THE EFFECT OF FIXED EXCHANGE RATES ON MONETARY POLICY*

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Abstract:

To investigate how a fixed exchange rate affects monetary policy, this paper classifies countries as pegged or non-pegged and examines whether a pegged country must follow the interest rate changes in the base country. Despite recent research which hints that all countries, not just pegged countries, lack monetary freedom, the evidence shows that pegs follow base country interest rates more than non-pegs. This study uses actual behavior, not declared status, for regime classification; expands the sample including base currencies other than the dollar; examines the impact of capital controls, as well as other control variables; considers the time series properties of the data carefully; and uses cointegration and other levels-relationship analysis to provide additional insights.

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* E-mail: Jay.C.Shambaugh@Dartmouth.edu. I thank Maury Obstfeld for his support and suggestions in the development of this work. I also thank Barry Eichengreen, David Romer, Andrew Rose, Michael Jansson, Alan Taylor, Doug Staiger, participants at the Berkeley International Macro Workshop and Macroeconomics Seminar, and participants at many 2002 job market seminars for helpful comments on the preliminary stages of this paper. I also thank the John L. Simpson Memorial Research Fellowship in International and Comparative Studies for financial support. All errors are mine.

I. Introduction

In many ways, the choice of the exchange rate regime is the fundamental macroeconomic policy choice, especially for small open economies. The decision of whether to peg or not may determine monetary policy options and/or the ability to maintain open capital markets. Despite the importance of this choice, economists are not in agreement over the implications of fixing the exchange rate. Recent research has been mixed regarding whether any economy outside the four or five largest actually has monetary freedom. If not, then fixing the exchange rate does not generate a loss of monetary flexibility, as most countries would not have freedom even if they floated. This paper seeks to understand the effect fixing the exchange rate has on monetary policy by establishing the extent to which interest rates in pegged countries follow base country interest rates and whether this is any different for countries without fixed rates. The assumption being that the short term interest rate is a measure of monetary policy and that autonomy can be measured by movements in these rates.

The paper tests a basic proposition of international macroeconomics, the notion of the open-economy trilemma. This tenet says countries can pursue two of three options, fixed exchange rates, domestic monetary autonomy, and capital mobility. In a country with open capital markets and a credible fixed exchange rate regime, where there is no expected change in the exchange rate, the interest rate must equal the interest rate of the base economy, adjusting for differences in risk and liquidity in the investment options. If there were a deviation, investment funds should pursue the higher returns and force changes in the interest rate until parity is restored. Thus, a country which pegs its exchange rate cannot pursue domestic goals with its monetary policy; it has no autonomy. When the exchange rate is not fixed or capital markets are closed, countries should be able to set interest rates based on domestic considerations.

On the other hand, it is possible that capital markets are so tightly integrated that non-pegged countries also lack monetary freedom. This could occur if any interest rate policy, other than following the base interest rate, generated immediate exchange rate fluctuations beyond the amount most countries are willing to tolerate. This is the general thrust of the fear of floating literature started by Calvo and Reinhart [2002]. In this case, there is an open economy dilemma, not trilemma: the choice is to have monetary freedom or open capital markets. Since most countries have liberalized capital flows, or are in the process of doing so, this would suggest that few countries have monetary freedom in today's world. Under this scenario, all countries would display a tight connection to the relevant base economy; there would be no difference between pegged and non-pegged countries.

In this paper, I find strong empirical evidence that pegged countries do in fact follow changes in the base country's interest rate and that there is a significant difference between pegged and non-pegged countries. This suggests that there is in fact a tradeoff between electing to peg the exchange rate and the ability to make autonomous adjustments in monetary policy.

I use a sample of over 100 developing and industrial countries from 1973 through 2000. Rather than following the declared exchange rate regime reported to the IMF, I create a *de facto* coding system which focuses exclusively on the volatility of the exchange rate and divides countries into pegs and non-pegs. This two-way classification generates results that disagree with the reported IMF status only about 12 percent of the time. Most countries that claim to float do so at least to some degree, although clearly some are mislabeled. Thus, the paper does not dispute the core contention of the fear of floating literature. It is clear that some countries that claim to float, in fact, keep a fixed exchange rate. Also, for those countries that in practice allow exchange rate flexibility, I do not enter into the debate on the extent to which they float when coding regimes. The argument of the paper is that some countries do float, at least to some extent, and these countries have more monetary autonomy than pegs.

Other *de facto* coding systems focus on reserves changes or parallel market considerations. A pegged country need not intervene with reserves, though, and some floats intervene at the margin. Also, the central bank makes no official commitment to the parallel exchange rate, so if capital controls decouple the parallel rate from the official, monetary policy is not forced to try to stabilize this rate. Thus, I argue in section IV, that a system focusing on official exchange rate behavior is the appropriate one for the issue at hand.

Once it is established which country/year observations should be considered pegged and which currency is the relevant base currency for each country,[1] I examine the interest rate behavior of pegged economies compared to non-pegged economies. While Frankel *et al* [2000, 2002] and Hausmann *et al* [1999] have recently argued that floating regimes do not provide monetary autonomy, I find that countries with fixed exchange rates do in fact follow the interest rate of the base country more closely. Due to strong persistence in the levels of the interest rate series (unit roots or near unit roots), I focus on the differences in rates. I test the effects of changes in the base interest rate on changes in local interest rates, and find that pegged countries' interest rates respond more to base interest rate changes.

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[1] The base country is the economy to which a country is pegged, or, for non-pegged countries, the economy to which they would peg if they did so. Exchange rates against all major currencies as well as important regional currencies are tested to insure that countries are compared to the correct base currency. The choice is described in Section V.

The results show that pegged economies have an elasticity of roughly .5 and non-pegged economies of roughly .3. When countries whose interest rates never change are removed from the sample, the difference between pegged and non-pegged countries rises to .3. In addition, the explanatory power of the regression is stronger for the pegged countries, and virtually none of the non-pegged country variation is explained by the base rate. This implies the non-pegs have significantly more room for autonomy.

Since the trilemma implies the relationship may only hold if capital markets are open, I also consider the effects of capital controls on these results. As expected, both a fixed exchange rate and open capital market increase the responsiveness to the base interest rate. One may be concerned that other confounding factors such as common shocks to pegs and the base could bias the results. Attempts to control for time or trade share, not explored previously, do not alter the general conclusions.

Finally, just as theory predicts a pegged country's interest rate will respond to changes in the base interest rate, it predicts that there will be a long run relationship between the interest rate of a pegged country and the base interest rate. I use a technique developed by Pesaran, Shin, and Smith [2001] which allows an examination of levels relationships and dynamic rates of adjustment without definitive knowledge of the order of integration of the variables. This method shows stronger levels of significance and much faster rates of adjustment for pegs and countries with open capital markets. Also, cointegration analysis is used on the levels of local and base country interest rates to see if the two non-stationary series are in fact stationary when combined. It is more likely for pegged-rate countries to be cointegrated with the base interest rate than for non-pegs or sporadic pegs, and, again, the rate of adjustment to shocks in the foreign interest rate is faster in pegged countries. These results both support the previous results using different assumptions about time series properties and show that even if some floating countries follow the base in the long run, they seem to have more autonomy in the short run than pegs.

In summary, this work suggests that the open-economy trilemma is alive and well. Countries cannot have fixed exchange rates, domestic monetary autonomy, and open capital markets all at once. Fixed exchange rate countries either by choice or out of necessity cede much of their monetary policy autonomy to the base country. Floating rate countries' interest rates are correlated with the base country to some extent, but not as much as fixed exchange rate countries' rates. Some recent research has claimed that even floating economies do not have monetary flexibility and thus there is no trilemma, only a dilemma. The empirics of this paper disagree and suggest that floating economies are substantially less tied to a base country rate than their pegged counterparts, giving non-pegs substantially more room for autonomy than pegs.

The next section discusses the way the exchange rate regime can affect policy. Section III reviews previous empirical work studying fixed rates. Section IV establishes which countries are pegged. Section V discusses the empirical results, and Section VI discusses the implications of the results and concludes.

II. Effects of Fixed Rates on Monetary Autonomy

1. Comparing Pegs and Non-pegs

To understand how the base interest rate should affect local rates in both pegged and non-pegged economies, we can use the interest parity equation. When capital markets are open:

$$(1) \qquad R_{it} = R_{bit} + E_t[e_{t+1} - e_t] + \rho$$

where R is the domestic nominal interest rate, R_b is the base country interest rate, e_t is the log of the nominal exchange rate, and ρ is the difference in risk on the two assets. If investment opportunities in two countries are equally risky, then ρ equals zero and the interest rate differential should equal the expected change in the exchange rate.

For our purposes, it is not crucial that uncovered interest parity hold in all cases. The important condition arises when there is a credibly pegged exchange rate. In this case, $E_t[e_{t+1}] = e_t$, and any risk associated with currency volatility is removed. Thus, we see that the local interest rate must equal the base rate plus any risk differential. If the risk differential is extremely small or does not change with the change in interest rates, (a constant risk premium or one with shocks not correlated with changes in R_{bit} would suffice):[2]

$$(2) \qquad \Delta R_{it} = \Delta R_{bit}$$

The base interest rate is assumed to be exogenous and is set based on shocks to the base economy. The local rate, though, must follow all changes in the base rate. Given this, we could test the equation:

$$(3) \qquad \Delta R_{it} = a + B^{*}\Delta R_{bit} + u_{it}$$

The coefficient B should equal 1 if the peg is credible, based on equation *(2)*. If there are changes to the risk premium, or there are expected changes in the exchange rate because the peg is not credible, then *(2)* becomes:

$$(4) \qquad \Delta R_{it} = \Delta R_{bit} + \Delta E_t[e_{t+1} - e_t] + \Delta\rho$$

The essential thing about a fixed exchange rate is that e_t cannot change, meaning the third term becomes $\Delta E_t[e_{t+1}]$. Thus, the local interest rate must change whenever the base rate changes, whenever the expected future exchange rate changes, or when the risk premium changes. This means that the local rate must still move one for one with the base rate, all else equal, but it may move for other reasons as well. If investors doubt a peg will last, R must rise to compensate for this, or the peg breaks. Now if we test the equation in *(3)*, the estimated B coefficient is:

$$(5) \qquad 1 + \frac{[Cov(\Delta R_{bit}, \Delta E_t[e_{t+1}] + \Delta\rho)]}{[Var(\Delta R_{bit})]}$$

If increases in the base rate make investors doubt the peg's stability, then the B coefficient could be greater than one (due to positive correlation of ΔR_{bit} and $\Delta E_t[e_{t+1}]$). Alternatively, if base rates tend to fall during times of global uncertainty, ΔR_{bit} and $\Delta E_t[e_{t+1}] + \Delta\rho$ may be negatively correlated. It would be ideal to include measures for $\Delta E_t[e_{t+1}]$ and $\Delta\rho$ in the estimated regression, but they are unobservable.

A final possibility for pegged rates is that the exchange rate is not pegged precisely but floats within small bands, allowing the spot rate to change by small amounts without ending the fixed exchange rate regime. This will allow some limited autonomy. Svensson [1994] details a model which demonstrates that allowing the nominal exchange rate to change within bands reduces the degree to which the local interest rate must precisely follow the base rate. If the exchange rate floats within bands, $\Delta E_t[e_{t+1} - e_t]$ does not have to equal zero even when the peg is credible. Now, e_t can change by the amount of the band. If the band is +/-2 percent, and the exchange rate begins at the edge

[2] The work of Frankel and Froot [1987] seems to suggest most of the bias in a forward market or any difference in R and R_b can be better explained by expectations, not a risk premium, making this a reasonable assumption.

of the band, the nominal exchange rate could change by as much as 4 percent. This does not provide long-term monetary autonomy, as policies must be maintained which keep the central parity credible, but in the short term, the movement of the exchange rate allows the pegged country some latitude.[3]

If the local country uses this flexibility to smooth interest rates, that is, if they allow instant depreciations causing an expected appreciation after an increase in the base rate, the base interest rate and the expected exchange rate change will be negatively correlated. By *(5)*, we see that this should lower the estimated *B* coefficient. In addition, costs to arbitrage may allow small differentials even if there are no bands. In this case, a country may only partially adjust to changes in the base interest rate generating a *B* less than 1.

Under a floating exchange rate, we still can use equation *(1)* to discuss the way the rates are connected. In this case, the local interest rate does not have to respond to changes in the expectations, but can instead allow the spot exchange rate to adjust immediately so that the expected change in the exchange rate is equal to any interest rate differential that develops. That is, we can rearrange *(1)* to say

$$(6) \qquad E_t[e_{t+1} - e_t] = R_{it} - R_{bit} - \rho$$

Under a fixed exchange rate, if the future exchange rate changed due to long-run factors, the local interest rate had to adjust so that the spot exchange rate stayed constant. Now, the local interest rate can be set and other factors can adjust to it. There is no reason for the local interest rate to react to the base rate or to expectations or to the risk premium. Uncovered interest parity has not usually succeeded when tested empirically,[4] but we see that it is not necessary for it to hold for this result to arise. The only important point is that unless the exchange rate is fixed, nothing forces the local rate to follow the base rate.

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[3] For example, if originally the exchange rate is in the middle of its band, and the foreign interest rate rises by 2 percent, one possible outcome would be for the pegged country to allow the exchange rate to instantly depreciate to the top of the band generating an expectation of a 2 percent appreciation over the next year (back to the center). In this case, the interest rate on a one-year maturity asset in the local country can now stay 2 percent below the base rate and the expected return on the local and base assets is the same. The possibility that the exchange rate could be expected to move by the entire width of the band (4 percent) within a short period, say three months, means there could be a difference of 4 percent in the rate on three-month maturity assets. When those rates are annualized, the difference could be up to 17 percent. This implies that the differential on overnight rates becomes essentially unbounded. It is unlikely, however, that the exchange rate will be expected to make such large moves if the peg is credible, meaning such wide differentials are unlikely to appear in practice

[4] See for example Froot and Thaler [1990].

On the other hand, there may be other reasons that base and local interest rates are correlated, generating a non-zero *B* coefficient. The change in the exchange rate may enter the monetary policy rule as some countries may choose to limit exchange rate volatility, and there may be common shocks. We could say, then, that monetary policy is a function of the expected change in the exchange rate and domestic shocks. For pure pegs, the weight on domestic shocks is zero. Even countries without any intentional exchange rate policy, though, will most likely respond to changes in the exchange rate because the exchange rate will be one of the factors driving domestic inflation, and as such, would be a part of most monetary rules.[5] Thus, many floats will show some reaction to foreign shocks and may not appear to be pure floats. Even for pure floats, if the shocks facing each country are similar, we could expect the interest rates to still be correlated. Then, an estimate of *B* would simply equal the correlation of base and local shocks. In addition, though, we would expect a much lower R^2 because so many other factors can drive the local rate.

Alternatively, despite the lack of a formally or rigidly fixed exchange rate, the local country may try to minimize exchange rate movement. In this case, the weight on ($E[\Delta e]$) in their policy rule rises. As it gets larger, floats may look very much like fixed exchange rates, or more likely, like non-credible fixed exchange rates. In addition, one could say that exchange rate expectations could be set in such a manner that if the base rate changes and the local does not follow, there is a large shift in the expected future exchange rate. This would generate a large change in the spot rate as well. Under these conditions, we might expect a "fear of floating" type behavior in which local rates move with base rates to minimize exchange rate volatility. Since the policy may lack credibility, we could see a case where floats react to the base rate more than pegs.

Thus, the standard theory suggests that non-pegged countries should have more monetary autonomy than fixed exchange rate countries. In terms of a regression like *(3)*, that implies that the R^2 and the *B* coefficient for pegged countries should be significantly larger than for non-pegged countries. If a peg is rigid (no bands) and perfectly credible, capital markets are open and arbitrage costless, risk premiums constant, and investors are optimizing, we would expect the coefficient and R^2 for the pegs to be 1. For non-pegs, standard theory would imply a *B* driven by the correlation of shocks and a very low R^2. Alternatively, recent arguments regarding fear of floating or a general lack of autonomy for

[5] Parrado and Velasco [2002] show that optimal monetary policy response to a foreign interest rate shock for a small open economy is to partly adjust, ward off some of the change in the exchange rate, but not entirely. Svensson [2000] shows that depending on the type of inflation targeting pursued, the reaction function coefficient on the foreign interest rate could be quite close to one.

non-pegs as well as pegs would imply that non-pegs should look a great deal like pegs, with high R^2 and *B* coefficients.

An important caveat is that all these results rest on capital mobility. If interest rates are set administratively or if there are barriers to international capital movements, nothing forces $\Delta R_{it} = \Delta R_{bit}$, and thus there is no reason for pegged economies to lose monetary autonomy. This is the logic of the open-economy trilemma. Countries with open capital markets cannot pursue domestically oriented monetary policy and fixed exchange rates at the same time. On the other hand, if capital markets are closed, the two are no longer incompatible. Thus, we should only expect these results to hold for countries with open capital markets.

In addition, it is possible that the exchange rate regime is not the driving factor behind the coefficient *B*. Instead, the correlation of interest rates for all countries, not just non-pegs, could be driven by common shocks. Thus, it is important to consider factors such as measures for common shocks. Otherwise, we may mistakenly attribute differences in the *B* coefficient or R^2 to the exchange rate regime when they arise due to differences in the correlation of shocks.

Before proceeding to estimation, it is necessary to consider some time series issues which have implications for the methods of estimation.

2. Time Series Issues

Estimation of equation *(3)* or a similar equation written in levels is influenced by the time series properties of the data. As with many time series of macroeconomic data interest rate data are often treated as if the series have a unit root.[6] Because nominal interest rates are bounded below by zero and do not go to infinity in practice, we know they are not pure unit roots, but they appear close to unit roots in finite samples.[7] In tests presented later, for most of the interest rate series used in this paper, I cannot reject unit roots in the data using augmented unit root tests such as those suggested by Elliot, Rothenberg, and Stock [1996]. Furthermore, I can reject stationarity for most of these series using a KPSS test. These tests, though, may not be entirely accurate. Unit root tests have low power, and even the test of the hypothesis of stationarity may miss important properties of interest rates. As Caner and Kilian [2001] discuss, the stationarity tests may reject the null too often if the true model is a highly persistent process, a near unit root.

[6] See Wu and Zhang [1997] and citations therein.
[7] Stanton [1997] shows that mean reversion is close to zero when interest rates are within a central band, but that when interest rates are extremely high, they do exhibit mean reversion. Likewise, the fact that nominal interest rates are bounded below by zero means that they must exhibit some mean reversion at extremely low rates.

Spurious correlations can arise between two independent integrated series.[8] Thus, if the data in question have unit roots and are not cointegrated, regressions on levels may generate incorrect results. Differencing the data removes the problem of spurious correlations and is the recommended procedure under this assumption. Furthermore Phillips [1988] discusses the fact that in finite samples, near-unit root processes are actually closer to the asymptotic theory for non-stationary processes than for stationary processes. He demonstrates that the issue of spurious regressions arises for near unit root processes just as for unit root processes. Once again, differencing the data and proceeding with standard estimation techniques should yield sound results.[9]

Alternatively, the data may have unit roots and the base and local rates may be cointegrated. This presents additional issues. An error-correction representation would be the proper form. If we assume R_{bit} is set based on its own shocks, and thus is exogenous to R_{it}, we can write:

$$\Delta R_{it} = \theta(c + R_{it-1} - \gamma R_{bit-1}) + B\Delta R_{bit} + u_{it} \quad (7)$$

(including lags of ΔR_{bit} and ΔR_{it} as necessary)

This representation allows us to examine the short run reactions (the B coefficients) but also take into account the forces generating a long-run return to equilibrium. The θ parameter describes the speed of the adjustment back from any deviation from the long-run relationship. Note that the first term on the right hand side would be missing from a simple differences regression and is equal to u_{it-1}. This means that a regression on differences alone would lose information regarding long run trends, mix short run and long run effects, and encounter an autocorrelated error term. It should be noted, though, that this error-correction form is problematic when the variables are not cointegrated. By definition, if the variables are not cointegrated, $(R_{it-1} - \gamma R_{bit-1})$, is non-stationary.

The first implication is that if the interest rates are unit root series or close to unit roots, the estimation of a levels regression may yield spurious results. Second, though, if the series are unit roots, then the proper estimation technique will be different depending on the cointegration properties of the data. This is problematic if one believes that a pegged country's interest rates will be cointegrated with the base country's interest rates, but the non-pegged country's will not. Under those

[8] Granger and Newbold [1974] and Phillips [1986] show that the use of data with unit roots may generate spurious correlations between two independent integrated series, and that, in general, unit roots cause inference problems for standard statistical testing. Granger and Newbold demonstrate through simulations that significant results are the norm, not the exception, when examining two independent integrated series, and that very high R^2 and low Durbin Watson statistics are to be expected from such regressions.

assumptions, the proper estimation technique is different for the two samples making comparison difficult.

A methodology developed by Pesaran, Shin, and Smith (PSS) [2001], and used in Frankel *et al* [2002] offers one more approach. One can estimate equations in the error correction form and examine the statistical significance of the coefficient on the levels relationship (θ in the equation above). They provide different critical values for I(0) and I(1) processes. The advantage is that if the test statistic lies above or below both values, one can assert whether there is statistically significant evidence of a levels relationship without having to take a stand on the order of integration.

I proceed in the following manner. First, given that the series may be only close to unit roots, or at least may be non-stationary but not cointegrated, I difference the interest rate series and test the data with standard regression techniques. Second, I pursue a PSS analysis to examine the statistical significance of the levels relationship and the speed of adjustment. Finally, I test for cointegration. If pegged countries are more likely to be cointegrated, that is further evidence that they are more likely to follow the base country closely. In addition, I use the error-correction form for the cointegrated countries to examine the dynamics of the system. The PSS and cointegration techniques are close with the PSS technique allowing us to consider both I(1) and I(0) settings. I examine the cointegration status as well to allow a variety of testing frameworks and assumptions beyond those in equation (7). The differences based OLS tests do not factor in dynamics which may vary strongly across countries. By studying annual horizons, we hope the dynamics have largely settled and we can pool the data across countries. Thus, these results allow for more broad based generalizations rather than restricting ourselves to single country dynamic studies as with the PSS or cointegration tests.

III. Review of the Literature

Over the years, there have been numerous studies comparing the macroeconomic effects of the exchange rate regime. More recently, researchers have examined the way the exchange rate regime affects monetary policy flexibility. Calvo and Reinhart [2002] have shown many declared floating countries limit exchange rate flexibility and may not have or use the autonomy attributed to floating rates. Other authors have more specifically studied whether pegs or non-pegs have more autonomy over interest rates. In a widely cited paper, Frankel *et al* [2000] find that at first glance, theory holds and pegged countries follow the base more than intermediate or floating countries. They say, though,

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[9] Banerjee *et al* [1997] recommend treating series which are close to unit roots as if they had unit roots. Thus differencing helps avoid the problems described of spurious results. Differences also appear to generate more precise

that on closer analysis, it seems that the link is not strong, and in the 1990's it is not present at all. They state that one "cannot reject full transmission, even for countries with floating regimes," and that no countries outside of the major few really have or at least pursue monetary independence. Frankel [1999] provides early versions of these regressions on individual Latin American countries in the 1990's and comes to the conclusion that the more firmly pegged, the smaller the reaction to changes in the U.S. interest rate. He notes, though, that there may be unit roots in the data and shows regressions on differences which are less supportive.

However, the main regressions in Frankel *et al* [2000, 2002] are run on levels of interest rate data at the monthly frequency, and may encounter the possibility of spurious results.[10] These time series complications make interpretation of the results unclear. Second, the *de jure* classification system is used on a sample in which few countries are declared pegged (500 out of 9000),[11] and countries in the pooled regressions are compared to the U.S. interest rate despite the fact that some countries may follow a different base interest rate.[12] In addition, in recreations of the results, when hyperinflations in Argentina and Brazil are removed from the sample, the results can change.[13]

Frankel et al [2002] avoid the time series issues in the second section of their paper by applying the PSS technique. They examine twenty developing and industrial countries in the 1990s to see if there is a consistent levels relationship and test the speed of adjustment to foreign interest rate shocks.[14] They find slower adjustment for non-pegs implying more monetary autonomy, but still find levels relationships significant and close to one for all countries except Germany and Japan. They do not note, though, that in their results for floating developing countries, the t-stat only rejects the null of no levels relationship under the assumption of I(0) data. If the data are non-stationary, or close enough to it that the I(0) critical values are inappropriate, their results show no significant long-run

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estimates than levels regressions on near integrated data.

[10] DW statistics are not reported, but in this author's recreation of the results, the pooled samples show R^2 on the order of .7 and DW statistics below .6 implying the results may be spurious. Recreations use an almost identical sample and use the same techniques. Frankel *et al* report Newey-West standard errors which control for autocorrelation. The problem, though, is not autocorrelation generating incorrect standard errors, but unit roots or near unit roots in the data generating incorrect results overall.

[11] In the Frankel *et al* [2002] version, they test using LYS coding as well. See the next section and the Appendix for a discussion of problems with using the LYS coding for this type of analysis.

[12] Frankel *et al* test European countries against the German interest rate in individual regressions, but the pooled regressions use the U.S. interest rate for all but the 1990s.

[13] A recreation of the results using *de facto* coding and relevant base interest rates as opposed to only the Unite States generates the result that pegged countries follow base interest rates more closely (.71 vs. .33), but the DW statistics are below .6 implying the results are tenuous.

[14] This paper was originally completed before Frankel et al [2002] was published and independently had used cointegration analysis and error correction estimation to test long run relationships and speed of adjustment.

relationship for the developing floating countries. That is inconsistent with the general results of their paper and is more in line with the results of this paper.

In addition, if one simply re-classifies the countries studied to a *de facto* exchange rate regime standard, the results look even more supportive of the idea that pegging generates a loss of monetary autonomy. Thailand is considered an intermediate regime despite appearing fixed during the sample. When it is moved to the fixed rate group, there is a stark dividing line for the developing countries between fast adjusting significant fixes and slower less significant intermediate regimes and floats. Likewise, for the industrial countries, all EMS countries are considered intermediate, but some (Belgium, Denmark, the Netherlands) were pegged throughout most of the 1990s while others (Portugal and Spain) were not as consistent. While Portugal looks like the pegged countries, Spain is slow to adjust and one cannot reject no long run relationship under any assumption of order of integration. It does appear that pegs and non-pegs react differently to the base and that more countries than Germany and Japan have exercised monetary autonomy in the 1990s, and this is confirmed on a broader sample in this paper.

This paper will differ from the pooled Frankel *et al* [2002] results by using differences not levels and a *de facto* classification, and this paper generates directly opposite results. The contrast with the PSS section of Frankel *et al* [2002] is more one of interpretation, by focusing on I(1) critical values and more carefully considering the exchange rate status, as well as sample, by adding a large number of episodes. The PSS results in this paper and Frankel *et al* [2002] are more similar than the pooled results with both finding that pegs adjust more quickly than floats, but disagreeing about the long run coefficients for the floats with this paper finding far more countries with insignificant or negative levels relationships.

Goldfajn and Olivares [2001] update Frankel *et al* [2000] by including a measure for capital controls to help explain the different reactions of different countries. They find a negative, but statistically insignificant coefficient on the multiplicative term foreign interest rate times capital controls. While a sensible result, the analysis is on the same sample and same technique as Frankel *et al* and thus encounters the same problems discussed above.

Hausmann *et al* [1999] study a number of features of fixed versus floating regimes including their reaction to foreign interest rates. For 1997-9, they find that Argentina (the peg) reacts the least to U.S. interest rate changes, Mexico (the float) the most, with Venezuela (a weak peg) in the middle. They conclude that this is evidence that the monetary freedom associated with floating exchange rates does not in fact exist for all countries. In addition, they study real interest rate reactions for a total of eleven countries and find that pegged countries react to U.S. real interest rates less or equal to floating countries.[15] Again, though, the regressions are run on levels leaving the interpretation unclear. In addition, it is difficult to generalize about the effects of fixed and floating exchange rates on monetary autonomy from the small sample.

Borenzstein *et al* [2001], on the other hand, find some support for the general theory. They are answering a slightly different question, though. They avoid problems of spurious correlation by considering the changes in local interest rates but examine them in response to monetary policy shocks in the base, as identified through event studies or standard monetary policy VARs, not in response to interest rate changes. Their concern is that common shocks might generate the appearance of correlation. On the other hand, since the estimated shocks measure only unexpected changes and conceivably can identify a shock as a change in policy that did not occur when data (say rising inflation or an overheating economy) predicts that it should have, it is not clear that pegged countries should always respond to these shocks. Thus, their paper is an excellent test of monetary autonomy in the face of certain types of shocks, but does not answer the question of whether fixed rates enforce R=R* as theory says it should. Focusing on a small sample, they find that in general pegs respond to shocks in U.S. monetary policy more than floats, and conclude that their evidence is much more supportive of the notion that floating provides some monetary autonomy.[16]

This paper tries to clarify the role of the exchange rate regime in monetary policy. Before proceeding to estimation, I briefly discuss the exchange rate classification system which is used.

IV: Coding Exchange Rate Regimes

There are a number of ways to classify which countries have fixed exchange rates. The IMF tracks the declared status of all member nations. This *de jure* classification system has been the typical method used for coding which countries have fixed exchange rates for many years. Recently, though, some researchers have started to examine the *de facto* behavior of countries rather than their declared

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[15] Because real exchange rates may change dramatically for countries with fixed nominal rates or stay the same even when there are large swings in nominal exchange rates, it is unclear whether one would expect the exchange rate regime to predict the extent of correlation of real interest rates.

[16] They find Hong Kong (peg) reacts more than Singapore (non-peg) while Argentina and Mexico are more similar and both over-react to shocks. One concern might be that this is not a test of pegs versus non-pegs but of currency boards against intermediate regimes. They also add Australia, Canada, and Chile for comparison and find that Hong Kong has a much stronger reaction than the other industrial countries.

intent because many countries do not characterize their behavior accurately. Some countries do not declare a peg despite maintaining one, and some do not maintain a declared peg.

1. Options

The chief considerations when creating a *de facto* coding are the degree to which the exchange rate can move and the amount of evidence required that the fixed rate is the consequence of active policy not a lack of shocks. One can simply look at the change in the exchange rate itself to determine the degree of stability of the rate. Some, though, such as Levy-Yeyati and Sturzenegger (LYS) or Ghosh *et al*, worry that a constant exchange rate alone cannot distinguish between exchange rate commitment and a lack of shocks. One can use intervention data, both direct intervention in currency markets and changes in domestic interest rates, to insure that the government is actively managing the exchange rate. Unfortunately, since the hypothesis under consideration is the interest rate behavior under fixed rates, it seems inappropriate to use interest rate behavior to define pegs. Alternatively, one can set the length of time required to constitute a peg at a sufficient length that it becomes highly unlikely that shocks would not have changed the exchange rate were it not intentionally pegged.

Many previous methodologies, while useful for their own purposes, are not ideal for this study. Calvo and Reinhart [2002] examine floaters' behavior by comparing the volatility of the exchange rate, reserves, and the interest rate for declared floating countries to the volatility for the world's major economies that float relatively freely. Their methodology is quite useful for examining whether countries that claim to float are in fact pure floats as opposed to managed floats, but it does not provide as clear a message on how to code a country as fixed or not fixed. LYS group countries based on the change in the exchange rate, the change in the change in the exchange rate, and the change of reserves / M2. However, they classify countries as pegs even if the exchange rate changes as long as there is sufficient volatility of reserves. In addition, they drop countries with stable exchange rates that do not intervene with foreign reserves. These aspects of the system make it inappropriate for this study. The IMF itself has also changed the *de jure* system in the last few years to reflect country behavior more accurately. Fischer [2001] uses the new official *de facto* system to discuss current arrangements as well as IMF staff analysis of 1991 arrangements coded retroactively to provide a comparison.[17] The three categories described in Fischer [2001] would be inappropriate in this study since long-term pegs

[17] This classification divides countries into hard pegs (currency unions or currency boards), a broad intermediate range including pegs, and floating (managed or free). The rules for this classification are not explained; IMF staff judgment is used.

that are not currency boards are considered intermediate despite the fact that the market has reason to believe the exchange rate will not change.

Finally, Reinhart and Rogoff [2002] have created an exhaustive coding based on parallel exchange rates. While this is quite useful for the study of trade flows or other variables which depend on exchange rate stability, this method is not as useful for our purposes because exchange rate commitments are made with respect to the official rate. Whether the parallel rate changes or not, the constraint on monetary policy comes from stabilizing the official rate. Countries with fixed official rates and floating parallel rates have found another route (capital controls) to allow monetary freedom. In this paper, I choose to separate capital controls and exchange rate regimes to more explicitly look at the predictions of the trilemma. In addition, Reinhart and Rogoff base their classifications on the odds the exchange rate will deviate from bands over a moving five year window allowing more short term flexibility than in my system, which is not ideal if one wants to focus on countries which have limited exchange rate flexibility in a particular year.

For the purposes of this paper, it is important that countries coded as fixed have very stable official exchange rates. Investment flows force countries whose exchange rates do not move to keep interest rates in line with the base country. A large degree of intervention is irrelevant if the exchange rate is still quite volatile. Thus, I choose to focus on the stability of the exchange rate itself, much like Obstfeld and Rogoff [1995], and use a sufficient length of time to be considered a peg to eliminate the possibility of misidentifying calm rates as pegs. Finally, one could argue that whether the peg is intentional is, in fact, irrelevant and that using a *de facto* classification shows what the interest rate behavior must be for exchange rates to be stable whether that is a goal or not. While this system seems a logical choice for the issues in this paper, I also present results using the other classifications.

Below, I describe the exchange rate based procedure which is used in this paper. Moreno [2001] also focuses on the exchange rate alone, and while conceptually similar, the two methodologies differ in the details largely due to the different purposes for which they are used.

2. Classification Procedure

Similar to Obstfeld and Rogoff [1995], I focus on whether the exchange rate stays within a band. To determine the base country, I examine the exchange rate against the dollar, all major currencies, and major regional currencies to find any potential fixed exchange rate relationship. It should be noted that many previous studies assume the United States is the relevant base interest rate or currency for all countries. When using a larger sample, though, many countries peg to countries other than the dollar. When a country pegs or occasionally pegs, determining the relevant base currency is simple. It becomes more difficult to assign a relevant base for non-pegged observations of countries that generally float, do not peg for a substantial amount of time, or switch base currencies. In these cases, judgment was used, and I chose a currency with historical importance for the local country, the nearby dominant economy to which other currencies were pegged, or the dollar as a default if nothing else was clear.[18]

For annual classifications, I determine if the exchange rate stayed within +/-2 percent bands against the base currency.[19] In addition, to prevent breaks in the peg status due to one-time realignments, any exchange rate that had a percentage change of zero in eleven out of twelve months is considered fixed. Using 155 countries from 1973-2000, I have 4338 country/year observations with exchange rate data, and 2220 are coded as pegged. The decision of 1 percent compared to 2 percent bands and the decision to include single peg breaks do not influence the results substantially. 53 percent of the country/year observations coded as pegs show no change at all over the entire year, and another 34 percent are within 1 percent bands. Only 9 percent are between 1 percent and 2 percent bands, and only 4 percent are realignments. In addition, single year pegs are dropped as they are quite possibly not intentional pegs. When the data are differenced, the first year of a peg is dropped so as not to difference from peg to non-peg.

For monthly samples, the test is similar, simply examining if the exchange rate is within +/2 percent bands over the last 12 months. To avoid coding as pegged the occasional month that qualifies, pegs must be at least 6 months long. Once again, if only one month in the last 12 has a non-zero percentage change, then the observation is still coded as pegged, and again, this is only 4 percent of the observations.

3. Comparison to other classifications

This system generates results which are quite similar to the IMF *de jure* coding, only 12 percent of the observations show up as incorrectly declared (see table I below).[20] Many of the incorrect declarations are basket pegs that are really one country pegs as can been seen by the fact that

[18] This is, in fact, rarely a problem because so many countries peg on and off to a particular base currency, but there were a few difficulties. I compared my results to LYS for countries they have in their sample and referred to the IMF exchange rate arrangement descriptions and the Global Financial Database reference guide [Taylor, 2000] which includes a history of all currencies.

[19] Technically, it tests whether the max and min of the log of the exchange rate are within .04. While Obstfeld and Rogoff tested that the same bands were maintained for as long as a country was pegged, this methodology tests only that they are within 2% bands in a given year.

131 of the mismatches are baskets which should appear to float against any one country, but are actually *de facto* pegs. Thus it appears the declared status is not as bad an indicator as some have claimed. By and large, countries that claim to peg do so, and countries that claim to float do as well. This also demonstrates that the concern of identified pegs being random and resulting from a lack of shocks seems overdone. 85 percent of the *de facto* pegs are also declared, and if one includes declared basket pegs, 91 percent are declared.

The differences to other de facto classifications are also not large. Comparing my system to the Reinhart and Rogoff system, 19 percent of the observations are classified differently while 16 percent of the observations are classified differently from the LYS system (comparisons with the LYS and Reinhart and Rogoff systems are discussed at greater length in the appendix). Thus, the simple official exchange rate based classification system described above is used because it seems the most appropriate for determining constraints on monetary policy, but this classification is not radically different from the others.

I now test the effects of the exchange rate regime on monetary autonomy using this classification procedure.

V: Empirical Results

1. General Results

Before examining regression specifications, simply looking at the interest rate differential between local and base interest rates is informative. Table II demonstrates that the means and standard deviations of the differential (expressed $R_{it} - R_{bit}$) for the annual interest rate series are both smaller and more stable for pegged rates using the 103 countries with available interest rate data. The differential is positive, on average, implying that base countries tend to have lower interest rates. This simple exercise suggests that the interest rates of pegged countries are more stable, closer to the base interest rate, and move in a more coordinated fashion with the base interest rate when compared to non-pegged countries. Furthermore, the gap widens in the 1990's in contrast to arguments, made by Frankel *et al* [2000,2002] among others, that floating countries follow base rates more today than in the past.

Since different countries respond to changes with different lags, pooled samples with high frequency data yield highly imprecise estimates. After a year, more of the dynamics have settled and

[20] The figures cited are for the entire 155 country sample. The smaller sample of country year observations for which interest rate data is available also yields disagreements in 12% of the observations.

short term differences in adjustment are less problematic. Thus, for the pooled sample differences specifications, I use the annual data. The first specification tests equation *(3)* using OLS.

$$(3) \qquad \Delta R_{it} = a + B^{*}\Delta R_{bit} + u_{it}$$

The specification was run for the entire sample of 1920 country / year observations as well as for various sub-samples. The inclusion of fixed country effects had little impact on the results, as a non-zero constant would imply a constant rate of change in the level of the interest rate. This is unlikely to appear over a thirty-year sample. Given that the data are panel data, both heteroskedasticity and serial correlation could be problems. Since the data are differenced, though, serial correlation seems to be absent with DW statistics being slightly above 2 in all the subsequent regressions reported. I report robust standard errors because the data are cross-sectional, but the uncorrected standard errors are always extremely close to the robust standard errors. The results are shown in table III below.

If non-pegs $B > 0$ and $R^2 > 0$, that is evidence of fear of floating or common shocks. If the coefficient and R^2 are higher for the pegged sample, that is supportive of the exchange rate constraint posited by the trilemma. As can be seen from the positive and statistically significant *B* coefficients, both pegged and non-pegged interest rates are correlated with the base country interest rate either due to common shocks or policies of intentionally following the base country. As the trilemma predicts, pegged countries have a higher coefficient and higher R^2 than non-pegs (.46 vs. .27 and .19 vs .01) with the non-peg point estimate below a two standard error band around the peg point estimate.[21] In the 1990's, the overall sample is more tightly integrated, but again, fixed countries follow the base country more closely, and, once again, in the 1990's, the pegged sample has a higher R^2. The non-peg coefficient in the 1990's is not estimated very precisely and is not significantly different from zero. Developing country non-pegs in the 1990's, the group that some say have no autonomy, have very little connection to the base (B = .17, std error = .34, R^2 = .00), a result counter to fear of floating arguments. [22]

[21] In addition, analysis was often run after removing some non-market interest rates from the sample. Some countries have perfectly constant interest rates over long periods of time. Given that these are most likely set administratively rather than through a market, these rates may not be relevant in the economy in question and certainly will not respond to economic forces. Excluding these countries, the results for pegs become (B = .59, std error = .04, R^2 = .26) and for non-pegs (B = .28, std error = .08, R^2 = .01).

[22] When the money market and treasury bill samples are separated, the money market sample has higher coefficients for all sub-samples when compared to the treasury bill data. This may be a function of capital markets being more open in the countries which have money market data or may simply mean these rates respond more closely to the

It is noteworthy that neither *B* nor the R^2 for the pegged sample is close to one. Clearly the parity condition is not enforced perfectly. The next section demonstrates that capital controls are part of the reason for this result, but it may also be weaker impediments such as information asymmetries or transaction costs. As discussed, bands around the exchange rate peg or changes in risk premia or exchange rate expectations could also explain the incomplete pass-through into pegged countries' interest rates. Finally, it may be that some countries that peg try to exercise monetary policy inconsistent with the base despite the incompatibility with successfully maintaining the peg. Many pegs in the sample break quite often, perhaps because of a refusal to subordinate domestic goals.

The extremely low R^2 for non-pegged countries shows that reacting to base rate changes is not a dominant part of the interest rate policies of non-pegged countries. In some ways, the R^2 is the more relevant statistic. Even if local rates for non-pegs react to base rates, if they can also change for other reasons, then there is room for autonomy. Floating may not insulate rates from all foreign interest rate shocks, but it would allow for the ability to direct interest rates to domestic purposes. The low R^2 does not explicitly imply autonomy as much as show that other factors are involved in determining local rates for non-pegs. These factors could be other external forces. It would be ideal to more formally model the interest rate process and see if countries were pursuing particular policies. However, most countries did not follow did not follow a simple Taylor Rule or some other monetary policy rule. Financing budgets, responding to terms of trade shocks and current account deficits, or attempting to stimulate the economy for political purposes were all factors in the policy process. This, combined with a lack of data, makes it difficult to explicitly determine the policy process, though such a project is worthy of further study.

2. Capital Controls

The open economy trilemma predicts a loss of monetary autonomy for pegged countries only if they have open capital markets, implying some measure of capital controls should be considered. The capital control data come from the IMF line E2, which signifies "restrictions on payments for capital transactions."[23] While this measure is clearly imperfect due to the fact that it cannot measure intensity

market and international arbitrage. Within each interest rate data sample, the gap between pegging and not is over .2 and the R^2 gap is .12 for the treasury bill sample and .31 for the money market sample. Theoretically, the shorter maturity on money market rates should allow larger and more variable spreads compared to treasury bill rates. Peel and Taylor [2002] discuss the fact that banks may be willing to commit arbitrage funds in short-term markets making covered interest parity holding more closely. This may be a similar effect.

[23] The data was generously provided by Michael Klein from the Klein and Olivei [1999] paper. I extended the data to cover countries not in their sample and to cover earlier years using the IMF exchange rate arrangements tables. After 1995, the IMF stopped reporting this series and reported disaggregated information. The series is extended for 1996-

of controls or controls on interest rates in particular, it has the advantage of being available across the entire sample. In addition, many more sophisticated measures rely on examination of correlation of returns with other countries or other measures which use the behavior of the interest rate itself.[24] Since I am testing the interest rate behavior, I prefer not to use the correlation of the interest rate to major world interest rates as a measure of capital controls.[25]

I segment the sample and consider four types of regimes: pegged countries with open capital markets, non-pegged countries with open capital markets, pegged countries with closed capital markets, and non-pegged countries with closed capital markets. Results are in table IV.

There appears to be either fear of floating behavior or a significant amount of common shocks because the non-pegs without capital controls (column 2) have a large *B* coefficient. At the same time, though, the R^2 is noticeably lower for the non-pegs, implying the base interest rate is not a dominant part of their monetary policy processes. Also, capital controls do not make the exchange rate regime choice moot; pegs with capital controls show stronger correlation with base interest rates and can be explained better by base interest rates than the non-pegs. Since the capital controls do not generally completely shut countries off from world markets but merely interfere with capital flows, this is not surprising. These patterns hold for the 1990's sample with the gap between pegs and non-pegs with no controls rising to .24. Coefficients for non-pegs with controls are very imprecisely estimated in the 1990's and are not significantly different from zero.

3. Pooling across regime type

The different regimes can be compared more systematically if the different regimes are included in one sample with multiple right hand side variables:

$$\Delta R_{it} = a + B_1 * \Delta R_{bit} + B_2 * Peg_{it} * \Delta R_{bit} + B_3 * noCapCon * \Delta R_{bit} + u_{it} \quad (8)$$

2000 using changes in the disaggregated coding and descriptions in the yearbook to determine changes in the binary codes.

[24] See Eichengreen [2001] or Edison *et al* [2002] for discussion of the different options. Edison *et al* also demonstrate that this declared version is quite similar to the coding of Quinn [1997]. Because it is available for more countries and years, I use the IMF coding.

[25] I also have used the measure for interest rate controls developed in Demirguc-Kunt and Detragiache, [1998]. This measure focuses more directly on whether interest rates are set in a market, and is thus quite useful (perhaps more so for the money market samples). Unfortunately, the sample is much smaller and reduces my number of observations to one-fourth the current size. Using the D&D control data shows similar, but stronger, results. In part, this stems from the sample, as the results for pegs and non-pegs are stronger using their sample. Open capital market pegs generate results of .94(.09) .49 while open capital market non-pegs generate .47(.14) .04.

where *Peg* is a dummy variable signifying a pegged exchange rate and *noCapCon* is a dummy signifying no capital controls. This regression presents the same information as a test of *(3)*, but allows easier tests of statistical significance of the exchange rate regime and capital control effects and allows us to add more controls. When B_1 is significantly different from zero, that is evidence of either fear of floating or common shocks. If B_2 and B_3 are different from zero, that is evidence supporting the predictions of the trilemma. Results are shown in Table V. Some specifications use the full sample, but in most, the observations with apparently non-market interest rates are removed because these observations seem to add more noise than information and to keep the interpretation of the B_1 variable straightforward.[26]

In all samples, B_2 is positive, and in the full time samples, it is significantly different from zero at conventional confidence intervals. When only the interaction with the exchange rate regime is included, B_1 is significantly different from zero, but when the capital controls are considered, B_1 shrinks and loses significance, implying that the closed capital market non-pegs do not significantly follow the base, and appear to have autonomy.[27] In some regards, the third column sums up the predictions of the trilemma. Pegging and having open capital markets leads to following the base more closely and without either, one barely follows at all. For the 1990's only samples, the coefficients stay largely the same as in the full sample, but the standard errors rise a great deal. This is either a sign that the relationship is weakening over time or simply that the smaller data sample is unable to speak as clearly. The B_1 coefficient does rise in the 1990s providing limited evidence that fear of floating or common shocks have risen, but the standard error rises equivalently making strong comments on this fact unreliable. As noted in the discussion of table IV, the non-pegs in the 1990's are estimate fairly imprecisely. Thus, it is not that we are unclear if the pegs follow the base, but that it is difficult to say with certainty that they follow more closely than the non-pegs because we have trouble saying with confidence how closely the non-pegs follow.[28]

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[26] These are the countries with constant interest rates over long periods of time. When the results are run including these observations, but including an interaction term to mark the questionable interest rate observations, the results are virtually unchanged.

[27] In addition, when dividing the sample into developing countries and industrial countries, industrial countries have a statistically significant B_1, implying links across all industrial countries, but also a significant B_2, implying that pegged industrial countries follow the base currency more. For developing countries, the B_2 coefficient is significant, but the B_1 is not, demonstrating less common shocks or fear of floating.

[28] Upon inspection of the data, it seems in a few non-pegged countries (which may have been unofficially pegging or pegging loosely), the money market rate over-reacted in large ways to base rate changes on a few occasions and was generally unstable. These incidents both raise the coefficient and the variance for the non-pegs. When they are included in pooled samples, they lead to higher standard errors for all coefficients.

When using this specification, we lose the ability to examine the difference in R^2 across sub-samples. We can, though, compare the residuals of the regression for pegs and non-pegs to demonstrate that there is a statistically significant difference in how well the base rate changes predict domestic rate changes. Taking the estimated errors from a regression in the form of (8), one can then regress the squared estimated errors on a constant and a dummy for peg or capital control status. The results show a negative coefficient statistically significantly different from zero at better than 99% confidence for pegs and no capital controls, alone or in combination. This implies errors for the pegs are smaller than for the non-pegs, and errors for the non-capital control observations are smaller than for those with capital controls. While perhaps less intuitive with regards to magnitudes than the R^2 results above, this shows there is more present in the errors for non-pegs, either other external shocks or some semblance of autonomy, and supports the findings on differences in R^2 presented above.

4. Adding More Controls

While the analysis appears to show a significant role for the exchange rate regime and capital controls in determining the importance of base interest rates to the local interest rate, as discussed above, numerous factors should affect the estimate of *B*. If pegged and non-pegged countries were identical, comparing the two samples is an adequate test of the impact of the exchange rate regime. In addition to capital controls, the level of industrialization seems to have an impact on the correlation of local and base rates, perhaps due to tighter integration. Thus, interaction terms of the change in the base interest rate times a dummy for level of industrialization ($Ind*\Delta R_{bit}$) is added. Common worldwide or regional shocks could also generate correlations in interest rates. To control for worldwide shocks, time dummies can be included. Previous studies which used only one base country are unable to control for time effects because the base country interest rate series and the time dummies will be collinear. In addition, shocks could be more localized. This may mean countries which trade with the base country more would follow the base country interest rate more tightly. Thus, trade share ($trd*\Delta R_{bit}$) is added to control for common shocks.[29] One may also expect that countries which are financially fragile or at the mercy of international capital markets are more responsive to base interest rate changes. Thus, I add a measure of financial exposure (external debt /

[29] I use the bilateral trade with the base as a percentage of all trade from the "World Trade Flows 1980-97" database from UC Davis and Statistics Canada. I do not use direct measures of shocks derived from GDP time series properties because if countries follow the same monetary policy, they may appear to have correlated shocks. The actual shocks and policy shocks are difficult to untangle.

GDP)[30] interacted with base rate changes (*FinExp*ΔR_{bit}*). Finally, as discussed earlier, some interest rate series are unchanged throughout much of the sample, most likely because they are not market rates, so I remove them some the sample.[31]

Thus, the final specification is:

(9)
$$\Delta R_{it} = a + B_1*\Delta R_{bit} + B_2*Peg_{it}*\Delta R_{bit} + B_3* noCapCon_{it}*\Delta R_{bit} + B_4*Ind_i*\Delta R_{bit} + B_5*Trd_{it}*\Delta R_{bit} + B_6 FinExp*\Delta R_{bit} + YearDummies + u_t$$

As can be seen in table VI, the B_2 and B_3 coefficients, signifying the importance of the exchange rate regime and capital controls, are in the same range as in the basic specifications without controls shown in table V. The B_1 coefficient moves even closer to zero, implying that developing countries that do not peg and have capital controls have almost no connection to the base interest rate. Again, this is not a wholesale rejection of fear of floating. Some countries may be afraid to float and appear as *de facto* pegs in this sample. On the other hand, it suggests that those countries that actually do allow the exchange rate to change do have monetary autonomy. Looking at column 1, we see that an average non-pegged developing country with capital controls would not respond much to base interest rates (.10). If the country were to peg, the response would rise by .4 to around .5; if it dropped capital controls, it would be around .8, if it were industrial, the response would be almost unity.[32] The addition of the industrial dummy has weakened the capital controls coefficient slightly, most likely because open capital markets in industrial countries are even more open than in developing. In trying to consider common shocks, we see that the addition of time dummies slightly lowers the size of the peg coefficient.[33][34] The addition of the trade variable appears to do little (column 3) unless time

[30] External Debt is defined as debt in foreign currency. The data for most countries is the World Bank series "External Debt" divided by the series for GDP. Some countries (mostly industrial) use the IFS series "Foreign Debt" divided by GDP. This series is foreign debt (foreign currency or owed to foreigners) owed by the government. For overlapping observations, the two series are correlated above .9. The IFS series tends to be a bit lower. I tried creating a different series which increased the IFS observations by .2 (the constant from a regression of one series on the other), but it made no difference in the results.

[31] Again, unreported results that simply control for these observations are largely unchanged.

[32] I also tested adding an interaction term of peg*capital controls to the table VI specifications. The coefficient tended to be insignificantly different from zero and generally negative, most likely because the effect is not purely additive; if pegs and capital controls each generate coefficients of .3, the combined seems to be around .5 (when time dummies are included).

[33] The time dummies are significant in some cases. For example in 1974, 1976, and 1980-2, it seems world interest rates went up. This matches expected results. The time dummies do not, though, seem to alter the main variables substantially. To see if fear of contagion and crises affected the results, dummies for crisis years (1982,95,97,98) interacted with the base rate were tried, but were always insignificant and did not change other variables.

controls are added as well (column 4). It seems that the trade coefficient is estimated very imprecisely, and when a combination of variables are added that lead it to have a non-zero coefficient, that can weaken other variables that are correlated with it.[35] In general, it appears that controlling for common shocks does not overturn the results.[36] In addition, in all specifications, the peg dummy generates a negative coefficient in a regression on the estimated errors, implying that the pegs' interest rates can be explained by the base rate with more precision than non-pegs'.

Financial exposure generates a negative coefficient, implying that more exposure to foreign debt leads to a smaller response to foreign interest rates. While seemingly counter-intuitive, the result is understandable. Developed economies that are very open to capital flows and have higher interest rate pass-through also have low foreign debt to GDP. Thus, low exposure tends to be correlated with high pass-through. The inclusion of this variable weakens the effect of capital controls, most likely because of the negative correlation between the two variables. A variety of alternative definitions of financial exposure were tried. Dropping the industrial countries or setting their financial exposure to zero did not change results, neither did including only foreign debt owed to the base country (this specification necessarily dropped industrial countries since the World Bank series is only available for developing countries). Finally, it is possible that countries with high short-term debt are leery of changing interest rates due to the effect on debt burden. Including only short-term debt changes the coefficient on financial exposure, but only by the amount that the mean of the variable changes, leaving the effect unchanged. Finally, I included short-term debt separately as an additional variable, but while it had a negative coefficient, the standard error was three times the size of the point estimate. In all specifications, the other variables are left unchanged from the specification listed.[37]

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[34] A variety of other measures to control for common shocks were explored. Adding a measure of world interest rates (average of United States, Germany, and Japan) instead of time dummies did little to the results despite the correlation of the base rate and world rate (because the United States and Germany are often the base country). Also, for non-U.S. based countries, pegs and non-pegs show a similar reaction to U.S. rates (a different proxy for world rates) but pegs show a much tighter relationship to the base.

[35] Another measure of susceptibility to common shocks would be distance. The distance measure uses the log of the distance of the center of one country to the next. This is typical of work using gravity models. The data are available on Andrew Rose's website. The expected sign on the coefficient is negative, showing that the farther away a country is, the less correlated its interest rate is with the base country. Coefficients are in fact negative, but generally not significant and are similar in their impact on the overall results to including trade controls.

[36] Still, the controls for common shocks are not perfect. Between time controls and world interest rate controls, it seems clear that the importance of the exchange rate regime is not driven by common world shocks. Localized common shocks are more difficult to estimate. Correlations of GDP or business cycles are influenced by policy coordination (the test of this paper) and we are left with trade and distance which seem to be imprecisely estimated in their effects.

[37] It may be worth noting that when financial fragility is included with the trade variable and time controls, the peg variable does not lose its statistical significance as it does in the column 4 specification.

Finally, column 6 shows the results for the D&D capital controls. As can be seen, the number of observations drops dramatically. The results are similar, but even stronger. Coefficients on peg and no capital controls are roughly .5 and significant at 99 percent.

Some may question whether the exchange rate regime is the relevant measure or whether it is simply a proxy for volatility of the exchange rate. The black and white definition of pegged and non-pegged may only be a surrogate for a continuous variable of volatility of the exchange rate. When volatility (standard deviation of percentage change in the exchange rate) is included without the peg variable, it is negative implying the more volatile an exchange rate the less correlated the local interest rate with the base. This measure, though, is not significant in most specifications and shrinks when the peg variable is included. In addition, if regressions are run on the divided sample of pegs and non-pegs, the volatility measure has almost no effect. This implies that it is the peg or non-peg variable that is important, and a continuous volatility variable is a proxy for the exchange rate regime, not the other way around.

5. Robustness to Other Exchange Rate Regime Classifications

While section IV argues that the regime classification used is the most appropriate one, the results are also run with alternate coding schemes; results are in table VII. The *de jure* coding leads to similar results, but somewhat weaker. The coefficient on the peg variable is not as large and is more easily diluted by various controls (comparable results are in the second column of table V and fourth of table VI). If one believes that many of the observations in the *de jure* coding are mislabeled, this is the expected result as some *de facto* pegs are coded as *de jure* floats, thus raising B_1 and some *de facto* floats are coded as *de jure* pegs, thus lowering B_2. When run on separate samples, the gap in the R^2 is also smaller than using the de facto classification.[38]

Alternatively, we could use the Reinhart and Rogoff classification, comparing their pegs to all other groups (from crawling pegs to freely falling). These results are in fact even stronger than the main ones presented in the paper. In part, this is due to the sample. Using the set of observations for which Reinhart and Rogoff codes are available, but using my coding yields stronger results than in the main paper. In addition, though, it is logical that Reinhart and Rogoff codes would generate stronger results as their pegs are pegged both for official and unofficial rates making it more likely that they

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[38] When claimed peg status is considered, it is interesting to note that countries that claim to fix to a basket show a moderate connection to the base currency when the first equation, *(3)*, is run (B=.18 (.08)), but with equation *(8)*, it is clear that the result is driven by those countries that actually peg to a single currency. For the declared basket countries, B_1 = .08 (.08) and B_2 = .67 (.15).

have somewhat open capital markets.[39] Finally, the LYS codes do not follow the pattern of the others. This too is unsurprising. Given that their pegs have been intervening, they may have tried to substitute following the interest rate of the base country with sterilized intervention. In addition, the general fact that LYS pegs may have somewhat volatile exchange rates means we may not expect the interest rates to be moving perfectly together.[40] Thus, it does not appear that the results are purely a creation of the coding used, but for a definition of a fixed exchange rate which requires the exchange rate to stay stable, we do find that pegs follow the base more closely than floats.

6. Individual Country Examples

As discussed, different dynamics make pooling countries using differenced monthly data inappropriate. At the same time, within countries pooling across exchange rate regimes would not make sense. We can, however, examine individual regime episodes. Three different types of country/regime episodes are examined. First, countries with pegs lasting for at least three years are considered long pegs.[41] Second, occasional or inconsistent fixed exchange rate countries are defined as countries with at least 3 separate episodes of a fixed exchange rate under 3 years each which yield a total of at least 30 months pegged. Finally, countries that do not peg for at least 10 years are considered non-pegs. The first year after and last year before a peg for these countries are dropped. The time required for floats is longer to avoid classifying the non-pegged interludes of occasional pegs as floating periods. This generates 67 pegs, 25 occasional pegs, and 32 non-pegs.[42][43]

Regressions using the basic specification of equation *(3)* but including lags of the change in the base rate generate results consistent with the annual differences regressions. On average, the sum of

[39] The Reinhart and Rogoff coding has a range of categories, and in fact, running the regressions on separate samples for each yields the predictable result that the coefficient (std error) and R^2 for the pegs, crawling pegs and managed floats line up as expected. Pegs [.58(.04),.27] are higher than crawling pegs [.49(.10),.08] which are higher than the managed floats [.20(.08), .01]. The other categories have small samples and are not estimated precisely, but adding them in with the managed floats makes it even less precise.

[40] Once again, the sample is an issue. Their sample is much smaller, and for that sample even using my *de facto* classification, the differences between pegs and non-pegs are weaker.

[41] Klein and Marion [1997] study 16 Latin American countries and find a median peg length of 10 months and an average of 32. Thus, it seems pegs of three years or more are fairly long. In addition, they find the probability of a peg breaking declines with time after 7-9 months. This implies that for the longer pegs, on average, in any given month, credibility is probably fairly high and the expected change in the exchange rate should be close to zero. This may not be true for the occasional pegs that peg on and off and last for a short time

[42] In 28 cases, similar episodes arise in both the money market and treasury bill sample because data is available for both (though, often the dates are different due to different data availability). Thus, there are 99 unique episodes. It is not uncommon for the money market and treasury bill episodes to yield somewhat different information, in part due to the different dates, so I include both. Finally, countries where the interest rate is unchanged for the entire period are eliminated. Once again, the assumption is that these rates are not representative of true market rates and are probably not the relevant rate to analyze for these countries.

the coefficient on ΔR_{bit} and its lags are higher, the R^2 is higher, and the sum of coefficients is significantly different from zero more often for pegs when compared to occasional pegs and non-pegs. Not all pegs follow the pattern; some pegs show almost no relation to the base rate, although these are often Caribbean countries with close to flat interest rates. The money market rates, where the interest rate is more likely to be set in a market related manner, show an even stronger pattern. The results for the sporadic peg countries are interesting. The fact that even when using the observations of only the time these countries are pegged, they generate lower coefficients and R^2 than long pegs either signals that refusing to maintain interest parity will force the peg to break over and over or that the market accurately did not trust these regimes and the expected change in the exchange rate was not equal to zero and thus the interest rate differential was not zero. If these regimes faced speculative pressure from time to time, it might have forced changes in the interest rate separate from those in the base country, lowering the coefficients and raising the standard errors.

Taking the estimates of the sum of the coefficients on ΔR_{bit} and its lags as a proxy for the responsiveness of local to base interest rates, we can then try to see what factors might explain the differences across country episodes. The candidates are the same ones as the controls added in the previous section. Thus the main specification is: (some variables are omitted in different specifications, as shown in the table).

10)

$$B = a + \kappa_1 {*} Peg + \kappa_2 {*}\, noCapCon + \kappa_3 {*} Industrial + \kappa_4 {*} Trade + \kappa_5 {*} FinEx + \kappa_6 {*} Peg {*} noCapCon + e$$

The occasional peg episodes use the full episode results, that is, the entire period over which they occasionally peg. The peg variable is 1 for pegs, zero for non-pegs and the percentage of time a country was pegged for the occasional pegs. There are some episodes where the coefficients are estimated less precisely than others. To counter this issue, I use weighted least squares, weighting by the inverse of the standard error on the sum of the coefficients.[44] The results are in table VIII. An alternative specification, using least absolute deviations instead of OLS, was tried generating results similar to those reported below (with the exception of the fact that the R^2 was noticeably lower for the

[43] Once again, we can consider alternate classifications. The coding of Reinhart and Rogoff generally supports the coding of the episodes. LYS, though, generally do not code the episodes in the same manner. See Appendix.
[44] To make the results comparable with table VI, I eliminate any episode that has a constant interest rate over part of the sample. Controlling for these episodes instead of eliminating them generates similar results.

LAD regression). Also, simply eliminating the seven episodes with large standard errors and running OLS generated similar results.

The results support the idea that the response of local rates to base rates is in large part driven by the exchange rate regime. The peg variable is unchanged in response to other dummies except in the 6th specification when the peg variable is included in an interaction term as well. Similarly, the capital control variable shows that open capital markets are a good indicator of correlation with the base rate. When the capital control variable is included, the level of industrialization becomes much less important suggesting the level of industrialization may simply have been a proxy for capital controls. The specification in column 6, in many ways, sums up the open-economy trilemma. The interaction of peg and capital controls is the important feature. The variables on their own essentially drop out.[45] The financial exposure variable is not available for many observations (almost one third) and is weak, though in this case positive. Again, using distance not trade shows little impact as does controlling for whether the base is the dollar or not or if the interest rate used is money market or treasury bill.

7. Levels Relationships and Dynamics

Rather than trying to estimate the relationship between the two series, we may instead try to first ascertain whether there is in fact a long-run relationship at all as well as try to understand the dynamics of that relationship. If the dynamics die down relatively quickly, our annual differences regressions should give us similar results to the long-run results. As discussed above, persistence in interest rate series makes simple levels regressions problematic and requires different techniques, and thus, I check the time series properties of the episodes. It is clear that the data are close to unit roots based on simply examining the autocorrelation coefficients. The average estimated coefficient for the local rates is .92 with a median of .96, while the average estimate for the base rates is .96 with a median of .97. Even if the data reject unit roots, it is clear the data are near unit roots and levels regressions on non-cointegrated variables could be problematic. The differenced series have an average of .08, median of .02, for the local interest rates, and an average of .22, median of .25, for the base rates.

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[45] I use the interaction term here instead of dividing the data into four separate dummies because the peg variable is not binary in this case, but includes the partial pegs.

Tests of both the null of a unit root in the form of Elliott, Rothenberg, and Stock [1996][46] and of the null of stationarity in the form of Kwiatkowski, Phillips, Schmidt, and Shinn [1992][47] support the contention that the data is I(1) or close to it. As noted above, Caner and Kilian [2001] have shown that the KPSS test may too often reject the null of stationarity if the true process is highly persistent. Since even if the interest rates are stationary, they are most likely highly persistent, this means that the KPSS test may signal non-stationarity too often. Thus, the results showing unit roots may be better interpreted as showing that the series are at the very least close to unit roots or act like unit roots over limited samples. There are 127 episodes tested (some episodes used in the OLS analysis had to be split due to breaks in the series), with a base and local rate for each. Only 7 of the 254 levels series are able to reject the null of a unit root using the ERS test and yet 204 are able to reject the null of stationarity using the KPSS test, implying at a minimum the data are highly persistent.[48]

While the evidence does not reject the assumption of unit roots, there are a substantial number of cases (40/127) where one series or the other cannot reject stationarity using KPSS, making it less clear whether treating the data as unit roots and pursuing cointegration analysis is necessarily appropriate. The PSS technique can be used on data which are either I(0) or I(1) making it quite useful in this circumstance.

As discussed, the PSS technique tests the equation:

$$\Delta R_{it} = \theta(c + R_{it-1} - \gamma R_{bit-1}) + B\Delta R_{bit} + u_{it} \quad (7)$$

(including lags of ΔR_{bit} and ΔR_{it} as necessary)

θ demonstrates the response to an interest rate differential; if R_{it} reacts to close an increase in the differential, then θ should be negative. One can examine the significance of the levels relationship based on the t-stat of θ and critical values provided by Pesaran, Shin, and Smith [2001] (table C2iii). In addition, one can examine the levels relationship based on γ, and the speed of adjustment based on θ. The larger in absolute value θ is, the faster the adjustment, with a value of -.5 implying the half life of a shock to the differential coming from the foreign interest rate is one month. A country without any monetary autonomy should have $\gamma = 1$ and θ rather large. I estimate *(8)* for each country regime

[46] I use the modified AIC to determine lag length as suggested by Ng and Perron [2001].
[47] KPSS tests are quite sensitive to the number of lags included in the test. I follow the advice listed in the original paper and use a measure between 4(T/100)^.25 and 12(T/100)^.25. I use 7(T/100)^.25 (implying lags of anywhere from 6-11 in my sample). Many of the results, especially those regarding stationarity of the differential, are actually invariant to lag choice of anywhere from 1 to 12 lags, but the sensitivity to the choice of lags remains a problem with the analysis.
[48] I also use augmented Dickey Fuller tests. These tests reject unit roots slightly more often (9% of the time).

episode, including lags based on the minimization of the Akaike information criteria, as is done in Frankel *et al* [2002].

The results show that pegs are more likely to reject the null of no levels relationship, have a coefficient close to one, and have a high adjustment speed. In short, they appear to have less autonomy. If one assumes the data are I(0), 37 percent of pegs reject no levels relationship at 95 percent confidence compared to 20 percent of sporadic pegs and 22 percent of non-pegs. If one assumes the data are I(1), the results are 24 percent, 16 percent, and 16 percent respectively.[49] As with all the results, we see that not all pegs react perfectly with the base, and many do not have statistically significant levels relationships. More notably, the estimated levels relationship is much closer to 1 for pegs. Pegs have an average value for γ of .93, compared to .68 for sporadic pegs and -.43 for non-pegs. The final result suggesting that on average, non-pegs move against the base country in the long run.[50] These disparities in exchange rate regime are more extreme than the estimated gaps based on pooled annual differences. In large part, this may be because these results are for sustained pegs and sustained non-pegs. Those pegs that last for long periods are more likely to be following a monetary policy consistent with the base country. Likewise, the pooled non-pegs include the non-pegged observations of countries that occasionally peg, making them more likely to follow the base than the long term non-pegs.

Most telling are the adjustment speeds. The average for pegs is -.19, for sporadic pegs, -.11, and non-pegs, -.06, implying half lives of 3, 6, and 10 months respectively. Looking at the implied half lives in a more disaggregated fashion (table IX) shows that almost no non-pegs react quickly to the base rate, while over 30 percent of pegs react quickly and 80 percent have half-lives of under a year. In addition, while Frankel *et al* claim that only Germany and Japan show true independence, these results show numerous non-pegged countries that cannot reject no levels relationships and have slow adjustment speeds.[51]

Finally, we can examine the results considering the capital control status as well. Table X shows that the only type of episode that generates a fast adjustment and levels relationship close to one

[49] These statistics do not include the few examples where there is a statistically significant levels relationship, but the estimated coefficient is negative, implying the local rate moves opposite the base rate.

[50] As discussed. Frankel *et al* [2002] do not see as large a difference in part because they focus on the I(0) critical values, in part because of the smaller sample of floats and in part because of the way they classify some *de facto* pegs as floats. Simply reclassifying their countries moves their answer closer to that of this paper. Expanding the sample to broaden the number and type of floats generates the rest of the difference.

is the pegs with no capital controls. The pegs with capital controls often have levels relationships quite close to one, and the average is 1.36, but the adjustment is much slower than that of the pegs with no capital controls. Likewise, the open capital market non-pegs have a coefficient close to one, .80, but again the adjustment is quite slow, -.05. The closed capital market non-pegs have a backwards levels relationship and slow adjustment implying considerable room for freedom. Since there are only 127 episodes tested, it is difficult to have too much confidence in the averages when they are cut into such small baskets, but the results as they are support the general contentions of the paper. We can learn more about the long-run relationships looking at cointegration analysis.

8. Cointegration

Testing for cointegration tests the possibility that for the equation:

$$Z_t = a + bt + R_{it} - \gamma R_{bit} \tag{11}$$

Z is a stationary variable where both R_{it} and R_{bit} are non-stationary. One of the problems one can encounter is that many tests must first estimate γ and then check the stationarity of Z, a problem also encountered in the PSS tests. This involves a loss of power [Zivot, 2001]; an alternative would be to impose γ based on theory and then test Z. I have argued that based on investment flows, if R_{it} and R_{bit} have a long run relationship, they must move together in a one-for-one fashion in the long run. This is especially true if the series act in a non-stationary fashion, otherwise the series would be forever diverging. In cointegration tests, we have to assume non-stationarity; therefore, I impose the condition that $\gamma = 1$, and test the stationarity of the residuals. This amounts to testing whether the differential of R_{it} and R_{bit} is stationary. I again test for stationarity using both ERS and KPSS. Rejecting the null of non-stationarity in the differential using the ERS test implies rejecting no cointegration. Conversely, being unable to reject the null of stationarity in the differential implies being unable to reject cointegration. The ERS test rarely rejects a unit root in the differential (~10 percent in pegs and non-pegs and only once for the sporadic pegs).[52] The KPSS test of the differential is the weakest test; it simply means that we fail to reject no cointegration. Under this measure, 49 percent of pegs, 38

[51] A large range of non-pegged countries do not reject no levels relationships under both I(0) and I(1) assumptions and have adjustment speeds with half-lives over 12 months. These include: Algeria, Australia, Brazil, Ghana, Lebanon, Mauritius, Malawi, South Africa, Uganda, the UK, and Zimbabwe.
[52] An ADF test rejects a unit root in the differential more often: 34% in pegs, 13% in occasional pegs, and 10% in non-pegs.

percent of occasional pegs, and 10 percent of non-pegs show cointegration. Thus, even with this lax standard, almost no non-pegs show a long run relationship.[53]

As a check on the methodology, I use an augmented Engle Granger test, where the cointegrating coefficient is estimated, not assumed, to see if the estimated coefficient *g* is close to one.[54] This test estimates the cointegrating relationship with dynamic least squares, using:

$$R_{it} = a + gR_{bit} + \sum_{s=-p}^{p} B_s \Delta R_{bit-s} + e_{it} \qquad \textit{12)}$$

Of the 121 episodes where ERS tests do not reject unit roots for both local and base interest rates, in 29, the null of no cointegration is rejected at 95 percent: 23 of 68 pegs, 3 of 24 occasional pegs, and 3 of 29 non-pegs. The average estimated cointegration coefficient (the *g*) for episodes showing cointegration is .76 (.84 for the pegs), with over half being between .8 and 1.2. This both helps confirm the theory that the long run relationship should be close to 1 and suggests that the choice of imposing 1 as the cointegrating coefficient was reasonable.

While, in total, these results support the idea that pegs are more likely to be cointegrated, only a minority of pegs can reject no cointegration implying that for the majority of our data, regressions on simple differences are appropriate. In addition, as long as the adjustment of the cointegrated systems is fairly quick, cointegrated series should not present too large a problem when using differences for annual data.

Finally, as discussed earlier, we can use the error correction form shown in equation *(7)* to test the dynamics of the cointegrated systems. Despite assuming $\gamma = 1$ rather than allowing it to vary by episode, the results regarding adjustment speed are quite consistent with the PSS analysis whether we limit ourselves to those episodes where cointegration is present or if we examine the error correction adjustment speeds for all episodes.

Overall, the annual pooled regressions, the individual country regressions on monthly data, the PSS analysis, and the cointegration analysis provide strong evidence that fixed rates

[53] Abraham [1999] also examines the possibility of cointegration between the interest rate of a pegged country and the base country. The paper is not an attempt to examine fixed versus floating or monetary autonomy but is a straightforward study of the time series properties of U.S. and Saudi Arabian interest rates. The results show that both series have unit roots. The null of no cointegration, though, is not rejected. A possible explanation, not discussed in the paper, is that the sample is from 1988 to 1994 and thus includes the Gulf War which may have led to instability in the relationship.

generate a closer relationship between pegged countries and the relevant base country, implying that fixed exchange rates require a sacrifice of monetary autonomy above and beyond that of non-pegged economies. These long run results support the earlier OLS regressions by finding less significant long run relationships for non-pegs than pegs and complement them by showing that even given a certain level of long run correlation with the base, floats react more slowly than the pegs and seem to have more short run autonomy as well as long run autonomy. At the same time, the finding that not all pegs are cointegrated and that the pegs which are cointegrated adjust with a small lag may be further evidence of the effects of capital controls, exchange rate bands, or some capital market segmentation.

VII: Conclusion

Despite the importance of the decision of whether to fix the exchange rate or not, economists are decidedly uncertain over the effects of such a choice on monetary policy. Recent studies have argued that countries that claim to float in fact display a fear of floating, and other studies have suggested that floating rate countries must react to changes in international interest rates more than fixed rate countries, not less. The idea that fixed rates could generate more policy autonomy and more exchange rate stability at the same time makes them appear quite attractive.

The general result of this paper, though, is that fixed exchange rates do in fact force countries to follow the monetary policy of the base country more closely than floating rate countries and that in general, the trilemma presents a sensible framework for policy analysis. Hard pegs are not a panacea; they come with costs. In particular, this paper demonstrates that fixed exchange rates involve a loss of monetary policy autonomy.

Evidence is seen in annual pooled regressions, in investigations of individual countries at the monthly frequency, and in levels analysis. In the pooled analysis, various controls (time, trade, volatility of the exchange rate, and various measures of foreign debt) do not alter this general conclusion. The exchange rate regime along with capital controls seems to explain the extent to which a country follows the base interest rate. Not only is the correlation of pegged country interest rates with the base higher than for non-pegged countries, pegged samples consistently a have much higher R^2. The R^2 of non-pegged samples is generally extremely low, demonstrating that factors other than

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[54] Hamilton [1994, p.613] describes this modification of Engle Granger, where leads and lags of the differenced independent variable are added to the regression which estimates the cointegrating coefficient. The lags are again chosen by the rule of min(AIC) +2 as is standard in TSP's EG process.

the base interest rate are the significant drivers of non-pegged countries' monetary policies. This implies these countries may have a reasonable amount of monetary autonomy.

Perhaps the most stark demonstration of monetary autonomy is differences in the speed of adjustment to a shock to foreign interest rates. Both the PSS analysis and the cointegration analysis show that the interest rates of pegged countries tend to react more quickly to changes in base interest rates. The non-pegs react quite slowly, often with half-lives of over a year.

Even pegs without capital controls, though, do not appear to move perfectly with the base rate. The response is less than one for one and the R^2 is significantly below one. Whatever flexibility this may afford them, though, it is less than the flexibility available to non-pegged countries. Furthermore, in the 1990's, pegs without capital controls are even more closely linked to the base, implying this flexibility may be eroding.

The paper also suggests a potentially fruitful area of further research. Exogenous monetary policy shocks have been something of a holy grail of empirical macroeconomics in the last decade as researchers try to estimate the effects of monetary policy. If a pegged country follows base country interest rate changes, then monetary policy in the pegged country is not set with regards to local conditions. In addition, there is no feedback from the economy into the policy-setting rule because the base rates are set without regard to their impact on the local economy. Thus, the study of pegged economies may add another means of testing the effects of monetary policy. While more work needs to be done on separating the common shock responses from the responses to base interest rate changes, and data limitations still presently limit the analysis, preliminary results are suggestive that the economic impact of exogenous changes in interest rates could be substantial.

APPENDIX A: **Data**

I examine 155 countries for which there is exchange rate data over the period 1973-2000. I use IFS data for the monthly end of period exchange rate to the U.S. dollar (computing cross rates as necessary). Of these 155 countries, 40 have both money market and treasury bill data, 28 have only money market data available, and 35 have only treasury bill data available.[55] Thus, for a series combining the two, data are available for 103 countries. When both series are available for any one country, I use the series that has data available for a longer time span with treasury bill rates being the default if both series are equally long.[56] In the combined series, I use treasury bills for 56 countries and money market data for 47.

Three adjustments are made to the data and sample. First, because interest parity is technically derived using the form log(1+R) not R, and following Frankel *et al*, as well as convention, I use log(1+R) in place of R, where 10 percent is written .10. This translation of the data has a minute effect on any interest rate that is under 20 percent, and results were run without this operation and the change had a small effect. In addition, because the change in interest rates during hyperinflations can be on the order of hundreds or even thousands from one year to the next, these observations overwhelm the quarter and half point changes in other countries. Thus, I eliminate three hyperinflations for which data are available.[57] Finally, the United States was removed from the sample as it did not seem to have a relevant base currency for comparison. Any correlation of the United States to another country seems more likely to be the other country following the United States rather than vice versa.

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[55] Interest rate data are from the IFS, datastream, and Global Financial database. The money market rate is generally overnight and is an average of monthly values in almost all cases. Some cases have slightly longer maturities. The treasury bill rate is 3 month period average. Again, a few countries have slightly longer maturities and some use period end. The end of period rates, though, are for countries with fixed interest rates, meaning they are identical to a period average. The difference in maturities is not as problematic as it first appears since the correlation of 3 month treasury bills and overnight money market is well over .95 for most of the countries that have both rates available.

[56] There are only eight cases where the length was the same. For the base country interest rate, I use whichever series is being used for each particular country. That is, if country i uses money market rates, its base country rate will be money market, if country j is pegged to the same base, but uses treasury bill rates, then the base interest rate for country j will use treasury bills.

[57] The removed time periods are: Argentina from 1981 to 1992, Brazil from 1983 to 1995, and Israel from 1983 to 1986. I define the periods broadly to prevent having to start and stop the data and to prevent the start and finish of the periods from having a huge change which affects the results. Eliminating these three periods removes all changes over 100 points in absolute value from the money market and treasury bill series and all of over 50 from the combined series. Removing the hyperinflations does change the results significantly, changing coefficients from being in the range of 2000 down to the range of 1.

APPENDIX B: **Discussion of Alternate *De Facto* Classification Systems**.

In recent years, the LYS methodology [see Levy-Yeyati and Sturzenegger, 2000b] has become a popular alternative to the IMF *de jure* system,[58] and more recently, the Reinhart and Rogoff methodology has gained considerable attention.[59] The LYS methodology uses cluster analysis of the change in the exchange rate, the change in the change in the exchange rate, and the change in the ratio of foreign reserves to M2 to group countries into different regimes. Countries with low exchange rate volatility but large reserve volatility are considered pegged; countries with non-zero but relatively constant rate of change in the exchange rate and high change in reserves are considered intermediate; countries with high levels of exchange rate volatility but low levels of reserve volatility are considered floats. A country with a constant exchange rate but with low reserve volatility is considered inconclusive. The cluster analysis takes place in multiple rounds to identify the groupings that fit together.

In many ways the LYS strategy is appealing due to its emphasis on actual behavior over declared intentions. LYS, though, require intervention to take the form of changes in foreign reserves and draw no distinction between sterilized and unsterilized intervention. A country can maintain a pegged exchange rate without ever changing its reserves, and a country that changes reserves dramatically may not really be showing concrete commitment to its exchange rate peg.[60] In addition, highly unstable M2 may make the reserve ratio volatile. LYS state that theory suggests highly variable reserves for a country that is truly pegged, but if a country maintains its exchange rate by constantly changing its interest rate, as, for example, Bahrain does, there will be no change in reserves. One could argue that such a country is exhibiting a much stronger commitment to its exchange rate than a country that continually exercises sterilized intervention – changing reserves but unwilling to allow its domestic money supply to be changed in defense of the exchange rate.[61]

This problem becomes clear when one examines the countries that are listed as inconclusive by LYS. Countries such as Bahrain, the Bahamas, or Hong Kong, that have very strong fixed exchange

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[58] Masson [2001], testing the "hollowing out" hypothesis uses the LYS classification as well as Ghosh *et al*'s [1997] interpretation of the declared status; Fischer [2001] refers to the LYS work, Frankel *et al* [2002] uses it, and many works in progress use the LYS coding. The LYS data is kindly provided on their website.
[59] The Reinhart and Rogoff data is available on Carmen Reinhart's website.
[60] If a country employs an unsterilized intervention in defense of its exchange rate, it sells foreign assets and removes money from the money supply. In a sterilized intervention, the central bank re-injects the money into the economy by buying an offsetting amount of domestic assets. A third type of defense could be made, though, without ever changing foreign asset levels. The central bank could simply sell domestic assets and withdraw money from the money supply. The subsequent increase in the interest rate should help support the exchange rate peg.

rates against the dollar are not listed as pegged in the first round or even the second round of their exercises. This is because their reserves are not highly volatile; they simply adjust their money supply at all times to avoid the need for intervention, or they are so trusted in their regime that they are rarely challenged. [62] On the other hand, countries that intervene dramatically but break their peg, such as Ireland in the EMS crisis, or float but have sufficient reserves volatility, such as New Zealand, are listed as fixed.

My system and the LYS two–way system disagree roughly on 16 percent of the observations for which we both have data (LYS have data for only about half of the observations in my sample due to the need for reserves data to make their classifications). The regime episodes, though, often do conflict. LYS generally do not code EMS episodes as sustained pegs, while I generally do. Alternatively, LYS code a number of African countries with volatile exchange rates as pegs, probably due to reserves or M2 volatility. While the disagreements are relatively rare, as seen in the paper, the LYS system does generate different results on the key questions of the paper. The LYS methodology helps identify floats and dirty floats, but the focus on reserves intervention leaves this an incomplete exercise. More importantly, the exclusion of pegs due to lack of intervention and the coding of countries that intervene as pegs even if the exchange rate is volatile both seem to be potentially misleading when one is trying to classify pegs. Thus, the LYS coding is not ideal for the questions of this paper and it is not surprising the results do not hold well for the LYS methodology.

The Reinhart and Rogoff methodology is more similar to the one used in this paper in that it focuses on the exchange rate, but the exchange rate used is the parallel market rate (where it exists) and the standard is based on the odds of the exchange rate being outside a band over a five-year window, not the strict one year unbreakable bands used in this paper. The five year window is a small problem for this paper, as a country which pegs sporadically may be classified as a crawling peg in Reinhart and Rogoff.[63] As it turns out, the two methodologies agree 81 percent of the time. 89 percent of their pegs are pegs in my system, and 97 percent of their pure floats are floats in mine. Far more disagreement comes on the intermediate regimes. For this study, there is some concern that Reinhart

[61] As discussed, one could use an index including both interest rate behavior and reserves changes, but that would require essentially assuming the hypothesis of the paper to classify the regimes.
[62] LYS also create a two-way classification that adds inconclusives that exhibit no movement in the exchange rate to the pegged category but tend to focus on their three-way classification in their results.
[63] Canada is an example. While I find Canada to be a floating rate country that occasionally tried to anchor to the dollar, Reinhart and Rogoff label it as a crawling peg throughout the sample.

and Rogoff are merging two axes of the trilemma by focusing on the parallel rate, and in fact, 86 percent of the no capital control countries generate agreement. A country with capital controls may appear pegged in my classification, but could be coded as managed floats to Reinhart and Rogoff. The Reinhart and Rogoff system may be better suited to studies of trade or general macroeconomic experience where the behavior of the exchange rate used in transactions is relevant. Here, though, we are more interested in the constraint on central bank policy and the official rate seems more relevant. As seen, though, using the Reinhart and Rogoff coding generates results supportive of this paper. As it turns out, most of my extended pegs and floats (the country / regime episodes) are coded the same in Reinhart and Rogoff. The five year versus one year window leads to slight differences for sporadic peg countries, but we both code them as sporadic, just with different dates.

In general, then, the systems differ somewhat, but not extensively, and based on the year to year focus and separation of capital controls and exchange rate regimes, the proposed system seems more useful for this paper.

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Tables:

Table I. *De Facto* vs. *De Jure* Exchange Rate Descriptions

Declared Status	Total	pegged in my coding	not pegged in my coding	declared status implies	disagree	percent disagree
$ peg	1008	909	99	peg	99	10%
limited flex to $	80	79	1	peg	1	1%
Franc peg	396	395	1	peg	1	0%
Other peg	332	311	21	peg	21	6%
EMS	190	129	61	peg	61	32%
SDR peg	202	10	192	not peg	10	5%
Basket peg	583	131	452	not peg	131	23%
Crawling	39	4	35	not peg	4	10%
horizontal bands	20	9	11	peg	11	45%
Floating	1335	183	1152	not peg	183	14%
base countries	56	0	56	not peg	0	0%
not declared	97	60	37			
total declared	4241	2160	2081		522	12%

Table II: Diagnostics of the Interest Rates and Differential (R_{it}– R_{bit})

	Full Sample		Pegged Countries		Non-Pegged Countries		Industrial Countries		Developing Countries	
Time	*all*	*1990's*	*all*	*1990's*	*all*	*1990's*	*all*	*1990's*	*all*	*1990's*
(R_{it}– R_{bit}) mean	.04	.06	.005	.01	.06	.09	.03	.02	.04	.08
(R_{it}– R_{bit}) std dev	.09	.10	.03	.03	.11	.12	.04	.03	.10	.11
R_{it} std dev	.08	.10	.03	.03	.10	.12	.04	.04	.10	.11
R_{bit} std dev	.03	.02	0.3	.03	.03	.02	.03	.02	.03	.02

Table III. Local vs. Base Country Interest Rate Changes.

	Full Sample	Pegs	Non-Pegs	Full 1990's	Pegs 1990's	Non-pegs 1990's
# observations	1920	748	1103	886	327	525
B	.36[aa]	.46[aa]	.27[aa]	.44[aa]	.56[aa]	.35
Std. Error	.05	.04	.08	.15	.06	.25
R^2	.026	.19	.009	.013	.13	.006

aa = significant at 99%, a = significant at 95%, b = significant at 90%

Table IV: Impact of Capital Controls

	Pegged / no capital controls	Non-pegged/ no capital controls	Pegged / with capital controls	Non-pegged/ with capital controls
# observations	214	338	531	738
B	.67[aa]	.56[aa]	.41[aa]	.15
Std. Error	.09	.08	.04	.11
R^2	.27	.07	.18	.00
1990's observations	141	190	186	333
1990's B	.79[aa]	.55[aa]	.44[aa]	.25
1990's Std. Error	.09	.14	.08	.36
1990's R^2	.30	.03	.08	.00

Table V: Testing the Trilemma

	Full Time Sample *coef (se)*	Full Time Sample No Constant R *coef (se)*	Full Time Sample No Constant R *coef (se)*	1990's *coef (se)*	1990's No Constant R *coef (se)*	1990's No Constant R *coef (se)*
# observations	1848	1627	1598	850	751	749
B_1	.26[aa] (.08)	.28[aa] (.08)	.18[b] (.11)	.36 (.24)	.40 (.25)	.30 (.33)
B_2 peg	.19[a] (.09)	.30[aa] (.10)	.33[aa] (.10)	.18 (.24)	.31 (.26)	.29 (.25)
B_3 No Capcontrols			.37[aa] (.10)			.31 (.26)
R^2	.03	.03	.03	.01	.02	.02

Table VI: Additional Control Variables (no constant R)

	Full Time Sample *coef (se)* *no time dummies*	Full Time Sample *coef (se)* *time dummies*	Full Time Sample *Data missing* *coef (se)* *no time dummies*	Full Time Sample *Data missing* *coef (se)* *time dummies*	Full Time Sample *Data missing* *coef (se)* *time dummies*	Full Time Sample **with D&D cap controls** *time dummies*
# observations	1598	1598	1523	1523	1225	485
B_1	.10 (.14)	.05 (.16)	.08 (.17)	-.02 (.17)	.24 (.19)	-.41[b] (.24)
B_2 peg	.38[aa] (.12)	.30[aa] (.12)	.33[a] (.15)	.23 (.15)	.32[a](.13)	.51[aa] (.14)
B_3 No Capcontrols	.30[aa] (.10)	.29[aa] (.10)	.33[aa](.10)	.32[aa](.10)	.13 (.12)	.50[aa] (.19)
B_4 Industrial	.24[a] (.11)	.19[b] (.10)	.26[a] (.12)	.19[b] (.11)	.14 (.15)	.42[a] (.18)
B_5 Trade	-	-	.03 (.72)	.24 (.71)	-	-
B_6 Fin. Exposure	-	-	-	-	-.39[b] (.23)	-
R^2	.04	.08	.04	.08	.08	.11

Table VII: Alternate Exchange Rate Classifications (no constant R)

	***De Jure* classification** *coef (se)*	***De Jure* classification** *coef (se) time dummies*	**Reinhart and Rogoff classification** *coef (se)*	**Reinhart and Rogoff classification** *coef (se) time dummies*	**LYS classification (limited sample)** *coef (se)*	**LYS classification (limited sample)** *coef (se) time dummies*
# observations	1667	1563	1393	1316	1146	1075
B_1	.32[aa] (.09)	.01 (.20)	.31[aa] (.09)	-.03 (.24)	.45[aa] (.13)	.30 (.30)
B_2 peg	.22[a] (.10)	.12 (.11)	.38[aa] (.10)	.37[aa] (.13)	.07 (.14)	-.04 (.18)
B_3 No Capcontrols		.31[aa] (.11)		.33[aa] (.12)		.09 (.14)
B_4 Industrial		.16[b] (.09)		.22[b] (.12)		.20 (.14)
B_5 Trade		.41 (.64)		.35 (.83)		.30 (.79)
R^2	.03	.08	.04	.09	.04	.09

Table VIII: Explaining the different responses to base interest rates

Specification:	**1**	**2**	**3**	**4**	**5**	**6**
# observations	117	115	115	109	87	115
A	.28[aa] (.08)	.13 (.09)	.04 (.09)	.01 (.12)	.27 (.17)	.20[b] (.12)
κ_1 (peg)	.20[a] (.10)	.29[aa] (.10)	.36[aa] (.10)	.32[aa](.10)	.32[aa] (.11)	.14 (.15)
κ_2 (no capital controls)		.25[aa] (.09)	.10 (.10)	.14 (.10)	.04 (.12)	-.17 (.14)
κ_3 (industrial)			.33[aa] (.09)	.35[aa] (.09)	.23 (.14)	. 31[aa] (.08)
κ_4 (Trade)				-.01 (.35)		
κ_5 (financial exposure)					-.49 (.33)	
κ_6 (peg*noCapCon)						.45[a] (.21)
R^2	.03	.08	.15	.16	.20	.18

Table IX: Adjustment Speeds to Shocks in Base Interest Rate

	Pegged	Occasional Pegs	Non-Pegs
half life < 3 months	31%	16%	3%
half life from 3 to 12 months	50%	56%	41%
half life >12 months	19%	28%	56%

Table X: PSS results by episode type

$[\theta, \gamma]$	No capital controls	intermediate	Capital controls
Pegged	-.26, .70	-.23, -.16	-.12, 1.36
Occasional Pegs	-.11, .57	-.08, .44	-.13, .86
Non-Pegs	-.05, .80	-.07, -1.59	-.06, -.38

Table XII: Details of Data Sample

	Money	Market	Treasury	Bill		interest rate used
country	start	end	start	end	base country	in combined series
ALGERIA	1994	2000	1980	2000	French Franc	treasury bill
ANTIGUA AND BARBUDA			1980	2000	U.S. Dollar	treasury bill
ARGENTINA	1980	2000			U.S. Dollar	money market
AUSTRALIA	1973	2000	1973	2000	U.S. Dollar	treasury bill
AUSTRIA	1973	1998			German DM	money market
BAHAMAS, THE			1973	2000	U.S. Dollar	treasury bill
BAHRAIN	1986	2000	1987	2000	U.S. Dollar	money market
BARBADOS			1973	1999	Pound (73-4) / $ (75-99)	treasury bill
BELGIUM	1973	1998	1973	2000	German DM	treasury bill
BELIZE			1977	2000	U.S. Dollar	treasury bill
BENIN	1976	2000			French Franc	money market
BOLIVIA	1995	2000	1994	2000	U.S. Dollar	treasury bill
BRAZIL	1973	2000	1973	2000	U.S. Dollar	money market
BURKINA FASO	1976	2000			French Franc	money market
CANADA	1973	2000	1973	2000	U.S. Dollar	treasury bill
CHINA,P.R.:HONG KONG	1973	2000	1992	2000	U.S. Dollar	money market
COLOMBIA	1995	2000			U.S. Dollar	money market
COTE D IVOIRE	1976	2000			French Franc	money market
DENMARK	1973	2000	1976	1988	German DM	money market
DOMINICA			1980	2000	U.S. Dollar	treasury bill
DOMINICAN REPUBLIC	1996	2000			U.S. Dollar	money market
EGYPT			1997	1999	U.S. Dollar	treasury bill
EL SALVADOR	1997	2000			U.S. Dollar	money market
ETHIOPIA			1978	1999	U.S. Dollar	treasury bill
FIJI	1982	2000	1975	1999	U.S. Dollar	treasury bill
FINLAND	1978	1999			German DM	money market
FRANCE	1973	1998	1973	2000	German DM	treasury bill
GERMANY	1973	2000	1973	2000	U.S. Dollar	treasury bill
GHANA			1978	2000	U.S. Dollar	treasury bill
GREECE	1994	1998	1974	2000	$ (74-80) / DM (81-00)	treasury bill
GRENADA			1980	2000	U.S. Dollar	treasury bill
GUATEMALA	1997	2000			U.S. Dollar	money market
GUYANA			1973	1999	Pound (73-5) / $ (76-99)	treasury bill
HAITI			1997	2000	U.S. Dollar	treasury bill
HUNGARY			1988	1999	$ (88-91) / DM (92-99)	treasury bill
ICELAND	1987	2000	1988	2000	$ (87-90) / DM (91-00)	money market
INDIA	1973	1997			Pound (73-9 / $ (80-97)	money market
INDONESIA	1974	2000			U.S. Dollar	money market
IRELAND	1973	2000	1973	1998	Pound(73-8) /DM(79-00)	money market
ISRAEL	1993	2000	1984	1999	U.S. Dollar	treasury bill
ITALY	1973	2000	1974	2000	German DM	money market
JAMAICA			1973	2000	U.S. Dollar	treasury bill
JAPAN	1973	2000	1973	2000	U.S. Dollar	treasury bill
KENYA			1973	2000	U.S. Dollar	treasury bill
KOREA	1977	2000			U.S. Dollar	money market
KUWAIT	1979	2000	1980	1997	U.S. Dollar	money market
LAO PEOPLE'S DEM.REP			1995	2000	U.S. Dollar	treasury bill
LEBANON	1982	2000	1977	2000	U.S. Dollar	treasury bill
LESOTHO			1981	2000	South African Rand	treasury bill
LIBYA	1973	2000			U.S. Dollar	money market
LUXEMBOURG	1980	1998			Belgian Franc	money market

MADAGASCAR	1990	1998			French Franc	money market
MALAWI			1981	2000	U.S. Dollar	treasury bill
MALAYSIA	1973	1999	1973	2000	U.S. Dollar	treasury bill
MALDIVES	1984	2000			U.S. Dollar	money market
MALI	1976	2000			French Franc	money market
MALTA			1987	1999	French Franc	treasury bill
MAURITIUS	1978	2000			British Pound	money market
MEXICO	1977	2000	1978	2000	U.S. Dollar	money market
MOROCCO	1980	2000			French Franc	money market
MOZAMBIQUE	1999	2000			U.S. Dollar	money market
NAMIBIA			1992	2000	South African Rand	treasury bill
NEPAL			1981	1999	$ (81-2) / Rupee (83-99)	treasury bill
NETHERLANDS	1973	1998	1973	2000	German DM	treasury bill
NETHERLANDS ANTILLES			1982	1999	U.S. Dollar	treasury bill
NEW ZEALAND	1973	2000	1978	2000	Australian Dollar	money market
NIGER	1976	2000			French Franc	money market
NIGERIA			1992	1999	U.S. Dollar	treasury bill
NORWAY	1973	2000			German DM	money market
PAKISTAN	1973	2000	1992	2000	U.S. Dollar	money market
PAPUA NEW GUINEA			1974	1999	Aus $ (74-85) / $ (86-99)	treasury bill
PARAGUAY	1991	2000			U.S. Dollar	money market
PHILIPPINES			1976	2000	U.S. Dollar	treasury bill
POLAND	1991	2000	1992	2000	German DM	money market
PORTUGAL	1975	2000	1980	1997	German DM	money market
ROMANIA			1996	2000	U.S. Dollar	treasury bill
SENEGAL	1976	2000			French Franc	money market
SEYCHELLES			1980	1999	U.S. Dollar	treasury bill
SIERRA LEONE			1973	2000	Pound (73-7 / $ (78-00)	treasury bill
SINGAPORE	1973	2000	1973	2000	Malaysian Ringgit	treasury bill
SOLOMON ISLANDS			1981	1999	Aus $ (81-85) / $ (86-99)	treasury bill
SOUTH AFRICA	1973	2000	1973	2000	U.S. Dollar	treasury bill
SPAIN	1974	2000	1978	2000	German DM	money market
SRI LANKA	1978	2000	1981	1999	$ (78-92) / Rupee(93-99)	money market
ST. KITTS AND NEVIS			1980	2000	U.S. Dollar	treasury bill
ST. LUCIA			1980	2000	U.S. Dollar	treasury bill
ST. VINCENT & GRENS.			1980	2000	U.S. Dollar	treasury bill
SWAZILAND	1989	2000	1975	2000	South African Rand	treasury bill
SWEDEN	1973	2000	1973	2000	German DM	treasury bill
SWITZERLAND	1973	2000	1980	2000	German DM	money market
TANZANIA			1993	2000	U.S. Dollar	treasury bill
THAILAND	1977	2000	1977	2000	U.S. Dollar	money market
TOGO	1976	2000			French Franc	money market
TRINIDAD AND TOBAGO			1973	1999	Pound (73-5) / $ (76-99)	treasury bill
TUNISIA	1981	2000	1990	2000	French Franc	money market
TURKEY	1987	2000	1985	1999	U.S. Dollar	treasury bill
UGANDA			1980	2000	U.S. Dollar	treasury bill
UNITED KINGDOM	1973	2000	1973	2000	German DM	treasury bill
URUGUAY	1994	2000	1994	1997	U.S. Dollar	money market
VANUATU	1985	2000			franc (85-9) / $ (90-00)	money market
VENEZUELA, REP. BOL.	1996	2000			U.S. Dollar	money market
ZAMBIA			1973	1999	U.S. Dollar	treasury bill
ZIMBABWE	1975	2000	1973	2000	U.S. Dollar	treasury bill