It is in practice impossible, politically [and] economically . . . to prescribe in advance for all contingencies the behavior of future presidents, legislators, and central bankers. It is . . . not credible that responsible officials will not react to the circumstances of the day as they and their constituents perceive them. It is in practice impossible to draw a line between responsive 'feedback' rules and discretion."

In a word, I fear that if academic economists insist on playing intellectual parlor games about how best to replace the Federal Reserve Board and the president by a Fortran statement, we will lose what little credibility we still have.

References

Albert Ando and Arthur Kennickell, "'Failure' of Keynesian Economics and 'Direct' Effects of Money Supply: A Fact or a Fiction," mimeo, University of Pennsylvania, March 1983.

David Backus and John Driffill, 'Inflation and Reputation: mimeo, Queen's University, February 1984.

George DeMenil and Jared Enzler, 'Prices and Wages in the FRB-MIT-Penn Econometric Model: in O. Eckstein, ed., ***The Econometrics of Price Determination,*** Board of Governors of the Federal Reserve System, 1971.

David Kreps and Robert Wilson, 'Reputation and Imperfect Information: ***Journal of Economic Theory,*** 1982.

B.T. McCallum, "Inventory Fluctuations and Macroenomic Analysis: A Comment: mimeo, March 1984.

Frederic Mishkin, 'Does Anticipated Monetary Policy Matter? An Econometric Investigation: ***Journalof Political Economy,*** February 1982.

James Tobin, discussion of a paper by Alan Blinder, in ***Monetary Policy in the 1980s,*** Federal Reserve Bank of Kansas City, 1982.