Reflections on Monetary Policy in the Open Economy: Comment*

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There are many reasons why these "Reflections" can be highly recommended to both academic and policy-minded readers, and in this note I briefly comment on some of them.

The best way to approach this paper is perhaps to start from its final section on the real-time forward-looking Taylor rule. The first chart in this section (Figure A) plots the time series of the Federal funds target rate against a standard Taylor rule applied to U.S. data in the 21st century. What transpires from the visual inspection of the two series is that the basic Taylor rule does not seem to provide a good description of the Greenspan-Bernanke years. Clearly, in the last two occasions the U.S. monetary stance was eased this happened at a substantially faster pace than required by the Taylor rule, and the tightening since 2004 started at a much later time than called for by the Taylor rule. Of course, the graph has little to say on whether or not U.S. policymaking has been 'behind the curve' in recent years, as the basic Taylor rule makes no claim whatsoever to provide a normative benchmark

*The views expressed here are those of the author, and do not necessarily reflect the position of the Federal Reserve Bank of New York, the Federal Reserve System, or any other institution with which the author is affiliated.

for optimal monetary policy. The only message of Figure A is that the Taylor rule yields an ineffective description of recent U.S. monetary policy.

For a relatively new, and more informative way to look at policy rules, the paper suggests to rely on data from the inflation-indexed bonds markets. In fact, at the very core of the paper is the suggestion that the empirical fit of the U.S. funds rate can be dramatically improved by adjusting the Taylor rule with a forward-looking measure of the inflation gap based on TIPS breakeven rates, with a time-varying measure of the natural real interest rate based on real yields in the inflation-indexed bonds markets, as well as with a measure of prospective output gap based on expected unemployment data.

This is a successful strategy. TIPS data provide high-frequency information on market conditions and expectations, and this is why they are widely used in actual monetary policy analysis. Needless to say, breakeven rates do not provide an ideal indicator of anticipated price dynamics for a series of reasons, mostly related to the difficulty of disentangling movements in risk premia from actual variations in inflation expectations. The approach considered in the paper is simply to assume that risk premia are constant over time. This strategy has obvious analytical advantages, but is perhaps too simplistic. At any rate, visual comparison of Figures A and B reveals that it is possible to obtain a dramatic improvement in the positive description of the Fed funds rate behavior (and similar considerations are valid to some extent in the European case as well).

Why do we do so much better?

The author's answer is because time-varying real rates are driven by global factors. In his own words: "The neutral real interest rate and potential output cannot be defined, modeled, or proxied without reference to an explicit global framework... Bernanke himself has attributed the fall in real interest rates to a 'global saving glut'. This indicates that the Fed takes the global

influence on U.S. neutral real interest rates seriously. Greenspan during his tenure alluded to the 'conundrum', a situation in which the Fed's influence over long term interest rates is much diminished compared to previous periods, a phenomenon which has been attributed to the globalization of the financial markets in a world of (explicit or implicit) inflation targeting."

This is insightful and, I believe, definitely on the right track. However, not everybody agrees. For instance, there may be factors different from global interdependencies and spillovers that affect the neutral rate (such as movements in risk and term premiums, or domestic cycles and bubbles in the housing and other financial markets). So we need more than a simple graphical analysis. We need an interpretive framework to elucidate the links between openness and macroeconomic fundamentals. We need a choice-theoretic normative approach to the design of optimal policy in an open-economy context. And we need statistical evidence to discriminate among alternative stories.

As a first pass in these directions, the paper provides a model of optimal monetary policy in an open economy. Does the model succeed in shedding light on how global spillovers affect the design and conduct of domestic monetary policy? The answer is mixed. The paper shows convincingly that the appropriate monetary stance for an open economy requires responding to changes in the time-varying real (natural) rate and forward-looking inflation gap, consistent with the aforementioned graphical analysis. But the model is less useful when it attempts to characterize the international mechanism of shock transmission, and its implications for monetary rules.

The theoretical framework is a variant of the highly regarded Clarida, Gali and Gertler (2002) model, in which optimal monetary policy is described by a Taylor-style rule such as:

$$r_t = \overline{rr}_t + \theta E_t \pi_{t+1} \tag{1}$$

where $\overline{rr}_t$ is the domestic natural real interest rate, r_t is the nominal short-term rate, $E_t\pi_{t+1}$ is expected domestic inflation and θ a parameter greater than one. Different from the standard Taylor rule, the above formula provides a stylized normative benchmark for optimal policy. In fact, it maximizes a quadratic approximation to the social welfare function expressed in terms of inflation and output gap. Incidentally, one may wonder whether this traditional optimal-control approach (based on a linear quadratic certainty-equivalence framework) provides the appropriate way to rationalize and explain the Greenspan-Bernanke years, or whether an alternative robust-control analytical apparatus (with emphasis on risk management and 'insurance' against tail risks and worst-case scenarios) should be considered instead. But this is material for future ISOM papers.

The similarity between the theoretical optimal rule (1) and the adjusted Taylor rule of Figure B in the paper is striking. One would expect there is much more to be learned from this formula. For instance, the model suggests specific values for the coefficient of the inflation gap θ, a function of structural parameters including the degree of openness. However, in the chart this value is set to 1.5 (the basic Taylor value), leaving open the question of what would happen if one took seriously the theoretical prescription and suitably calibrate the empirical rule according to (1). Also, the model suggests a specific value for the coefficient of the output gap (that is, zero!). In Figure B, instead, the weight on the output gap is 1. Again, one is left curious on whether or not it would still be possible to explain the time-series behavior of the Fed funds rate with a lower output gap weight, much closer to the theoretical prescription.

Next, look at $\overline{rr}_t$. The model provides a well-defined link between natural rate and domestic and global output levels. But this information is not exploited further in the paper. In particular, it would be interesting to test

whether the relationship between TIPS real rates and measures of global output are consistent with the theoretical framework. By the same token, the model provides a well-defined notion of potential output as a function of world GDP, to be used in the definition of output gap. But the empirical section of the paper overlooks this result. Figure B uses a measure of potential output based on a transformation of the domestic unemployment rate, without incorporating global spillovers.

The paper strongly hints that one should not attempt to follow the theoretical prescription too closely, since what matters is the spirit of the model, and not quite its letter. In fact, according to the author, "the main points presented in this paper are robust to the illustrative model reviewed here, and in particular such key inputs to monetary policy as the neutral real interest rate and potential output cannot be defined, modeled, or proxied without reference to an explicit global framework". I tend to agree, although this claim needs to be taken with some caution. It is true that global variables are bound to have a significant impact on the "key inputs to monetary policy". But it is also true that the theoretical results are less robust than suggested, as relatively minor variants of the model can lead to very different predictions for the sign and the size of the macroeconomic spillovers.

To make my point in the simplest possible way, consider one of the key propositions in the paper regarding the relation between foreign output and the natural rate of domestic output in the open economy. When the elasticity of intertemporal substitution is realistically low (or $\sigma > 1$ in the notation of the paper), a rise in foreign output uncorrelated with domestic productivity *lowers* home natural output.

With reference to the algebra of the paper, recall how do we get to this result. First, consumption depends on global output:

$$C \propto Y^{1-\gamma} Y^{*\gamma} \qquad\qquad 0 < \gamma < 1 \tag{2}$$

Second, marginal costs (in real terms) depend on real wage and terms of trade:

$$MC \propto \frac{W}{P} S^{\gamma} \tag{3}$$

Third, the terms of trade are a function of relative output:

$$S = \frac{Y}{Y^*} \tag{4}$$

Fourth, the real wage (that is, the relative price of leisure in terms of consumption) is a markup over the marginal rate of substitution (the ratio of marginal disutility of labor to marginal utility of consumption):

$$\frac{W}{P} = markup \ * \ \frac{-V_N}{V_C} \tag{5}$$

The paper assumes additive separability in preferences:

$$V = \frac{C^{1-\sigma}}{1-\sigma} - \frac{N^{1+\phi}}{1+\phi} \tag{6}$$

so that:

$$\frac{W}{P} \propto Y^{\phi} C^{\sigma} \tag{7}$$

Putting together the pieces, we get:

$$MC \propto Y^{\sigma(1-\gamma)+\gamma+\phi} Y^{*\sigma\gamma-\gamma} \tag{8}$$

Finally, the real marginal cost in the flexible-price equilibrium is a constant:

$$\overline{MC} = const \tag{9}$$

From the previous two equations, home natural output $\overline{Y}$ (i.e., the flexible-price equilibrium level of potential output) is a negative function of foreign output Y^* as long as $\sigma > 1$.

In a nutshell: home output and marginal costs are positively related. Foreign output expansions lower marginal costs (improving home terms of

trade) by $-\gamma$. But foreign output expansions also increase marginal costs (by increasing consumption, which reduces the marginal utility of consumption, which raises real wages) by $\sigma\gamma$. If $\sigma > 1$ the second effect prevails. In the flex-price equilibrium with unchanged real marginal costs, foreign growth lowers home potential output, raising the neutral domestic real interest rate.

Now, consider a small modification of the model and assume that preferences are not additively separable but rather follow the specification suggested by Greenwood, Herkowitz, and Huffman (1988) (which, incidentally, has desirable properties for modelling open economies as it helps generating relatively high volatility in consumption and counter-cyclical trade balances, consistent with empirical stylized facts):

$$V = \frac{1}{1-\sigma}\left(C - \frac{N^{1+\phi}}{1+\phi}\right)^{1-\sigma} \tag{10}$$

Based on this specification, the equations for the real wage and the real marginal cost are now modified as follows:

$$\frac{W}{P} \propto Y^{\phi} \tag{11}$$

and

$$MC \propto Y^{\gamma+\phi}Y^{*-\gamma} \tag{12}$$

The implication is that, regardless of the size of σ, foreign output expansions always lower domestic marginal costs through the terms of trade effect. So, in the flex-price equilibrium, foreign growth always *increases* home natural output, exactly the *opposite* result of what was predicted by the model of the paper. This sensitivity analysis warns against the supposed robustness and generality of the paper's conclusions. The simplest modification in the parameterization of utility turns out to change the predicted sign of the relation between key macrovariables, even though all relevant elasticities remain the same!

This is not the only reason why one can be a bit skeptical about the consistency of the model of Sections 2-5 with the analysis of Section 6. Quoting once again from the main text (my italics), "according to this analysis, *variations in the neutral real interest rate, perhaps due to the 'global saving glut' and enhanced financial integration* in a world of inflation targeting central banks, *have played an important role* in Fed policy this decade".

The problem here is that the model assumes Cobb-Douglas consumption indexes, that is, a unit elasticity of intratemporal substitution between home and foreign goods. This is not an innocuous assumption. Let's be clear about this point: Cobb-Douglas indexes are a wonderful modeling tool when one wants to focus on the effects of relative price changes. Actually, relative prices play such a key role with Cobb-Douglas consumption baskets that they make redundant any other mechanism of adjustment in the asset market. In fact, as Cole and Obstfeld (1991) have emphasized, in a Cobb-Douglas global economy movements of the terms of trade are sufficient to generate full risk-sharing worldwide even in a regime of financial autarky, that is, even though no asset is internationally traded. So, in the world economy described by the model there is absolutely no difference between balanced trade and complete financial markets. Financial globalization is, quite simply, totally irrelevant. Perhaps, this is not the most useful theoretical framework to shed light on the effects of financial integration on resource allocation and monetary policy...

To make the same point in a slightly different way: in the model of the paper the current account is always balanced. This implies that national saving equals national investment under Cobb-Douglas indexes. But there is no capital accumulation in the model. Therefore investment is always zero. Therefore saving is zero as well. Perhaps, this is not the most appropriate theoretical framework to analyze the effects of a global saving glut...

In conclusion, there seems to be a bit of 'decoupling' between what the

model actually says and what the empirical analysis suggests. The challenge for future research that will build on the insights of this paper is to bridge the gap between these two parts. What remains undisputedly true is that, as the author succinctly puts it, "policy makers ignore open economy influences at their peril". And no one interested in the current complexities of monetary policy can afford to overlook this message.

References

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