

Recent Thinking About Exchange Rate Determination and Policy

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There is a certain irony in the title of the paper that I was asked to write for this Conference. While there has, of course, been a lot of recent work on exchange rate determination, this work has had little impact even on sophisticated policy analysis. Indeed, it is striking that policy discussion has been based on an underlying model of exchange rate determination that has changed little since the mid-1970s. The workhorse model of policy analysis remains the Mundell-Fleming approach, modified mainly to allow for some expected regression of the exchange rate towards a long-run 'normal' level.

Consider, for example, the way in which most commentators discuss the exit of the United Kingdom from the exchange rate mechanism of the European Monetary System (EMS). We say that the purpose of the exit was to allow the Bank of England to cut interest rates; that this cut in rates inevitably led to a fall in the pound sterling against the deutschemark (DM); and that the combination of more competitive UK exports and the investment effects of the lower interest rates have allowed the United Kingdom to begin an economic recovery even while the rest of Europe continues to slump. What is the underlying model? Clearly, it is IS-LM with high capital mobility plus regressive expectations on the exchange rate, right out of an undergraduate textbook (say, to take an arbitrary example, Krugman and Obstfeld (1991)).

Why isn't there a more up-to-date canonical model of exchange rate determination? It is not for the want of trying. Indeed, there was a 'heroic age' of exchange rate theory from the mid-1970s to the early 1980s. During that era there were major competing schools of exchange rate theory (Dornbusch's overshooting model, the monetary models of Frenkel and Mussa, the portfolio models of Kouri and Branson) that attracted both efforts at theoretical extension and a considerable amount of empirical work. By about 1979, one would have described exchange rate economics as a field dominated by innovation, and one in which ideas were rapidly changing.

Unfortunately, that heroic age ended when empirical work succeeded in refuting not one or two but *all* of the contending theoretical approaches. From my point of view, the two key papers were Hansen and Hodrick (1980) and Meese and Rogoff (1983). The Hansen and Hodrick paper was the first of what was to become a host of papers refuting the proposition that forward premia are efficient, unbiased, or even halfway reasonable forecasts of subsequent changes in exchange

rates. I'll go into the reasons why that result was so devastating below, but for now, let me simply note that the assumption of rational expectations was central to all of the new models of exchange rates. Without financial market efficiency, one was back in the old-fashioned world of macroeconomic models with *ad hoc* assumptions about expectations, a world that young exchange rate theorists thought they had put behind them.

The Meese and Rogoff paper stepped back from the efforts of empirical modellers to show that their preferred approach to the exchange rate did a better job than the others, and to ask whether any of the approaches did well enough to warrant consideration. There is still controversy over the philosophical basis of their test, which compared out-of-sample forecasts using the actual values of explanatory variables with a random walk. However, the paper highlighted the extremely poor performance of all models to such an extent that it became difficult to present another set of weak regression results without embarrassment.

The theory of exchange rate determination has never recovered from the empirical debacle of the early 1980s. While there has been considerable work, both on theoretical models and on empirical testing of propositions about exchange rates, I think that it is not unfair to say that for the most part international monetary economists have given up, at least for now, on the idea of trying to develop models of the exchange rate that are both theoretically interesting and empirically defensible. Quantitative policy analysts must have something to determine exchange rates in their empirical models, so they either have an exchange rate equation that more or less fits the data or simply impose some mechanism, but they make little pretence that they have solved the riddle of exchange rates. Theorists work with models in which the exchange rate is determined by certain fundamentals, but there is little effort to confront these models with evidence.

And yet, serious and useful work on exchange rates goes on. Instead of trying to find a working model of the complete process of exchange rate determination, however, this research tends to break off questions that are pieces of the puzzle. In the remainder of the paper I discuss three such questions that seem to me to be of crucial importance: the speculative efficiency of the foreign exchange market, the real effects of nominal exchange rate changes, and the role of real exchange rates in the trade balance.

On each of these questions I follow the same format. I pose the question and ask why it is important, then summarise what I take to be the current state of the evidence. This is followed by a description of some popular 'rationalisations' - that is, claims that the evidence does not show what it appears to. Finally, I discuss some recent concepts that have given the controversy a new lease on life.

1. Is the Foreign Exchange Market Speculatively Efficient?

1.1 Why it Matters

The speculative efficiency hypothesis - the view that the market takes into account the available information and does not make systematic mistakes - is a powerful idea. Indeed, it is so powerful that economists are extremely reluctant to abandon it even in the face of adverse evidence. This is true in finance and in macroeconomics but, above all, it is true in the economics of exchange rate determination, where the question of whether or not markets are speculatively efficient is critical to both theory and policy.

The reason why speculative efficiency is so crucial to theory sounds somewhat discreditable, but it should not. Without rational expectations, it is difficult to do much in the way of theory at all. In exchange rate models from overshooting to smooth pasting, the assumption of speculative efficiency is crucial to tying down the behaviour of asset markets. In the Dornbusch overshooting model, for example, one imposes *ad hoc* but plausible assumptions about price behaviour and money demand; the interesting results occur when one shows that given this assumed behaviour, the *rational* response of the exchange market is to produce overshooting, with all of its implied real exchange rate volatility. If one does not assume rationality, by contrast, the behaviour of the exchange market is as much imposed by the theorist as anything else: the magician essentially pulls out of the hat the same rabbit the audience has seen him stuff in a few minutes earlier.

As I have already indicated, this sounds like a spurious reason to want to believe in speculative efficiency, but I think that it has considerable justification. Physicists tend to prefer hypotheses about nature that constrain their theories rather than leave a large number of free parameters; a preference that has tended over time to steer them in the right direction. Why shouldn't economists show the same kind of preference?

On a more practical level, the question of whether markets are speculatively efficient is crucial to the debate over exchange rate regimes. The practical man's argument against floating exchange rates, from Ragnar Nurkse to George Soros, has always been that they are subject to destabilising speculation - that markets get carried away in one direction or another, giving rise to excessive and presumably costly variation in exchange rates. One of the most effective counter-arguments is the claim, originally made by Milton Friedman, that markets don't work that way, and that rational speculators will try to burst bubbles rather than create them. While there has been some confusion over the Friedman hypothesis, both from high theory (the possibility in some models of rational bubbles) and more prosaic concerns (how do we define stabilising speculation?), his basic argument stands: the kind of irrational herding behaviour that opponents of floating exchange rates fear, should be ruled out by speculative efficiency.

It would, then, be both an easier world to model and a more comfortable world to live in if exchange markets were speculatively efficient. What is the evidence?

1.2 The Basic Evidence

The simplest form of the speculative efficiency hypothesis assumes both efficiency and risk-neutrality, so that the forward premium is the best available predictor of the subsequent change in the exchange rate. Since investors are not risk-neutral, one would not be surprised if the test were failed in a technical sense. The actual failure is, however, such a blowout that it is very difficult to rescue the hypothesis in any other form.

I will not try to survey all of this enormous literature; the survey by Froot and Thaler (1990) covers much of the ground. Basically, it turns out that for exchange rates in which the US dollar (\$US) is one of the currencies, not only is the forward premium not an efficient predictor of the subsequent change in the exchange rate, its correlation with that subsequent change is actually perverse. In terms of the basic regression:

$$\Delta s = \alpha + \beta f + u \quad (1)$$

where we ought to find $\alpha = 0$ and $\beta = 1$. In fact, we typically find $\beta < 0$. That is, the forecast errors are very strongly negatively correlated with the forward premium itself. For other currency pairs, the results are not quite so striking but, in terms of the formal criterion - that no information available to investors at the time they sign a contract should be correlated with the forecast error - the results remain devastating.

It is tempting to argue that this test proves little, since we know that investors are not risk-neutral. In fact, however, before the tests were first performed, people did think that β would come out at least close to 1, and when some early regressions were misinterpreted as showing that this was true, they were claimed as evidence in favour of speculative efficiency. Moreover, there is substantial evidence that risk aversion alone cannot explain away the results. Expected exchange rate changes from surveys look much more like the forward premia than like the expected actual outcome given available information. To explain the perverse correlations one needs large and shifting risk premia, yet there is no plausible explanation of such shifts.

The most favourable thing that one could possibly say is that the data give no positive support to the idea of speculative efficiency. Most people would view things even less favourably: the assumption of efficiency is something that must be maintained in the face of seemingly unfavourable evidence. In my view, the situation is even worse: there is no plausible way to reconcile the assumption of speculative efficiency with the data.

Unfortunately, that is such an uncomfortable conclusion from the point of view of both modelling strategy and policy prescription that even after a decade of negative results theorists are still looking for a way out.

1.3 Rationalisations

Leaving aside the possibility of large, shifting risk premia, I would identify two ways in which serious efforts have been made to rationalise the failure of simple efficiency tests.

The first is the ‘peso problem’. This problem takes its name from the futures market in the Mexican peso during the years before the 1977 devaluation, which put the peso at a consistent discount. As a number of people have pointed out, any test of the efficiency of that market before 1977 would have concluded that it was inefficient, since the futures price of a peso consistently mispredicted the actual price, which itself had very little variance. Yet, that discount was the result of a belief by investors that there was some probability of a large devaluation, a belief that turned out to be justified.

In general, the peso problem may be defined as the presence of potential large events, whose possibility affects behaviour significantly even though they occur rarely. Thus one might have 60 months of observations, which seems like a large sample; but it is quite possible that throughout this period there is a varying but real chance of a 100 per cent devaluation over the next month, producing a large capital gain to holders of foreign currency. If the devaluation does not happen to materialise during the sample period, the market appears inefficient.

From a statistical point of view, the peso problem is simply that shocks may not be normal; they may have very fat tails. As a result, our usual statistical tests may be misleading.

It is worth noting that stories that attempt to explain exchange rate volatility by invoking such exotic things as rational bubbles can confront the failure of the efficiency tests only by bringing in particular versions of the peso problem: in the rational bubbles case, the efficiency test may appear to fail because of the potential large exchange rate change when a bubble bursts.

Leaving aside such stories, however, the natural way to confront the peso problem is to ask what large event the markets might have been worried about. In some cases there is a natural event of this kind: in the case of the German hyperinflation, studied by Krasker (1980), stabilisation provided the obvious ‘peso’ event. In the case of ‘Southern cone’ stabilisations, studied by Kaminsky (1982), the prospect of eventual collapse of the crawling peg regimes plays a similar role. However, for the industrial country experiences since 1973, peso problems seem hard to rationalise given the absence of probable abrupt shifts in regime.

Furthermore, the apparent bias in forward rates was so large during the 1980s that either the mysterious potential shock must have been unreasonably large, or it must have been perceived as fairly likely; in that case, the actual experience, that the shock never materialised, is a highly unlikely draw and we are again in the position of finding apparent inefficiency in the market.

While it is hard to argue definitively against the possibility of something that might have happened but did not, on the whole, the ‘peso problem’ explanation of apparent inefficiency is not convincing for recent exchange rate experience.

The other main way to make sense of the apparent irrationality of exchange markets is to argue that investors have been engaged in a process of learning about the structure of the world in which they live. If investors initially do not know the parameters of the economy, and must base their expectations on a model that is continually updated in the light of experience, it might be possible to look back over the data and find consistently biased expectations even though everyone was in fact doing the best he or she could given the available information. On this view, the standard tests for efficiency would apply only to a long-run equilibrium situation in which agents have had an arbitrarily long time to revise their expectation-forming mechanisms; in the real world, one would expect to fail the tests.

This is a highly reasonable argument and has been elaborated by Lewis (1989). The problem with it is that it does not justify the use of rational expectations models, since a world in expectational disequilibrium may behave very differently from the ‘ergodic’ models favoured by theorists. Indeed, there is no guarantee that this ergodic world emerges even in the long run. Perhaps, beliefs and the actual behaviour of exchange rates drive each other in endless circles (or strange attractors!). In other words, the learning/disequilibrium hypothesis may save the idea of investor rationality, but only by making that idea much less useful as a help in understanding the world.

In summary, while there are sophisticated ways to rationalise the apparent failure of exchange markets to meet efficiency criteria, they either lack plausibility or create as much trouble as they solve.

1.4 The Return of Efficiency: Target Zones

In spite of the disastrous failure of the speculative efficiency hypothesis when tested directly, in the late 1980s and early 1990s that hypothesis was once again fashionable in theoretical models. The ‘smooth pasting’ literature originally developed to analyse target zones could be criticised on many grounds, but one central problem was that it presumed the same risk-neutral speculative efficiency that the data seemed to reject. The reason for this assumption was, of course, its power to constrain the theory: with speculative efficiency one could derive a

pleasingly elegant and initially surprising solution, without it the theory would be so unconstrained as to be uninteresting.

Given what we know about the empirical evidence on exchange markets generally, it is not surprising that the target zone model has fared badly in the face of the data. (See Svensson (1992) for a useful survey.) Interestingly, however, most of the proposed solutions have involved rational behaviour by investors given more complex processes; here, as elsewhere, we have difficulty in facing up to the implications of the apparent irrationality of the financial markets.

2. Do Nominal Exchange Rates Have Real Effects?

2.1 Why it Matters

Since the early 1970s, macroeconomics, in general, has been divided into two schools variously known as new classical and new Keynesian, fresh water and salt water, equilibrium and disequilibrium. The core disagreement between the two schools is over the flexibility of nominal prices. In Keynesian macroeconomics, of course, sluggish price adjustment is crucial to the story of why demand shocks affect the level of output. The new classical attack on Keynesian ideas asserts that prices cannot be sticky because that would represent irrational behaviour and that business cycles must, therefore, have another explanation.

The debate over the real effects of nominal exchange rate changes must be seen in the context of this broader dispute. Can a nominal devaluation lower the relative prices of the devaluing country's goods and/or labour? If so, this is of direct importance for the choice of exchange rate regime, because flexible exchange rates can then be a useful way to ease the process of adjustment to a variety of shocks. It is also, however, of considerable importance to the general macroeconomic debate: if the nominal exchange rate can affect the real exchange rate, then nominal variables may be presumed to have real effects in general. If, on the other hand, we are convinced that 'the exchange rate is the relative price of two monies, not of two goods' (Mussa 1979), and that exchange rate changes are normally matched by an offsetting combination of inflation in the depreciating country and/or deflation in the appreciation country, we also reinforce the case for new classical macroeconomics in general.

2.2 The Basic Evidence

The evidence on the real effects of nominal exchange rate changes is, at first sight, overwhelming: for industrial countries, especially since 1980, nominal exchange rates have been reflected in nearly one-for-one changes in the relative prices of goods and labour.

The \$US is the most obvious example: from 1980 to 1985 its trade-weighted nominal exchange rate rose a logarithmic 49 per cent, its real rate by 44 per cent; then, from 1985 to 1990, the nominal rate fell 47 per cent, and the real rate fell 43 per cent. The \$US experience is not, however, unique. To take the most recent example, the exchange rate adjustments that took place within Europe following the EMS crisis of 1992 were not, to any visible extent, offset by inflation in the depreciating countries or deflation in the countries that remained pegged to the DM; thus, these nominal exchange rate changes appeared to have very nearly one-for-one impacts on real exchange rates.

The overwhelming evidence, then, is that depreciation and appreciation are *not* reflected in corresponding inflation or deflation. Indeed, the evidence is so strong and straightforward that it can be regarded as ‘Exhibit A’ in the case for Keynesian macroeconomics and against the new classical view.

2.3 Rationalisations

Classical macroeconomics is extremely attractive, for the same reasons that the hypothesis of speculative efficiency is attractive: a world of clearing markets and rational agents is much more satisfactory from the point of view of the theorist who wants definite answers than a world in which one must make *ad hoc* assumptions like sluggishly adjusting prices. Thus classical macroeconomists have been reluctant to accept even the seemingly ‘in-your-face’ truth of the proposition that nominal exchange rate changes produce nearly equal changes in relative prices.

There is essentially only one way to argue with this evidence: to claim that the causal relationship actually runs the other way: that is, real shocks cause real exchange rates to change and these real shocks dominate the movements in nominal rates as well.

There is no question that real shocks occur and that they do move real exchange rates. Most notably, changes in the prices of oil and other raw materials have clearly played an important role in the movements of the exchange rates both of raw materials exporting nations and to some extent of highly import-dependent nations like Japan. One can also argue that changes in fiscal policy (like the emergence of US deficits under Ronald Reagan or German deficits after reunification) are, in effect, real shocks as well. The question is whether such real shocks can explain away the striking correlation between nominal and real exchange rates. The answer is almost surely no, for at least three reasons.

Firstly, while a reverse causation from real shocks to nominal exchange rates can explain a correlation between nominal and real exchange rates, the actual correlation is not a modest one - it is virtually perfect, with a coefficient of almost unity. This is just too much to explain unless one is willing to suppose that there

are virtually no exchange rate changes that are *not* the result of real shocks, a view that is difficult to defend.

Secondly, the timing of exchange rate changes - closely matched to political events and seemingly unrelated to real shocks - is hard to reconcile with the reverse causation story. To take the most conspicuous recent example, what was the real shock that abruptly reduced the equilibrium relative price of UK goods and services by 10 per cent during the week following September 16th, 1992? (Should George Soros be considered an exogenous real variable?) The same questions arise when one considers the fall in the \$US after the Plaza Accord.

Thirdly, it is difficult to reconcile a 'real shocks' story of real exchange rates with the observed changes in behaviour across exchange rate regimes. It is a glaringly obvious fact that real exchange rates have been much more volatile since 1973 than before, and that the adoption of the Exchange Rate Mechanism (ERM) of the EMS was associated with a sharp reduction in the volatility of intra-European exchange rates. It is implausible to argue that the nature of real shocks changed dramatically right on cue as the regime changed.

It may be worth pointing out one serious diversion in the discussion of the evidence on the relationship between nominal and real exchange rates: the issue of an alleged unit root in the real exchange rate. In the exchange rate discussion, as in other parts of macroeconomics, initial tests that seemed to fail to reject the hypothesis of a unit root in the real exchange rate were taken by some economist as evidence of the predominance of real shocks. The argument went like this: models that depend on short-run price stickiness, like the Dornbusch overshooting model, exhibit mean reversion in the real exchange rate, whereas a real shock could be permanent. Thus, the absence of evidence for mean reversion is treated as evidence for real rather than nominal shocks.

At this point I think that the wrongness of this argument is generally appreciated, but it may be worth re-emphasising. Firstly, while some real shocks could lead to permanent changes in the real exchange rate, many should not. Indeed, the idea that relative prices should follow a random walk even in a flexible price world seems more like a knee-jerk carryover from asset-pricing theory than a carefully thought-out proposition.¹ Secondly, a point emphasised by Jeffrey Frankel among

1. I have been puzzled by the importance attached by so many economists to unit root findings, often in the face of what seems like obvious evidence that there is, in fact, a tendency to return to a long-run trend. For example, in some studies a unit root is found in US real GDP. But the US unemployment rate shows a clear, unambiguous tendency to return towards the mean; and Okun's Law, the relationship between changes in unemployment and output growth, is one of the most solid empirical relationships in economic analysis. Why ignore this evidence from the labour market, which seems so obviously to validate the conventional trend-and-cycle view of growth?

others, if mean reversion is slow (as it will be if prices adjust slowly) it will normally take very long time series to reject the hypothesis of a random walk; ironically, the further the economy is from flexible prices, the longer the time series that is necessary. Finally, when sufficiently long time series are used, one *does* find the mean reversion one expects from sticky-price models.

In summary, the evidence on the real effects of nominal exchange rate changes is about as clear as anything in economics: nominal depreciation produces a sustained reduction in the relative price of the depreciating country's goods, services and labour. If that evidence sits uncomfortably with influential macroeconomic doctrines, so be it: it is the doctrines that need changing, because the evidence stands.

2.4 The Return of Price Flexibility: Credibility

Even though there is overwhelming evidence that nominal exchange rates have real effects, there is an influential policy doctrine that urges countries to act as if any depreciation will lead to an immediate offsetting rise in prices. This is the doctrine of credibility, which argues that wages and prices are largely determined by expectations of future inflation and that a fixed nominal exchange rate can be a peg on which to hang a credible commitment to future price stability. The credibility doctrine is most influential among European policy makers who use it to justify the sacrifices needed to remain within or (for non-members) 'shadow' the ERM; but it is also influential in stabilising developing countries such as Mexico and Argentina.

Policy makers who believe in the credibility doctrine argue that any devaluation of their currencies will fail to produce the desired gain in competitiveness because it will simply lead to a burst of inflation. This sounds like the classical view that nominal variables cannot have real effects. The chain of argument is, however, somewhat different: it is not argued that nominal variables are inherently neutral in a market system, but rather that wage and price-setters, who may have considerable market power, will respond to the depreciation by increasing their demands. The conclusion, however, is the same: depreciation simply doesn't 'work' in real terms.

The doctrine of credibility is widely held, but seems to be based more on anecdotes and selective interpretation of history than on careful examination of the evidence. For example, in Sweden, it is widely noted that the effects of the 1982 devaluation were wiped out by inflation after a few years. Yet, it is not often observed that the inflationary boom of the mid-1980s was largely due to a mishandled deregulation of the banking system, not the devaluation.

In any case, recent events seem to suggest that the credibility doctrine was, at the very least, oversold. It was widely asserted, for example, that any depreciation

of the pound would lead only to an explosion of inflation; given the experience since Black Wednesday, this fear looks irrational. In general, the emergence of a policy consensus during the late 1980s, that nothing is lost by fixing exchange rates, seems to be an example of how a conventional wisdom can take hold based on little or no evidence.

3. Do Real Exchange Rates Matter For Trade Flows?

3.1 Why They Matter

The idea that a weaker \$US will reduce the US trade deficit, or that a stronger yen will reduce Japan's surplus, seems straightforward. Yet it is an idea that receives startlingly hostile attacks from both the Right and the Left.

During the 1980s most of the attacks came from the Right. Why would the *Wall Street Journal*, for example, feel that it was important to refute the idea that a lower \$US would spur exports? Presumably because any suggestion that currency depreciation does something useful threatened their case for a return to the gold standard or some simulation thereof. Similarly, more moderate monetary conservatives in Europe were unwilling to acknowledge that exchange rates play any useful role in trade adjustment because that amounts to a potential argument against the European Monetary Union.

The characteristic conservative argument against any role for exchange rates is essentially to point to the famous identity $S - I = X - M$. While there is a naive view that this identity somehow allows savings-investment balances to translate into trade balances without any intervening real variables (what John Williamson has called the doctrine of immaculate transfer) the more sophisticated view holds that markets for goods and services are sufficiently well integrated that a change in the national composition of world demand requires hardly any change in relative prices.

At present, however, the more important critics of the idea that the exchange rate affects the trade balance are on the Left: advocates of 'managed trade', especially in the United States, are unwilling to accept the idea that overall trade imbalances can be corrected without direct imposition of quantitative restrictions. Indeed, an assertion of the ineffectiveness of the exchange rate is crucial to demands by the United States, at the time of writing, that Japan accept numerical targets not only for a number of particular imports but for its current account as a whole.

These critics from the Left essentially have the same view as the 'elasticity pessimists' of the early post-war period: the responses of trade flows to prices are too small to be helpful, indeed, small enough that the Marshall-Lerner condition may not be satisfied. These alleged small trade responses are blamed both on

market imperfections and on what is perceived as a world in which trade is increasingly managed.

So the proposition that exchange rate depreciation spurs exports and limits imports bothers a wide range of opinion across the political spectrum.

3.2 The Basic Evidence

The best evidence in favour of the role of exchange rates comes from the United States, where since the early 1970s, depreciations have consistently been followed by an improved trade performance and appreciations by worsened performance.

Consider the \$US cycle of the 1980s. The \$US hit a trough in 1980, a peak in 1985, then fell rapidly. All empirical studies suggest a substantial lag in exchange rate effects; the picture for the 1980s looks best with a two-year lag. And the numbers are very striking. From 1982 to 1987, US real exports of goods and services rose at an annual rate of only 4.1 per cent, while imports rose at an annual rate of 10.2 per cent. From 1987 to 1991, exports rose at an annual rate of 9.8 per cent while imports went up by only 2.5 per cent annually. It is difficult to think of any explanation of such a radical change in trade performance other than the fact that the \$US rose for the first half of the decade and fell for the second.

Robert Lawrence once pointed out that the \$US cycle of the 1980s played out almost like a controlled experiment on the effects of exchange rate fluctuations. Initially, the \$US soared in a real appreciation which, in magnitude, has few precedents; then it fell to roughly its starting position. The result has been to allow much tighter fitting of standard trade equations and also, seemingly, to confirm their usefulness: while predictions have not been perfect, exchange rates seem to play a conventional role in determining trade flows.

But as in the case of our previous question, yes is an answer that many people are unwilling to accept.

3.3 Rationalisations

Those who remain unconvinced of the effectiveness of exchange rates in affecting trade generally make their case by pointing to experiences other than that of the United States. For the conservative critics, the case in point is trade adjustment within Europe; while for the critics from the Left, it is Japan's persistent surplus.

For Europeans who think that exchange rates do not matter, the case in point has been the shift in current account balances since 1989. Since reunification, they point out, Germany has moved from a large surplus to a large deficit *vis-à-vis* its European partners even though it has remained under a fixed exchange rate regime, with only small differences in inflation rates. Doesn't this show that

changes in savings-investment balances can be translated into changes in trade balances without the exchange rate as a transmission mechanism?

The short answer is yes, it does - but only because changes in real output took place instead. The effect of German reunification was a fiscal-led boom in West Germany even while high interest rates squeezed demand elsewhere. If European markets had been very well integrated, the increased German demand would have been spread evenly around Europe. In fact, from 1989 to 1991 the West German unemployment rate fell sharply while the rate in the rest of Europe rose, and West German industrial production rose 9 per cent while production fell in the rest of Europe. The German economy is very open, relying heavily on industrial goods produced elsewhere in Europe, especially in the Low Countries, so that changes in demand fall more on imports than is the case in, say, the United States; thereby, the German boom was more effective at turning a current account surplus into a deficit than a comparable boom would have been in the United States or Japan. But nothing in that experience suggests that at constant rates of unemployment, a reduction in Germany's $S - I$ could have been translated into a corresponding change in her $X - M$ without a substantial real appreciation.

On the whole, the attempts to use European experience to debunk the apparent role of exchange rates in trade balance determination look increasingly unconvincing. My guess is that they will look even less convincing a year from now, when the effects of the depreciation of the pound have had time to work themselves through the pipeline.

Perhaps surprisingly, the neo-structuralists who point to the Japanese experience are somewhat harder to answer. The raw facts look like this: in 1985, there were about 240 yen to the \$US, and Japan had a current account surplus that peaked in 1987 at 4 per cent of GDP. In 1992, the yen averaged 125 to the \$US, yet Japan was once again running a current account surplus of more than 3 per cent of GDP. Doesn't this show that at least in the case of Japan, exchange rates cannot move the trade balance?

I do not think that Japanese trade is really insensitive to the exchange rate, but the defence is fairly complicated - exculpatory factors, rather than a one-sentence answer. The factors include the following: lower inflation in Japan than in the United States, so that the real yen has not appreciated as much as the nominal; a long-term upward trend in the equilibrium real yen, due to rapid technological progress concentrated in tradeable sectors; and a massive favourable shift in Japan's terms of trade, due to falling real prices of raw materials. Equations fitted to Japanese manufacturing trade show price elasticities comparable to those for US trade, a fact that leads me to conclude that it is these special factors rather than an absolute lack of exchange rate response that explains the persistence of Japan's surplus. Still, in this case, we do not have the kind of overwhelming, simple

evidence that we have in many of the other issues discussed in this paper.

Perhaps the important point to remember, however, is that if there is any question about the impact of exchange rates on trade, it applies only to Japanese trade - and in spite of some US paranoia, Japan is only a small part of the world trade picture. Broadly speaking, the evidence is still that exchange rates do work in the adjustment process. Indeed, the experience of the 1980s makes that evidence stronger than ever before.

4. Implications For Policy

4.1 Does Exchange Rate Theory Give Any Policy Guidance?

It is notoriously difficult to translate exchange rate models into policy prescriptions. Like all plausible monetary models, exchange rate models generally lack a clear microeconomic foundation, making it impossible to do any rigorous welfare economics. Nor is it as easy to assign plausible criteria for ranking outcomes as it is in domestic macroeconomics, where we have little trouble concluding that everyone would prefer output to be high and stable.

Suppose, for example, that it had turned out that Dornbusch's overshooting model fitted the data quite well. Should one have concluded from this that floating exchange rates are a bad thing, because they tend to be inherently very volatile? Or should we have concluded that the observed volatility shouldn't worry us because it may be driven by fully rational behaviour? At this point the answer is moot, because the model turns out not to be validated by the evidence; but the question helps illustrate why it is hard to go from conclusions about exchange rate determination to conclusions about policy.

Nonetheless, there is a framework that is widely used to discuss the welfare economics of exchange rate policy, namely the optimum currency area discussion. So it is worth asking how the current state of thinking about exchange rate determination has a bearing on that framework.

4.2 Optimum Currency Areas

The optimum currency area theory essentially views the choice of exchange regime as a trade-off between macroeconomic flexibility and microeconomic efficiency. If exchange rates are flexible, then nations that for some reason have managed to get their wages and prices out of line need not suffer extended recessions in order to work them down; they can simply devalue or allow their currencies to depreciate. That was the argument Keynes made in 'The economic consequences of Mr Churchill'; it is the same as the argument that prevailed when Norman Lamont pulled the United Kingdom out of the ERM.

On the other hand, flexible exchange rates expose nations to the microeconomic costs imposed by unstable units of account, costs that are that much greater if currencies are subject to irrational, destabilising speculation. Concern about these microeconomic costs, and worries about destabilising speculation, underlay both the creation of the Bretton Woods system and the creation of the EMS.

So what can we say about this trade-off? Unfortunately, the evidence on exchange rates seems to cut both ways.

There was a time when many economists, both monetarist and Keynesian, accepted Milton Friedman's argument that flexible exchange rates would work smoothly, because rational speculators would act to stabilise rather than destabilise exchange rates. In the light of the evidence, however, Friedman's argument does not seem very reassuring: the traditional concern about irrational, destabilising speculation seems as relevant as ever.

On the other hand, until very recently, many economists (and policy makers) would have denied the usefulness of exchange rate flexibility, on the grounds either that nominal realignments do not produce real realignments, or that real realignments play no role in the adjustment process. Here again, however, the evidence seems to suggest that the traditional concerns remain as valid as ever.

In brief, flexible exchange rates are useful but unreliable; a fixed exchange rate regime may buy stability at the price of a lack of adjustment.

4.3 Exotic Proposals

One surprising aspect of recent thinking about exchange rate policy has been the re-emergence of what we might call exotic proposals for exchange regimes; proposals that try to reconcile the conflicting claims of fixed and flexible rates by adding other policy instruments, such as some form of capital controls.

The apparent justification for such schemes should be clear. Suppose that you are convinced that the foreign exchange market is unreliable, subject to irrational speculative bubbles. Then you may be unwilling to allow exchange rates to float freely, preferring at any given time to peg the exchange rate. Yet a permanently fixed exchange rate means abandoning the help that exchange rate flexibility can provide in adjustment. A seemingly useful compromise, then, is an adjustable peg: an exchange rate that is normally fixed, but adjusted when it seems clear that a change in relative prices is necessary.

There is, however, a major problem with such an adjustable peg system in a world of capital mobility: it is subject to massive speculative attacks whenever the market suspects that a realignment is in prospect. So what do you do? Economists have repeatedly come up with the same answer, which has now been revived in a more sophisticated form by Eichengreen and Wyplosz (1993): capital controls that discourage such speculative attacks.

It is astonishing, in a way, that such proposals are now resurfacing. During the 1980s, most countries developed a strong commitment to ‘clean’ foreign exchange policies: currencies were to be fully convertible, with the maintenance of any target exchange rate a job essentially for monetary policy; capital flows were a microeconomic issue and were not to be meddled with for macroeconomic purposes. Only a few years ago, the complicated schemes of the Bretton Woods era, with their capital export taxes, deposit requirements, and so on, seemed antique and foolish. What we have now discovered, however, is that the problematic trade-offs that motivated these schemes are still there.

My personal gut feeling - I cannot really call it a judgement - is that even sophisticated forms of capital controls are a mistake, except in crisis; that the microeconomic costs of such controls tend to grow over time and/or their effectiveness tends to be eroded. However, this may be more an aesthetic judgement, a preference for clean policies and a strict separation between macro and microeconomics, than a reasoned policy conclusion.

So let me conclude on a somewhat dispiriting note: recent thinking about exchange rate determination has taught us a great deal about how the world works, but has given us remarkably little guidance for policy. We are much better at explaining and quantifying the dilemmas of exchange rate policy than we were 20 or so years ago, but the dilemmas themselves remain much the same.

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