Economic Policy
Web Essay

The failure of empirical exchange rate models: no longer new, but still true

Kenneth Rogoff

Economic Counsellor and Director of Research, International Monetary Fund

One of the most remarkable facts about G-3 exchange rates is that they are so seemingly immune to systematic empirical explanation. This does not mean that we, as a profession, have given up on exchange rates and indeed, there has recently been a resurgence of interest in the topic. The interest was marked by a recent conference whose central theme was to see what the profession has learned since the 1983 publication of a rather nihilistic paper – entitled "Empirical Exchange Rate Models of the 1970s: Do They Fit Out of Sample?" – in which Richard Meese and I showed that random-walk forecasts outperform economic models of exchange rates.

To make a long story short not only have a subsequent twenty years of data and research failed to overturn the Meese-Rogoff result, they have cemented it, as readers can confirm from Jeff Frankel and Andy Rose's definitive survey in the 1995 *Handbook of International Economics*, the May 2000 edition of the IMF's *World Economic Outlook*, or the papers posted on the September conference's web site (Empirical Exchange Rate Modelling). The only consistent qualification (which Meese and I noted in our second paper) is that the models seem to outperform the random walk at very long time horizons of three to four years (a fact demonstrated most convincingly in Nelson Mark's 1995 paper in the *American Economic Review*). However, the notion that short run exchange rates are hard or impossible to predict was not always accepted.

Exchange rate modelling in the early 1970s

Toward the outset of the 1980s, Richard Meese and I were working as staff economists at the International Finance Division of the US Federal Reserve Board in

Washington. At the time, the modern post-Bretton Woods flexible exchange rate experience was still in its infancy, and the Board was still reviewing its general approach to the matter. The new Fed chairman, Paul Volcker, had cut his teeth as an international policymaker by helping broker the transition to flexible exchange rates, and it is fair to say that no one quite understood all the implications at the time. Our exchange rate forecasting group met in secrecy, since not only did the Federal Reserve want to protect its private exchange rate forecasts, it did not even want the outside world to know that it even thought about the matter. Much has changed now, of course, not least with the advent of increased transparency. Today people are much more likely to worry about whether the Federal Reserve Board thinks about exchange rates enough!

Meese and I, as the young "quants" in the group, were asked to see whether any structural models of exchange rates would be of systematic forecasting value. Both of us had just emerged from graduate school, and we certainly were aware that there was a lot of excitement and optimism in the academic community at the time about the potential for building useful models of exchange rates. The "asset approach to exchange rates," pioneered by Dornbusch, Frenkel, Mussa and others, seemed to hit the nail on the head in explaining why flexible rates had turned out to be so surprisingly volatile. Simply put, the new approach showed that (in theory) the exchange rate must depend not only on the current demand and supply for imports and exports (as in the classic "elasticities" approach), but also on market expectations of future developments in the "fundamentals" including outputs and money supplies. The logic was impeccable and the most attractive feature of the new theories was that they seemed to offer an explanation for why exchange rates are so volatile. Step A, emphasized especially by Jacob Frenkel, was that monetary policies themselves were quite volatile, as indeed they were during the 1970s, partly due to a misunderstanding of how to respond to oil price shocks. Frenkel argued that a change in today's money growth rate often signalled further growth rate changes, leading to a sharply compounded impact effect on the exchange rate. Dornbusch famously took the argument one step further in his "overshooting" paper. Dornbusch showed that if domestic goods prices are "sticky" (a radical assumption at the time), then the freely moving exchange rate would have to bear all the burden of adjustment to monetary shocks. Under seemingly quite plausible conditions, the exchange rate under sticky goods prices was even more volatile than under flexible prices.

Early empirical work: a limited cause for optimism

With only seven to eight years of data to work with (depending whether one marked the formal start of floating in March 1973 or later in January 1974), neither Meese nor I necessarily expected to turn up anything thoroughly convincing to our colleagues, even though we, too, were inspired by the new models. But there was limited cause for optimism, not least because there had already been quite a bit of empirical work on the asset approach, all of it seemingly very positive. Among the most influential papers in the literature included Jeffrey Frankel's September 1981 *American Economic Review* paper "On the Mark," as well as empirical papers by University of Chicago professors

Jacob Frankel and John Bilson. Not least, Peter Hooper and John Morton of the Federal Reserve had extended the Dornbusch model to factor in the effects of current accounts, again with what appeared to be very promising results. The typical empirical specification in this literature consisted of a "semi-reduced form" exchange rate equation in which the nominal exchange rate would appear on the left hand side and the right hand side explanatory variables would consist of factors such as relative (home versus foreign) outputs, money supplies, interest rates and cumulated current account (wealth positions.)

A more ambitious focus

Now, Meese and I had a somewhat different and more ambitious focus than these earlier authors. Our charge was not only to explain why exchange rate movements occurred, but to try to predict them. Presumably, being staff members at the Federal Reserve Board, we would have some advantage in this dimension, especially if monetary policy shocks were, as argued, a central driving force in exchange rate movements. But the truth was, being very junior staff members, we really had no special inside information, and had to rely on more systematic methods. The obvious thing to do was to use a vector autoregression model for the fundamentals, a method that had been newly advocated by Minnesota econometrician Christopher Sims in his highly influential 1980 *Econometrica* article. Given forecasts for the fundamentals, together with our estimated structural parameters, we would then be able to construct exchange forecasts. Meese and I already understood the severe limitations of the Sims' VAR approach, namely that it is not nearly as useful for policy analysis as for forecasting. We knew that even if our approach was successful, we would run into trouble as soon our forecasting group colleagues turned to us and said "OK, now what happens if we follow a tighter (looser) monetary policy." But we figured we would cross that bridge when we came to it. But as it happens, we never got that far. Instead, our research took an accidental turn in another direction.

It seemed natural to us to base our forecast simulations on "rolling regressions" so that, in our sample, each forecast was based on data actually available at the time of the forecast. (As a practical matter, a true rolling regression is almost impossible with macroeconomic data since data on real variables such as output and current accounts is constantly being revised, and it is virtually impossible to find a consistent historical international data set that allows one to know, say, exactly what 1982 GDP was thought to be in early 1983. Not only are ex-post revisions sizable, but when one starts talking about *real* GDP, the technical problems become even larger as methodologies for constructing price indices have changed over time, with back-dated series continually being substituted for original ones.) Given the parameters of the underlying semi-reduced form exchange rate equation (e.g., the interest rate and income elasticities), our plan was to then substitute them in VAR (time series) forecasts of the explanatory variables, again constructed by rolling regressions. First, however, we took the intermediate step of constructing forecasts based on actual realized values of the explanatory variables, rather than VAR forecasts. Any exchange rate forecasts produced

in this way were not, of course, true forecasts, since they were based on information no one could ever have had in real time, revisions or no revisions. Since our baseline was to be a random walk model, it was natural to complete this consistency check by comparing the model forecasts to those of a random walk. Incredibly, there did not seem to be any improvement, in fact the model forecasts seemed worse.

A couple thousand regressions later, we began to realize that this result was VERY robust to data set, model specification, error term specification, estimating technique, choice of theoretical model, etc. Though I admit we were more than a little amused by our failure, we were not sure how to convey our findings to our colleagues in the exchange rate forecasting group, especially after having set off so sure that our knowledge of the new theoretical literature on exchange rates would prove invaluable. Of course, the reader will realize if she does not know already, that all we had really done is to show that models do not fit at all well out of sample.

The forecasting interpretation

But it was when we realized the forecasting interpretation of the results that they became truly interesting. No one need be particularly shocked that empirical exchange rate equations fail to decisively outperform a random walk model. After all, if money supplies are hard to predict (and over the seventies, they certainly were), then one should not blame the models if exchange rates are hard to predict. On the contrary, if a suitably weighted average of the explanatory variables follows a random walk, so too will the exchange rate. But our results said something different. In essence, our simulation models asked the following question: "We will *tell* you what money supplies, interest rates and outputs are going to be one year hence. You have to predict the exchange rate, under the constraint that you must adhere to what the model says the effect should be, based on historical parameters." (Later, in a follow-up paper, we threw an even softer pitch: the only constraint we imposed on the ex post-based model forecasts was that they adhere to the theoretical sign of the coefficients. That is, if your model tells you a relative rise in the US money supply leads to a depreciation, you can call for a depreciation of any magnitude, just not an appreciation.) Put this way – using this forecasting interpretation – our results seemed almost impossible to believe.

An incredulous reaction

And, for a long time, no one did believe them. The editor of the *American Economic Review* (Robert Clower) sent our manuscript back in return mail with a scathing letter saying that the results are obviously garbage and if we wish to remain in the economics profession, we had better develop a more positive attitude. My thesis advisor, Rudiger Dornbusch, delivered the same message in a kinder way. He acknowledged the results as interesting, but made clear that he doubted they would hold up once another year's data rolled in. Was this really a good investment of research time? People outside international macroeconomics typically found the results even more implausible. One then young and now pre-eminent MIT macroeconomist, when told the findings,

forcefully commented (with a French accent) "You just cannot possibly have done it right." Others, however, especially those who had played a bit with data themselves, were more ready to believe our findings. Paul Krugman described our paper as "an anti-neutron bomb" that left the model designers alive and well, but destroyed all their structures.

Though I did not agree with Dornbusch's assessment that another year of data would turn it all around (no one actually working with the data at the time would be so optimistic), I certainly told all my colleagues that, surely, by the year 2000, the problems identified in my research would be overcome. They have not.

The sources of failure

Now briefly, what is one to make of the failure of our canonical empirical exchange rate models? This is, of course, a topic of ongoing research and, even twenty years, later, I am still optimistic that an answer will eventually be forthcoming. But, I think, there are at least two important elements. First, a key link in any extent model involves the connection between prices and exchange rates. This link is not instantaneous – that was the important premise of Dornbusch (1976), but it is the foundation of virtually any existing theory of the nominal exchange rate, all of which build in some form of (perhaps real-shock adjusted) purchasing power parity. That is, there should be some long-run relation between national price levels and exchange rates. It is the PPP relation, after all, that really lies at the heart of our belief that it should be easier to explain the level of exchange rates than the level of stock prices. If there is any macroeconomic relation in which we have confidence, it is the long run relationship between monetary policy and prices (in 1980, it was a relationship between money and price levels, today it is a relationship between interest rates and inflation). If one can explain why national price levels move differentially, one ought to be able to say something significant about the level of exchange rates. The problem with this logic, as we now far better understand today than we did in 1980, is that purchasing power parity is, at best, a very long run relationship. Casting aside technical debates about issues such as whether the right model includes linear or non-linear adjustment, it is clearly the case the deviations from PPP dissipate only very slowly, as documented in my own 1995 Handbook paper with Ken Froot and in my 1996 *Journal of Economic Literature* paper "The Purchasing Power Parity Puzzle." The standard finding in the literature is that the half-life of PPP deviations is three to four years, at least in linear models.

The second element has to do with the lack of connection between exchange rates and fundamentals, which in my *NBER Macroeconomics Annual* 2000 paper with Maurice Obstfeld on "The Six Major Puzzles in International Macroeconomics: Is there a Common Cause?" terms "the exchange rate disconnect puzzle." That is, not only do macroeconomic fundamentals fail to explain exchange rates, but it is not easy (for major currencies) to systematically trace back the effects of exchange rates to economic fundamentals either. (The seminal paper on this topic is by Baxter and Stockman, 1989 *Journal of Monetary Economics*). The explanation Obstfeld and I give, is that market segmentation across industrialized countries is much greater than commonly believed,

and is consistent with a level of international transactions costs for *all* GDP (not just goods actually traded) averaging 25% or more.

So, as of this writing, explaining the yen, dollar and euro exchange rates is still a very difficult task, even ex-post. The policy implications of this finding I leave to a subsequent debate on these pages.

References

Baxter, M. and A. Stockman (1989). 'Business Cycles and the Exchange Rate Regime: Some International Evidence,' *Journal of Monetary Economics*.

Dornbusch R. (1976). 'Expectations and Exchange Rate Dynamics,' *Journal of Political Economy.*

Frankel, J. (1981). 'On The Mark: Reply,' *American Economic Review*.

Frankel, J. and A. Rose (1995). 'Empirical research on nominal exchange rates', in G. Grossman and K. Rogoff (eds.), *Handbook of International Economics*, North-Holland, Amsterdam.

Froot, K and K. Rogoff (1995). 'Perspective on PPP and long-run real exchange rates', in Grossman and Rogoff (eds.), *Handbook of International Economics*, North-Holland, Amsterdam.

Meese, Richard, and Kenneth Rogoff (1983), 'Empirical Exchange Rate Models of the 1970s: Do They Fit Out of Sample?' *Journal of International Economics*.

Nelson Mark (1995). 'Exchange Rates and Fundamentals: Evidence on Long-Horizon Predictability," *American Economic Review*.

Obstfeld, M. and K. Rogoff (2000). 'The Six Major Puzzles in International Macroeconomics: Is there a Common Cause?" in B. Bernanke and K. Rogoff (eds.) *NBER Macroeconomics Annual*, Cambridge MA.

Rogoff, K. (1996). 'The Purchasing Power Parity Puzzle', *Journal of Economic Literature.*

Sims, C. (1980). 'Macroeconomics and Reality,' *Econometrica*.