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Commentary

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I would like to start by saying how commendable I think it is that the Federal Reserve Bank of St. Louis is conducting this work on the monetary base. In that regard, it might be useful to describe one set of facts that illustrates how important it is to make adjustments for changes in reserve requirements. About a year ago, the IMF published a small study that I did with Monica Hargraves in which we tried to promote use of the growth rate of the monetary base minus predicted growth in the permanent component of base velocity as an indicator of the medium-term stance of monetary policy.¹ We compiled such measures for all the G-7 economies going back to 1960 and argued that this measure provided a better description than contending indicators of the relative “looseness” or “tightness” of economic policy in these countries over time. In doing this, we had to make adjustments to the basic base series of currency plus reserves. That came as no surprise to me, as I had previously found a need for such adjustments in my 1993 study of Japanese monetary policy. But even so, I was surprised by the extreme nature of this need in the case of France in the 1970s.

The IMF publishes a measure of the unadjusted base that it calls Reserve Money (line 14 in International Financial Statistics). The value of this reserve money at the end of 1974 was 152.3 billion French francs (FF), and at the end of 1975 it was 119.4 billion, which is a drop of well over 20 percent. On the face of it, this change would seem to indicate a monetary tightening of truly epic proportions, but there was no sign of any such tightening in the statistics for nominal GDP, the price level, or

even monetary aggregates such as M1 or M2. The reason, we discovered, was that reserve requirements on sight deposits were decreased over the year from 17 percent to 2 percent, which “freed up” about 44 billion of reserves. When we took account of that decrease by developing our series for an adjusted monetary base, the data made very good sense. Our adjustments were very simple compared to what is done by the St. Louis Fed, but they seemed to work reasonably well. But if more nations had well-developed and published magnitudes for the adjusted monetary base, it would be much easier for us, as researchers, to do our work. Also, I suspect that the value of the monetary base as a measure of monetary policy actions would be more widely recognized.

I didn't originally intend to say more about the McCallum-Hargraves study, but Don Kohn's rather strong comments concerning the weakness of the base as an indicator variable have prompted me to add a few more sentences, because our results were much more encouraging in that regard. I think he would agree that there have been some significant mistakes in monetary policy in the G-7 economies since 1960, so I would like to ask Don, first, to specify in which years policy was too tight and in which years it was too loose for each of these economies—i.e., applying his judgment in retrospect to the information that we have now. Second, I would ask him to specify his favorite policy indicator, which might be a fixed-weight index of several macroeconomic variables. And I will conjecture that our velocity-adjusted monetary base growth rate, which can be calculated on a real-time basis, will have done a better job than his favorite indicator in signaling past policy errors as he, himself, judges them.

DUEKER-SERLETIS STUDY: A VALUABLE STARTING POINT

The study by Dueker and Serlitis performs a valuable service by exploring the

¹ McCallum and Hargraves (1995).

effects on relationships and other studies of the difference between the new and old measures of the St. Louis adjusted monetary base. There will be and should be more such studies, but these authors have made a good start.

I am especially interested in the first section of the article, since it concerns the policy rule for controlling the monetary base so as to provide smooth non-inflationary growth on nominal GDP—a rule that I have been promoting for several years. In my own studies² I have tried to determine how the rule would have performed historically in the face of estimated shocks to the macroeconomy of the United States and some other countries, and the question of how the revision in base statistics would affect those studies seems highly relevant and quite interesting. The same could be said for the Judd and Motley (1991, 1992) extensions of my work, which feature stochastic rather than historical simulations, and also the studies by Dueker (1993) in which he examined the rule's robustness with respect to stochastic time variation in the parameters of a base-velocity model (which is what he uses as a macro model).

This is not quite what is done in the Dueker-Serletis study, however. Instead, the authors have backed out estimates of what implicit nominal GDP growth targets would have been, given observed base growth rates, if these had been generated by a policy process that was using a rule somewhat like mine to target GDP growth. They conduct this exercise using both the old and new adjusted base measures, and they conclude that the match between the implied target GDP growth rates and actual rates since 1988 is closer with the new base measure.

I am frankly not sure what to make of that, given that their rule actually differs from mine in two significant respects (which I will discuss further on in this presentation) and also given that their analytical framework involves an assumption that the Fed has had nominal GDP targets that have switched between two target growth values in response to some unobserved state variables. In fact, I would have to say that the authors do not give

enough description of their setup to make it easily accessible to the reader without study of some other works. What they have done constitutes a sophisticated and interesting exercise, but at its end I still don't know whether the new base series gives results more encouraging or less encouraging for the properties of my proposed rule.

The two ways that the version of the rule studied by Dueker and Serletis differs from mine are that (1) they include no feedback term to provide adjustments in response to target misses, and (2) they use a procedure for estimating future velocity growth that is unlike my use of average velocity growth over the most recent four years. With respect to the latter, their implicit objective, taken from Dueker and Fischer (1996), is different: Their velocity growth term is designed as a predictor of the next quarter's velocity growth. Mine, by contrast, is supposed to be a predictor of average velocity growth over the indefinite future. The point is that, in my rule, a deliberate attempt is made to keep responses to cyclical conditions quite separate from responses to slower-moving (and quasi-permanent) institutional changes. By contrast, a good predictor of velocity growth over the next quarter will take account of cyclical conditions. I would guess that it is this feature of the Dueker-Serletis velocity term that leads the authors to find the target-miss feedback term unimportant.

Another point that should be mentioned is that for several years I have been favoring a version of my rule that emphasizes GNP growth rate targets, rather than growing level targets, thereby treating past target misses as bygone.

In the second part of their article, the authors investigate effects on several results previously obtained from vector autoregression (VAR) studies. They nicely document that most of these results are qualitatively unaffected by replacing the old base and reserve measures with the new ones. I find it a bit puzzling that they devote so much of their discussion to an attempt to unravel some of the substantive puzzles thrown up by the VAR studies (under both new and old measures), for that seems like a different

² Earlier studies are referred to in McCallum (1993) and (1995).

agenda from the one their paper is designed to address. Also, it is, in my opinion, a bit unfortunate that they devoted so much of their attention to these VAR studies, because, in the process, they implied that the studies' innovations in either the fed funds rate or some reserve variable is a good measure of monetary policy actions. My objection to that assumption is this: Do they (or other researchers) believe that only the surprise component of the Fed's actions is relevant? For real output, that is an interesting hypothesis, but it is one that would certainly not be accepted by most practical policymakers and, with respect to price-level movements, it would be a downright bizarre hypothesis. But if output or prices respond to the anticipated component of policy-instrument movements, then the impulse response function and variance decomposition measures (of the type found in the VAR literature) are not measures of policy effects, but only one component of those effects. Furthermore, I would think that the regular and predictable component of movements in the Fed's instrument is much greater than that of the unpredictable surprise component. If my assumption is correct, then these studies, as a class, leave out most of what they are supposed to be studying. Thus it would seem that the portion of the Dueker-Serletis article that is devoted to this type of study is a bit excessive.

The third section of the article is concerned with the recent Haslag-Hein (1995) study, which suggests that macroeconomic variables respond differently to movements in the unadjusted base than to movements in the reserve adjustment magnitude (RAM). The authors recognize, but do not emphasize, that the Haslag-Hein findings implicitly constitute an attack on the usefulness of the adjusted St. Louis monetary base, since that variable is designed to provide a single summary index of the net effect of Fed policy actions via open market operations, discount-window lending, and reserve-requirement changes. In this regard, one might say that the St. Louis Fed's Research Department deserves praise for being willing to publish work that is so damaging to one of its own most famous products. I, myself,

would find their results very discouraging, since I am inclined to think of the base as a good summary measure, except that the results in question come from VAR studies that are open to the objections that I developed above. Also, the results are open to the objection that the identifying assumptions used by Haslag and Hein are not terribly compelling.

In conclusion, let me express the hope that these comments do not obscure the fact that the Dueker-Serletis study is skillfully executed and serves a valuable function.

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