The Trilemma in History: Tradeoffs among Exchange Rates, Monetary Policies, and Capital Mobility

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Abstract

Recently, the political economy of macroeconomic policy choice has increasingly been guided by the simple prescriptions of the classic trilemma. For example, policymakers often speak of the hollowing out of exchange rate regimes in a world of unstoppable capital mobility; and policy autonomy and a fixed nominal anchor present an unpleasant dichotomy for emerging markets beset by the fear of floating. Yet the trilemma is not an uncontroversial maxim, and its empirical foundations deserve greater attention. Some authors (e.g., Calvo and Reinhart 2001, 2002) have argued that under the modern float there could be limited policy autonomy given the rapid international transmission of interest rate shocks; others (e.g., Bordo and Flandreau 2003) that even under the classical gold standard there actually was considerable policy autonomy given the gold point spread and the use of gold devices and other tricks. Such arguments turn the trilemma on its head. Resolving this debate is ultimately an empirical matter, where the broadest span of data should be scrutinized. Using new techniques to study the coherence of international interest rates at high frequency, in conjunction with an examination of capital mobility policies and a data-based classification of exchange rate regimes, we propose to look at the empirical content of the trilemma based on consistent data over 130+ years. We aim to show whether the clean predictions of this influential adage are borne out by history.

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The controversies that swirl around economic globalization—implicit in that sound-bite sobriquet "golden straightjacket"—tend to take one of two forms.[2] We can either argue about how golden the jacket is, or else about how straight it is. The former question has occupied many applied economists, who have endlessly studied the relationship of openness to growth and so forth. The latter question has been much less studied, and yet it appears equally important for any overall assessment of the costs and benefits of open-economy policies. The goal of this paper is to examine the tradeoffs in the macroeconomic policy arena in an open economy setting and we extend a very small extant literature on this question both temporally (using long-run historical data) and methodologically (via appropriate econometric techniques).

As noted in previous work, at the most general level, policymakers face what Obstfeld and Taylor (1998) termed a *trilemma*. Typically they are confronted with three desirable, yet contradictory, objectives:

1. to fix the exchange rate, for relative price stabilization purposes;
2. to have free capital mobility, for efficiency and flexibility purposes;
3. to engage in activist monetary policy, for output stabilization purposes.

The fact that only two out of the three policies can be mutually consistent leaves policymakers having to decide which one they wish to give up: this is the trilemma. The theoretical foundations of the trilemma need not be repeated here at length. If monetary policy activism (3) is taken to mean the ability to drive local interest rates away from the world rate (a criterion we shall employ here), then arbitrage in capital markets (2) and simple interest parity under a credible fix (1) clearly defeat the objective. Something has to give. These ideas have long been a commonplace, and are sometimes referred to as the "inconsistent trinity."

The trilemma would seem to place stark constraints on the political policy equilibrium, yet despite the clarity and simplicity of the theory, one is often surprised by the frequency of episodes in which the lessons of the trilemma seem to be disregarded by policymakers, even today. Perhaps the tendency to sometimes ignore this dictum reflects the fact that there have been relatively few empirical studies that show how tight the constraints are and how well the trilemma describes the tradeoffs facing policymakers. This might not be surprising, however, since in a hunt for evidence of the trilemma, one confronts major empirical challenges when trying to measure each of the three components.

[2] The quotation is from Friedman (1999).

The first element, the exchange rate, is perhaps the simplest to measure, where one might employ the volatility of the exchange rate and an indicator, or a simple binary indicator of fixing or floating. However, even here some disputes arise. Should one employ unconditional or conditional volatility measures? If using an indicator variable, should the exchange rate regime be classified by it *de jure* or *de facto* properties? It is now clear that a country's actual exchange rate regime choice can often depart from its self-reported status, as published by the IMF. Hence, the preferred approach now is to examine what countries do, not what they say (Obstfeld and Rogoff 1995; Calvo and Reinhart 2001, 2002; Levy-Yeyati and Sturzenegger 2000; Reinhart and Rogoff 2002).

The second element of the trilemma, capital mobility, is also hard to measure. Postwar empirical analysis has tended to rely on the IMF's reported classifications of capital mobility restrictions, which only have broad coverage in recent decades. These are general qualitative indices and, as with the *de jure* exchange rate reports, questions have been raised about their *de facto* accuracy. For an historical study like ours, we face the further obstacle that there exist no comparable sources on pre-1945 capital controls. Accordingly, this will be the weakest link in our study.

The third element, measuring the activism of monetary policy, is also very problematic. The approach taken by Rose (1996), perhaps the only antecedent in this literature, uses a classic monetary model of exchange rates to search for the "Holy Trinity." He noted that the quantity theory implies an exchange rate response to shocks to "fundamentals" such as money, output, and interest rates. He then tested how well the model fits the data (in the second moments) to see how exchange rate flexibility is related to "monetary divergence" in the two countries, with the optional additional of linear and interaction controls based on capital mobility indices. His results were "somewhat favorable but surprisingly weak" (p. 926). Still, as many papers have pointed out, we are poorly equipped to identify monetary policy shocks. Using monetary aggregates is dubious when one cannot easily distinguish between demand and supply shocks to money, and also when the stability of velocity has to be assumed.

The ineluctable problems of policy measurement will likely persist and the quest for the Holy Trinity might prove as arduous as that for the Holy Grail. Undeterred, in this paper we try a different approach. First, our measure of monetary independence will be based not on quantity aggregates, but on observed short-run market interest rates. This has intuitive appeal, since a primary channel for monetary policy is via the credit market. Furthermore, to a large extent monetary policy (a few experiments aside) has almost always taken the form of interest rate targeting or manipulation, with little reference to quantity targets. The question we will pose is whether the exchange rate regime influences the extent to which local interest rates diverge from the "world" interest rate (in some base market).[3]

Second, our approach will vastly expand the sample range of experience studied. In particular, our study of international interest rate transmission will encompass historical episodes as far back as 1870. Specifically, we will try to assess the potency of the trilemma as an overarching explanation of policy constraints in both the pre-World War One gold standard period (1870–1913), the convertible Bretton Woods period (1959–73), and in the post-Bretton Woods era (1974–present). [We hope to add an analysis of the interwar period in a future draft.] In comparison, Rose (1996) studied the period 1967–92, and Shambaugh (2001) and Frankel et al. (2000, 2002), the modern post-1970 period through the end of the twentieth century.

An enlargement of the data universe is attractive on a number of grounds. First, we can see whether the trilemma has endured over a long period as a useful characterization of policy choice. The more durable it can be shown to have been over the long course of history, the more seriously its constraints should be taken by policymakers. Second, the larger historical sample is simply a welcome expansion of the database, and adds useful additional variance to the data. This variance takes a number of forms. Adding the whole of the Bretton Woods convertible period allows us to study countries in a period of widespread capital controls. Adding the gold standard period introduces a different benchmark, an era with essentially no capital controls, but with a different dominant wisdom about the proper role for monetary policy. These two eras offer a useful and clean contrast with the recent era, when countries to greater or lesser degree dismantled their postwar systems of capital controls. Third, for all the talk of the recent revolutionary globalization, more nuanced scholarly work finds considerable evidence to suggest that the pre-1914 era was one of perhaps even greater market integration than today in certain key ways.

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[3] As noted below, this follows the approach of Shambaugh (2001) and is closely related to the work of Frankel (1999) and Frankel et al. (2000, 2002). A delicate question arises, of course, over whether an observed non-divergence of such interest rates can be seen as evidence of a tight market constraint forcing the issue, rather than just a policy choice to follow the base country. We consider this question below.

Thus, a study of history might reveal some meaningful benchmarks for exactly how tightly the straightjacket of globalization might eventually fit in different times and places. Already there is suggestive evidence of the different solutions to the trilemma adopted in these earlier epochs, and the basic findings of this paper confirm conventional accounts, as told using more descriptive evidence on capital flows, interest rates, monetary policies, and the like. According to various historical narratives (e.g., Eichengreen 1996; Obstfeld and Taylor 1998, 2003), the classical gold standard was a period of mostly fixed rates, unfettered capital mobility, and, hence, limited monetary independence. The interwar period was a time of divergent strategies, including floating and exchange controls, as monetary independence started to be asserted by domestic policymakers. The architecture of Bretton Woods was set up to preserve this autonomy, while preserving relatively stable fixed-but-adjustable exchange rates, necessitating strict limits on capital mobility. Finally, since the collapse of the Bretton Woods system, capital mobility has reasserted itself; some countries have adopted floating as a means to maintain independence, some have fixed and tied their hands, and others have endured crises and confusion as they vacillated between the two "corner solutions."

Are such narratives supported by the data? We think so. In this study we find pronounced and rapid transmission of interest rate shocks during fixed-rate episodes under the classical gold standard period, perhaps to an extent that has not yet been equaled even in the contemporary period. This once again confirms the classical gold standard regime as a useful benchmark for the study of globalization. In marked contrast, the Bretton Woods era reveals an episode where fixed exchange rates did not provide much of a constraint on domestic interest rates whatsoever, a clear by-product of capital controls. Now, in the contemporary post-Bretton Woods era, there are signs of reversion to the more globalized pattern, with increased interest rate transmission among fixed-rate countries. Still, an alternative solution of the trilemma is also clearly present in our findings: floaters, both before 1914 and in the present, have enjoyed considerably more monetary independence than fixers.

Overall, as witnessed in the systematic variation in policymakers' room for maneuver, we find strong evidence in support of the trilemma, which deserves to be recognized as a long enduring and still very relevant constraint on the political policy equilibrium.

Methodology

We examine a number of specifications to test the degree to which a local country's interest rate follows a canonical base country's interest rate. The time series properties of the data are quite important to the choices made. Nominal interest rates tend to behave in ways very close to unit roots, especially over finite samples. They are not literally unit roots; if they were, some series would wander into negative territory, and others would approach infinity. We do not observe this in practice, so clearly there are some bounds on the behavior of the data. As Stanton (1997) discusses, nominal rates seem to behave like non-stationary data most of the time unless they reach very high or very low levels, in which case there is some mean reversion.[4] Given the lack of power of most unit root tests,[5] and the necessity of using relatively short time series in some cases to isolate individual peg or non-peg episodes, we cannot posit unambiguously the time series properties of the data. Thus, we pursue a variety of analyses under different assumptions.

If the data are non-stationary, any simple regression of the levels of one series on another leaves open the possibility of spurious regression (Granger and Newbold 1974). Phillips (1988) demonstrates that analogous problems arise with stationary but near-integrated data as well. If the data are truly non-stationary (or close) and are not cointegrated, the proper approach would be to difference the data and examine a simple equation such as:

$$\Delta R_{it} = a + b\Delta R_{bit} + u_{it}, \qquad (1)$$

where R_{it} is the local interest rate at time t, R_{bit} is the base rate at time t, and Δ is the difference operator.

With perfect capital mobility and an exchange rate permanently and credibly pegged within a band that is literally of zero width, we would expect to find beta = 1 above: home and base-country interest rates would always move one-for-one, and the pegging country's monetary independence would be nil. In practice, however, "fixed" exchange rates are fixed only up to a possibly narrow fluctuation band, and our methodology for selecting pegs (described below) must allow for this. As a result, even under a peg, beta could conceivably be below 1. How far below 1 could it be? We have experimented with simulations of Krugman's (1991) target zone

[4] See also Wu and Zhang (1997).

model, using Svensson's (1991) term-structure model to derive interest rates for non-infinitesimal maturities when the fluctuation band is quite narrow (plus or minus 1%). We find that beta coefficients well below 1 are likely to arise even when domestic authorities mildly smooth short-term domestic interest rates in the face of foreign interest-rate shocks.[6] We will describe these simulations in detail in our next draft. Their results underscore, however, the importance of a having the classical gold standard (itself a target zone system because of gold points) as a quantitative benchmark for results from later periods. Because the gold standard is widely acknowledged to be an era in which the exercise of monetary independence was strictly limited, results that look similar to those found in pre-1914 data, even if they entail a beta below 1, can be construed as supporting the hypothesis that pegs greatly limit monetary autonomy.[7]

Despite the fact that the data are in panel form, fixed country effects are not needed because such an effect would assume a constant rate of change in the interest rate for an individual country, a highly unlikely scenario.[8] Given the fact that the response to a change in the base rate may not be immediate and may vary across countries, examining the differences with high-frequency data in pooled fashion generates unclear results. At an annual frequency, though, there appears to be sufficient similarity across the countries to allow for pooling the data.[9] While this basic specification cannot tell us much about the dynamics of the relationships nor about individual episodes, it can at least inform us about general patterns across the different eras and across exchange rate regime types.[10]

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[5] Caner and Kilian (2001) show that tests with stationarity as the null are likely to entail spurious rejections when the data are stationary but highly persistent.

[6] Of course, Krugman's (1991) model assumes effectively infinite foreign reserves, a factor that may exaggerate the ability of some countries to smooth the path of domestic interest rates.

[7] Thus, a beta below 1 is not sufficient for monetary independence. We caution the reader that it is not strictly necessary either. An economy buffeted by permanent real shocks, for example, will be stabilized by a floating exchange rate even if its interest rate never deviates from the foreign rate. An important (and unanswered) question is whether independence at the short end of the term structure but not at the long end, as in a fairly narrow target zone, confers on a central bank much leverage over the economy. Some scholars of the gold standard argue that the gold points allowed considerable monetary independence, a contention which, if true, would make the gold standard an unacceptable benchmark for judging the degree to which a pegged exchange rate binds monetary policy. The dynamic cointegration results we report below throw doubt, however, on the hypothesis of substantial monetary independence under gold.

[8] Likewise one could question the need for a constant in equation (1). In practice, however, the estimated constant for this equation was very close to zero.

[9] This also means that if some of the series are cointegrated with the base rate, the differences specification is less problematic for annual data as the dynamic adjustments are likely to have settled down to a large extent after a year.

[10] There are, of course, many other factors one could expect to affect the degree to which a country follows the base country interest rate. Common shocks, world or regional trade shares, capital controls, level of industrialization, level of debt, etc. could all have some impact. Shambaugh (2001) considers the impact of these factors in studying

Alternatively, one could assume the data are stationary and that the persistence is not so dramatic as to require treating the data as if they have unit roots. In this case, levels analysis is appropriate, though a correction for the high levels of autocorrelation is necessary. In addition, since there are quite likely to be differences in the levels of risk or liquidity in the different countries in any sample, it is necessary to allow for fixed effects. The regression therefore becomes[11]

$$R_{it} = a_i + bR_{bit} + u_{it}. \qquad (2)$$

Again, we can look across both eras and exchange rate regime episodes to see differences in the levels relationships. In practice, we find that the series are so close to unit roots that uncorrected levels regressions seem problematic and any AR(1) correction comes close to a differences specification. Thus, we do not report the levels regressions.

Finally we can try to examine the dynamics of individual country/exchange rate regime episodes and test for the presence of significant levels relationships. Again, the technique depends on the properties of the data. If one is convinced the data are non-stationary and that the individual local series are cointegrated with the base interest rate, then one can use an error-correction form to analyze the dynamics. Given the uncertainty over the order of integration of the data, the technique developed by Pesaran, Shin, and Smith (2001) and also used in Frankel, Schmukler, and Serven (2002) is quite helpful. Rather than assume the order of integration, one can test a specification like the error correction form, but examine the critical values provided by PSS to test the significance of the levels relationship. Different critical values apply in the I(0) versus the I(1) case. Thus, if the test statistic either passes both critical values or fails to pass both critical values, we can either reject no long run relationship or not without having to take a stand on the order of integration of the data. Only when the test statistic is in the middle are we

the post Bretton Woods era. With the exception of capital controls, which are quite important, the exchange rate regime tends to be the major determinant in how closely a country follows the base interest rate.

[11] Again, to accommodate the autocorrelation in practice, we estimate this equation on adjusted data using a simple AR(1) correction.

thrust back into the position of having to ascertain the order of integration to make judgments about the data.[12]

To employ the PSS test, we adopt the specification

$$\Delta R_{it} = a + b\Delta R_{bit} + q\,(c + R_{i,t-1} - gR_{bi,t-1}) + u_{it}, \qquad (3)$$

where we can include lags of ΔR_{bit} and ΔR_{it} as necessary, and g is a cointegrating coefficient. We can then test the significance of the adjustment speed (q) to determine whether there is a significant long run relationship. If the local interest rate adjusts to restore the equilibrium relationship after shocks to the base interest rate, we would find $q < 0$. The size of the coefficient shows the speed of adjustment with $q = -0.5$ implying a half-life of one month.

In contrast to the PSS test, an error correction form would involve testing for cointegration and then running equation (3), with g set equal to the estimated slope coefficient from a cointegrating regression. If the data are nonstationary, we would expect that a long run relationship would be (roughly) one of equality. Any other would imply the possibility of an ever widening spread in the levels of the interest rate. Thus, one could also impose $g = 1$ at the start.

The next section describes the data and the following section discusses the results from these different tests.

Data

Data Sources

The short-term interest rate data for the Gold Standard era reflect the arduous collection efforts of Neal and Weidenmier (2003), whom we thank for sharing the resource they have assembled. The Neal-Weidenmier data are available for 15 countries plus the UK. Before World War I, the UK interest rate is used as the base rate for comparison to the other countries. The data begin in 1870 for many countries and later for others, and all series end in 1914. The interest rate data for the Bretton Woods era consist of both short-term money market rates (interbank and call money rates) and short term treasury bill rates (generally three-month rates), all taken from the IFS CD-

[12] It is difficult to try to analyze the pooled data with PSS or EC techniques as the data are quite unbalanced with certain countries pegging at certain times and not others. Furthermore, the dynamics appear to differ widely across

ROM.[13] Availability determines which data we use. While the maturity on the two types of rates are slightly different, when available for the same country they are extremely highly correlated. The base interest rate under Bretton Woods is the US interest rate that matches the interest rate used for each local country. Due to a lack of monthly data before 1957 and the expansion of convertibility that took place in 1959, the Bretton Woods data that we examine are limited to the years 1959–70.

For the post Bretton Woods era, the interest rate series used are once again short-term money market and short-term treasury bill rates from the IFS CD-ROM. Those data are augmented with data from Datastream and Global Financial Data. The base rate chosen for the post-Bretton Woods period varies by country and is taken from Shambaugh (2001). For pegs, the interest rate is obviously the country to which the local country pegs. For non-pegged countries, the base country is the country to which the local country would have pegged if it were pegging. This choice is determined by previous pegging history, which is possible in almost all cases, but otherwise by the dominant currency in the region (the one to which neighboring countries peg). Information from the IMF's Annual Report on Exchange Arrangements and Exchange Restrictions and from B. Taylor (2000) is used as well. Because almost all countries peg at some point in the sample, and those that switch bases generally switch from one peg to another, the assignment of a base currency is generally quite simple. Once again, the base interest rate used is always of the same type (money market or treasury bill) as the one for the local country.

All interest rates are expressed in the form $\ln(1+R)$. While this has a trivial impact on low to moderate interest rates, it does shrink the impact of outliers. In addition, hyperinflations are excluded from the post Bretton Woods sample due to the excessive weight they take in the regressions.

Exchange-Rate Regime Coding

The exchange rate regimes for the Gold Standard are determined both based on the legal commitment of countries to gold (the *de jure* status) as well as the *de facto* behavior of the exchange rate. *De jure* coding is based on Meissner (2001), Eichengreen (1996), Global

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countries, making pooling inappropriate.

[13] Data for the US are extended back in time using information from FRED on the St. Louis Federal Reserve website.

Financial Data, and Hawtrey (1947).[14] The *de facto* standard follows the coding used for the post-Bretton Woods era developed in Shambaugh (2001). We test whether the monthly exchange rate stays within 2% up or down bands over the course of a year. In addition, single realignments are not considered breaks in the regime as long as the transition is immediate from one peg to another. Finally, single year pegs are dropped as they are quite likely a simple lack of volatility and it is unlikely that there exists either commitment on the government's part or confidence in the market that the rate will not change.[15] For the Bretton Woods period, all countries with the exception of Canada in 1960–61 and 1970 and Brazil throughout the period are pegged both *de jure* and *de facto*. Finally, the post-Bretton Woods era coding comes from Shambaugh (2001) as described above.[16] While we will adopt the terms peg and float to describe countries' regimes, we do not suggest that the countries without pegged rates are pure floats or that they have no exchange rate management policy; they are probably best described as non-pegs.

Individual Country Episodes

For the PSS analysis, individual country/regime episodes are examined using monthly data. Exchange rate regime coding follows much the same pattern as for annual data, checking that the exchange rate has stayed within +/- 2% bands over the last 12 months. The episodes for the Gold Standard and Bretton Woods eras are listed in the Appendix. Short episodes of less than three years are excluded due to lack of data. There are 13 defined peg episodes and 7 non-peg episodes based on *de jure* status under the Gold Standard, and 20 pegs and 5 non-pegs based on *de facto* status. Under Bretton Woods, there are 13 pegs and only one non-peg (Brazil). In addition, in the post-Bretton Woods era, there is a considerable amount of flipping back and forth from peg to non-peg for many countries. For this era, a separate category of "occasional peg" is created. Occasional pegs have at least 3 short pegs lasting less than three years, and the episode is defined from the start of the pegging until the last peg period breaks. To prevent short non-peg episodes that are really simply the middle of these occasional pegs from being counted

[14] Bimetallic regimes are treated as fixed; we recognize that this is somewhat arbitrary, but this choice affects only a small number of observations.

[15] When pursuing differences regressions, we also drop the first year of a peg to insure we are not differencing the interest rate data across non-pegged and pegged observations.

[16] Shambaugh (2001) provides an extensive discussion of different options from IMF coding to other de facto classifications. Recent work by Reinhart and Rogoff (2002), which uses data on parallel market exchange rates, does not appear to be directly relevant for the present paper. Since countries commit to keep only the official rate pegged, the behavior of the parallel rate is not a constraint on monetary policy.

as floats, floats must last at least 10 years in this era. There are 70 pegs, 25 occasional pegs, and 32 non-pegs during this era.

Capital Control Status

In some regressions we will also want to code countries as either having or not having capital controls. This is a non-trivial matter. *De facto* classifications are difficult to use for a number of reasons. Most are available for a limited number of countries and a limited amount of time. Some rely on interest rate differentials (the phenomenon we study) and thus are not appropriate. While *de jure* codes are available for many countries, they are available only after 1973. We proceed by assuming all countries have open markets in the Gold Standard era, no countries are open during Bretton Woods, and that the IMF coding from the Exchange Rate Arrangements yearbooks can be used as a reasonable approximation of capital controls for the post Bretton Woods era.[17]

Unit Root Properties of the Data

While the methodology section considers the fact that many time series of nominal interest rate data are difficult to distinguish from unit roots, this is not necessarily true for the Gold Standard era. Figures 1a, 1b, and 1c show the base interest rates during the three time periods considered. Simply viewing the figures shows that the UK interest rate in the Gold Standard era appears to be far more stationary than the US post-war rates and that the post Bretton Woods US interest rate is even less stationary than the Bretton Woods rate. This is to be expected given that the main component of the nominal interest rate generating nonstationarity is the level of inflation. A credible commodity standard is more likely to produce stable inflation and a stationary nominal interest rate than a discretionary fiat standard.

Simple tests on monthly base and local interest rates for the individual episodes back up the ocular evidence. The tabulation below shows simple autocorrelation coefficients for the base and local rates over the different eras; recall that these estimates will be biased down if the true value is near or equal to one. While all the data are persistent, The post World War II data appear to be much closer to containing a unit root than the Gold Standard data.

[17] After 1995, the IMF switches from a binary coding (line E2) to a disaggregated coding. Following Shambaugh (2001) we use changes in the disaggregated coding and descriptions in the yearbooks to determine changes in the binary codes.

	Gold Standard	Bretton Woods	Post-Bretton Woods
Autocorrelation coefficient for local rates	.82	.93	.92
Autocorrelation coefficient for base rates	.80	.98	.96

We employ unit root tests in the form of Elliot, Rothenberg and Stock (1996) using the modified AIC suggested by Ng and Perron (2001) to determine the appropriate number of lags to include. Over the entire Gold Standard period we reject a unit root in the UK interest rate (at the 5% level). Subsamples of the UK rate data that start in 1890 or after often do not reject a unit root despite covering more than 20 years. A look at figure 1a shows that this is not necessarily surprising: the UK rate shows somewhat less mean reversion starting in 1890. For the US Federal funds rate, one cannot reject a unit root over the full Bretton Woods period. For the US Treasury bill rate one can reject the unit root null using the full 1948-73 sample, but not in the 1959-70 sample that we use. Over the full post-Bretton Woods era, one cannot reject a unit root in US interest rates and in only 1.6% of the episodes can a unit root be rejected for the base interest rate.

Local rates follow a similar pattern. While in 8 out of the 25 episodes in the Gold Standard one can reject a unit root in the local interest rate (including all of the long series), one can reject a unit root in only 1 of the 14 Bretton Woods episodes and in only 5 out of 127 episodes in the post-Bretton Woods era.[18]

Below, the results from each of the tests are discussed for each era. Due to the high persistence of the data in the post-war eras, we see that levels regressions on the Bretton Woods and post-Bretton Woods era run the risk of spurious regression, and the differences or PSS tests seem more appropriate, and we thus present evidence on these tests. At the same time, cointegration analysis on the Gold Standard data may not be appropriate as the interest rates do appear to be closer to stationarity.

[18] Using an ADF unit root test generates a similar pattern as those above with a slightly higher number of rejections. Likewise, using the optimal lag length of Ng-Perron 1996 rather than the MAIC allows a few more rejections in the Gold Standard era. The post Bretton Woods era are generally so far from rejecting a unit root that lag length is irrelevant.

Discussion of Results

In some sense, the most basic test of the data is to look at annual differences. This specification tests whether a change in the base interest rate is followed abroad. The reason for using annual data when examining pooled differences is that with higher frequency data, differences in the dynamics across countries tend to wash out the results. After one year, however, one may hope more of the dynamics have settled down and the short-term differences in adjustment across countries are less salient.

The results of estimating equation (1) are shown in Table 1, which presents striking differences across the time periods. If we simply pool data within periods, with no distinction across exchange rate regimes, we see that the Gold Standard and post-Bretton Woods eras have fairly similar slope coefficients (0.42 and 0.36, respectively), especially when compared to the Bretton Woods era (–0.11).[19] The big difference between the Gold Standard and the current era appears in the R^2, where Gold Standard base-rate changes can explain a large fraction of local-rate changes, but modern base rate changes cannot (R^2 = 0.26 versus 0.03, respectively). For the Bretton Woods era, the R^2 is 0.01, showing that the capital controls of the era seem to have essentially shut down the mechanism by which local countries are forced to follow the base country.

While there are not enough floating episodes under Bretton Woods to examine the floats separately, the pegs and floats do appear to behave differently in both the Gold Standard and modern eras. Intra-regime differences, though, are more striking under the Gold Standard. While the coefficients (and associated standard errors) on the base rate are similar for pegs in the two eras, the R^2 is much higher under the Gold Standard, (0.37 versus 0.19), implying fewer interest rate changes for reasons other than following the base rate. The lower R^2 that we find for modern pegs could reflect the relatively greater credibility of gold standard exchange rate commitments or the more frequent presence of capital controls in the modern era, although we can draw no firm conclusions without further investigation. In neither era do we see slope coefficients or R^2s close to 1, which a model with no exchange rate bands, costless arbitrage, and perfect regime credibility would imply. One of the most useful roles of the Gold Standard data is as a benchmark. Looking at the Gold Standard results, we should not be surprised by the somewhat low slope coefficients and R^2s that we find for pegs in the current era. As we have noted, even in the essentially capital-control free era of the Gold Standard, pegs did not show a perfect correlation with the base interest rate due to exchange rate movements within the gold points, which gives latitude for short-term interest-rate divergence (Svensson 1994). Now as then, most exchange rates that we consider to be pegged actually do move within specified narrow bands.

The more significant difference between the Gold Standard and modern eras appears in examining the floats. The Gold Standard floats on average show almost no connection to the base rate (b = 0.05, std. error = 0.11, R^2 = 0), while the slope coefficient for the modern era is much closer to that for the pegs (b = 0.27). The R^2 is still quite low (0.01) implying considerable room for maneuver for reasons other than following the base rate, but the gap in the coefficient is nowhere near as large for the modern era as it is in the Gold Standard data.[20]

This point is seen more sharply when one pools the data and includes and interaction term for pegging times the base interest rate:

$$\Delta R_{it} = a + b_1 \Delta R_{bit} + b_2 PEG_{it} \Delta R_{bit} + u_{it}. \tag{4}$$

While under the Gold Standard, b_1 is close to zero (0.05) and is not statistically significantly different from zero (std. error = 0.08), the coefficient on b_2 is 0.43 (0.08). With current data, b_1 is estimated as 0.26 (0.08) and b_2 as 0.19(0.09).[21]

The general message from these tests is that the exchange rate regime does affect the extent to which a country follows the base, but that the capital controls of the Bretton Woods era seemed to stop or significantly slow these reactions. The results from the Gold Standard shed some light on the modern results first by showing that the lack of a coefficient or R^2 close to 1 is not a surprise. Our comparison with the Gold Standard also shows, though, that the non-pegs of the modern era seem to respond more to changes in the base than was true up to 1914.

Thus, the within era information points to a significant role of the exchange rate regime in the extent to which a country follows the base. Conversely, the across era comparisons help

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[19] We refer in the text to the *de facto* results, though *de jure* is always close.

[20] It should be noted that some interest rate series in the modern era are entirely flat (see Shambaugh 2001). When these are excluded, the results for pegs become (b = 0.59, std. error = 0.04, R^2 = 0.26) and for floats (b = 0.28, std. error = 0.08, R^2 = 0.01). Thus, removing these mostly small Caribbean countries increases the gap between the two and raises the R^2 on the pegs.

[21] Again, excluding the interest rates which are constant over the entire sample generates b_1 = 0.31(0.08), b_2 = 0.27(0.09).

show the role of capital controls. As a final check, we pool the eras and test the importance of the exchange rate regime and capital control status. These results are reported in table 2.

First, we divide the sample into pegs and floats and then capital controls and no capital controls. Looking first at the exchange rate regime, we still see a stark difference between pegs and floats with coefficients of 0.44 vs. 0.27 and R^2s of 0.19 vs. 0.01. Similarly, we see an impact of capital controls with non-capital control countries showing a coefficient of 0.56 and R^2 of 0.10 and capital control countries showing a coefficient of 0.27 and R^2 of 0.01. Thus, both legs of the trilemma appear to be validated. Tables 2b and 2c test more carefully the interaction of these two sides.

While, as expected, pegs with open capital markets have the highest coefficient and R^2, there is some evidence of fear of floating as open capital market floats have a coefficient of 0.52 compared to the pegs' 0.59. On the other hand, the R^2 still shows a considerable gap with pegs having an $R^2 = 0.29$ and floats only 0.06. Thus, despite moving with the base country to some extent, only a small portion of the changes in the floats' interest rates can be explained by the base rate. In addition, we do not find that capital controls are completely isolating. Pegs with capital controls have higher coefficients and R^2s than the floats with closed markets.

The pooled data (table 2c) shows that both the exchange rate regime and capital controls matter, though in columns 1 and 2, capital controls appear to be more important. Once again, removing the countries in the modern era for whom interest rates never change (and thus appear to be purely administratively set or not relevant rates) increases the importance of the peg variable (columns 3 and 4). We also see in columns 2 and 4 that the impact of capital controls and exchange rate regimes are not purely additive as the interaction term of the two is negative. This fits with the logic of the trilemma as either policy will provide a good degree of independence and adding the second increases the independence, but not by as much as the policy on its own.

Thus, this evidence in total supports a modified view of the trilemma. While both the exchange rate regime and capital controls clearly affect autonomy, the combination of floating with capital controls seems to provide unfettered autonomy and removing either side compromises some autonomy. Still, floats with open markets and pegs with closed seem to have some autonomy, and, as expected, pegs with open markets have the least of all.

Levels Analysis

Finally, we can learn considerably more about the relationships of the local countries to the base countries by examining specific individual country relationships and testing both the level relationships and the dynamic adjustment. Rather than assuming a levels relationship exists and estimating it, we test for the presence of level relationships and examine the dynamics of the system. Table 3 shows the results for individual country regime episodes as well as averages across pegs and floats by era. This is probably the most useful specification for testing whether there was monetary independence under the Gold Standard. We see that in only two cases are we forced to assume the order of integration as in all others the test statistic either passes both critical values or fails to do so.

Looking at both the *de jure* and *de facto* classifications, we see that there were floating rate episodes that demonstrate considerable independence. Perhaps the clearest evidence comes from the fact that the levels relationships have negative signs, implying that rates moved opposite one another over the long run for two of the seven floats in the *de jure* and all five under *de facto*. These backwards levels relationships make the average half lives somewhat misleading as an indicator of how closely these countries followed the base in that many of the floats moved away from the base. Portugal, Russia, and Spain (all *de facto* and *de jure*) show considerable independence from the UK interest rate.

At the same time, there certainly were some countries that exhibited what today has been called a fear of floating. Despite not being on the Gold Standard and thus officially floating, some countries certainly followed the base interest rate quite closely. In some cases, such as the Netherlands 1870–75, the exchange rate is in fact pegged despite no announcement, so it is unsurprising to see the interest rate move so tightly with the base. Austria *de facto* pegs on and off during its official float and it too shows a tight relationship with the base.[22]

In general, the pegs show little independence. With the exception of India's early pegging experience, all episodes show the expected sign, and adjustment tends to be quick. The only episodes with slow adjustment (Spain's early *de facto* peg and Portugal's late *de facto* peg) are

[22] Readers may notice a large difference between estimates for India's *de facto* and *de jure* floats despite a difference of only 18 observations. Early in India's *de jure* float, there were some large changes in the interest rate that affect the results. India was in a *de facto* peg at the time and thus these observations are not in the *de facto* float. In addition, this episode is quite sensitive to the lag length chosen. While the standard deviation of the levels estimate across different lag specifications is no more than .02-.05 for most episodes, it is quite large (over 1) for the Indian float.

short ones. The long-time members of the Gold Standard show levels slope relationships ranging from 0.4 to 0.6 and half-lives of adjustment from two to five months.

The fact that the levels relationship is less than unity in all cases may seem odd until one considers the era. Because the UK interest rate always resided within reasonably small bands, countries could partially adjust to the UK rate change and use the margin afforded by the gold points and arbitrage costs to cover the rest. If the UK rate continued long in one direction, a foreign country would eventually have to adjust fully, but because the UK rate reverted toward its mean, the estimated level relationship can be less than one. This reflects the moderate independence in exchange rate target zones of the kind described by Svensson (1994). At the same time, though, countries had to move quite quickly to adjust to any change at all. If one imposes a long-run levels coefficient of unity, then the adjustment speeds appear to slow down because in truth, the rates never did fully adjust. Thus, Bordo and MacDonald's (1997) depiction of monetary independence under the Gold Standard due to slow adjustment speeds may require re-examination. (Their study covered only the UK, France, and Germany, with a data set starting in 1880.) By assuming nonstationarity in the Gold Standard interest-rate data and imposing a unit cointegration coefficient between national rates (consistent with their estimates), the Bordo-MacDonald study may give an exaggerated impression of adjustment speeds. In our data most levels coefficients are significantly below 1, and allowing for this apparently raises estimated adjustment speeds. There was some room for independence under the Gold Standard, but it seems to appear more obviously for countries that were floating. Some floats followed the UK rate anyway, but some did not and exhibited their potential for monetary independence.

The limited room for maneuver under the Gold Standard appears in the long-run levels coefficients that are below 1. Should one consider a levels coefficient of 0.6 and a half life to shocks of under three months to be evidence of monetary independence? Given the insignificant levels relationships and/or half-lives of over a year that are found in some floats, we would judge that pegs did not have extensive independence under the Gold Standard.

The Bretton Woods era provides little room for comparison across regime types, since only Brazil is a non-peg country. Brazil certainly shows independence with an insignificant levels relationship and a very slow adjustment speed (half-life of over 17 months). At the same time, capital controls appear to have isolated some of the pegs. Jamaica, Japan, and Pakistan all have negative levels relationships and South Africa has an insignificant one with a very high half life. Even in some countries where the levels relationship is close to one (e.g. Germany and Canada), adjustment is slow enough that the relationship is not significant. Sweden, France, and the Netherlands have levels relationships near one and half-lives from 2 to 5 months, implying a tighter relationship than even the Gold Standard pegs. On average, though, for only 38% of the pegs can we reject the hypothesis that there is no levels relationship. The average adjustment speed for pegs is over seven months, demonstrating far more flexibility than under the Gold Standard, which, of course, was the reason for the design of the Bretton Woods system in the first place.

There are too many post Bretton Woods episodes to consider individually. For pegs and occasional pegs, there are very few instances of negative levels relationships making the averages more reliable. Compared to the other eras, we see three striking features. The first is that far fewer of the episodes are statistically significant. Not only are fewer significant when assuming I(0), but because the data are so close to being nonstationary, one should probably consider the I(1) critical value as the more relevant one. This means that even fewer episodes are significant, especially compared to the Gold Standard. This finding, though, may partly result from the slightly shorter time spans of the modern episodes.

Secondly, the adjustment speeds are slower in the post Bretton Woods era. This is especially true under the floats for which the average half-life is over 30 months. Over 50% of the floating rate episodes show half lives over a year, implying a considerable amount of autonomy. Like the low R^2s in the differences regressions, these results seem to point to a significant amount of autonomy for floating countries and a substantive difference between pegs and floats, in contrast to previous work on the subject arguing that only a few large countries can pursue independence regardless of exchange rate regime. The PSS results show even more independence for the floats, though, as the average non-peg levels relationship is in fact negative. This suggests that on average, long episodes of non-pegs today show as much independence as those in the Gold Standard.[23] This result is directly opposite to the suggestions of Frankel et al. (2000, 2002) and the message of the "fear of floating" literature in general.[24] Pegs are also

[23] While this result may appear to be inconsistent with the differences regressions (which show non-pegs having a tighter bond with the base today than in the past), the incongruity is understandable. The PSS results are only for long-standing non-pegs, while the pooled results include as non-peg the non-peg years of countries that flip back and forth.
[24] See Shambaugh (2001) for a more detailed discussion of these results. Borenzstein et al. (2001), using a different methodology and country sample, obtain results on monetary independence that support our conclusions.

somewhat slower to adjust than during the Gold Standard, with an average adjustment speed of −0.19 compared to the *de facto* Gold Standard's of −0.27. Still, the average adjustment in the current period is faster than under Bretton Woods.

While adjustment is slightly slower for the modern era, the levels relationships are closer to one. The average for pegs is 0.93, compared to between 0.4 and 0.5 for the other eras . Once again, the explanation may lie in the rapidly moving and near unit root base interest rates characterizing the modern era. In addition, even if capital controls are present, if countries do not adjust fully over time, the exchange rate may not last.

Comparisons across different eras show that the while the Gold Standard saw pegs with low levels relationships but fast adjustment and the modern era has seen countries with slower adjustment but levels relationships closer to one, the Bretton Woods era had both slower adjustment speeds and lower levels relationships. Comparing the Gold Standard and modern eras, we see that there appears to be room for floats to have independence especially when compared to the fixed exchange rate countries. The high levels relationships for pegs during the current era are a switch from the past. But the adjustment speeds seen today do not necessarily demonstrate the exigencies of today's capital markets, because those speeds in fact appear a bit slower than in the past.[25]

While useful for different types of data, the different tests all seem to lead to similar conclusions. Countries that peg do indeed have less monetary freedom than floaters, although the capital controls of Bretton Woods did succeed in weakening the linkages among national interest rates.[26] In addition, despite gold points and less sophisticated arbitrage and communications technology, peggers under the Gold Standard arguably had even less freedom than do peggers today.

We must acknowledge that we cannot speak to the intent of the monetary authorities regarding this evidence towards independence. It is possible that common shocks keep the pegs moving together not the constraint of the exchange rate regime. Alternatively, floats may not be demonstrating independence, but simply may be buffeted by market changes. Evidence in Shambaugh (2001) suggests that common shocks are not behind the correlation of pegs' interest

[25] It should be noted that the adjustment speeds are not simply lower because the levels relationships are higher. Even when one imposes the level at one, the Gold Standard data shows a faster adjustment as can be seen on the EC half-life line in the tables.

rates, but without a fully specified model of monetary authorities' intentions and data sufficient to test it, we are left to acknowledge the caveat and move on.

Concluding Thoughts

The overall lesson is that the trilemma makes sense as a guiding policy framework. Exchange rate pegs do result in a substantially closer connection to the base country interest rate than do floats. They react more to changes in the base rate; the base rate can explain more of the changes in the local rate for pegs; and, the pegs react more quickly and have a stronger long run relationship than floats. Absent capital controls, countries choosing to peg lose considerable monetary independence. At the same time, floaters appear to have a reasonable amount of autonomy even when there are no capital controls. Pegs are rarely completely handcuffed (in any era) though, because of exchange rate bands and, possibly, arbitrage costs. Conversely, floats are never purely free, as floaters do often choose to follow the base to some degree.

The results also show some interesting perspectives across eras. Pegs in both eras of open markets show fairly similar relationships with the base interest rate. In particular, pooled regressions look somewhat similar, though the R^2 for the Gold Standard is always higher. In line with conventional wisdom, the Gold Standard was not an era with extensive independence for the pegged countries. The pooled slope coefficients on interest-rate differences are strong, and adjustment speeds faster, in the Gold Standard era. The Gold Standard is a useful benchmark from which to judge the relationships between countries today. We see that slope coefficients and R^2 for pegs significantly below unity should not be a surprise.

Based on the differences regressions, the floats of today do appear to have more of a connection to the base rate than in the past (especially absent capital controls). But there are many countries, not just a few large ones, which consistently move their interest rates in ways that imply no long run levels relationship with the base and show much slower adjustment to shocks. The longer run floats that are used as episodes show a very weak connection to the base with negative or insignificant relationships and slow adjustment. The fact that there are a few examples of countries exhibiting "fear of floating" under the Gold Standard strengthens the

[26] Shambaugh (2001) finds that capital controls are quite important in distinguishing the degree to which a country follows the base in the modern era as well.

argument that it is not that floats cannot exert independence (for some certainly could in the Gold Standard) but that some simply choose not to do so.

Finally, we can also see in interest rate data that the architects of Bretton Woods achieved their goal of exchange rate stability with more room for autonomy. Despite fairly rigid pegs, the Bretton Woods era shows both weaker levels relationships and slower adjustment speeds to the long run. As capital controls became more porous over the 1960s, the combination of exchange rate pegs and monetary independence became untenable. Despite recent challenges to the trilemma as a concept, we see its lessons borne out over a broad range of historical experience.

Appendix: List of data available and episodes:

Gold Standard:

Data available:

	from	to
France	1–1870	6–1914
Germany	1–1870	6–1914
Netherlands	1–1870	6–1914
Belgium	1–1870	6–1914
Italy	1–1885	6–1914
Austria	1–1870	6–1914
Spain	1–1883	6–1914
Portugal	1–1885	6–1914
Russia	1–1870	8–1900
Denmark	5–1884	6–1914
US	11–1880	6–1914
India	5–1884	6–1914
Switzerland	12–1892	6–1914
Sweden	12–1892	6–1914
Norway	1–1894	6–1914

Gold Standard de jure Episodes

Pegs

	actual		with data available		
	from	to	from	to	
France	1–1870	6–1914	2–1872	6–1914	
Germany	12–1871	6–1914	12–1872	6–1914	
Netherlands	6–1875	6–1914	6–1876	6–1914	
Belgium	1–1870	6–1914	1–1870	6–1914	
Italy	1–1884	12–1893	1–1885	12–1893	
Austria	8–1892	6–1914	8–1893	6–1914	
Portugal	1–1870	6–1891	1–1885	6–1891	
Denmark	1–1873	6–1914	5–1884	6–1914	
US	1–1879	6–1914	11–1880	6–1914	
India	1–1898	6–1914	1–1899	6–1914	
Switzerland	1–1870	6–1914	12–1892	6–1914	
Sweden	1–1873	6–1914	12–1892	6–1914	
Norway	1–1873	6–1914	1–1894	6–1914	
Russia	*1–1897*	*6–1914*	*1–1897*	*8–1900*	*insufficient data*
Spain	*1–1870*	*12–1882*	*none*		*no data*
Non-Pegs					
Netherlands	1–1870	5–1875	3–1871	5–1875	
Italy	1–1894	6–1914	1–1894	6–1914	
Austria	1–1870	7–1892	2–1871	7–1892	
Spain	1–1883	6–1914	1–1883	6–1914	
Portugal	7–1891	6–1914	7–1891	6–1914	
Russia	1–1870	12–1896	2–1871	12–1896	
India	5–1884	12–1897	5–1884	12–1897	
Germany	*1–1870*	*11–1871*	*insufficient data*		

Sweden, Norway, Denmark, US, Australia all end float before data becomes available

Gold Standard de facto Episode

Pegs	From	To
France	4–1872	6–1914
Germany	7–1872	6–1914
Netherlands	1–1870	6–1914
Belgium	12–1872	6–1914
Italy early peg	1–1885	2–1892
Italy late peg	10–1902	6–1914
Austria early peg	6–1879	8–1888
Austria late peg	5–1894	6–1914
Spain early peg	1–1883	1–1890
Spain late peg	6–1910	6–1914
Portugal early	1–1885	1–1890
Portugal late peg	6–1910	6–1914
Russia	9–1894	8–1900
Denmark	5–1884	6–1914
US	1–1883	6–1914
India early peg	1–1899	3–1903
India late peg	5–1904	6–1914
Switzerland	12–1892	6–1914
Sweden	12–1892	6–1914
Norway	1–1894	6–1914
Floats		
Spain	8–1891	11–1901
Portugal	8–1891	11–1901
Russia 1	12–1875	2–1882
Russia 2	11–1887	9–1893
India	10–1890	12–1897

Bretton Woods:

All countries are pegged except Brazil (full) and Canada (1960–61, 5-1970ff)
Data available: (annual data is available for all countries from 1959–70)

	From	To	series used
Belgium	1–1959	12–1970	T-bill
Germany	12–1959	12–1970	mon.mkt
France	1–1964	12–1970	mon.mkt
India	1–1959	12–1970	mon.mkt
Jamaica	5–1961	12–1970	mon.mkt
Japan	1–1959	12–1970	mon.mkt
Netherlands	1–1960	12–1970	mon.mkt
Pakistan	1–1964	12–1970	mon.mkt
South Africa	1–1959	12–1970	T-bill
Sweden	12–1962	12–1970	T-bill
Trinidad and Tobago	12–1964	12–1970	T-bill
UK	1–1964	12–1970	T-bill
Canada	1–1959	12–1970	T-bill
Brazil	1–1964	12–1970	mon.mkt

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Table 1. Differences Regressions on Annual Data[27]

Gold Standard *de jure* classification

	pool	peg	Float	pool
# observations	499	350	140	490
b	.42	.52	.16	.16
b std error	.03	.04	.06	.06
b_2				.36
b_2 std error				.07
R^2	.26	.36	.05	.30

Gold Standard *de facto* classification

	pool	peg	float	pool
# observations	499	402	82	484
b	.42	.51	.05	.05
b std error	.03	.04	.10	.10
b_2				.46
b_2 std error				.11
R^2	.26	.38	.00	.31

Bretton Woods

	pool	peg	float	pool
# observations	154	138	na	na
b	-.11	-.05		
b std error	.14	.12		
b_2				
b_2 std error				
R^2	.01	.00		

Post Bretton Woods

	pool	peg	float	pool
# observations	1920	748	1103	1848
b	.36	.46	.27	.26
b std error	.05	.04	.08	.08
b_2				.19
b_2 std error				.09
R^2	.03	.19	.01	.03

[27] DW statistics never indicate autocorrelation problems for the differences regressions. Due to the cross-sectional nature of the data, robust standard errors are reported.

Table 2. Annual differences on pooled sample

Table 2a

	pool	peg	float	no capital controls	capital controls
# observations	2573	1288	1257	1090	1454
b	.35	.44	.26	.56	.27
b std error	.04	.03	.08	.05	.06
R^2	.03	.19	.01	.10	.01

Table 2b

	peg & no capital controls	peg with capital controls	float & no capital controls	float with capital controls
# observations	630	655	478	753
b	.59	.38	.52	.15
b std error	.05	.04	.07	.11
R^2	.29	.15	.06	.00

Table 2c

	pool	pool	pool no constant R	pool no constant R
# observations	2542	2542	2319	2319
b	.21	.19	.23	.20
b std error	.09	.11	.10	.11
b_2	.15	.19	.23	.30
b_2 std error	.09	.11	.09	.12
b_3	.29	.35	.26	.36
b_3 std error	.08	.13	.08	.13
b_4		-.14		-.23
b_4 std error		.14		.15
R^2	.03	.03	.03	.03

b = coefficient on R*
b_2 = coefficient on peg * R*
b_3 = coefficient on no capital controls * R*
b_4 = coefficient on no peg * capital controls * R*

no constant R signals that interest rates constant over the entire sample have been removed

Table 3. PSS results

Gold Standard *de jure*

	# obs	opt. lag	θ	γ	tstat θ	sig at 0	sig at 1	half-life	<3	3 to 12	>12	EC θ
Pegs												
France	520	0	-0.19	0.56	-7.37	1	1	3.30	0	1	0	-0.07
Germany	499	8	-0.20	0.59	-4.20	1	1	3.14	0	1	0	-0.19
Netherlands	457	0	-0.18	0.63	-7.26	1	1	3.46	0	1	0	-0.13
Belgium	529	1	-0.22	0.60	-7.33	1	1	2.80	1	0	0	-0.14
Italy	107	0	-0.23	0.47	-3.48	1	1	2.69	1	0	0	-0.17
Austria	251	7	-0.10	0.27	-2.76	0	0	6.82	0	1	0	-0.04
Portugal	77	0	-0.08	1.03	-1.68	0	0	8.00	0	1	0	-0.10
Denmark	361	1	-0.09	0.57	-4.09	1	1	7.14	0	1	0	-0.08
US	401	1	-0.66	0.41	-10.00	1	1	0.64	1	0	0	-0.35
India	198	8	-0.28	-0.13	-3.72	1	1	2.08	1	0	0	-0.21
Switzerland	258	0	-0.22	0.43	-5.33	1	1	2.79	1	0	0	-0.04
Sweden	258	2	-0.14	0.39	-3.87	1	1	4.73	0	1	0	-0.07
Norway	245	0	-0.11	0.54	-4.41	1	1	5.82	0	1	0	-0.05
Floats												
Netherlands	60	0	-0.34	0.96	-4.19	1	1	1.67	1	0	0	-0.37
Italy	256	0	-0.21	0.49	-6.16	1	1	2.89	1	0	0	-0.11
Austria	264	0	-0.19	0.50	-5.41	1	1	3.35	0	1	0	-0.10
Spain	386	0	-0.09	0.06	-3.94	1	1	7.76	0	1	0	-0.02
Portugal	276	6	-0.05	-0.10	-2.03	0	0	13.41	0	0	1	-0.01
Russia	309	0	-0.19	-0.06	-6.14	1	1	3.36	0	1	0	0.00
India	163	1	-0.23	0.94	-5.12	1	1	2.60	1	0	0	-0.18
floats			-0.19	0.40	-4.71	86%	86%	5.00	43%	43%	14%	-0.11
pegs			-0.21	0.49	-5.04	85%	85%	4.11	38%	62%	0%	-0.13

opt lag	the optimal lag length chosen based on Akaike information criteria
θ	the adjustment speed to shocks in the levels relationship
γ	the levels relationship
t-stat θ	the t-stat on the adjustment speed which is used to determine the significance of the levels relationship.
sig at 0	signifies whether we can reject no levels relationship if we assume the data is stationary
sig at 1	signifies whether we can reject no levels relationship if we assume the data is non-stationary
half-life	the half-life of a shock based on the adjustment speed
EC θ	the adjustment speed when one runs the data in error-correction form assuming a level relationship equal to 1.

Table 3. PSS results (cont.)

Gold Standard *de facto*

	# obs	opt. lag	θ	γ	tstat θ	sig at 0	sig at 1	half-life	<3	3 to 12	>12	EC θ
Pegs												
France	507	4	-0.13	0.45	-4.45	1	1	4.90	0	1	0	-0.07
Germany	504	8	-0.20	0.59	-4.22	1	1	3.12	0	1	0	-0.19
Netherlands	529	0	-0.21	0.69	-8.85	1	1	2.98	1	0	0	-0.16
Belgium	499	4	-0.17	0.65	-5.18	1	1	3.64	0	1	0	-0.13
Italy early peg	95	0	-0.29	0.44	-4.16	1	1	2.04	1	0	0	-0.06
Italy late peg	141	1	-0.19	0.38	-3.41	1	1	3.31	0	1	0	-0.14
Austria early peg	111	7	-0.70	0.27	-4.85	1	1	0.57	1	0	0	-0.13
Austria late peg	242	7	-0.09	0.28	-2.59	0	0	7.35	0	1	0	-0.04
Spain early peg	93	0	-0.02	1.46	-0.95	0	0	28.96	0	0	1	-0.04
Spain late peg	49	0	-0.32	0.31	-3.03	1	0	1.05	1	0	0	-0.06
Portugal early	60	0	-0.06	1.14	-1.01	0	0	11.61	0	1	0	-0.10
Portugal late peg	49	3	-0.64	0.01	-25.02	1	1	0.69	1	0	0	0.00
Russia	72	0	-0.26	0.29	-3.26	1	1	2.32	1	0	0	-0.05
Denmark	361	1	-0.09	0.57	-4.09	1	1	7.14	0	1	0	-0.08
US	377	1	-0.66	0.42	-9.64	1	1	0.64	1	0	0	-0.35
India early peg	60	6	-0.32	-1.15	-2.43	0	0	1.83	1	0	0	-0.11
India late peg	122	8	-0.49	0.25	-3.09	1	0	1.04	1	0	0	-0.39
Switzerland	258	0	-0.22	0.43	-5.33	1	1	2.79	1	0	0	-0.04
Sweden	258	2	-0.14	0.39	-3.87	1	1	4.73	0	1	0	-0.07
Norway	245	0	-0.11	0.54	-4.41	1	1	5.82	0	1	0	-0.05
Floats												
Spain	112	0	-0.10	-0.14	-2.39	0	0	6.78	0	1	0	-0.02
Portugal	112	2	-0.15	-0.08	-2.53	0	0	4.31	0	1	0	-0.01
Russia 1	59	0	-.18	-0.09	-2.04	0	0	3.39	0	1	0	.02
Russia 2	71	2	-.13	-0.41	-2.12	0	0	4.97	0	1	0	-.03
India	145	6	-0.22	-0.16	-3.34	1	1	2.75	1	0	0	-0.18
floats			-0.16	-0.18	-2.44	20%	20%	4.44	20%	80%	0%	-0.04
pegs			-0.27	0.42	-5.28	80%	70%	4.79	50%	45%	5%	-0.12

Table 3. PSS results (cont.)

Bretton Woods

	# obs	opt. lag	θ	γ	tstat θ	sig at 0	sig at 1	half-life	<3	3 to 12	>12	EC θ
Belgium	144	1	-0.12	0.56	-3.18	1	0	5.30	0	1	0	-0.05
Germany	132	0	-0.10	1.10	-2.99	1	0	6.66	0	1	0	-0.10
France	83	0	-0.19	1.09	-3.35	1	1	3.36	0	1	0	-0.15
India	144	3	-0.40	0.17	-6.06	1	1	1.35	1	0	0	-0.11
Jamaica	115	1	-0.06	-0.14	-2.45	0	0	11.23	0	1	0	-0.01
Japan	144	2	-0.13	-0.29	-2.42	0	0	4.96	0	1	0	-0.04
Netherlands	131	0	-0.13	1.07	-3.34	1	1	5.06	0	1	0	-0.11
Pakistan	83	0	-0.05	-0.11	-1.62	0	0	12.50	0	0	1	-0.02
South Af	144	4	-0.03	0.46	-2.24	0	0	25.47	0	0	1	-0.02
Sweden	96	0	-0.34	0.94	-4.59	1	1	1.66	1	0	0	-0.24
Trin and Tob	72	0	-0.13	0.13	-2.16	0	0	5.15	0	1	0	-0.01
UK	83	1	-0.19	0.60	-3.51	1	1	3.37	0	1	0	-0.10
Canada	89	2	-0.14	0.90	-2.60	0	0	4.65	0	1	0	-0.13
Brazil (float)	83	0	-0.04	0.07	-1.32	0	0	17.72	0	0	1	-0.05
floats			-0.04	0.07	-1.32	0%	0%	17.72	0%	0%	100%	-0.05
pegs			-0.15	0.50	-3.12	54%	38%	6.98	15%	69%	15%	-0.08

Post Bretton Woods Averages

	θ	γ	tstat θ	sig at 0	sig at 1	half-life	<3	3 to 12	>12	EC θ
floats	-0.06	-0.43	-2.21	31%	25%	35.16	3%	41%	56%	-0.04
occasional pegs	-0.11	0.68	-2.38	24%	20%	10.56	16%	56%	28%	-0.08
pegs	-0.19	0.93	-2.56	43%	27%	7.84	31%	50%	19%	-0.11

Figure 1a. Base in Gold Standard era

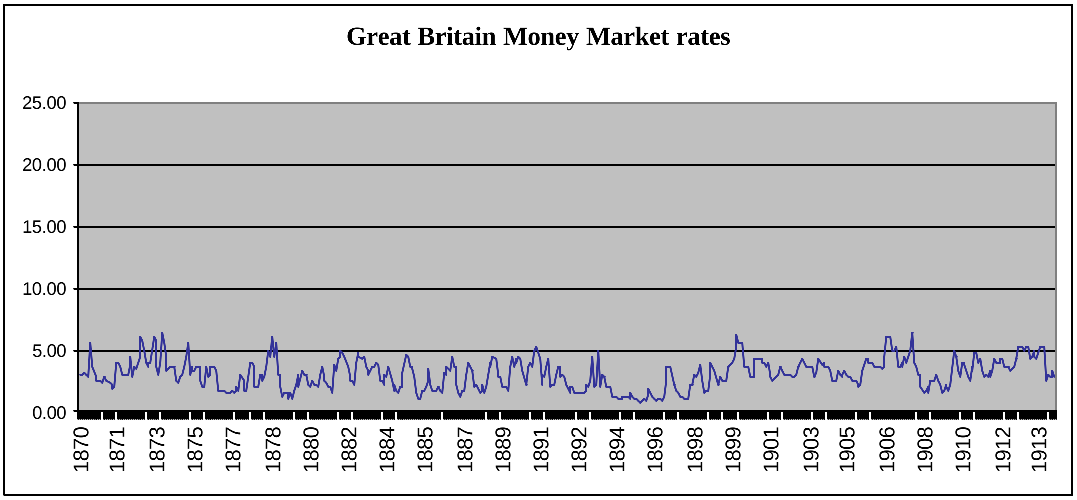

Figure 1b. Base in Bretton Woods era

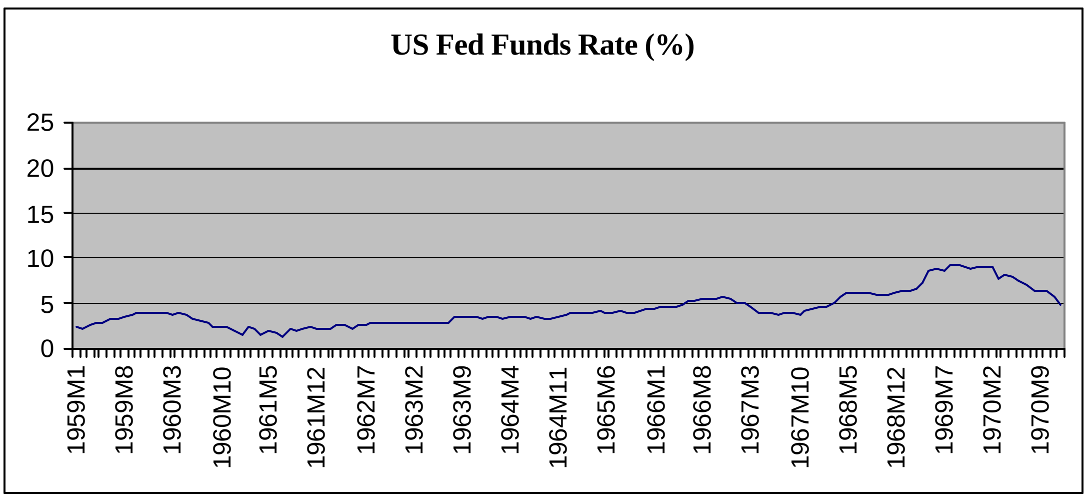

Figure 1c. Base in Post Bretton Woods era *(not all countries are based to US, but it is an example)*