Referee Report on <u>The Exchange Rate Targeting of Central Banks Revised: The Role of Long-term Interest Rates.</u>

The paper revisits a classic problem in international monetary theory namely whether central banks should target exchange rates. The authors take a New Keynesian macro model in which they substitute short term for long term interest rates in the Euler equation. This substitution is not innocuous since it implies that aggregate demand now depends on long term interest rates rather than short term rates. The flipside of this is that the central bank has more difficulty controlling aggregate demand given that it is assumed to control short term interest rates.

The authors calibrate their model to euro area data and then simulate their model using various simple interest rate rules. They find that the central bank can achieve the minimum of its loss function – consisting of an aggregate of inflation and output volatility – if it includes an exchange rate target in its interest rate rule. The main advantages of targeting exchange rates appear to be a marked reduction in inflation volatility rather than output volatility (c.f results in Table 1 and in Table 2).

The result in the paper is interesting and has also some policy relevance given the current focus on the USD/EUR exchange rates within central banking policy circles on both sides of the Atlantic. The main mechanism behind their results appears to be as follows: The model economy is subject to a myriad of shocks (6 in total) which triggers movements in the endogenous variables. Of particular importance is the open economy channel in their model. Exogenous UIP shocks move the nominal exchange rate around while exogenous term premia shock move long rates around. The combination of volatile long rates and exchange rates creates volatility in both output and inflation.

What can the central bank do to offset this volatility? Since demand responds little to short term interest rates, his options for limiting output volatility is limited. But targeting exchange rates would limit import price inflation and hence lower CPI inflation volatility. This appears to be the mechanism behind the result in the paper.

There are a number of problems with the approach in their paper which I would need to see the authors deal with before I would recommend accepting the paper for publication.

1. While it is refreshing that their model allows aggregate demand to respond to long term interest rates, I am not entirely convinced that the aggregate relation (equation 2 and 8) is derived from first principles. Typically the aggregate demand function in a standard New Keynesian model comes from rewriting the Euler equation. You can clearly see that consumption/ output today is a function of expected consumption/output tomorrow and the expected real return of a bond maturing tomorrow. It is not obvious whether the same relation can be derived in their specification.[1]

2. Given that aggregate demand responds only to long term interest rates, a standard monetary policy reaction function is not very potent in their model. This also implies that an exogenous monetary policy shock (i.e. a shock to ε_{rt} in their equation (1)) would have very little effect on inflation and output. How can we reconcile this theoretical result with the solid empirical evidence on the real effects of monetary policy shocks documented by say Christiano, Eichenbaum and Evans (JPE 2005)? Or put it differently can the model in this paper produce impulse responses for monetary policy shocks similar to those found in Christiano, Eichenbaum and Evans (JPE 2005)? I am not sure given how the monetary transmission mechanism works in their model. I think the authors would have to argue more clearly why they think their monetary transmission mechanism is more realistic than the one that has found acceptance in the academic literature. As a first pass it would be interesting to see impulse responses for monetary policy shocks in their model (i.e. a shock to ε_{rt} in their equation (1))

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[1] A minor comment on the same equation: I think there should be a foreign output measure in the aggregate demand equations (2). That's the case in standard New Keynesian open economy model such as Gali and Monacelli (RES, 2003) or Monacelli (JMCB, 2005),

3. The authors claim that they calibrate the model to Euro area data. It would be interesting to know how well their model fits Euro area data? Table 1 shows only the volatility of inflation and output from model simulations using various interest rate rules. But how does this match up with actual euro area data?

4. It would be helpful if the authors documented the shock volatility of all the exogenous processes. Page 10 mentions that there are 6 different shocks but there is no mention of the size of shocks. It would also be nice to see a variance decomposition of the simulated variables to give us a better understanding of what's driving the result.

5. I believe that there is price level indeterminancy in their model (The authors mention that some of their variables contain a unit root). This is a well-known problem in models with interest rate rules. The authors can try to substitute the interest rate rule with a simple price level targeting rule say where the central bank simply sets the CPI level equal to a constant. Alternatively, the authors would have to rewrite the model in real terms (i.e. divide through with a price level and rewrite all prices in relative terms).

6. The authors would need to argue why the term premium is linked to deviation from PPP. This is a quite stark assumption especially if you are familiar with the macro-finance literature which links the term premium to the autocorrelation of consumption. (see den Haan, JEDC 1995).

7. I would recommend that the authors pitch the paper closer to the recent literature on monetary policy targeting in an open economy (see various papers by Benigno and Benigno, Corsetti and Pesenti and Devereux and Engel) instead of Svensson (1998) and Ball (1999). This would also allow them to update their welfare measure. I believe that the current state of the art is to evaluate welfare using 2nd order approximation techniques (see Benigno and Woodford, NBER 2006) rather than ad-hoc loss functions.

As stated previously, I think these problems would have to be dealt with before the paper can be published. However, I do not see these problems as insurmountable and I would certainly encourage the authors to continue working on this very interesting policy question.