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Comment

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Taylor and Kim's paper uses a very sophisticated methodology to model the time series of six real exchange rates (the United Kingdom, Switzerland, Denmark, France, Austria, and Italy, all relative to Germany) over two time periods: January 1991–December 1998 and January 1999–June 2007. The empirical model has two basic features. First, the real exchange rate is modeled to revert not to a constant but to a time-varying fundamentals level. Second, the adjustment is modeled as nonlinear. The authors, in turn, propose two different models of the underlying equilibrium real exchange rate. One is based on relative productivity levels as in the Harrod-Balassa-Samuelson theory, and the other is a more atheoretical model derived from a diffusion index.

I think the modeling and estimation are impressive, but it is a bit difficult for me to put this exercise in context. My comments mostly raise the question of how the model performs compared to simpler alternatives.

I begin by simply noting that the paper does not present a standard test for the null of a unit root in the real exchange rates. The reason this is of interest is that one might wonder whether, for these real exchange rates, a simple stationary linear autoregressive model fits the data. Do we need to model reversion of the real exchange rate toward a time-varying equilibrium rather than a constant? Do we need to model the reversion as nonlinear rather than linear?

The paper does test for whether the parameter γ, which controls the speed of nonlinear reversion to the equilibrium real exchange rate, is zero. The paper states, "In fact, a test of H_0: $\gamma = 0$ is effectively a test for a linear unit root in the real exchange rate against the alternative hypothesis of nonlinear mean reversion toward a (constant or time-varying) long-run equilibrium." But the paper never considers the possibility of a constant long-run equilibrium. Under both specifications of the model, the long-run equilibrium real exchange rate has a unit root. That

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is explicitly assumed in the modeling of the real exchange rate using the diffusion index. But I guess it would be hard to reject the null that the relative productivity levels used in the Harrod-Balassa-Samuelson model have a unit root. Thus, whether $\gamma = 0$ or not, the real exchange rate has a unit root. In the models considered here, if $\gamma = 0$, the real exchange rate is a pure random walk. If $\gamma \neq 0$, the real exchange rate is nonlinearly cointegrated with the measure of the equilibrium real exchange rate.

So the first question is whether a simple linear stationary model can describe these real exchange rates. My guess is that the answer to this question is ambiguous, because with very short time series (8 years in the first subsample and 8.5 in the second), it will be hard to reject either a unit root or stationarity. But it would still be helpful to see how well a linear, stationary model does in describing these real exchange rates.

Next, there are two ways of increasing the level of sophistication of the model by one step. Again, it would be interesting to see how these possibilities performed: (1) a linear model in which the real exchange rate converges toward a time-varying equilibrium level and (2) a nonlinear model in which the real exchange rate converges toward a constant.

Taylor and Kim's paper settles on a nonlinear model of the real exchange rate, converging toward a time-varying constant, but does not help us understand how well the alternative models would do. I raise this issue because these particular real exchange rates do not seem like ideal candidates for a model of nonlinear adjustment. None of these real exchange rates are very variable (compared, e.g., to the U.S.-German real exchange rate or the real exchange rate of the other six countries relative to the United States). The Austrian and Danish real exchange rates with Germany are nearly constant over the entire period, and the Swiss real exchange rate also is very quiescent. Even the U.K. and Italian real exchange rates seem quite stable.

As a statistical matter, the volatility of the real exchange rate has no bearing on whether a nonlinear model is appropriate. The intuition of the nonlinear model is simply that adjustment is faster when the real exchange rate is farther from its equilibrium rate. That could happen even if the variation of the real exchange rate were tiny.

But the economic motivation for the nonlinear model, given by the authors in the second paragraph of Section II, seems to rely on the notion that there is a lot of variability in real exchange rates. There are two theories given for why the real exchange rate adjusts more quickly when it is far from its equilibrium value. According to the first, noise traders control the real exchange rate movement when it is close to equilibrium. But far from the equilibrium, it becomes worthwhile for fundamentals traders to come in and exploit mispricing of the exchange rate, which drives it back toward its equilibrium. In the second theory, central banks are motivated to intervene more strongly when the real exchange rate deviates a lot from its equilibrium. It seems to me that economically, both of these stories hinge on the notion that the real exchange rate is volatile, so that the real exchange rate deviations that are in the tails are substantial. They must be large to make it worthwhile either for the fundamentals traders to enter the market or for the central bank to intervene.

It seems, in other words, that it might be natural to look at a nonlinear model to explain the dynamics of a volatile real exchange rate. But a nonlinear model would not seem like such a natural choice for such stable real exchange rates. Hence the question, how well do linear models perform?

Taylor and Kim's figure 3 does make it seem as though there is a relation in some cases between the relative productivity levels and the real exchange rate. But is there reversion of the real exchange rate toward the relative productivity levels? Let me suggest another simple alternative that could be compared to the model of this paper: The real exchange rate follows a random walk, but innovations in the real exchange rate are correlated with innovations in relative productivity levels, $a_t - a_t^*$.

Apparently, the bootstrap procedure for testing the null that $\gamma = 0$ in the nonlinear model does not allow for correlation under the null between shocks to the real exchange rate and shocks to either the relative productivity levels $a_t - a_t^*$ (in the first model of the equilibrium real exchange rate) or the diffusion indexes $f_t - f_t^*$ (in the second model of the equilibrium real exchange rate). Note 16 says, "Since the parameterization (30) implies that the real exchange rate is independent of the time-varying equilibrium when $\gamma = 0$, the simulated real exchange rate data can be generated independently under the null hypothesis." I believe this footnote means that under the null in the bootstrap, the innovations in equation (30) (e_t) are uncorrelated with innovations in $a_t - a_t^*$ or $f_t - f_t^*$. The bootstrap therefore constructs a test of the assumption that $\gamma = 0$ jointly with the assumption that e_t is independent of innovations in $a_t - a_t^*$ or $f_t - f_t^*$. Hence, I raise the question of whether a simple random walk model for the real exchange rate with correlation of the innovations in the real exchange rate and the economic fundamentals would be adequate, rather than having reversion to an equilibrium real exchange rate based on the fundamentals.

Ultimately, then, the question is why this specification for the real exchange rate was chosen. Is there evidence that this model fits better than alternatives that might seem more natural for such stable real exchange rates? If not, can we be confident of the conclusions?