"Monetary and Portfolio-Balance Models of Exchange Rate Determination"

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MONETARY AND PORTFOLIO-BALANCE MODELS OF EXCHANGE RATE DETERMINATION: EPILOGUE

Much has happened in the few years since the exchange rate models of the 1970s were developed and tested.

The early 1980s saw a wave of pessimism among international economists as to the empirical performance of the existing models, or indeed as to the possibility of ever constructing a model that would perform well. Haache and Townend (1981), Dornbusch (1983), Frankel (1984), and Backus (1984) were typical of the mounting pile of studies showing poor results by standard statistical criteria (incorrectly signed coefficients, insignificant magnitudes, low R^2, etc.). Recent surveys of the empirical models include Levich (1985) and Isard (1986).

Rendering the devastation seemingly complete was a series of papers by Meese and Rogoff (1983a,b; 1986). Meese and Rogoff (1983a) showed that the popular models of Frenkel (1976), Bilson (1978), Dornbusch (1976), Frankel (1979), and Hooper and Morton (1982), were of no use whatsoever in predicting exchange rates outside the sample in which the models had been estimated, that in every case a simple random walk predicted better than the structural models. In one sense, this finding should not have been at all surprising. A typical in-sample regression shows unsensible coefficient estimates (for example, near-zero or negative coefficients on the money supply variables, as in Table 3.1 above, attributable to simultaneity bias). Thus it should not have been surprising that the estimated equations made bad predictions out-of-sample.

But Meese and Rogoff (1983b) then tried an alternative to estimating the equations in-sample. They tried out an entire grid of possible combinations of parameter values, for example, a range of possible values of the semi-elasticity of money demand from -3 to -10. This way, any failure to predict could not be blamed on bad estimates arising from small samples or from simultaneity bias. The results were again discouraging. While many plausible combinations of parameter estimates did give predictions that beat a random walk, many other combinations did not, and the predictive performance was in no case very impressive compared to the total variation in exchange rates. What made these findings particularly humiliating is that the authors had from the beginning given the structural models the benefit of the doubt by using ex post realized values of the explanatory variables (money supply, income, interest rates, etc.), rather than making the models forecast them ex ante before forecasting the exchange rate.

Some economists tried to convert the inability of the structural models to predict from a liability into an asset. Their argument, in its least sophisticated form, was essentially a misunderstanding of the point by Dornbusch (1980) and Frenkel (1981) regarding the importance of "news" in determining exchange rates. The argument was that under the assumptions of high capital mobility and rational expectations, which almost all of the standard theoretical models share, new information regarding the money supply or other macroeconomic variables should have a big effect on the contemporaneous exchange rate, and this effect should not have

been predictable before the information is known. While this statement is true so far as it goes, it does not follow that the poor empirical performance of structural models is anything other than a major strike against the standard theory.

There are two respects in which the empirical results are disturbing from the viewpoint of standard theory. First, the proportion of exchange rate changes that we are able to predict over the short term seems to be, not just low, but close to zero. According to rational expectations theory we should be able to predict that proportion of exchange rate changes that is correctly predicted by participants in the foreign exchange market. For example, a country that has a record of high money growth and inflation should have a currency that can be predicted to depreciate, at a rate that is appropriately reflected in the expectations of market participants, in the forward discount, and in the interest rate. Yet the Meese-Rogoff papers found that a random walk beats, not only all the structural models, but also the forward exchange rate, as well as standard time-series techniques (ARIMA and VAR).[1] The finding that the forward exchange rate is of zero benefit in predicting which way the spot rate will move is confirmed in the very large literature testing unbiasedness in the forward exchange market. These studies typically regress the ex post change in the spot rate against the forward discount at the beginning of the period. Rather than getting a coefficient of 1.0 as would be implied by the hypothesis of unbiasedness, they usually get a coefficient much closer to zero, confirming the random walk. (For surveys of this literature, see Levich (1985), Boothe and Longworth (1986), or Hodrick (1987).) A new measure of the expectations of market participants, survey data, shows results similar to those for the forward rate. Expectations as reflected in the surveys are worse predictors than the contemporaneous spot rate; investors could improve their forecasts by putting more weight on the contemporaneous spot rate, perhaps even 100 per cent weight (Frankel and Froot, 1987; and Froot and Frankel, 1987.) The "random walk" results seem remarkably robust.

The second respect in which existing empirical results are disturbing is that, even if we accept that we are able to predict only a very small part -- or no part-- of exchange rate changes ex ante, for example because the predictable component is statistically dwarfed by the "news," we would still hope to be able to explain a large part of exchange rate changes ex post, after we are able to observe the realized values of the macroeconomic variables. This we seem unable to do, at least on a monthly basis (without in-sample overfitting).

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[1] It should be noted that Meese and Rogoff's "random walk" finding was that the simple spot exchange rate was a better predictor than the simple forward rate (or the simple forecasts of structural or ARIMA models). This is not quite the same thing as saying that these other predictors are of no help at all in predicting changes in the exchange rate. Meese and Rogoff did not test whether there might exist some convex combination of the forward rate (or model forecasts) and the spot rate that would out-predict the simple spot rate. This is what the large literature on bias in the forward discount tests for, and the answer is generally "no." But Somanath (1986) uses updated data sets (to December 1983) to test versions of the monetary models combined with the lagged spot rate, and claims forecasting performance superior to the simple lagged spot rate. Meese and Rogoff (1986) report some ability of the model based on real interest differentials to predict the direction of change in the 1980s.

The response of international finance economists to their inability to predict or explain exchange rate movements has been to redefine the problem. Many were predisposed in any case to move away from the money-demand or portfolio-balance functions that were assumed in the models above, considering them too ad hoc, and instead to derive investor behavior more rigorously from principles of optimization. This is the way the theory has proceeded in the 1980s. A demand for money is created, within the optimization framework, either by assuming that money enters the utility function directly, or by assuming a "cash-in-advance" constraint for transactions. (Examples include Stockman (1980), Lucas (1982) and Svensson (1985). For a survey, see the last section of Obstfeld and Stockman (1985).)

Whatever their motivation, these models have the distinct advantage, from the viewpoint of their evolutionary survival, that they are generally too abstract to be subjected to genuine empirical testing at all. In fact, proponents of these models, in the economists' public relations coup of the decade, have managed to claim as econometric verification their inability to explain changes in the exchange rate. Examples typical of modern macroeconometric logic are Roll (1979) and Stockman (1987), who argue that the very slow tendency of the exchange rate to return to purchasing power parity supports the optimizing ("equilibrium") models against the overshooting ("disequilibrium") models. It is ironic that the earlier incarnation of equilibrium models, those called "flexible-price monetary" above, claimed support from the alleged empirical observation that the speed of adjustment to purchasing power parity was near-infinite, while the current generation of equilibrium models claims support from the alleged empirical observation that the speed of adjustment to purchasing power parity is near-zero. (Meanwhile, proponents of overshooting have consistently claimed a slow, but positive, rate of adjustment.)

The argument goes essentially as follows. According to the optimization models, exchange rate changes are due to shifts in technology and tastes that, though known to all agents in the economy, are not known to the economist. In fact, the economist doesn't even care to commit himself on questions such as whether the trend in domestic productivity is greater or less than in foreign productivity. Thus, as far as he or she is concerned, the exchange rate could as easily move up as down: the theory--which is admitted to be in its infancy--as yet contains no information that could be used to explain specific changes in the real exchange rate. He then goes to "test" his theory "empirically" by seeing whether he can statistically reject the hypothesis that the real exchange rate follows a random walk. Rather than being humbled or embarrassed about his statistical failure to explain any movement in the macroeconomic variable that he is investigating, he proudly proclaims it as confirming his theory, on the grounds that the theory too did not explain any movement in the variable![2]

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[2] This disturbing trend in modern macroeconometrics is an extreme case of the old problem that a statistical failure to reject a null hypothesis does not entitle one to claim an interesting finding. The failure to reject may simply be due to low power in the test, especially if the null hypothesis is a weak one. Traditionally in econometrics, the goal is supposed to be to succeed in statistically rejecting one economically interesting hypothesis in favor of another, i.e., to get results that are "statistically significant at the 95 per cent level," rather than the reverse. What makes the trend away from this principle so remarkable is that the popular null

If the goal is considered to be to explain exchange rate changes rather than not to explain them, then the empirical developments of the 1980s, ironically, are in many respects more supportive of some of the structural models of the 1970s than were the empirical developments of the 1970s. In particular, the broad outlines of the 1981-85 appreciation of the dollar--roughly 50 per cent in either nominal or real terms--and its 1985-87 reversal, are consistent with the theory of exchange rates based on differentials in real interest rates (what is above called the "sticky-price monetary model"). Long-term real interest differentials now seem to explain the real exchange rate better than the short-term real interest differentials that were used in earlier specifications (e.g., Frankel, 1979), and there are good theoretical reasons for preferring them as well.[3] By a variety of alternative measures, the long-term real interest differential between the United States and its major trading partners rose by about 5 points between 1980 and late 1984.[4] Thus a ready account is provided by the overshooting theory: the increase in U.S. real interest rates--due, presumably, to a shift in the mix between monetary and fiscal policy--attracted capital into the country, causing the dollar to appreciate, until it had become sufficiently "overvalued" that expectations of future depreciation back toward equilibrium were sufficient in investors' minds to offset the interest differential. After 1984, the real interest differential declined, and the dollar followed.

A theory that only claims to explain exchange rate movements on the basis of 2 or 3 observations per decade is not very testable when only 15 years of data are available. A lot more work would be needed before we could claim to have explained exchange rates well. A number of recent studies on monthly or quarterly data have claimed a degree of success with long-term real interest differentials: Shafer and Loopesko (1983), Hooper (1984), Sachs (1985), Hutchison and Throop (1985), Golub et al (1985), and Feldstein (1986). Given how often in the past a model that appeared to work well for one sample period is observed to go awry subsequently, it would be foolhardy to claim too much from these or any other regression studies. But neither is it necessary for economists to abjure any ability to explain exchange rate movements at all.

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hypothesis of a random walk is so weak that a failure to reject it is nothing other than a failure to explain any movement in the variable of interest. It is true that a problem with the classical econometric criteria is that the author has an incentive to shift through many regressions, and the journal editor has an incentive to sift through many submitted articles, to come up with "good" results. But it is no solution to this problem to redefine the inability to explain something as a "good" result.

[3] The ten-year real interest differential, corrected for any risk premium, tells us how far the market expects the dollar to depreciate per year in real terms over the next ten years. The expected real exchange rate in ten years, unlike in 3 months, can in turn be interpreted as the long-run equilibrium. See, for example, Isard (1983).

[4] Frankel (1985).

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