

Comment John B. Taylor

My comments focus mainly on the policy aspects of this paper by Martin Feldstein and James Stock. The authors provide us with a thorough analysis of nominal GDP targeting culminating in a specific policy rule for the Federal Open Market Committee (FOMC) to follow when conducting monetary policy. Actually there are two alternative policy rules discussed in the paper. One rule is extraordinarily complicated. It would have the FOMC respond to several lagged values of every variable the authors bring into the analysis. This rule is computed using linear quadratic control methods based on an estimated vector autoregression.

The second policy rule is a very simple feedback rule in which the growth rate of M2 is adjusted in response to the deviations of nominal GDP growth from a stated target. When nominal GDP growth exceeds the target, the growth of M2 is slowed by the FOMC. When nominal GDP growth falls below target, the growth of M2 speeds up. The authors favor the second simple rule over the more complicated rule. For example, they do not even write down the more complicated rule in the paper. Hence, most of my comments are directed to this simple rule.

I find several features of the Feldstein/Stock policy analysis and their proposals for monetary policy to be very attractive. First, monetary policy actions are discussed entirely within a modern policy rule framework. The paper shows how sophisticated econometric analysis can be brought into the policy evaluation process and at the same time incorporate the advantages of policy rules, including credibility and greater certainty about policy.

Second, the policy rule they propose is an example of a responsive rule that contrasts with constant growth rate rules for the money supply, as proposed by Milton Friedman. The authors provide considerable evidence that this responsiveness would improve economic performance. This more general notion of a policy rule is a common feature of modern macro research.

Third, the rule implicitly entails a flexible exchange-rate system. Monetary policy—as described by their policy rule—is not guided directly by exchange rates or events abroad. That this is likely to be preferred to a policy rule that incorporates exchange rates is a finding which appears to be emerging from several research efforts, including my own.

Fourth, in analyzing the performance properties of their proposed rule—that is, how the policy rule would affect macroeconomic performance—the authors do not stop with a point estimate of the reduction in volatility of this target variable (nominal GDP). They also report statistical confidence measures. For example, they estimate that there is an 85 percent chance that the

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simple rule would improve performance by reducing the variance of nominal GDP. This is a welcome innovation in policy evaluation research.

Fifth, the authors' method of looking for a simple rule that approximates a more complicated rule derived from optimal control is a good one. It is certainly essential that a rule be fairly simple if it is to be used in practice. While the authors' simulations indicate that their simple rule would work well in reducing the volatility of nominal GDP, it would be useful to formalize the approximation method. It might be possible to improve on the approximation and show how analogous approximation methods could be used in other applications. For example, in Taylor (1981) I used results from David Livesey (1980) to approximate the more complex rules I had computed in an earlier paper (Taylor 1979).

Despite these valuable features of the Feldstein/Stock paper, I have several concerns about the results, especially when viewed as something for the FOMC to use in practice. I have some suggestions for future research based on these concerns.

One concern is methodological. In evaluating the effects of policy, Feldstein and Stock do not use a structural model. For example, they neither take a position on a credit or money view of monetary policy, nor do they say whether a sticky-price or sticky-transactions view underlies the monetary transmission mechanism; further, they state no assumptions about international capital mobility, which in many countries figures as a key issue in exchange-rate policy. Perhaps it is asking too much to provide a policy model in areas where there is still so much controversy, but in my view, depending entirely on reduced-form correlations is worse than using some structural model, or certainly worse than using several alternative structural models. Instead, the authors use an estimated vector autoregression (VAR). They simply replace the equation for M2 in the VAR and see how the stochastic-dynamic properties of the VAR change through stochastic simulation.

An alternative approach is to develop a structural econometric framework. Technically speaking, my concern is with the Lucas critique—that the parameters of the VAR will change with policy. I do not mean this criticism to be destructive, for I think using *structural* models is an alternative approach that deals with the Lucas critique. The framework I use includes staggered contracts, perfect capital mobility, and an interest-rate view of the monetary transmission mechanism. Even if you do not like this particular model, there are many structural models with which to do the analysis. For example, Ralph Bryant, Peter Hooper, and Catherine Mann (1993) have used a number of econometric models to comparatively evaluate the performance of policy rules like the one suggested by the authors.

Are these technical concerns quantitatively important to the analysis? Consider two examples. First, the authors' simulations seem to show that with their optimal policy rule, inflation would have gone into double digits in the 1970s,

and the 1982 recession would have been worse than it was. See figure 1.4 of their paper. But would not a money rule such as that which the authors suggest have been able to avoid the great inflation and the subsequent great disinflation of the early 1980s? Perhaps the use of a reduced-form correlation explains this finding. Second, using the reduced forms may explain why the performance improvement is so small; the volatility of inflation is reduced by only 6.6 per cent during the past twenty years.

I am also concerned with the authors' stated goal of policy. I found that the paper focused too much on *nominal* GDP growth rather than its two components. Should not the criterion of performance relate more directly to how the economy performs in the two dimensions we care about: inflation and real GDP? What are the implications of the policy rule for the fluctuations in inflation and real GDP? I am also concerned about not using the *level* of nominal GDP in the evaluation. Feldstein and Stock discuss this, but it seems to me that a good policy allows a speedup in growth (above potential GDP growth) after a recession. In my view, the faster growth in the United States compared with that in Europe just after the 1982 recession is an example of a better policy.

My preference is to examine policy in terms of (1) the deviations of real GDP from an estimate of potential GDP and (2) the fluctuations in inflation. Robert Hall and Greg Mankiw in their paper in this volume (chap. 2) call such a rule a *hybrid* nominal-income rule. I proposed such a rule in my 1985 paper (Taylor 1985) and called it a *modified* nominal-income rule. It might even be better to consider a rule that looks at the deviation of the price level from a target as well. But the point is that if we are concerned about inflation and economic fluctuations, then it would be useful to examine these features directly.

Another concern is the complete focus on M2 as the policy instrument. One of the appealing features of nominal GDP targeting is that it automatically controls for velocity shifts. If you use M2 as an instrument, you bring velocity shocks right back in. One could have considered a rule with the federal funds rate as the instrument. In fact, one rule I have found attractive has the federal funds rate adjusted up if GDP goes above target or if inflation goes above target, and vice versa. This rule comes fairly close to the type of decision the Fed actually makes, so it may be a more plausible place to begin. It also appears as a preferred instrument in the Bryant, Hooper, and Mann (1993) review of policy evaluation using structural models.

The paper addresses the *design* of a policy rule, not its *operation*. However, it raises some operational questions. How would such a rule operate in the context of the FOMC as currently constructed? Should the Fed publicly state the rule and give an explanation to Congress whenever policy does not conform to the policy rule? To get things started, one possibility, at least in the short term, would be for the FOMC to have the Fed staff put in their briefing books the M2 growth forecasts implied by the rule. Then the FOMC could at

least discuss policy in the context of the rule. However, with our current state of knowledge, some alternative rules—including an interest rate rule—would probably need to be placed alongside the Feldstein/Stock rule.

References

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Comment Bennett T. McCallum

The Feldstein and Stock paper is a stimulating and constructive addition to the growing literature on nominal income targeting. It includes some nice technical innovations, such as the derivation of the distribution of $\hat{\rho}$, the estimator of a variance-reduction performance measure. And from a substantive policy perspective, the spirit of Feldstein and Stock's paper is in many ways similar to that of my own work,¹ so there is much in it that I would applaud. But there are also some important differences which deserve to be pointed out.

In discussing these differences I will focus on Feldstein and Stock's simplified policy rule (13) rather than their "optimal" rule of form (9). Because of its comparative simplicity, the former is considerably more attractive from a practical policy perspective, given that it performs nearly as well as the "optimal" rule in the one particular model in which the latter is (by construction) optimal. Since there is no professional agreement on the "true" model, the

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1. The main items are McCallum (1988, 1990a). The first of these proposes four "principles" to be kept in mind when specifying a monetary rule: (i) neither theory nor evidence points convincingly to any one of the many competing models of the dynamic interaction between nominal and real variables; (ii) output and growth levels will be essentially independent, over long spans of time, of the average rate of growth of nominal variables; (iii) a rule should specify settings of an instrument variable that the monetary authority can control directly and/or accurately; and (iv) a rule should not rely upon the absence of regulatory change and technical innovation in the payments and financial industries.