Volume Title: NBER International Seminar on Macroeconomics 2008

Volume Publisher: University of Chicago Press

Volume ISBN: 978-0-226-10732-5

ISSN: 1932-8796

Volume URL: http://www.nber.org/books/fran08-1

Conference Date: June 20-21, 2008

Publication Date: April 2009

Chapter Title: Comment on "Real Variables, Nonlinearity, and European Real Exchange Rates"

Chapter Author: Kenneth D. West

Chapter URL: http://www.nber.org/chapters/c8240

Chapter pages in book: (p. 197 - 199)

Comment

Kenneth D. West, *University of Wisconsin and NBER*

Let me begin with a summary of Taylor and Kim's interesting paper before making some comments.

Let q be the real exchange rate. Let $\bar{q}_t$ be the long-run real exchange rate (allowed to vary over time, e.g., to allow a Harrod-Balassa-Samuelson trend to the real exchange rate). (Note that $\bar{q}_t$ is my notation and is not used in the paper.) The paper relies on an "exponential smooth transition autoregression" (ESTAR) model that has been used in a number of Mark Taylor's previous papers:

$$q_t = q_{t-1} - (q_{t-1} - \bar{q}_{t-1})\{1 - \exp[-\gamma(q_{t-1} - \bar{q}_{t-1})^2]\} + e_t. \tag{1}$$

Here, $\gamma > 0$ is a parameter to be estimated, and e_t is a shock. (I have suppressed the parameters called α and β that appear in the exposition of the ESTAR model in Sec. V of the paper because the empirical work sets these two parameters to zero.)

To understand what the term $1 - \exp[-\gamma(q_{t-1} - \bar{q}_{t-1})^2]$ implies for behavior, consider two extreme cases. First, suppose that $q_{t-1} = \bar{q}_{t-1}$. Then $(q_{t-1} - \bar{q}_{t-1})^2 = 0$, $\exp[-\gamma(q_{t-1} - \bar{q}_{t-1})^2] = 1$, $1 - \exp[-\gamma(q_{t-1} - \bar{q}_{t-1})^2] = 0$, and

$$q_t = q_{t-1} + e_t, \quad E_{t-1}q_t = q_{t-1}. \tag{2}$$

Thus, when $q_{t-1} = \bar{q}_{t-1}$, the real exchange rate follows a random walk. However, if q_{t-1} is far away from $\bar{q}_{t-1}$ (either above or below), then $(q_{t-1} - \bar{q}_{t-1})^2$ is big, $\exp[-\gamma(q_{t-1} - \bar{q}_{t-1})^2] \approx 0$, $1 - \exp[-\gamma(q_{t-1} - \bar{q}_{t-1})^2] \approx 1$, and

$$q_t \approx \bar{q}_{t-1} + e_t, \quad E_{t-1}q_t \approx \bar{q}_{t-1}. \tag{3}$$

Thus, when q_{t-1} is far away from $\bar{q}_{t-1}$, the real exchange rate is expected to revert to its (time-varying) mean. More generally, the farther q_{t-1} is from $\bar{q}_{t-1}$ (either below or above), the stronger the tendency for $E_{t-1}q_t$ to be centered around $\bar{q}_{t-1}$; the closer q_{t-1} is to $\bar{q}_{t-1}$ (either below or above),

978-0-226-10732-5/2009/2008-0042$10.00

the stronger the tendency for $E_{t-1}q_t$ to be centered around q_{t-1}. Also, all things equal, larger values of γ mean a stronger tendency to revert to the mean $\bar{q}_{t-1}$.

The paper estimates two empirical models for $\bar{q}$. In the first model,

$$\bar{q}_{t-1} = \mu_1(a_{t-1} - a^*_{t-1}) + c, \tag{4}$$

where a is labor productivity, asterisks denote foreign variables, and μ_1 and c are parameters. (The paper notes that ideally a should be productivity in tradables, but the data are not adequate.) In the second model for $\bar{q}$,

$$\bar{q}_{t-1} = \beta_1 f_{t-1} + \beta_2 f^*_{t-1} + c, \tag{5}$$

where f and f^* are home and foreign factors, constructed as the first principal component of a set of monthly real variables (output, employment, sales, etc.), and β_1, β_2, and c are parameters.

These two models are estimated for six bilateral real exchange rates versus Germany, with a consumer price index (CPI) based q and monthly data, 1991–98 and 1999–2007. Thus there are 12 sets of estimates (12 = 6 countries × 2 time periods) for each of the two models. Taylor and Kim's table 1 has estimates of γ, μ_1, and c; their table 4 has estimates of γ, β_1, β_2, and c. In general, if an estimate of μ_1, β_1, or β_2 was insignificant, that coefficient was dropped. The parameter γ was always included, whether or not its estimate was significant.

Let me now turn to my comments. Taylor and Kim's paper is on an interesting topic (behavior of real exchange rates) and has interesting ideas and motivation. But in my view it makes a weak case that real exchange rates show nonlinear reversion to a time-varying equilibrium value.

Consider first when the Harrod-Balassa-Samuelson effect motivates the time-varying mean (eq. [4] above). Only half the estimates of μ_1 were significant. This means that in the other half of the data sets, a constant equilibrium real exchange rate was imposed (United Kingdom, 1991–98; Switzerland, 1999–2007; France and Italy, both time periods). Also, the paper's exposition of the (standard) version of Harrod-Balassa-Samuelson (Sec. III) has

$$\mu_1 = (1 - \lambda)\frac{1 - \phi_N}{1 - \phi_T},$$

where, in both countries, ϕ_N and ϕ_T are capital shares in Cobb-Douglas production of nontradables and tradables, and λ is the share of tradables in the CPI. When μ_1 is not set to zero, the estimates of $\hat{\mu}_1$ in table 1 include three negative or very small values that do not seem consistent with plausible values of λ, ϕ_N, and ϕ_T ($\hat{\mu}_1 = -1.242$ [United Kingdom, 1999–2007], $\hat{\mu}_1 = -0.1177$ [Denmark, 1991–98], and $\hat{\mu}_1 = 0.019$ [Denmark, 1991–98]). (This is not to say that the paper argues that the estimates should be interpreted in terms of the standard model rather than an extended model with a distribution sector also presented in Sec. III. My basic point is that the paper's argument for a time-varying equilibrium value is weakened by the absence of an attempt to rationalize regression estimates in terms of an underlying economic model.)

Consider now when the time-varying mean is modeled via factors (eq. [5] above). Only seven of the 24 estimates of β_1 and β_2 in table 4 were significant and hence included in the final specification; three of the six countries (Denmark, France, and Italy) had a constant equilibrium real exchange rate ($\beta_1 = \beta_2 = 0$) in both time periods. The paper does not give us guidance to plausible values for β_1 and β_2, so it is not possible to comment on the reasonableness of the estimates that are not zero.

In sum, the paper presents little econometric or economic evidence for a prevalence across data sets of reversion to a time-varying equilibrium value. Let us now allow for reversion to a constant (as well as time-varying) equilibrium value. The paper presents evidence for pervasive nonlinearity. If we peruse the estimates of γ in tables 1 and 4, we see that there is only one negative value for $\hat{\gamma}$ (table 1, France, 1991–98). The 23 remaining values range from about .02 to .13, with about half the estimates significant at the 10% level. The simulations reported in table 5 often indicate perceptible differences from a linear model, in terms of speed of mean reversion.

I wish, however, that the paper had made a better case for nonlinearity in either econometric or economic terms. In econometric terms: How does the fit or forecasting ability of the nonlinear model compare to a univariate linear model, for example? In economic terms: The paper does not attempt to rationalize the estimates that it finds. For example, it notes (Sec. VII) that the estimates imply that the introduction of the euro caused an especially dramatic increase in speed of reversion to purchasing power parity for France-Germany relative to other bilateral German rates. Is there corroborating evidence for that finding? A persuasive case for nonlinearity requires answers to such questions.

Endnote

I thank the National Science Foundation for financial support.