Commentary: How Should Central Banks Reduce Inflation?—Conceptual Issues

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It was a pleasure to study Mervyn King's wide-ranging and ambitious paper, which raises a number of very interesting issues, some of which I will comment upon in the following paragraphs. In calling Mervyn's paper "ambitious," what I mean is that it tackles two or three distinct conceptual problems, each of which is important enough that a solution would be regarded as a major accomplishment for an academic researcher. I am amazed that Mervyn has the time and energy to do all this work while keeping British monetary policy from going astray. But maybe it is the case that the Chancellor's attitude toward Bank of England independence has the effect of leaving Mervyn with more time for such activities.

Why no inflationary bias?

In any event, there are several points that I will try to address. The first of these concerns Mervyn's position regarding the necessary existence of an inflationary bias in nontransitional monetary policy-making[1] and the related notion of an inevitable tradeoff between flexibility and commitment. His conclusion for nontransitional (that is, within regime) periods is the same as the one that I have promoted in a pair of recent papers (McCallum, 1995a, 1996), namely, that there is no necessary inflation bias or flexibility vs. commitment tradeoff. This conclusion differs, of course, from the one that is prevalent in the dynamic inconsistency literature.[2] It is important to

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note, however, that our reasons for reaching this conclusion are quite different. In particular, Mervyn assumes that the central bank has no *desire* to have unemployment rates below (or output rates above) their natural-rate values. My argument, by contrast, retains the literature's usual assumption that central banks would like to keep unemployment below the natural rate but suggests that thoughtful central bankers recognize that attempting to do so would be counterproductive on average; consequently they adopt a committed or "rule-like" manner of conducting policy. Naturally I believe that my assumption is the more realistic of these. I believe, that is, that central bankers would in fact like to keep unemployment below the natural rate value[3] *if* they could do so with no fear of inflationary consequences being generated thereby. For example, I suggest that the Fed's policymakers would prefer for U.S. unemployment rates to average around 4.0–4.5 percent rather than 5.5–6.0 percent if this would entail no inflationary pressures. In other words, I contend that it is realistic to specify *k > 0* in an objective function of the form

$$(1) \qquad \omega(\pi_t - \pi^*)^2 + (1 - \omega) \, [y_t - (\overline{y}_t + k)]^2$$

where π is inflation, y is the (log of) output, $\overline{y}$ denotes the natural-rate value of y, π^* is the target inflation rate, and the weight ω satisfies $0 < \omega \leq 1$. The point is that the first term, not the second, reflects the central bank's concern for inflation, which is distinct from its concern for unemployment.

This issue is really quite important, in the following way. Suppose that in fact k > 0 as most of the analytical literature presumes. Then the validity or invalidity of numerous conclusions in this literature hinges on the correctness of my contention that central bankers can, if they choose, behave in a committed fashion. If my contention is correct, for example, striking recent results by Debelle and Fischer (1995) concerning monetary-fiscal interactions and by Svensson (1995, 1996) concerning inflation targeting are fundamentally misleading, because they are based on the presumption that noncommitted behavior prevails.[4] With respect to the evidence, my hypothesis provides—as, admittedly, does King's—an explanation for the

absence of empirical findings of a tradeoff between inflation variability and output growth variability, as mentioned by Fischer (1994), among others.

Before going on, I think I should take a minute to explain what I mean by committed or rule-like behavior on the part of a central bank. Of course I agree with John Taylor (1993a) that literally adhering to a specific numerical formula for setting instrument values is not presumed; everyone knows that central banks cannot reasonably be expected to do that. Thus, what I mean by rule-like behavior is that in selecting interest rate (or other instrument) settings month by month, the central bank is systematic in its responses to prevailing conditions and also forward-looking enough to abstain from attempts to exploit existing inflationary expectations. It takes account, that is, of the fact that the private sector's expectations will not be systematically incorrect—although there will be substantial expectational errors occurring at random—which implies that it is fruitless to try to regularly exploit any short-run Phillips relationship. This does not mean, it must be emphasized, that the central bank forgoes short-run stabilization actions in responding to shocks. In terms of the model in Mervyn's appendix, the central bank does respond to shocks like his ε_t but it acts as if $\pi_t - \hat{\pi}_t$ is unaffected by its choice of the period-t instrument setting.

Monetary policy rules

So, with that interpretation, let me go on to the subject of some specific patterns of rule-like behavior and for the sake of brevity refer to them as policy rules. The most prominent such rule today is, of course, the one described by John Taylor in his 1993 Carnegie-Rochester conference paper, whereby the central bank sets its nominal interest rate instrument so as to produce an expected real rate equal to the long-term average real rate plus upward adjustments whenever inflation and/or output are above their target and natural-rate values (or downward adjustments when either is below). With respect to this rule of Taylor's, Mervyn emphasizes that it is essential to distinguish between positive and normative interpretations or

uses of the formula. With that proposition I would agree heartily. But Mervyn goes on to suggest that, because inflation and output deviations are in his model proportional to current shocks, any choice of inflation target and response to shocks yields the same interest rates as implied by Taylor's rule.[5] To quote from his paper, "Hence it is impossible to distinguish between those central banks which are following a Taylor rule and those which are not." This statement is evidently supposed to apply to charts like those that we have seen in several publications recently, charts that compare actual and Taylor-rule paths of the federal funds rate or the clearing banks' base rate in the United Kingdom. Well, I do not understand this claim. Of course, actual interest rates rise and fall with expected inflation, as do Taylor-rule rates, so their values will be reasonably close together when actual inflation is reasonably close to the target value. But if you look at the data from the 1970s, before some important central banks decided that inflation was, in fact, dependent upon monetary policy, then the rates called for by Taylor's formula are not at all close to actual rates. From the start of 1974 continuously until late 1980, John's rule always calls for a federal funds rate at least 300 basis points higher than actual, with a discrepancy of over 1,000 basis points for a while in early 1980. And the difference was even greater in the United Kingdom, showing clearly that policy was not anything like that called for by his rule.[6]

   All of this is not to suggest that I would favor Taylor's rule over all others. It seems to me a rather good guide to policy behavior, one that would almost certainly have prevented the inflation of the 1970s if it had been followed. But as a policy guide, I continue to be attracted to the nominal income growth rule, with a base money instrument, that I have been promoting for the past decade (see McCallum, 1995b). In this regard, a diagnostic application to the British economy of my rule as well as John's is provided in the recent Bank of England *Quarterly Bulletin* paper cited by Mervyn (Stuart, 1996). I have some complaints about that paper—it states falsely that "both rules require knowledge of the output gap" (which mine does not) and its handling of trend output in my rule appears inappropriate—but nevertheless, the reported exercises show accurately that my rule calls for much tighter monetary policy in the

United Kingdom during the years 1988-1990, whereas Taylor's does not.[7] In retrospect, I think that most observers would agree that U.K. monetary policy was, in fact, too loose during those crucial pre-exchange-rate-mechanism (ERM) years.

There are two more things that I would like to say about my proposed rule before returning to our main topic. First, the fact that I express it in terms of a monetary base instrument does not imply that it could not be implemented with a funds rate instrument. My (1995b) paper for the Bank of Japan's Seventh International Conference includes a study (which Mervyn wrote a comment upon) in which I explore a procedure for adjusting a funds rate instrument from week to week so as to hit quarterly time paths for the monetary base that are specified by my rule. Interest rate variability would be somewhat greater than at present but not very much. Second, the fact that GDP data is only published quarterly, and is then revised, is not a serious problem. What the logic of my rule actually calls for is some measure of nominal aggregate spending. I am confident that an index based on the monthly consumer price index (CPI) and industrial production indexes, for example, would serve quite well in that capacity, and perhaps some such constructed measure would even be preferable in principle.[8]

Transitions between policy regimes

Now let me back up to the earlier portion of Mervyn King's paper that is concerned with transitions following changes in policy regimes—such as a shift to a new, lower inflation target. Here Mervyn argues that there will typically be output losses during disinflationary transitions, basically because it takes time for the private sector to *learn* that the central bank's inflation target has changed, and that a reasonably slow gradualist path will result in less cumulative output loss than a sharp, abrupt movement toward the lower inflation target. These conclusions he justifies by analysis with a simple analytical model in which aggregate supply behavior is represented by a familiar equation that relates output (relative to its natural-rate value) to the surprise component of current inflation. The aggregate demand function is also familiarly obtained by

solving the nominal interest rate out from log-linear IS and LM functions.[9] This is a very standard model, but for the purposes of this analysis I would contend that its reliance upon the aggregate supply relation (1) is unsatisfactory. The problem is not that there is anything particularly wrong with this specification,[10] but rather that there are many other models of aggregate supply behavior in the literature and many of them would yield different implications in this context. To emphasize the multiplicity of competing specifications, let me quote from a recent paper of mine on inflation targeting (McCallum, 1996, p. 17):

> It is not just that the economics profession does not have a well-tested quantitative model of the quarter-to-quarter dynamics, the situation is much worse than that: we do not even have any basic agreement about the qualitative nature of the mechanism. This point can be made by mentioning some of the leading theoretical categories, which include: real business cycle models, monetary misperception models, semi-classical price adjustment models, models with overlapping nominal contracts of the Taylor variety or of the Fischer variety, models with nominal contracts set as in the recent work of Fuhrer and Moore, NAIRU models, Lucas supply function models, MPS-style markup pricing models, and so on. Not only do we have all of these basic modeling approaches, but to be made operational, each of them has to be combined with some measure of capacity output—a step that itself involves competing approaches— and with several critical assumptions regarding the nature of different types of unobservable shocks and the time series processes generating them. Thus there are dozens or perhaps hundreds of competing specifications regarding the precise nature of the connection between monetary policy actions and their real short-term consequences. And there is little empirical basis for much narrowing of the range of contenders.

Of course, if all these models had similar implications for Mervyn's disinflation analysis, the multiplicity would not matter. But they definitely do not, a point that I will illustrate by reference

to a recent article by Larry Ball (1994), which uses a different model of aggregate supply. In Ball's analytical experiment, a disinflation is announced and is assumed to be perfectly credible by the private sector, which is like Mervyn's case (1). But whereas Mervyn's model implies that there will be no expected output loss regardless of the disinflation path, Ball's implies that output depends sensitively upon the path and, in fact, that a well-chosen rapid disinflation can generate an *increase* in cumulative output.[11] Thus the results of the experiment are drastically different, just because of the different assumptions regarding aggregate supply. The one used by Ball is not an obscure or crazy choice, moreover, but is essentially the same as the overlapping nominal contracts model developed by Taylor (1979, 1993b). I might mention that a version of Ball's result was published much earlier by Taylor (1983).

My purpose here is *not* to suggest that Ball's analysis is superior to Mervyn King's. To the contrary, I find the latter's conclusion more plausible. The point, rather, is that we do not possess adequate knowledge of wage-price-output dynamics to permit any well-founded conclusions to be developed regarding disinflationary transition periods, even if specifics of the public's learning process were agreed upon (which they certainly are not).

This lack of knowledge concerning wage-price-output dynamics plays a very important role, it should be emphasized, in my way of understanding the desirability of an inflation-targeting regime or one that attempts to keep constant the growth rate of nominal income. If we did understand accurately this dynamic process, it is hard to see why more short-run activism would not be justified, as is argued by some critics of these more inflation-focused regimes. That is one reason why it is important not to be seduced by striking results generated in the context of one particular model—*any* particular model.

Opportunistic policymaking

Finally, I would like to conclude with a few remarks on the topic of "opportunistic policymaking," which Mervyn discusses briefly.

To put it bluntly, I think there is a basic inconsistency that is marring much of the current discussion of this subject. What I have in mind is a failure to distinguish between regime design and the process of transition between regimes. Thus the actual analysis in the paper by Orphanides and Wilcox (1996) is entirely concerned with regime design, that is, with the analysis of what type of objective function would rationalize an "opportunistic" regime. That analysis is skillful and, in my opinion, makes an opportunistic regime—policy rule, if I may use the term—look very unattractive. But the title of the Orphanides-Wilcox paper refers to "disinflation," which should be thought of as a transition to a regime with lower inflation than was accepted previously. Now it seems to me that a kind of opportunistic approach to that type of transition might make some sense, at least on political grounds. If a painful disinflationary move to a new regime is to be undertaken, it may be sensible to wait for an "opportune" time to begin. But that is quite a different matter from an opportunistic way of behaving within an established regime. In any event, clear thinking about this subject, among others, requires a clear distinction between regime design and the transition between regimes.

I suspect that actual central bankers may be inclined to object to that distinction, for the reason that these categories are not clear in practice. But I will argue that at the level of conceptual thinking—to use the term in the title of Mervyn's paper—it is necessary to make this distinction. For we have a well-developed theory of expectation formation within an ongoing regime, but virtually no theory at all of expectational behavior during transitions between regimes.

Mervyn's paper does respect this distinction quite nicely; my few objections to his analysis stem from other small matters. All in all, I found his paper very stimulating. I was initially inclined to think that, from the perspective of British monetary policy, it is somewhat worrisome that four years after the end of the U.K.'s ERM regime, Mervyn should still be giving so much thought to transitional issues. But, of course, that thought is inappropriate, since Mervyn's topic was assigned to him and it is one that certainly involves disinflationary transitions.

Endnotes

[1]Here I am referring to a central bank that has considerable independence and is not constrained by any externally imposed rule or target, but is sensitive to public opinion.

[2]See, for example, Fischer (1994) or Persson and Tabellini (1994).

[3]This wording should not be taken to imply that the natural-rate value is constant through time.

[4]Svensson's (1996) paper in this volume suggests that price level targeting dominates inflation targeting, but his reasoning presumes—I believe unrealistically—that discretionary policymaking prevails.

[5]Throughout, my comments pertain to the version of King's paper that was presented at the symposium.

[6]When actual inflation is 10 percent, the term $0.5(\pi_t-2.0)$ itself contributes 400 basis points.

[7]In that regard, see also the useful recent study by Dicks (1996).

[8]A word should be added about the choice between what Svensson (1996) terms "target rules vs. instrument rules," both Taylor's rule and mine being examples of the latter. Svensson argues that "with new information about structual relationships ... a target rule implies automatic revisions of the reaction function." This argument involves, however, a basic misunderstanding of a central part of my thinking, and I'm sure John Taylor's, namely, that our simple instrument rules are intended to be robust to model specification. It would be, I believe, a major mistake to design a rule on the basis of any specific model because the profession does not know what the correct model is. My rule and Taylor's are designed to work *reasonably* well in a wide variety of models, and thus (perhaps) in reality. So no change in rule would be made in response to new information. Furthermore, with regard to language, I would argue that this particular terminology is undesirable since it blurs an important distinction. Thus a target is just that, a target. The word rule should be used only with reference to a formula or guide for setting instrument values.

[9]There is actually a misspecification in the demand function since real and nominal interest rates in period t differ by the expected value of Δp_{t+1}, not Δp_t, but there is no need to dwell upon that here. (Here p_t is the log of the price level in period t.) On the other hand, the specification is richer than the most standard one, because King's IS function includes a real-balance effect.

[10]Although it performs rather poorly in empirical work.

[11]See Ball (1994), pp. 285-6.

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