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Abstract

My research explores the effect of monetary systems on how quickly developing countries recover from economic crises. Existing research typically assumes that countries that do not take on a given policy are an appropriate counterfactual for countries that do. Often, these two groups of countries are not comparable, and tests of this assumption in my research indicate these two groups are, on average, different across determinants of the outcomes of interest. Therefore, many established results are likely biased. I address these issues of non-random selection of macroeconomic policies using quasi-experimental methods, such as propensity score matching, coarsened exact matching, and synthetic counterfactuals, to estimate treatment effects. These econometric methods allow me to identify control and treatment group countries that are observationally similar across relevant factors, and therefore unbiasedly estimate the average treatment effect on the treated.

In my first chapter, I use the synthetic control method (SCM) to estimate the impact of official dollarization in Ecuador and quasi-dollarization, in the form of a currency board, in Argentina. The SCM is a relatively new econometric technique, developed by Abadie and Gardeazabal (2003), and it is uniquely suited for evaluating a monetary policy, such as dollarization, where the potential treated sample is so small as to make traditional econometric techniques ineffective. I show that these monetary arrangements were effective at controlling inflation in both countries. Interestingly, in contrast with previous research, I find these policies had no impact on real income. Despite the success of these policies, Argentina abandoned its currency board in 2002, devalued its

currency, and repudiated much of its outstanding government debt causing renewed inflation and a loss of significant consumer wealth. Ecuador, conversely, continues to operate under dollarization, fourteen years after implementation.

This line of research is important for policymakers who are considering joining a currency union, such as the European Union (EU) or the de-factor U.S. dollar or Sterling unions. In particular, my research confirms one must give careful consideration to government spending balance and the timing of one's business cycle with that of the other members of the union. One example is Scotland, which in September had referendum for independence and if passed, would have formed a de-facto currency union with Great Britain or join the EU. Other examples are countries, such as Poland, the Czech Republic, Croatia, etc., who are EU member countries but are still debating whether to adopt the Euro as their sole legal tender.

In the second chapter of my dissertation, I analyze the effect of exchange rate regime selection on currency depreciation and equity market shocks during a currency crisis, using the 1997 Asian financial crisis as a case study. Previous studies on this topic find that countries with fixed exchange rates at the onset of the crisis suffered significantly greater currency depreciation and more severe declines in equity market than countries with flexible exchange rates. I find fixed exchange rate countries and floating exchange rate countries are different across several key determinants of currency depreciation. After accounting for these differences, I find that a country's exchange rate regime at the onset of the crisis had no impact on how much its currency depreciated in 1997 vis-à-vis the USD but did cause stock returns to fall by approximately 40 percentage points relative to equity returns in countries with flexible exchange rates at the onset of the crisis. Moreover, I show the estimate effects of exchange rates regimes on currency depreciation and stock market performance during the Asian financial crisis vary depending on the type of exchange rate regime classification system used to sort countries into treated and control group.

Finally, using a sample of emerging market and developing countries, I analyze the financial consequences of having a fixed exchange rate at the onset of a speculative attack or currency crisis. Research typically treats exchange rate regime selection as exogenous. However, I find that countries that have a pegged exchange rate before a currency crisis are, on average, different from countries with a flexible exchange rate on variables that affect the outcomes of interest. After correcting for these differences, I find a country's pre-crisis exchange rate regime has no impact on real income, equity returns, or inflation during or immediately following the crisis. This paper contributes to the literature by considering all crises and attacks from 1972 to 2003, rather than using a case study approach, and by analyzing only the subset of countries that actually experience these events. Consequently, these findings are generalizable across all crises and better isolate the average treatment effect on the treated (ATT) of having a peg before a crisis on a country's performance during the crisis.

Chapter 1:

The Impact of Dollarization and Currency Boards on Income and Inflation: A Synthetic Control Analysis

Introduction

Do political and economic conditions ever become so dire that the optimal decision is to surrender control of monetary policy to the United States? In the decade from 1990 to 2000, two countries, Argentina and Ecuador, decided the answer to this question was "yes." From 1982-1991, annual inflation in Argentina averaged 794% and peaked at over 3000% in 1989. Inflation became such a pervasive and damaging problem that, in 1991, Argentina, under President Carlos Menem, adopted a currency board, with a one-to-one fixed peg and full convertibility between the Peso and the USD. In the 1990's, Ecuador also failed to control inflation, averaging 44% from 1991-2000. Like Argentina, Ecuador opted to tie itself to U.S. monetary policy. Unlike Argentina, however, Ecuador chose to dollarize formally in 2000, taking the USD as its official currency.

Prima facie evidence indicates these policies were effective at stabilizing prices. Inflation in Argentina averaged 4.2% per-year over the ten years after President Menem implemented the currency board, while in Ecuador, prices rose on average 8.6% annually in the decade following dollarization. Despite this apparent success, three questions remain unanswered. First, did the currency board in Argentina and dollarization in Ecuador causally reduce inflation? Second, did these policies reduce inflation at the cost of economic growth? Third, despite adopting similar monetary arrangements, why did Argentina's currency board-convertibility system collapse in 2002 while Ecuador shows no signs of abandoning the dollar? It is not clear that traditional economic techniques can generate an unbiased estimate of the impact of dollarization and quasi-dollarization policies because only a handful of countries have ever utilized these monetary arrangements.

In this article, we use the Synthetic Counterfactuals Method (SCM), developed by Abadie and Gardeazabal (2003) and Abadie *et al.*, (2010) to estimate the treatment effect of formal dollarization in Ecuador and quasi-dollarization, in the form of a currency board, in Argentina on inflation and real per-capita GDP, over the period 1980-2010. The SCM is ideal for this type of study because it allows for an estimate of the treatment effect of a policy where the sample size of treated countries is so small that standard economic techniques are unable to identify a proper counterfactual for the treated country. Augmenting the SCM with the Permutation Test (Pitman, 1937a & 1937b), we find that these monetary arrangements had a statistically significant and negative impact on inflation and, in contrast to previous findings, no impact on real per-capita GDP.

While neither country faced an inflation abatement-economic growth trade-off, Argentina’s currency board proved economically and politically unsustainable, while Ecuador’s dollarization arrangement remains in effect today. A number of factors likely contributed to the divergent outcomes, including the high unemployment rate and high debt levels resulting from Argentina’s lack of fiscal prudence, Argentina’s inflexible labor market, and its over-valued exchange rate. We show that dollarization and quasi-dollarization are effective methods of controlling inflation, but the divergent policy persistence implies these monetary arrangements require structural adjustments to the labor market that create labor market flexibility and fiscal discipline (Krueger, 2002; World Bank, 2010; Blustein, 2005).

For developing countries, high inflation can be a particularly worrying problem because it is often autocatalytic, easy to start and difficult to arrest, and damaging to growth through its effect on consumption and investment behavior (Ghosh & Phillips, 1998). In cases where, currency devaluation, profligate government spending, heavy indebtedness, and loose monetary policy cause high inflation, formal dollarization has been suggested as an effective, albeit extreme, solution (Berg & Borensztein, 2000; Calvo, 2001).

A few authors have tested if dollarization or adopting a currency board has a statistically significant effect on inflation or real per-capita GDP. Ghosh *et al.*, (1998) find that, for the ten countries in their sample with currency boards, inflation under currency boards is about 4 percentage points lower than average inflation under other fixed exchange rate regime. The effect occurs, they argue, primarily by currency boards slowing the growth of the money supply. They also find economic growth is higher for countries with currency boards.[1] Edwards & Magendzo (2003) find that dollarized countries and territories have lower inflation and slower economic growth than non-dollarized countries, while, Edwards & Magendzo (2006) find no treatment effect of dollarization on economic growth but a positive effect of growth volatility.

This paper contributes to the existing literature in the following ways. (1) We use a data driven SCM to generate an appropriate counterfactual for our treatment countries, Ecuador and Argentina, thus avoiding the common limitations associated with the small sample problem. (2) We estimate the impact of dollarization and quasi-dollarization on two medium sized countries. Typically, the previous literature has focused on small,

[1] They caveat their sample is small, most of their currency board countries are island nations, and they do not observed a currency board country during a crisis period.

primarily island, nations and territories, limiting the generalizability of the results. (3) We provide a series of falsification tests to show that divergence between the outcome of interest between a treated country and its counterfactual is due to the treatment and not due to some unobserved difference between the two.

Identification of a proper counterfactual for the treated countries is particularly challenging, if not impossible, using traditional econometric techniques, given the small sample size. When assessing treatment effect, we would ideally observe the treated individual both with the treatment and at the same time without. Since this is impossible and dollarization is a policy not randomly assigned to countries, then we must find a way to identify a proper counterfactual for the treated country. The SCM is ideal in this case because it offers a data-driven method for constructing a synthetic counterfactual for the treatment country using data from countries that never took the treatment. Therefore, even though there is no one country that is identical to Ecuador or Argentina in factors that affect inflation and real per-capita GDP, except for their currency arrangement, the SCM allows for the creation of one from a composite of suitable control countries.

The rest of the paper proceeds as follows. Section 2 describes the literature on the predicted policy effects on inflation and real per-capita GDP and the history of Argentina's currency board and dollarization in Ecuador. Section 3 briefly explains how the SCM is used to estimate the treatment effect and describes the data utilized. Sections 4 and 5 detail the results of our estimation. Section 6 offers explanations on why Argentina's currency board ultimately failed, while Ecuador's dollarization persists. Section 7 concludes.

Dollarization in Argentina and Ecuador

Predicted Policy Effects

Aside from the predicted price stabilizing effect of dollarization and quasi-dollarization, these policies are also predicted to positively affect growth by increasing consumption spending by reducing uncertainty regarding government exchange rate policy (Mendoza, 2001), increasing financial depth (Nicolo *et al*., 2005), and increasing trade with dollarized countries (Glick & Rose, 2002). In countries that are highly liability dollarized, formally dollarizing eliminates currency mismatch risk between debt and income (Berg & Borensztein, 2000). This type of mismatch is the reason that currency crises often become debt crises. Moreover, given that dollarization limits the central bank's ability to fulfill the role of lender of last resort, commercial bankers may become more conservative in their lending practices and thus reduce the probability of a banking crisis (Gale & Vives, 2002).

Given their similarity to currency unions, the optimal currency area (OCA) research offers relevant insight into the appropriateness and sustainability of any given currency union arrangement. The OCA literature argues the following four criteria are essential for a successful currency union (Mundell, 1961; McKinnon, 1963; and Frankel & Rose, 1998). First, labor and capital must be mobile across all member countries. Second, prices of final products and inputs should be flexible. Third, member countries should have similar business cycles or symmetric shocks across the union should dominate asymmetric shocks to individual member countries. On this point, Eichengreen (1994) argues countries with a large traded sector relative to GDP and with employment spread across multiple sectors would benefit the most from joining a currency union, as a country specific or industry specific shock is less likely to have a large, negative impact on the economy. Fourth, to offset the impact of asymmetric shocks, there should be an automatic fiscal transfer mechanism between member countries.

Unless these conditions are satisfied, the possible downside of dollarizing or adopting a currency board is that Ecuador or Argentina may have business cycles that are out-of-sync with the United States' or may experience local economic shocks. In these cases, the United States' monetary policy will not be appropriate (Berg & Borensztein, 2000; Alesina & Barro, 2002). Additionally, the central bank's ability to counteract the effect of a shock via its role as lender of last resort services is more constrained (Beckerman, 2001). Under dollarization, the central bank can only provide this role up to the amount of dollars it has on hand. Under currency boards, the central bank must at all times have enough dollars to cover the monetary base. Therefore, the central bank can only act as lender of last resort if it has excess dollars above the amount needed to cover the monetary base.

Unable to absorb shocks through exchange rate or monetary policy, economies will adjust primarily through the labor market. Consequently, slowdowns in productivity growth will have more immediate and negative impacts (Berg & Borenzstein, 2000). Therefore, Ecuador and Argentina may face an inflation abatement-growth trade-off.

History of Dollarization in Ecuador & Argentina's Currency Board

After repeated attempts by President Menem and his predecessors to curb inflation and rising government deficits in the 1980's were thwarted by external shocks and currency crises, in April 1991, President Menem implemented a peso-USD currency board in Argentina. The currency board fixed the Argentine peso at a one-to-one peg with the USD, with the peso convertible on demand to the USD at the fixed exchange rate. Many felt the currency board-convertibility system would act as a credible fixed exchange rate and provide an anchor of expansionary monetary policy in Argentina. (See World Bank, 2003 and Blustein, 2005) Theoretically, this type of system should have the same inflation curbing features of dollarization, but; unlike dollarization, a government can more easily unwind a currency board.

As in Argentina, the decision to dollarize in Ecuador was a final attempt to arrest rising prices and stabilize an economy in crisis. From 1998 through early 2000, Ecuador suffered one of the most severe economic crises of its history. In 1999, the banking system collapsed with 40% of banks failing, the government defaulted on its debt, and consumers and investor lost all confidence in the Sucre, Ecuador's currency, resulting in a massive sell-off. This flight from the Sucre generated uncontrollable hyperinflation, peaking at 30% per month. Estimates place the cost of the crisis at 20% of 2000 GDP. An inability to maintain fiscal discipline resulted in a highly indebted central government with an external debt of 106% of GDP in 1999. In the wake of the Tequila Crisis, financial capital quickly exited Latin America, driving up lending rates. The Central Bank of Ecuador's lending rate rose from under 30% in 1997 to over 50% by the end of 1998. Spillover effects from the Asian Financial crisis and the sharp fall in oil export prices from $20/barrel in early 1997 to around $7/barrel in December, 1998 made the government's debt position unsustainable and helped initiate the crisis

(Quispe-Agnoli & Whisler, 2006; Jacome H., 2004; Beckerman & Solimano, 2002; Beckerman, 2001).[2]

In January 2000, President Jamil Mahuad announced that Ecuador would fully dollarize, converting the Sucre at a rate of 25,000 Sucre to the dollar. Soon after, the military deposed Mahuad. Although the coup was ultimately unsuccessful, Mahuad did not return to office. Gustavo Noboa, Mahuad's former vice-president, took power and passed the legislation authorizing formal dollarization through congress in September 2000 (Quispe-Agnoli & Whisler, 2006).[3]

Dollarization is different from a currency union in several key aspects. Dollarization is the adoption of a foreign currency, typically the U.S. dollar, as the sole, legal domestic tender.[4] In essence, dollarization is a credible, one-to-one fixed exchange rate: credible because dollarization may be irreversible. It is thought to be irreversible because to de-dollarize the central bank must float a new currency, repurchase the existing stock of dollars, and convince depositors that the sins of the past that necessitated dollarization are never to be repeated (Ize & Levy-Yeyati, 2003). This latter point, in particular, may prove impossible, as financial dollarization is highly persistent, well after the end of high inflation periods.

By dollarizing, Ecuador entered a de-facto currency union with the United States. Under a currency union, each member country agrees to utilize a single currency, issued

[2] Proceeds from oil exports generated 25-30% of government revenue.

[3] In some sense, this legislation represented public policy catching up to financial reality. In 1999, 53.7% of deposits and 66.5% of loans were in denominated in dollars, but five years earlier only 15.6% of deposits and 20.3% of loans were dollar denominated (Beckerman, 2001). To put the 1999 figures into perspective, the stock of dollar deposits in 1999 was eight times larger than Sucre denominated deposits (Jacome H., 2004).

[4] Throughout this paper, it is understood that dollarization refers to a country adopting the U.S. dollar as its sole, legal tender. Countries may enact this type of financial and transactional currency substitution by formal agreement with the dollar nation or unilaterally.

by a central bank that distributes the seigniorage revenue across all members and considers the economic situations and outlooks of each member when setting monetary policy. Under dollarization, the U.S. Federal Reserve remits all seigniorage to the U.S. Treasury and sets monetary policy according to its dual mandate and the economic conditions of only the United States. Consequently, by dollarizing, Ecuador forfeits monetary policy as an economic tool.

In 1991, Argentina established a currency board in the form of a one-to-one hard peg with the U.S. dollar with, as is characteristic of currency boards, unlimited and on demand convertibility between the peso and the dollar at the one-to-one peg. Currency boards, of this type, are similar to dollarization, however; Argentina retains nominal control over its currency. We say, nominal, because under the convertibility system the Central Bank of Argentina can only expand the monetary base to the degree that it collects extra dollars. Consequently, the currency board forces Argentina, like Ecuador, to inherit U.S. monetary policy. In contrast to Ecuador, Argentina showed in 2002 that currency boards are reversible.

Synthetic Counterfactuals and Inference Testing

When assessing treatment effect of dollarization on inflation and real per-capita GDP, we would ideally observe the adopting country both with the treatment of dollarization and at the same time without this policy change. Since this is impossible and dollarization is a policy not randomly assigned to countries, then we must find a way to identify a proper counterfactual for the treated country.

In an ideal experiment on could compare Ecuador and Argentina to a counterfactual Ecuador and Argentina that are identical except the counterfactual countries never dollarized. If we could observe both, then the difference in their outcome data, Y_{it}, after dollarization would simply be the treatment effect.

$$Y_{it} = Y_{it}^{N} + \alpha_{it} D_{it}, \text{ where } D=1 \text{ in the post-dollarization period.} \qquad (1.1)$$

The Synthetic Counterfactuals Method (SCM), developed by Abadie and Gardeazabal (2003) and extended by Abadie *et al*., (2010), offers a data driven method for generating this counterfactual Ecuador, $\widehat{Y_{it}^{N}}$. We provide an outline of the method below, using Ecuador as an example.

First, we take a set of potential control countries that are never treated and do not experience a spillover effect from Ecuador dollarizing. We pick our control countries such that for some of the countries the value of their outcome variables (e.g. per-capita RGDP, etc.) is less than Ecuador's and for some it is greater than Ecuador's.[5] This is necessary to create, using weighting matrices, a synthetic country that closely approximates Ecuador's outcome data in the years before dollarization. Formally, to avoid extrapolation outside of the support of our data, we choose countries such that Ecuador's data falls within a convex hull of our control group's outcome data.[6]

Second, we select a series of explanatory variables with predictive power for our outcome of interest. Here we use the second set of weights, with the same restrictions as

[5] Throughout this article, we assume the so-called stable-unit-treatment assumption (Rosenbaum & Rubin 1983) holds for all of the countries used to form our synthetic Ecuador. Put simply, this means there are no spillover effects from Ecuador to the countries in our synthetic pool after Ecuador dollarizes.

[6] We also restrict weights to be nonnegative and to sum to one to prevent extrapolation outside the bounds of the data. Abadie *et al.,* (2010) elucidate the technical details.

above, to assign each variable's relative importance in predicting the outcome of interest. We choose both sets of weights to minimize the mean squared prediction error between Ecuador's outcome of interest and the Synthetic's in the pre-dollarization period. Once we have selected our weights, we generate the synthetic data over the entire sample period. In theory, if the synthetic's data deviate only slightly from Ecuador's in the pre-dollarization period, then the counterfactual may be reasonably expected to approximate what Ecuador would have looked like without treatment (i.e.$Y_{it}^{N} \approx \widehat{Y_{it}^{N}}$). In this case, any difference between the synthetic's data and Ecuador's in the post-dollarization period would be attributable to the causal impact of dollarization.

Abadie *et al.*, (2010) propose a series of placebo or falsification tests to strengthen the argument that the treatment causes the observed difference in the outcome data between the treated country and untreated synthetic. First, we check that the deviation we see between Ecuador's data and the synthetic's after the pre-intervention period is not due to poor out-of-sample properties of our synthetic counterfactual. We do this by shortening the training period of our model. If we do not immediately see a deviation between the synthetic's data and Ecuador's, then we have evidence that the deviation between the two datasets is not due to poor out-of-sample fit.

Next, we use the SCM to form a synthetic for each of the countries in the control country pool. Because they never received the treatment, one can think of these as placebo exercises. We then check if, for each placebo country, the synthetic's data diverges from control country's in the post-intervention period. In this falsification test, we use the same list of variables and training period to form each control country's synthetic that we utilize to create Ecuador and Argentina's synthetics. The idea is that if the treatment has an impact on our outcomes of interest in the treatment country, then one should see no similar treatment effect in countries that did not receive the treatment.[7]

To judge the magnitude of the treatment effect of dollarization on the outcomes of interest, utilize Pitman's Permutation Test (Pitman, 1937a & 1937b). The Permutation Test allows me to test if the average difference between the value of the outcome of interest for a treated country's and its synthetic in the post-treatment period is significant. Theory predicts dollarization and quasi-dollarization will reduce inflation so we utilize the one-sided variation of the Permutation Test. That is we reject the null hypothesis if the difference between country's test statistic and the average of the control countries' test statistics is negative and in the lowest 5% of the rank order. As mentioned in the introduction, the predicted effect of dollarization and quasi-dollarization on per-capita RGDP is ambiguous. Consequently, we use a two-sided hypothesis test.

For the Permutation Test, we construct the following tests statistics. First, we consider the post-treatment period average deviation between the treated and synthetic country for inflation and real per-capita GDP, separately.

$$TS_{1i} = \frac{1}{T}\left(\sum_{t=1}^{T} Y_{it} - \widehat{Y_{it}}\right) \qquad (1.2)$$

[7] We further perform a restricted version of this falsification test by limiting our sample to the placebo countries where the pre-intervention fit of the synthetic placebo to the placebo's observed data, measured by RMSE, is at least as good as the fit of the synthetic treatment country's model to the observed treatment country's data.

Here Y_{it} is country i's the observed data in period t and $\widehat{Y_{it}}$ is the synthetic country i's data in period t. For real per-capita GDP, we have ten years of post-treatment data for both Argentina and Ecuador. For log inflation, we have eight years of post-treatment data for Argentina and ten years for Ecuador. Next, we split our post-treatment sample into two halves. Using this split sample, we generate the average deviation between the treated and synthetic country for the first half the treatment period and again for the second half.

$$TS_{2i} = \frac{1}{T/2}\left(\sum_{t=1}^{T/2} Y_{it} - \widehat{Y_{it}}\right) \qquad (1.3)$$

$$TS_{3i} = \frac{1}{T/2}\left(\sum_{t=\left(\frac{T}{2}\right)+1}^{T} Y_{it} - \widehat{Y_{it}}\right) \qquad (1.4)$$

By splitting our sample in this way, we are able to see if dollarization and quasi-dollarization affect inflation and per-capita RGDP in a delayed manner.

Data and Sample

The SCM process requires selection of a potential pool of control countries that have economic processes similar to our treatment countries of interest, Ecuador and Argentina. Additionally, the control countries can never receive the treatment, dollarization, or spillover effects from the treatment. Consequently, we exclude countries that engage in currency substitution, adopt currency boards, or join a monetary union.

As a measure of inflation, we use national CPI data from the IMF's IFS database. The Argentina and Ecuador's inflation data exhibit massive positive swings, as high as 3000%, over the pre-intervention period. Therefore, we take the natural log to smooth out the data. As right hand side variables in our prediction equation for Argentina's annual inflation –actually the natural log of inflation— we use inflation, population growth, and the ratio M2 to GDP (table 1.1).[8] We have a pool of fifty-seven control countries. From the pool of control countries, the SCM algorithm selected two countries, Bolivia and Peru, to form our synthetic Argentina. The SCM algorithm gives Bolivia a weight of 19% and Peru a weight of 81%. Table 1.2 shows the average value of the inflation predictors over the pre-treatment period. For each of these predictors, Argentina and the synthetic Argentina have almost the same average value (table 1.2). This shows on these observable predictors, the synthetic is similar to the actual Argentina.

To predict Ecuador's annual inflation, we use population growth and the ratio of government spending to GDP. Additionally, annual inflation, in the control countries,

[8] Readers may notice that we do not use the same predictors for each treated country. This is because we are not trying to test hypotheses about causal relationships between individual right hand side variables and our outcomes of interest. As we are only trying to match optimally the pre-treatment period outcome data of our treated countries with their synthetics, this leads to some predictors being in one country's equation but not in another country's equation.

Additionally, we use outcome data from control countries as a predictor for treated country outcome variables. For example, we use inflation data from control countries as a predictor of inflation for Ecuador and Argentina. Abadie and Gardeazabal (2003) discuss the case of using the outcome variable as a predictor variable in the appendix. They argue it is best to use the outcome variable along with determinants of the outcome in the right hand side of the prediction equation. We take this approach. Using the prediction equations for inflation as an example, it is correct to say that inflation from the control countries is not a predictor of inflation in Ecuador or Argentina in the traditional sense that inflation in the control countries may determine inflation in the treated. Rather, we include control countries' inflation as a predictor variable because including inflation greatly improves the fit of our synthetic models, which would make sense if there were some underlying process that helps determines inflation in all Latin American countries.

enters as a predictor four times: once averaged over the entire pre-treatment period and again averaged across control group countries in 1991, '95, and '99 (table 1.3). We construct our synthetic from the pool of thirty-seven countries. From these, the SCM algorithm assigns positive weights to the Democratic Republic of Congo (.9%), Kenya (23.9%), Nicaragua (7.2%), Norway (.8%), Papua New Guinea (11%), and Turkey (57.1%). Table 1.3 shows the synthetic Ecuador and Ecuador are similar across the predictors of inflation.

As predictors of Argentina's real income, we use control countries per-capita RGDP, averaged over the entire pre-treatment period; per-capita RGDP in each of the years 1982, 1985, and 1990; M2 to GDP; inflation; and openness, the ratio of the sum of exports and imports to GDP. To predict Ecuador's real per-capita GDP, we use control countries per-capita RGDP, averaged over the entire pre-treatment period; per-capita RGDP in each of the years 1989, 1995, and 1999; M2 to GDP; inflation; and openness, the ratio of the sum of exports and imports to GDP. (Table 1.4)

From a pool of fifty-seven control countries, the SCM algorithm picks use the Democratic Republic of Congo (15.3%), Iceland (.1%), Peru (37.3%), Trinidad and Tobago (4.8%), and Venezuela (42.5%) to form our synthetic Argentina. For synthetic Ecuador, we use the Democratic Republic of Congo (33%), Gambia (7.2%), Norway (.7%), Sudan (36.5%), and Venezuela (20.7%) to construct the synthetic Ecuador. Both synthetic Argentina and synthetic Ecuador closely approximate actual Argentina and Ecuador, respectively, on the predictors of per-capita RGDP. (See tables 1.4 & 1.5)[9]

[9] One may be concerned about including Sudan and the Democratic Republic of Congo in the control pool, as they frequently experience war. However, because both countries were engaged in conflict over the training and post-treatment periods we are not concerned that war acts as a countervailing treatment to dollarization.

Argentina enacted its currency board-convertibility system in 1991. We have data on the predictors of inflation and per-capita RGDP for Argentina from 1980. Therefore, the SCM uses the eleven years of predictor data, 1980-1990, and the country weights listed above to calibrate the model and generate the synthetic. For Ecuador, which dollarized in 2000, we have twenty years of data, 1980-1999, available to generate the synthetic.

Argentina

Inflation

In the top panel of figure 1.1, we graph Argentina's log inflation (solid line) against the synthetic Argentina's (long dotted line). The vertical line at 1991 indicates when Argentina established its currency board-convertibility system. Argentina and the synthetic track closely throughout the pre-treatment period, but the two series diverge after 1991. Following currency board adoption, Argentina's log inflation is lower than that predicted by the synthetic's inflation throughout the entire eight-year post-treatment period.

To test the out-of-sample properties of the synthetic model, we shorten the training period to 1984, seven years before Argentina actually implemented the currency board. We allow the synthetic model to estimate its weighting matrices using data from 1980 to 1983. The top panel of figure 1.2 shows the synthetic correctly matches Argentina's observed data relatively closely through 1986 suggesting that the deviation between the two data sets is not due to poor out-of-sample properties of our synthetic model.

Next, we check if the divergence between Argentina and synthetic Argentina's log inflation series is large using the placebo-falsification test. We display the results of this placebo test in the top panel of figure 1.3. Thirty-eight placebo countries' synthetics fit their respective data at least as well as synthetic Argentina fit Argentina's log inflation data. Compared to the placebo countries, Argentina's log inflation falls sharply after quasi-dollarization. For more than half of the placebo countries, inflation increased relative to their synthetics after 1991. Around six countries experienced negative divergences in log inflation as large as Argentina's and only three had sustained down breaks in inflation. This provides visual evidence that the down break in Argentina's inflation rate after 1991 is due to the adoption of the currency board.

The permutation test results (top panel of table 1.7) show that the average deviations of Argentina's log inflation from its synthetic over the entire post-intervention period is negative and significant at the 10% level. More importantly, the effect is negative and significant at the 5% level the last four years of the treatment period. This is evidence that the effect of quasi-dollarization occurs with delay and builds over time.

Real Income

In the top panel of figure 1.4, we plot Argentina's real income (real per-capita GDP) (solid line) against the synthetic's (long dotted line). The vertical line at 1991 indicates when Argentina implemented the currency board. The synthetic, for the most part, closely tracks Argentina's real income data during the pre-treatment period. The two series diverge in 1991. In the period following the implementation of the currency board-convertibility system, Argentina's real per-capita GDP rises markedly above that predicted by the synthetic Argentina. In the late 1990's, the results indicate each Argentine is $1000 richer than he/she would have been had Argentina never adopted the convertibility system.

Figure 1.5, top panel, shows the results of the currency board adoption move test. We end the training period at 1986, five years before Argentina adopted the currency board. The two series follow each other closely through 1990, the year before Argentina adopted the currency board. This provides visual evidence that the deviation between the two data sets in the post-training period is not likely to be due to poor out-of-sample properties of our synthetic model.

We display the results of the placebo test in the top panel of figure 1.6. Thirty-eight placebo countries' synthetics fit their respective data at least as well as synthetic Argentina fit Argentina's real income data. In this Figure, the divergence between Argentina's per-capita RGDP and its synthetic's, appears larger than the divergence between most of the placebo countries and their synthetic's data. The results of the permutation test (table 1.7); however, show the average deviation is not statistically different from zero for any of the three cases we consider.

Ecuador

Inflation

The bottom panel of figure 1.1 shows Ecuador's observed log inflation (solid line) against the synthetic Ecuador's (long dotted line). The vertical line at 2000 indicates when Ecuador dollarizes. Ecuador's log inflation began diverging from the synthetic's in 1998 due to the economic crisis Ecuador experienced from late 1997 through early

2000. The crisis was not only one of Ecuador's most intense economic crises, but also, it was more severe than crisis affecting any of the control pool countries. This divergence peaks at around one log point in 2000. Following dollarization, inflation rapidly declines and ultimately falls below synthetic Ecuador in 2002, and it remains below the synthetic throughout the rest of the sample period.

To test the out-of-sample properties of the synthetic model, we shorten the training period to 1994, six years before Ecuador actually dollarized. We allow the synthetic model to estimate its weighting matrices using the data from 1980 to 1993. Figure 1.2, bottom panel, shows the synthetic tracks Ecuador's observed data relatively closely through 1997 providing evidence that the deviation between the two data sets is not due to poor out-of-sample properties of our synthetic model.

Next, we check if the divergence between Ecuador and synthetic Ecuador's log inflation series is large using the placebo-falsification test. We present the results of this test in the bottom panel of figure 1.3. Fourteen control countries' synthetics fit their respective data at least as well as synthetic Ecuador fit Ecuador's log inflation data. Compared to the placebo countries, Ecuador's log inflation falls sharply after dollarization. For more than half of the placebo countries, inflation increased relative to their synthetics. Only about four countries experienced negative divergences in log inflation as large as Ecuador's and none were sustained as long.

The permutation test results (top panel of table 1.7) show that while the average deviations of Ecuador's log inflation from its synthetic over the entire post-intervention period and the first half of post-treatment period are not statistically significant, the average deviation in last half of the post-treatment period, -.86 log points, is statistically significant at the 5% level. This indicates the effect of dollarization occurs with delay.

Real Income

In the bottom panel of figure 1.4, we graph Ecuador's observed real income (real per-capita GDP) (solid line) against the synthetic Ecuador's (long dotted line). The vertical at 2000 indicates when Ecuador dollarizes. Ecuador's real income began diverging from the synthetic's in 1998 due to the shocks described in the previous section. The divergence peaks at around $120. Following dollarization, real income converges with the synthetic's in 2003 and surpasses it until 2005. Afterwards, real per-capita GDP falls relative to synthetic Ecuador's throughout the remainder of the sample period.

To test if the observed deviation between the Ecuador and synthetic Ecuador's real income is due to poor out-of-sample properties of the synthetic model, we truncate the training period at 1993, seven years before Ecuador actually dollarized. We allow the synthetic model to estimate its weighting matrices using the data from 1980 to 1992. Figure 1.5, bottom panel, shows the synthetic tracks Ecuador's observed data relatively closely through 1998 providing evidence that the deviation between the two data sets is not due to poor out-of-sample properties of our synthetic model.

Next, we check if the divergence between Ecuador and synthetic Ecuador's real per-capita GDP series is large using the placebo-falsification test. We display the results of this placebo test in Figure 1.6, bottom panel. Sixteen control countries' synthetics fit their respective data at least as well as synthetic Ecuador fit Ecuador's real per-capita

GDP data. Compared to the placebo countries, Ecuador tracks the synthetic Ecuador quite closely for the first six years of the post-intervention period, then begins to diverge sharply. While the picture is illustrative, it is not formal evidence for statistical significance. The bottom panel of table 1.7 shows the results of the permutation test. The average deviations of Ecuador's real per-capita GDP from its synthetic over the entire post-intervention period, the first half of post-treatment period, and the last half of the post-treatment period are not significantly different from zero. The p-values in all three cases are near .9 so we have evidence that we are seeing true zero effects, rather than insignificant effects due to large standard errors.

Discussion: The Long-Run Sustainability of Dollarization

Argentina and Ecuador adopted similar monetary policies, enjoyed statistically significant reductions in inflation and, surprisingly, had no adverse shocks to real per-capita GDP, relative to their synthetic counterfactuals. However, the long-run outcomes could not be more different. Fourteen years on, Ecuador is still prospering under dollarization and shows no signs of de-dollarizing. Argentina, after defaulting on $132 billion dollars of outstanding debt, abandoned the currency board in January 2002, allowing the peso to float. Scholars have argued that Argentina's currency board was ultimately unsustainable because the central government was unable to institute fiscal disciple at the federal and provincial level resulting in persistent deficits and rapidly rising debt-to-GDP levels. Moreover, when Argentina pegged to the dollar, it over-valued the peso and compromised its international competitiveness. Finally, Argentina's labor markets were too inflexible in wages to adjust to external shocks so adjustments came in the form of rising unemployment and, commensurately, poverty rates increased throughout the 1990's (Krueger, 2002; Blustein, 2005).

Figure 1.7 shows, with the exclusion of 1993, Argentina ran an overall budget deficit every year it had the currency board, averaging 2.7% of GDP per year over the decade from 1991 to 2001. Furthermore, the data illustrate that neither the provincial governments nor the federal government were able to control spending over this period. Furthermore, government spending to GDP increased fourfold from 3.3% in 1991 to 14.2% in 2001. As Figure 1.7 illustrates, this was not a gradual rise, but a discrete increase in 1993. These two trends necessitated extensive borrowing, primarily from the international market in dollars. (See Blustein (2005) for a detailed description.) External debt to GDP increased twenty percentage points from 34.6% in 1991 to 55.7% in 2001. As a percentage of exports, typically a major source of hard currency tax revenue for paying external dollar denominated debt, external debt interest payments trended from 18% in 1991 to 30.5% in 2000.

To finance external debt payments, an economy needs to be running a trade surplus so the government can use export tax revenue to make scheduled interest payments. Unfortunately, Argentina ran a trade deficit for the entire decade it operated under the currency board, 3% of GDP on average (See middle panel of figure 1.7). Figure 1.7 also shows the Peso-Dollar real exchange rate, calculated using average CPI's, against Argentina's current account balance-to-GDP ratio, in percentage points. In real-terms, the peso appreciated 25% from 1990 to 1991, when Argentina pegged to the dollar. At its peak value in 1996, the peso was 41%, in real-terms, more valuable than in 1990. The graph provides visual evidence of a connection between the real appreciation of the dollar and the deterioration of the current account balance. Krueger (2002) and Blustein (2005) argue the appreciation of the Peso coupled with lagging labor productivity growth, relative to regional export competitors, cause a decline in Argentina's international competitiveness. This hints at a need to consider carefully one's monetary anchor.

Given fixed exchange rates, the economy adjusts to shocks through the labor market, either in terms of changes in unemployment or wages, or both. At the time of the currency board, Argentina's unions dominated the labor market and collectively bargained at the sector level. Moreover, collective bargaining agreements automatically renew if at the end of a giving contracting period the unions and firms cannot agree to new terms. Consequently, firms have no flexibility in adjusting wages in response to economic shocks, and unions have no incentive to negotiate in good faith with firms (IMF, 1998; Economist, 1998; World Bank, 2000).

Given these sticky wages, labor markets adjusted to reduced competitiveness by substituting away from labor and towards capital. (Krueger, 2002) The bottom panel of figure 1.7 shows the national average unemployment and under-employment rates. In 1991, the unemployment rate is 6% but steadily rises to a zenith of 18.1% in 2001, the year Argentina defaulted on a major portion of its debt. The World Bank (2000) points out that because Argentine labor markets was so inflexible, companies took to hiring people as part-time workers, not subject to the full fringe benefits required under union contracts. We see this in the data as well. Notice that the under-employment rate rises in-tandem with the unemployment rate, although the former is less volatile than the latter.[10]

This paper does not argue against viability of currency boards, in general, but in Argentina's specific case it is difficult to imagine now a scenario where Argentina could have maintained the currency board given its persistent deficit spending, over-valued currency, peg to a non-major trading partner, and inflexible labor market. In contrast to Argentina, Ecuador has maintained a level of fiscal discipline that has allowed it to pay down its external debt from 73% of GDP in 2000 to 20% in 2012. Correspondingly, the interest payments to exports ratio was only 2.7%, down from 13% in 2000. Given Ecuador formally dollarized, it does not have a mismatch between its revenues and its debt so maintaining a positive current account balance is not as critical, as in Argentina's case. Still here too, Ecuador outperforms Argentina. From 2000 to 2009, Ecuador ran a current account deficit-to-GDP ratio of .78%.

The World Bank (2010) attributes much of Ecuador's early success in the steady rise in oil prices since 2000, from around $30/barrel in 2000 to nearly $100/barrel in 2014,[11] and its fiscal responsibility laws. President Noboa enacted these laws as part of the reforms to institute dollarization. These laws created the Oil Stabilization Fund and earmarked oil revenue for specific government development projects, including infrastructure building in oil producing region. The Ecuadorian government uses the Oil

[10] One school of thought argues that fiscal imbalances and labor market inflexibility are only proximate causes for the unsustainability of Argentina's currency board. Calvo (2001) and Calvo *et al.,* (2004) argue that foreign capital flow reversals, or Sudden Stops, leading to a rapid rise in the real exchange rate is the root cause of the crisis that motivated Argentina to abandon its currency board. However, Calvo (2003) shows higher liability dollarization increases the probability of a Sudden Stop. Moreover, even researchers from this school of thought agree the fiscal deficits and labor market inflexibility exacerbated the crisis making fiscal discipline and labor market flexibility relevant policy goals (Calvo, 2001; Calvo and Mishkin, 2003).

[11] Crude oil prices quoted here are West Text Intermediate (WTI) from the Federal Reserve's FRED database.

Stabilization Fund to service outstanding government debt and finance social programs (Cueva, 2008). Since dollarizing, petroleum has constituted over 50% of Ecuador's exports, by revenue, and other primary commodities an additional 25%.[12] Consequently, a sudden decline in commodity prices would quickly deteriorate Ecuador's current account and fiscal positions (World Bank, 2010).

As far as the choice of nominal anchor, Ecuador's primary export is oil, and oil contracts are denominated almost entirely in USD. (Mileva & Siegfried, 2007) Additionally, Ecuador chose to currency substitute using the dollar because the country was already highly financially dollarized in 1999 so in that sense the change in 2000 was public policy recognizing the de-facto situation. Still there is the question of when Ecuador dollarized, did it convert Sucre to Dollars at a rate that ensured an appropriately valued real exchange rate? When the government formally converted over to dollars, it purchased Sucre at 25,000 to the dollar. This implies a real exchange rate, using CPI, of about 25,500 for the post-dollarization period from 2000 to 2012. Therefore, we can say the Ecuadorian government was effective at strategically devaluing when it dollarized.

In terms of labor market outcomes, Ecuador's unemployment has fallen since dollarizing in 2000. In 1999, unemployment was 14.4% but fell to around 7.8% in 2007 and remained at about that level through 2011.

[12] Based on UN COMTRADE data aggregation by the MIT Media Lab's Macro Connections

Conclusion

Using the SCM, we are able to estimate the effect of Ecuador's dollarization policy and Argentina's currency board on inflation and real per-capita GDP. The policies prove remarkably successful in taming inflation, when compared to a synthetic counterfactual country. In Argentina, the currency board reduced inflation, relative to the synthetic Argentina, -2.74 log points, on average for the period 1995-1998. In Ecuador, the average effect was -1.04 log points, for the period 2006-2010. In both cases, the effect appears only after several years. Interestingly, in contrast to previous research on the topic, we find neither policies hurt real per-capita GDP so neither country faced an inflation abatement-income trade-off.

Despite these results, the long-term outcomes are vastly different between the two countries. Because Argentina was unable to maintain a balanced budget, adopted an over-valued exchange rate, and could add sufficient flexibility to its labor market, Argentina could not maintain the currency board. It defaulted on large portions of its debt in 2001 and broke the peg in 2002. Ecuador, by comparison, strategically devalued when it dollarized so ensure its real exchange rate benefited its export industries. Ecuador has adjusted its fiscal spending levels such that it has paid down its external public debt from 70% to GDP to 20% to GDP.

We conclude that while dollarization and quasi-dollarization, in the form of a currency board, are effective methods of controlling inflation, for a currency board to be sustainable in the long-run, governments must implement structural adjustments to make the labor market more flexible and impose fiscal discipline.

Tables

Table 1.1:
Weights on Inflation Predictors

Variables	Synthetic Ecuador	Variables	Synthetic Argentina
ln(Inflation)	0.59	ln(Inflation)	0.03
ln(Inflation (t=1991))	8.23E-06	Population Growth	0.02
ln(Inflation (t=1995))	3.81E-05	M2/GDP	0.96
ln(Inflation (t=1999))	6.85E-06		
Population Growth	0.28		
Govt/GDP	0.13		

Note: Table shows the weights used, for each inflation predictor, to generate synthetic Ecuador and synthetic Argentina.
Data Source: Inflation: IMF's IFS database; population growth, G/Y, and M2/Y: World Bank's WDI database.
Data Range: Argentina (1980-1990), Ecuador (1980-1999)

Table 1.2:

Average Inflation Predictor Means

Variables	Actual Argentina	Synthetic Argentina
ln(Inflation)	5.83	5.26
Population Growth	1.49	2.32
M2/GDP	20.94	22.46

Note: Table shows the values of indicator variables and the average pre-dollarization outcome variable for Argentina and synthetic Argentina. All variables are averaged over the entire pre-dollarization period, 1980-1990. Population Growth and M2/GDP are in percentage points.

Table 1.3:

Average Inflation Predictor Means

Variables	Actual Ecuador	Synthetic Ecuador
ln(Inflation)	3.49	3.49
ln(Inflation (t=1991))	3.41	3.49
ln(Inflation (t=1995))	3.89	3.98
ln(Inflation (t=1999))	3.13	3.17
Population Growth	2.33	2.33
Govt/GDP	13.88	13.87

Note: Table shows the values of indicator variables and the average pre-dollarization outcome variable for actual and synthetic Ecuador. All variables are averaged over the entire pre-dollarization period, 1980-1999. The natural log of inflation enters as a predictor four times: once averaged over the entire pre-treatment period and again averaged across control group countries in 1991, ‘95, and ‘99. **Government spending to GDP is averaged over 1984, 1989, and 1999. Govt/GDP and Population Growth is in percentage points.

Table 1.4:
Real Income Predictor Means

Variables	Actual Ecuador	Synthetic Ecuador
RGDPpc	2672	2638
RGDPpc(t=1989)	2615	2614
RGDPpc(t=1995)	2742	2710
RGDPpc(t=1999)	2637	2733
ln(Inflation)	3.49	3.97
M2/GDP	17.74	17.78
OPEN	40.25	40.3

Note: Table shows the values of predictor variables and the average pre-dollarization outcome variable for actual and the synthetic Ecuador. Inflation is averaged over the entire pre-dollarization period, 1980-1999. RGDP per-capita is averaged over 1980-1998, and M2/GDP is averaged over 1990-1999. OPEN is ((EX+IM)/GDP). M2/GDP and OPEN are in percentage points.

Table 1.5:
Real Income Predictor Means

Variables	Actual Argentina	Synthetic Argentina
RGDPpc	3804	3800
RGDPpc(t=1982)	3831	4088
RGDPpc(t=1984)	3592	3631
RGDPpc(t=1990)	3262	3397
ln(Inflation)	5.83	3.99
M2/GDP	19.67	27.54
OPEN	16.06	43.81

Note: Table shows the values of predictor variables and the average pre-dollarization outcome variable for actual and the synthetic Ecuador. All variables are averaged over the entire pre-dollarization period, 1980-1990, except M2/GDP and OPEN. These two are averaged over 1986-1990. OPEN is ((EX+IM)/GDP). M2/GDP and OPEN are in percentage points.

Table 1.6:
Weights on Real Income Predictors

Variables	Synthetic Ecuador	Variables	Synthetic Argentina
RGDPpc	0.94	RGDPpc	0.82
RGDPpc(t=1989)	0.046	RGDPpc(t=1982)	0.0002
RGDPpc(t=1995)	0.005	RGDPpc(t=1984)	0.17
RGDPpc(t=1999)	0.002	RGDPpc(t=1990)	0.02
ln(Inflation)	1.41E-07	ln(Inflation)	6.77E-14
M2/GDP	0.0007	M2/GDP	0.0001
OPEN	0.003	OPEN	2.12E-06

Note: Table shows the weights used, for each predictor, to generate synthetic Ecuador and synthetic Argentina.
Data Source: Inflation: IMF's IFS database; real per-capita RGDP, M2/Y, Openness: World Bank's WDI database.
Data Range: Argentina (1980-1990), Ecuador (1980-1999)

Table 1.7:
Results: Treatment Effect and Permutation Test

	Inflation			
	Argentina		Ecuador	
	Average Deviation (log points)	P-Value	Average Deviation (log points)	P-Value
Full Treatment Period	-1.75	0.09	-0.47	0.13
First Half	-0.94	0.16	-0.09	0.43
Second Half	-2.56	0.02	-0.86	0.05

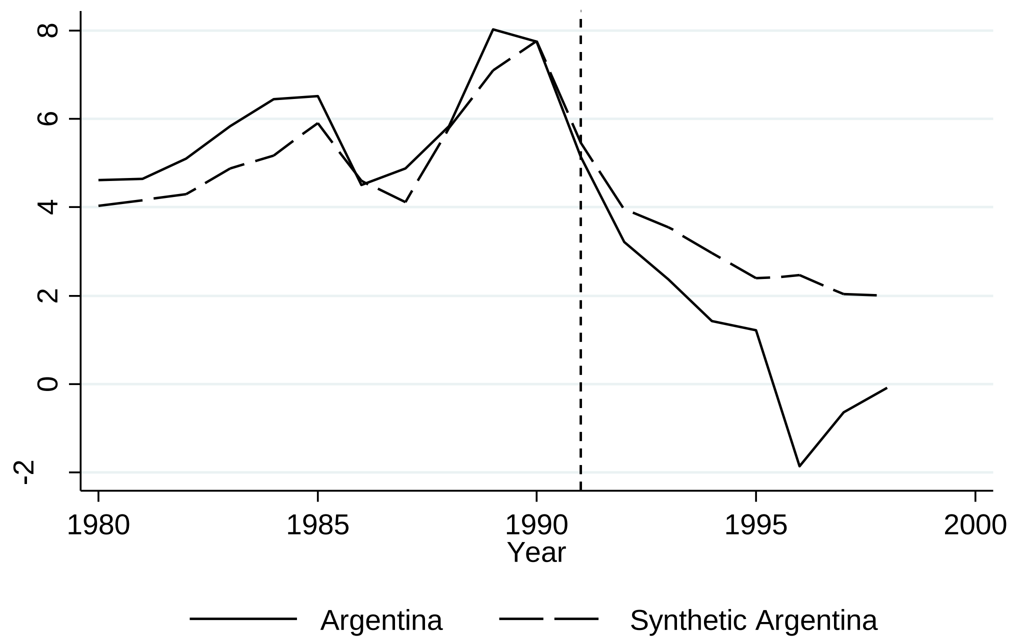
	Real Per-Capita GDP			
	Argentina		Ecuador	
	Average Deviation (2005 USD)	P-Value	Average Deviation (2005 USD)	P-Value
Full Treatment Period	681.16	0.37	-20.89	0.92
First Half	522.57	0.23	-28.71	0.84
Second Half	771.85	0.37	-13.08	0.90

Note (Top Panel): The table shows the average deviation of the treated countries observed inflation rate from the synthetic's over the entire ten year post treatment period, the first five years of the treatment period, and the last five, respectively. Next to each of these averages is the p-value, calculated via a permutation test, of seeing an observation at least that far below zero.

Note (Bottom Panel): The table shows the average deviation of the treated countries observed per-capita RGDP from the synthetic's over the entire ten year post treatment period, the first five years of the treatment period, and the last five, respectively. Next to each of these averages is the p-value, calculated via a permutation test, of seeing an observation at least that different from zero.

Figures

Figure 1.1: Impact of Dollarization/Quasi-Dollarization on Inflation



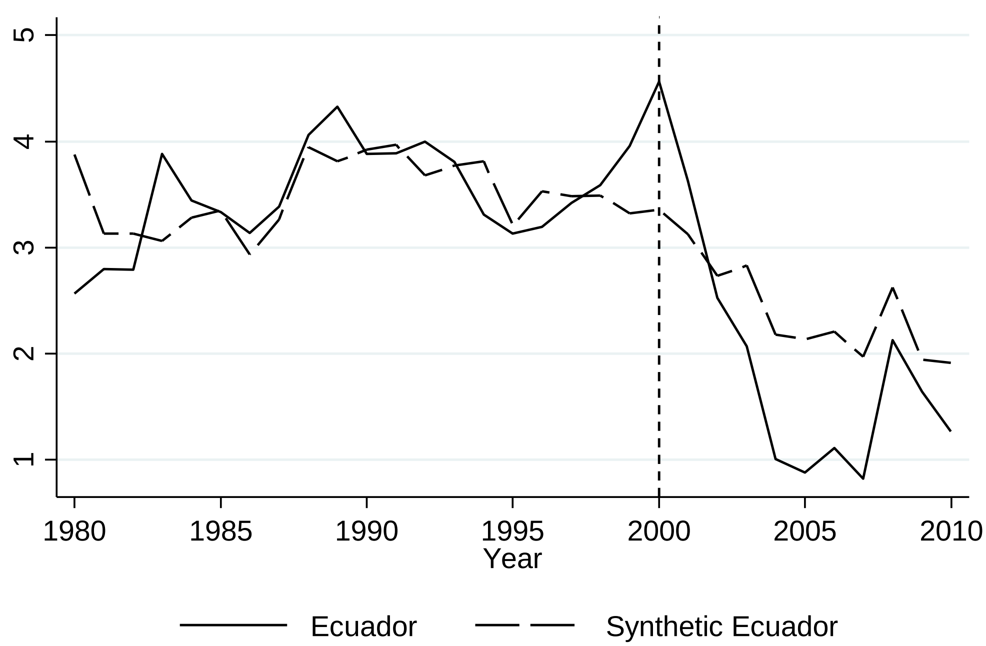
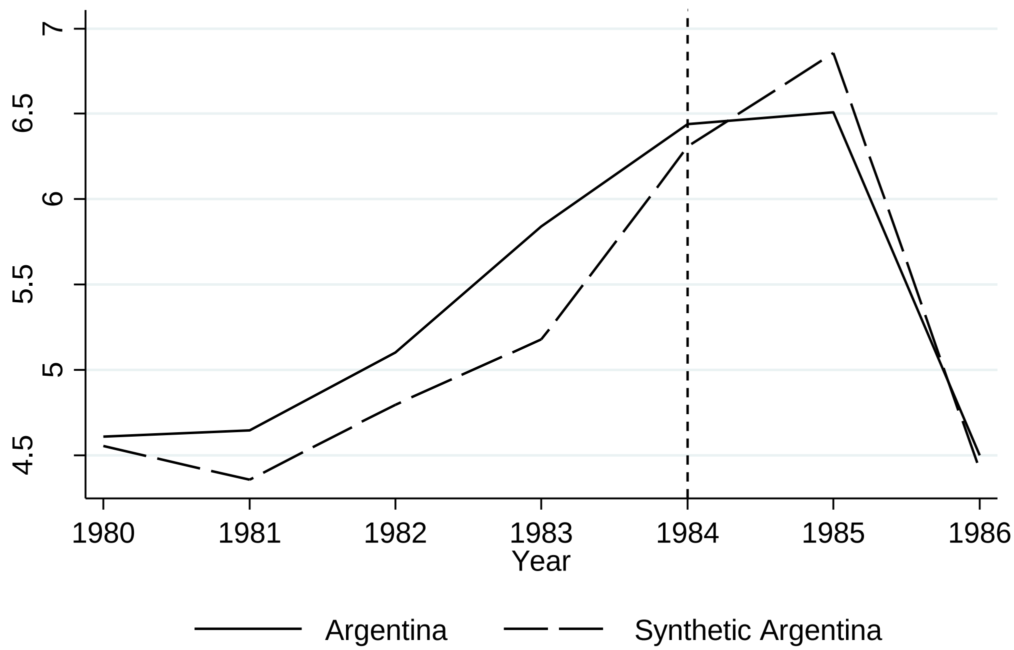


Figure 1.2: Within Sample Falsification Check



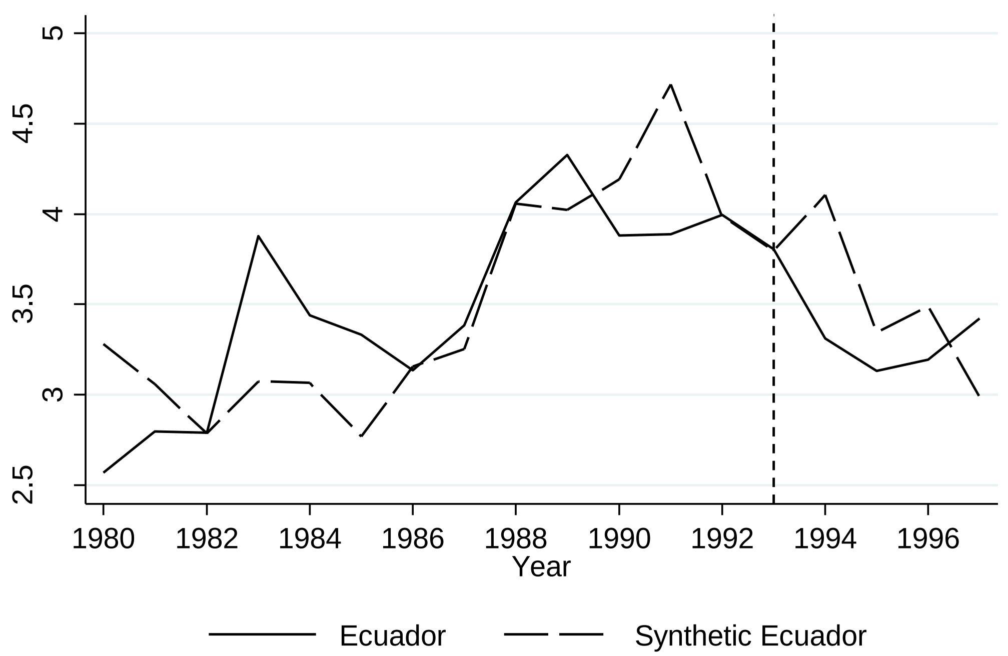


Figure 1.3: Placebo Test

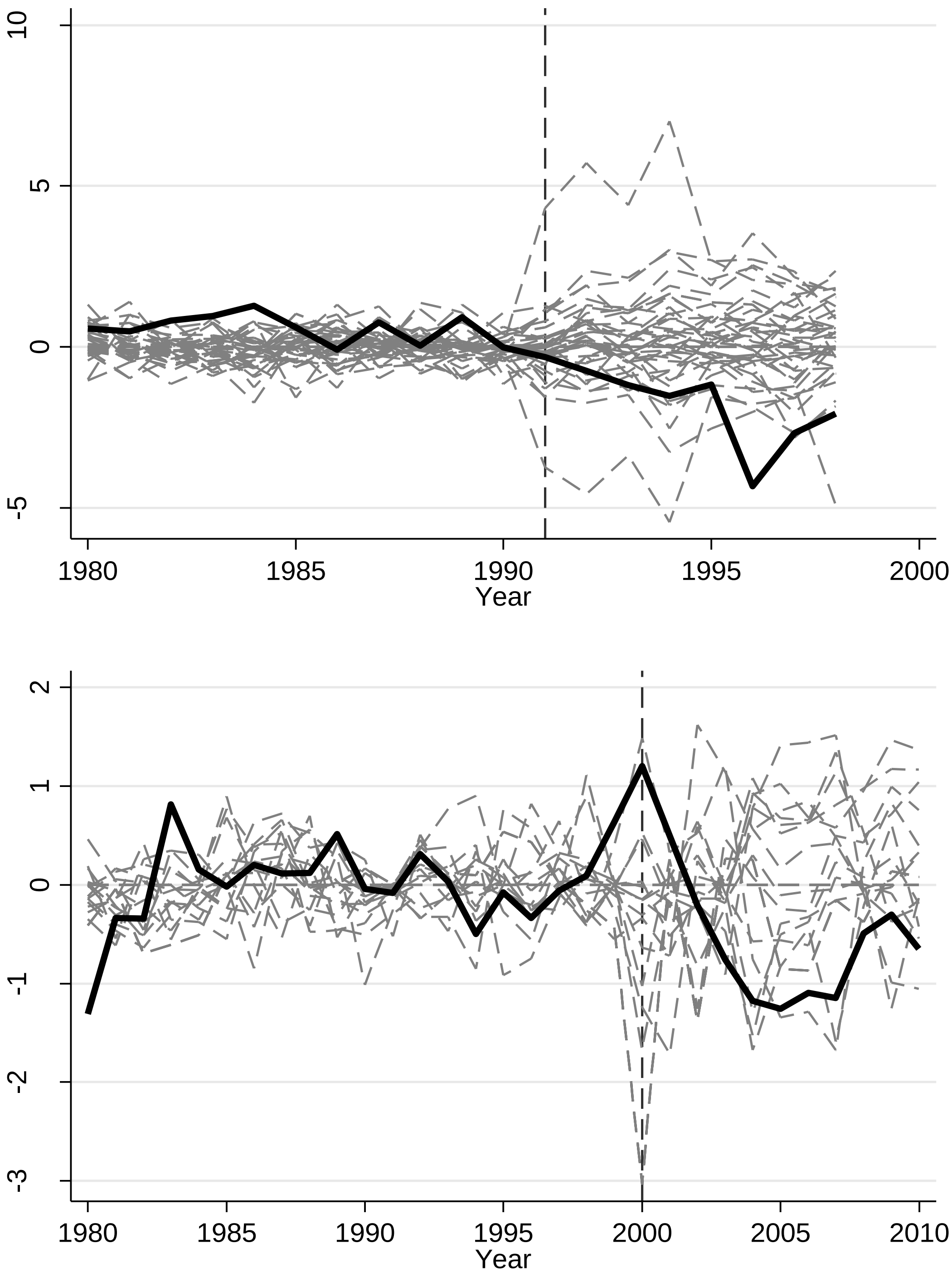


Figure 1.4: Impact of Dollarization/Quasi-Dollarization on Income

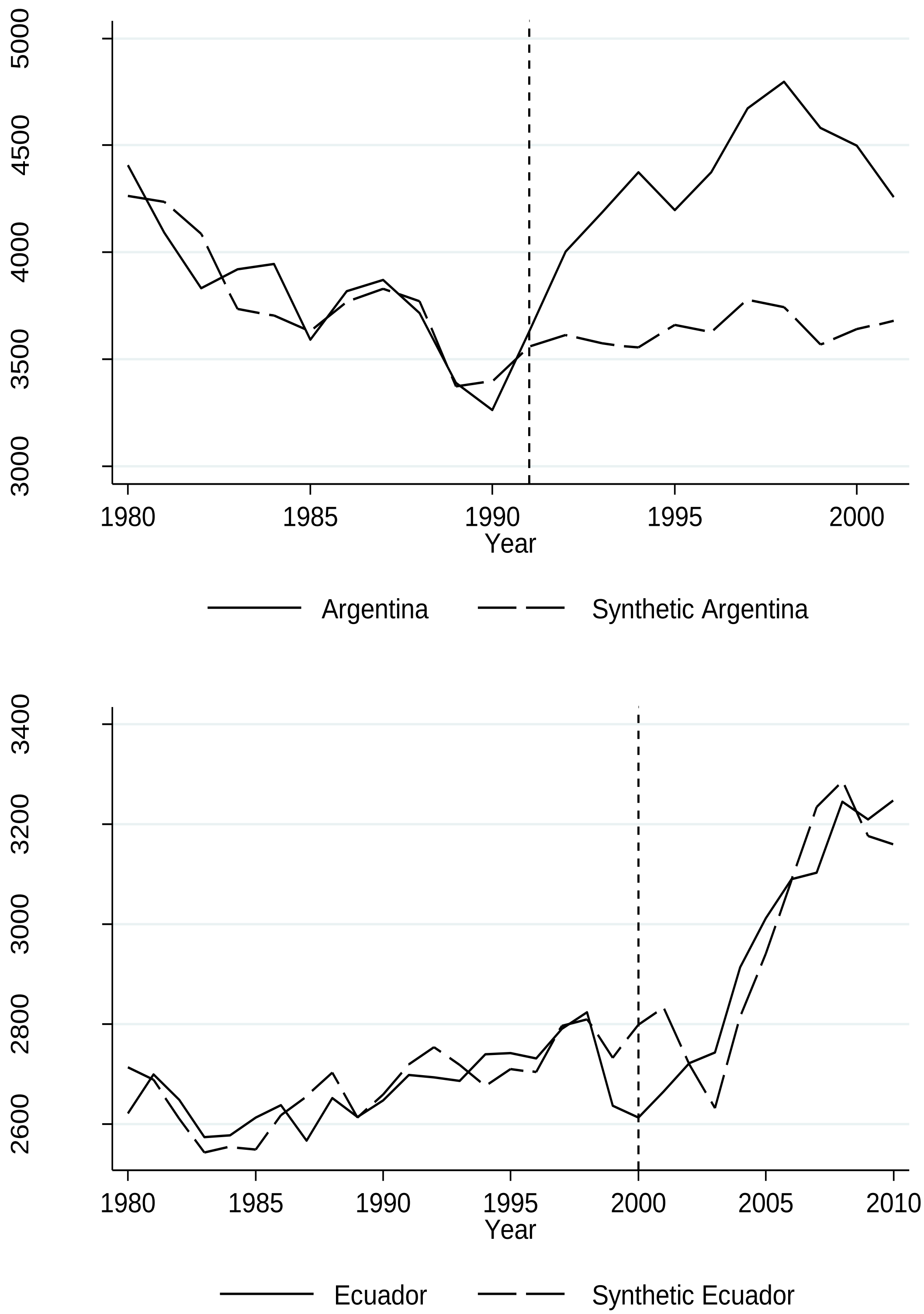


Figure 1.5: Within Sample Falsification Check

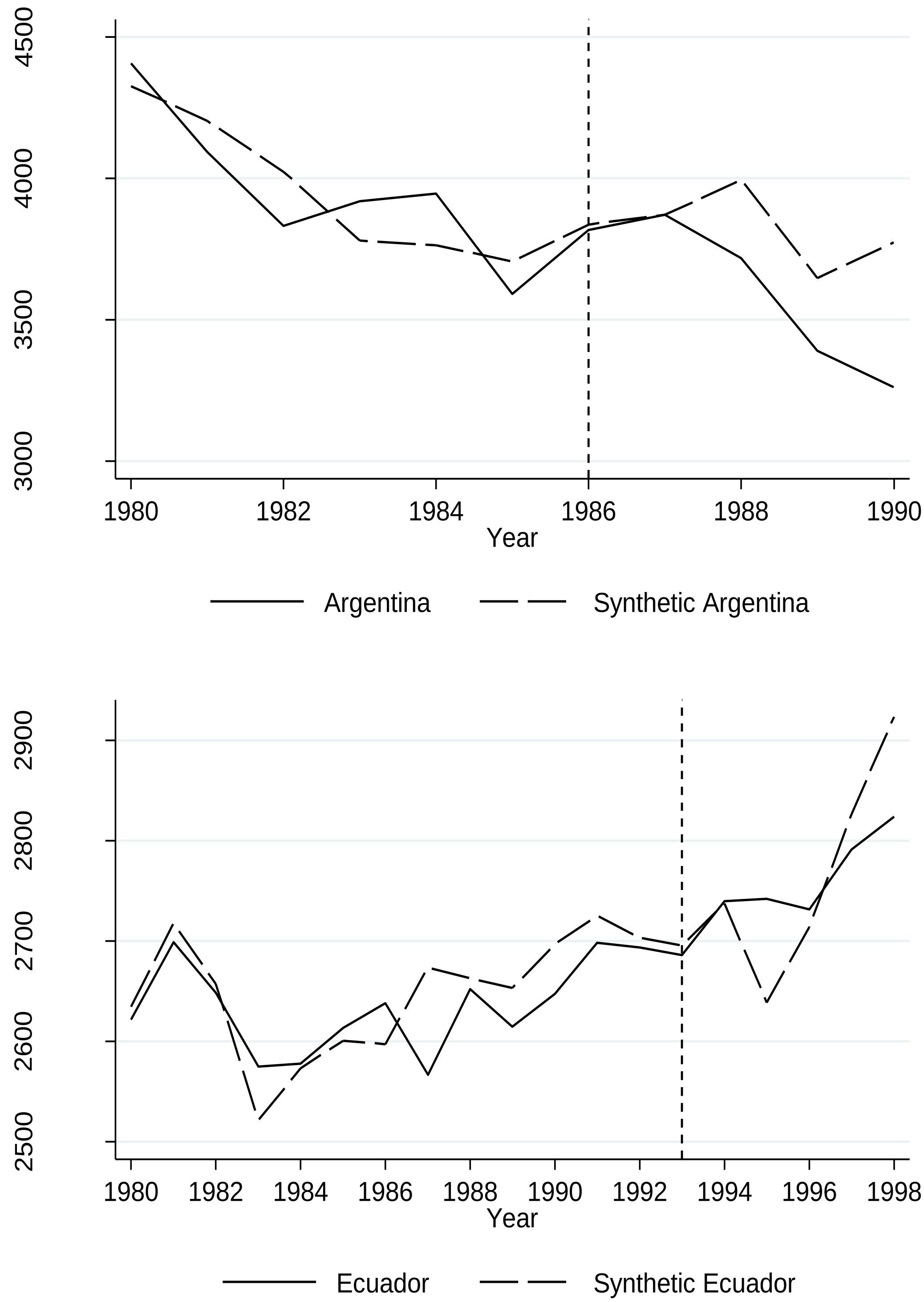


Figure 1.6: Placebo Test

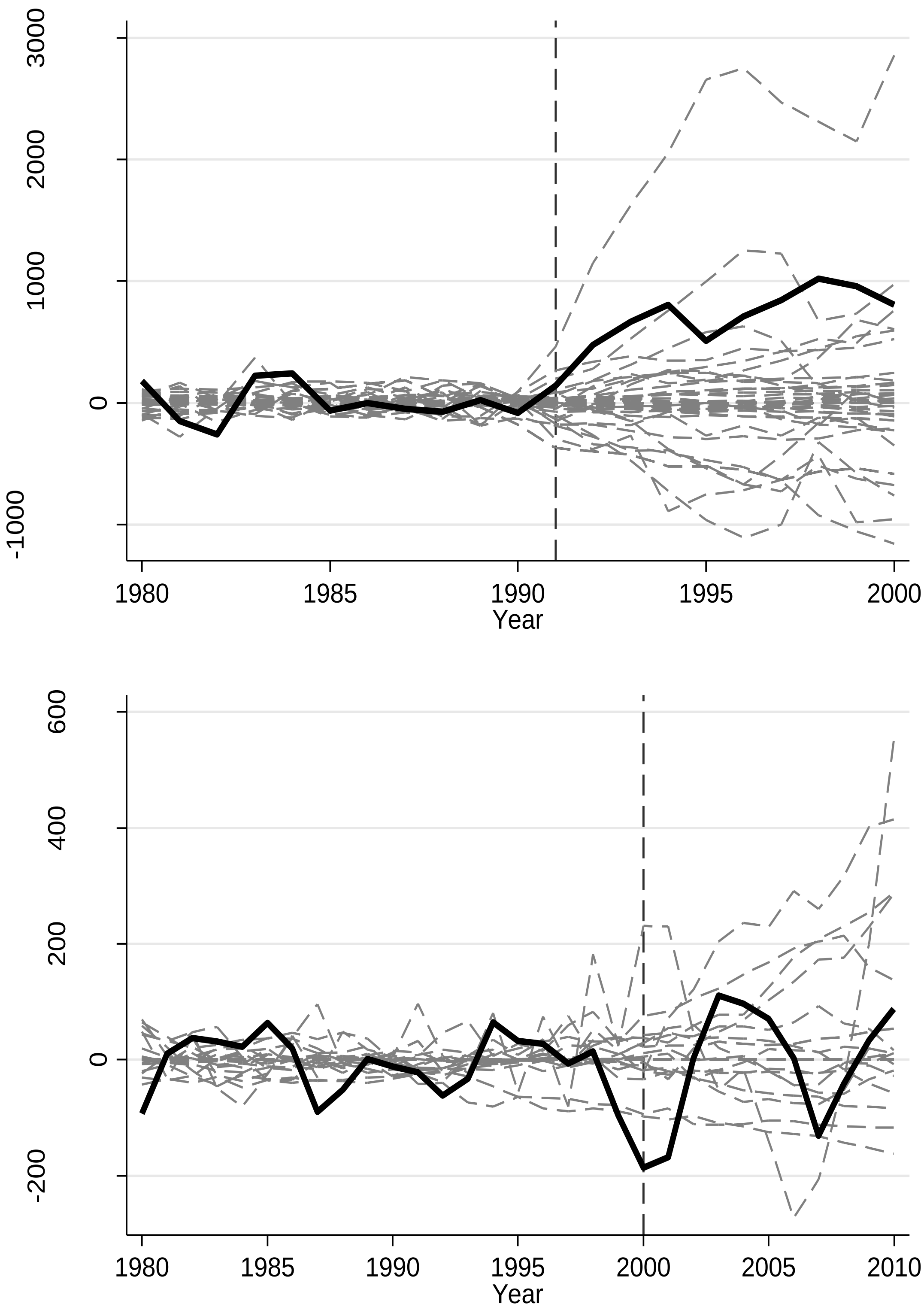
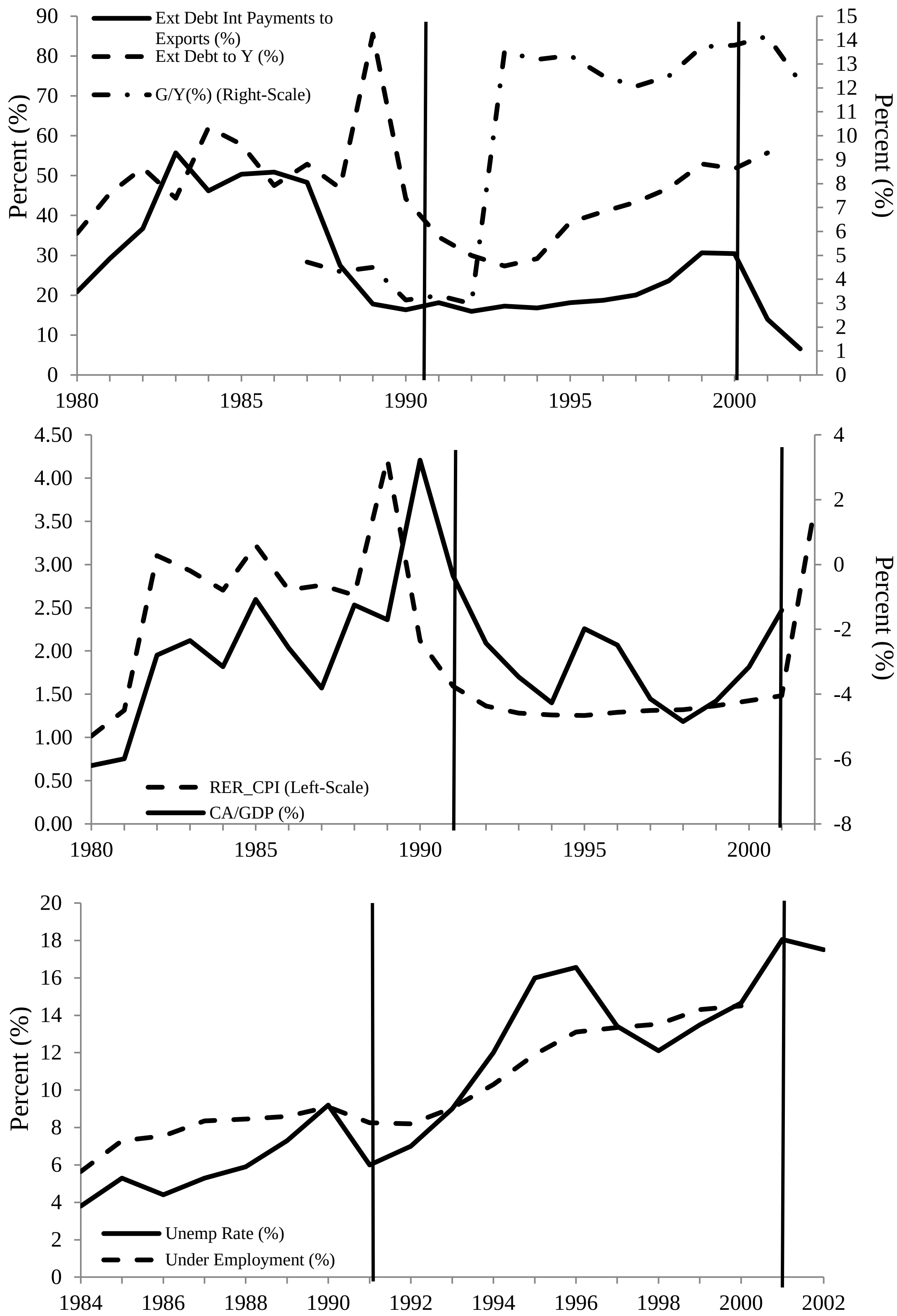


Figure 1.7: Argentina Fiscal Position



Chapter 2:

To Fix or To Float: Quasi-Experimental Analysis of Pre-Crisis Exchange Rate Regime Selection on Post-Crisis Recovery

Introduction

One major concern for a developing country on a high or steady growth path is a sudden crisis precipitating a growth reversal. While developed countries, such as the United States, tend to return to trend growth, excepting the 2008 financial crisis, most developing countries do not. Therefore, many researchers attempt to explain sudden growth accelerations and post-crisis growth stagnation or decline.[13] Here I address the question, "Did a country's exchange rate regime choice before the Asian Financial Crisis affect its subsequent growth path?" I find that having a de-facto flexible exchange rate the year before the Asian Financial Crisis countries had no statistically significant impact on the magnitude of the shock to RGDP per-capita but negatively affected the post-crisis growth rate of RGDP per-capita. Moreover, such countries did experience higher post-crisis inflation than did countries with pre-crisis fixed exchange rates.

Much of the contemporary empirical research on economic development and exchange rate regimes focuses on the effect of exchange rate regimes on long-run trend growth or trend growth volatility.[14] However, we should evaluate the effect of the various exchange rate regime types on economic outcomes in tranquil and crisis periods separately to test if exchange rate regimes affect a country's economic performance differently according to the contemporary economic conditions. Though economic crises, by type (i.e. liquidity, currency, banking, balance sheet, etc.), are highly

[13] Pritchett (2000); Hausmann, Pritchett, and Rodrik (2005); Jerzmanowski (2006); Cerra and Saxena (2007); Jones and Olken (2008); Eichengreen, Park, and Shin (2011); Berg, Ostry, and Zettelmeyer (2011)

[14] Calvo and Vegh (1992); King and Levine (1993) Svensson (2003); Edwards and Magendzo (2003, 2006); Alesina and Barro (2002); Calvo and Mishkin (2003); Levy-Yeyati and Sturzenegger (2003); Beetsma and Giuliodori (2010); Husain, Mody, and Rogoff (2005); Rose (2011)

heterogeneous with respect to their causes and dynamics (Claessens & Kose 2013), the evidence suggests that countries with de-facto fixed exchange rates have lower probabilities of experiencing an economic crisis. Moreover, fixed exchange rate countries recover more slowly than countries with flexible exchange rates that suffer the same crisis.[15]

In previous research on this topic, authors assume that countries cannot select their exchange rate regime, but rather that the exchange rate regime is determined exogenously, and therefore countries with fixed exchange rates should, on average, be appropriate counterfactuals for countries with flexible exchanges rates. Consequently, researchers estimated the treatment effect of exchange rate regime policy using a dummy variable.[16]

If this assumption is incorrect and countries, themselves, endogenously select their exchange rate regimes, then flexible exchange rate and fixed exchange rate countries may, on average, be different from one another on factors that affect the exchange rate

[15] See Grier & Grier (2001) and Calvo & Miskin (2003) for specific analysis of the effect of exchange rate regime selection and recovery from the Asian Financial Crisis. Domac & Peria (2003) find that developing countries with fixed exchange rates have lower probabilities of experiencing a banking crisis but recover more slowly. Tsangarides (2012) found countries with flexible exchange rates recovered more quickly from the 2008 financial crisis than fixed exchange rate countries. Perhaps because flexible exchange rates help absorb the real economic impact of shocks to terms of trade (see Levy-Yayati & Sturzenegger (2005)).

[16] Ghosh et al., (1997) find countries with de-jure fixed exchange rates tend to have lower inflation but higher real output volatility, though this result seems only to hold for developing countries with little access to international capital markets. Using de-facto exchange rate regime data, (the Levi-Yeyati & Sturzenegger classification method) Levi-Yeyati and Sturzenegger (2003) conclude, fixed exchange rates are, in emerging market countries, associated with slower growth and higher output volatility. In a review of the literature comparing growth outcomes across multiple de-facto exchange rate classification systems, Andrew Rose (2012) concludes cross country variation in exchange rate regime choices across similar countries does not correspond to statistically significant differences in macroeconomic outcome variables, such as economic growth and inflation.

regime decision and the growth outcomes of interest in this paper.[17] Consequently, previous papers on this topic might suffer from model misspecification and generate biased estimates of the treatment effect. I find evidence that countries do endogenously select their exchange rate regimes and failing to account for this endogenous selection does produce biased results of the treatment effect of having a flexible exchange rate regime before the Asian Financial Crisis.

This paper builds on the previous research by modeling the decision to adopt or maintain a flexible exchange rate regime in 1996, the year preceding the Asian Financial Crisis. Specifically, I use a PROBIT model to estimate the probability, or propensity score, that a country will have a flexible exchange rate in 1996. I then match treatment and control countries on their propensity scores. The propensity score matching approach compares countries that are observationally similar across factors relevant to exchange rate regime choice at the time when governments are making decisions regarding their exchange rate regime, and therefore the method is well suited to estimate the treatment effect of regime choice to post-crisis growth outcomes. I show that countries did endogenously select their exchange rate regime type before the Asian Financial Crisis, and that the bias from failing to account for this endogenous selection is large enough to produce misleading results.

The paper proceeds as follows. Section 2 describes the particulars of the propensity score matching method I use to get an unbiased estimate of the average treatment effect on the treated (ATT) of the endogenous exchange rate regime choice. Sections 3 and 4

[17] As early as 2002, Calvo and Reinhart (2002) forwarded the hypothesis that countries endogenously selected their exchange rate regimes, often defending a de-facto exchange rate that diverged sharply from the declared exchange rate. Subsequently, authors have attempted to estimate the regime selection decision. (see Vonhagen & Zhou (2007) and Levy-Yeyati & Sturzenegger (2010))

enumerate the data sources and specific variables employed. The paper concludes with a discussion of results.

Methodology: Treatment Effect and Endogeneity Bias

In treatment analysis, the goal is to estimate the average treatment effect on all of the individuals who received the treatment (ATT). Unfortunately, the empirical econometrician never observes an individual after receiving the treatment and the same individual at the same moment in time who has not received the treatment. Moreover, empirical researchers cannot work around the first problem by observing a subject over a fixed time-period without the treatment and then over a second fixed time-period, of equal duration, observe the individual with the treatment. This is because other factors in the environment that affect the outcome of interest may change over the observation period, and therefore, a country before the treatment may not be an appropriate counterfactual for the same country after the treatment.

If the treatment assignment were random, then one could calculate the ATT by differencing the average of the outcomes of the treatment group and the average of the outcomes of the control group (Caliendo & Kopeinig, 2008). The primary challenge with evaluating exchange rate policy is that countries themselves select their exchange rate regimes based on observable country characteristics and expectations of future economic conditions. For this reason, countries that do not enact the policy of interest may be, on average, different from countries that do enact the policy on factors that affect the outcome of interest. Therefore, countries that do not enact the policy will not be good counterfactuals for countries that do. Consequently, estimating the ATT of a given exchange rate regime switch via dummy variables in a linear model will produce biased results.

In his seminal work, Rubin (1974) first proposed the method of pair wise matching treated and control group individuals in a nonrandom treatment assignment study to generate an unbiased estimate of the ATT. He noted randomization of the treatment assignment reasonably ensures that, on average, the treatment and control groups are identical in all factors –observed and unobserved– that affect the outcome of interest, and therefore the average difference of the treated and control outcomes are an unbiased estimate of the ATT. Likewise, the average difference of pair wise matches of treated and control individuals, on the same observable factors, is also an unbiased estimate of the ATT. Unanswered is how to match individuals, given one could generate any number of possible matching combinations by changing the weights placed on each of the relevant variables. Moreover, the dimensionality problem of matching directly on covariates makes this method untenable as the number of covariates grows.

Rubin and Rosenbaum (1983) operationalized this proposed method by proving that matching via a scalar propensity score based on factors that affect the decision to adopt a given treatment and the outcome of interest will achieve the conditions necessary to estimate the ATT, unbiasedly in both small and large samples. These requirements are first that conditional on a well specified propensity score model the treatment assignment is independent of the potential responses and second that the probability of receiving treatment is less than 1 and greater than zero for all combinations of characteristic in the sample. If these two conditions are satisfied, one can say treatment is strongly ignorable.[18] Put plainly, these two conditions ensure that for two individuals with the same propensity score treatment assignment is essentially random.[19]

I offer the following example to illustrate the method. Suppose we have a sample of 100 countries that have adopted a floating exchange rate, and a much larger sample of countries, perhaps 1000, which remained fixed. For now, let us ignore the fact that this is six times the number of countries that actually exist. To estimate the treatment effect of this endogenous policy decision on, for example, per-capita RGDP growth one year after policy adoption, we can employ the Rubin and Rosenbaum matching method as follows. First, using a vector of observable characteristics we believe affect both the decision to adopt the policy and per-capita RGDP growth, we estimate the probability that a given country adopts the policy of interest via PROBIT, LOGIT, or a linear probability model. We then match each treated country with a country in the control group that has the same, or nearly the same, probability of adopting the treatment (i.e. propensity score). For each treatment-control pair we difference the per-capita RGDP growth rates in the year following policy adoption and take the average of the differences.

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[18] Showing that treatment is strongly ignorable is elsewhere referenced as solving the selection of treatment on the observables problem (Heckman, et al (1998)) or satisfying conditional independence of treatment assignment (Lechner (2002)).

[19] Implicitly, one must also assume the response of unit j to treatment T is independent of treatment given to some other unit k. This is the so-called stable-unit-treatment assumption **(**Rosenbaum & Rubin 1983)**.**

Data and Estimation

I perform my matching estimation of the ATT of having a flexible exchange rate regime the year before the Asian Financial Crisis via the following two-step procedure. First, I estimate the probability a sample country has de-facto flexible exchange rate in 1996, the year before the Asian Financial Crisis. Second, I match each flexible exchange rate country with a fixed exchange rate country with a similar propensity score of having a flexible exchange rate in 1996, and difference the post-crisis outcomes of interest: RGDP per-capita, RGDP per-capita growth, and inflation. The average of the differences for each outcome of interest across all matched pairs is the ATT of having a flexible exchange rate regime on the outcome variables in the post-crisis period.

There are a number of competing methods for classifying a country's exchange rate regime. The IMF provides a list of de jure (i.e. country declared) regimes, but previous works have revealed that de facto exchange rate regimes diverge markedly from the de jure. Reinhart and Rogoff (2004) and Levi-Yeyati and Sturzenegger (2005) (LYS), and Ghosh, et al (1997) elucidate just a few of the competing classification methods.

In both of these classification schemes, countries are finely divided by degrees of flexibility of the exchange rate: free floating, managed float, crawling peg, or hard peg. Because of my sample size of approximately 100 countries, I do not use a multinomial PROBIT to estimate the various treatment effects of a range of non-fixed exchange rates to the base case of a fixed exchange rate. Rather I define a country as having flexible exchange rate if a country has either a pure or a managed float. I list my sample countries by 1996 Reinhart & Rogoff exchange rate regime in table 2.1 and by 1996 Levy-Yeyati & Sturzenegger exchange rate regime in table 2.2.

A number of papers provide guidance on model selection for predicting exchange rate arrangement decisions.[20] Utilizing these insights, I estimate the probability that a country adopts a flexible exchange rate in year t, using a PROBIT model (equation (2.1)) and the variables presented in table 2.3. To avoid contemporaneous endogeneity between the left-hand side and right-hand side variables, I lag the right hand side covariates one year.

$$\begin{aligned} P(D=1|x)_{i,t} = \beta_0 &+ \beta_1 CA_{i,t-1} + \beta_2 CI_{i,t-1} + \beta_3 FINDEV_{i,t-1} \\ &+ \beta_4 OPEN_{i,t-1} + \beta_5 SIZE_{i,t-1} + \beta_6 LEVEL_{i,t-1} \\ &+ \beta_7 RES_{i,t-1} + \beta_8 NOMSHK_{i,t-1} + \beta_9 INF_{i,t-1} + \varepsilon_{it} \end{aligned} \quad (2.1)$$

In this equation, CA is a country's current account balance to GDP. CA should be positively correlated with having a flexible exchange rate, as large swings in current account balance would constitute a sudden shift foreign reserves stocks, making a fixed exchange rate more difficult to maintain (Krugman 1979).

The Chinn-Ito index (CI) captures the degree of capital account openness in a given country for a given year (Chinn & Ito (2006)). Previous studies find countries with more stringent capital controls are more likely to have fixed de facto exchange rate regimes (Carmignani, et al (2008), Edwards (1996), and Von Hagen & Zhou (2007).

My measure of a country's financial development (FINDEV) is M2 to GDP. One would expect that developing countries with flexible exchange rates are more likely to attract savings and foreign investment, as the country risk related to sudden devaluation

[20] Carmignani, et al (2008), Edwards (1996), Edwards & Magendzo (2003, 2006), Holden, et al (1979), Melvin (1985), and Van Hagen & Zhou (2007)

is less frequent and more predictable in the case of fixed exchange rate countries. Empirically, higher financial development is correlated with having a fixed exchange rate regime (Edwards (1996), and Von Hagen & Zhou (2007). However, Carmignani, et al (2008) define financial development as M2 over M1 and find this measure is negatively correlated with having a de facto fixed exchange rate.

OPEN is the ratio of the sum of exports and imports to GDP, and it measures the contribution of trade to a country's economy. According to Optimal Currency Area (OCA) theories, as countries increase their trade openness, they should enjoy greater benefits from adopting fixed exchange rates (Alberto & Barro (2002), Mundell (1961), and Carmignani, et al (2008)). Indeed, some empirical studies find greater trade openness is positively correlated with adopting a fixed exchange rate (Edwards (1996), Edwards & Magendzo (2003, 2006), and Carmignani, et al (2008)). OPEN is also a proxy for how exposed a country is to external shocks. One would expect that countries more exposed to external shocks would be more likely to adopt a flexible exchange rate regime to allow adjustments to occur in the exchange rate and not in the factors of production markets. Supporting this view, Von Hagen & Zhou (2007) find trade openness is positively related to adopting a flexible exchange rate.

Relatedly, the size of an economy should also be positively correlated with adopting a fixed exchange rate. Given a fixed level of openness, the larger the economy the more international transactions it will have so the greater the benefits of eliminating foreign exchange transaction costs related to adopting a common currency (Alberto & Barro (2002) and Mundell (1961). The variable SIZE is the natural log of real GDP in billions of USD and captures the size of a country's economy. LEVEL is natural log of real

GDP per-capita and proxies the country's standard of living. Empirical results are mixed: SIZE is positively correlated with a country adopting a flexible exchange rate (Carmignani, et at (2008) and Von Hagen & Zhou (2007)), but LEVEL is negatively correlated with adopting a flexible exchange rate (Edwards (1996) and Von Hagen & Zhou (2007).

The variable RES is the country's stock of foreign currency reserves to GDP. Empirically countries with higher stocks of foreign reserves are more likely to adopted fixed exchange rates. (Edwards (1996), Von Hagen & Zhou (2007))

Inflation (INF) is the annual percentage change in the GDP deflator, and NOMSHK is the absolute deviation of M2 growth from the four year moving average of M2 growth. Both Edwards (1996) and Von Hagen & Zhou (2007) find these variables are positively correlated with adopting a fixed exchange rate.

These data for the PROBIT model all come from the World Banks World Development Indicators database, except NOMSHK, which I calculate as in Edwards (1996), and the Chinn-Ito index.

I present the results of my PROBIT estimations in table 2.4. The results are mostly consistent with previous findings and across exchange rate regime classification methods. CA is insignificant under the R&R classification regime but negatively correlated with having a LY&S de facto floating regime. As expected, greater capital account restrictions reduce the probability of having a flexible exchange rate regime. Contrary to Edwards (1996) and Von Hagen & Zhou (2007), I find greater financial development is positively correlated with having a flexible exchange rate regime in 1996. Trade openness is significant in both estimations but with opposite signs. Size and Level are as expected. RESERVE, NOMSHK, and INF are all statistically insignificant.

After estimating the probability of having a flexible exchange rate regime in 1996, I use this propensity score to match treatment group countries with a country in the control group with the closest propensity score. I then calculate the mean differences of outcomes of interest, using these matched pairs.

There are many ways to match treatment and control group countries based on their estimated propensity scores: one-to-one nearest neighbor, radius matching, inverse propensity score matching, etc. I find that I achieve the identification requirements when I match via one-to-one nearest neighbor or inverse propensity score matching using a Gaussian kernel.

In the second method, a treatment group member's outcome is compared to a weighted average of nearby treatment group members' outcomes. The weight is proportional to a control member's "distance" from the treatment group member, in terms of the difference of their propensity scores. This matching method is preferred to other matching methods in cases where there are many individuals in the control group (Hirano, Imbens, and Ridder, 2003). Therefore, I report the Gaussian kernel-weighted matching results in the table and figures section, though the one-to-one matching results are qualitatively the same.

Results

First, I compare the mean values across my treatment and control groups for each of the regressors from my PROBIT model to show that the unmatched fixed exchange rate regime countries are not an appropriate counterfactual for the flexible exchange rate regime countries. The results in tables 2.5 and 2.6 show that several of the means of the right hand side covariates are statistically different from one another when we conduct an unmatched control-treatment group comparison.[21,22]

Notice in the R&R case, the average values of two of the right-hand side variables for the treatment group are statistically different from the corresponding average value of the control group at the 95% level. In the LYS case, there are two such regressors, and four at the 90% level. This means the treatment and control group are different, on average, from each other on variables that affect the outcomes of interest. Consequently, any estimation of the ATT via a dummy variable for exchange rate regime type would generate a biased and inconsistent result.

Next, I show I, indeed, have a properly specified propensity score model that accounts for the selection into the treatment group on observable factors. There are two methods to check if I have empirically satisfied the conditions of strong ignorability of treatment.

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[21]The reader may wonder using a long pre-crisis period to estimate the coefficients on the PROBIT model may be inappropriate because of either high persistence in exchange rates regime choices over time or because of the existence of other economic crisis over this period. I re-estimated my PROBIT model using only 1996 data and found estimates of the treatment effect that are not qualitatively different from those described in this section.

[22]Readers may worry about slippage or movement between the treatment and control group after the onset of the Asian Financial Crisis. Because this paper investigate the effects of a country's choice of exchange rate regime in 1996, only, and matches control and treatment countries on the 1996 propensity scores, it is unnecessary for me to account for treatment assignment slippage during the crisis.

If, for example, a country fixed in 1996 and due to the crisis had to break the peg, which resulted in a higher interest rates or capital flight and thus lower post-crisis period economic growth, vis-à-vis a 1996-pegged country, then my paper would attribute this result to the country's exchange rate regime choice in 1996. From the standpoint of my framework, any actions a floating exchange rate regime country is forced to take after 1996 that affects RGDP per-capita, per-capita RGDP growth, or inflation is part of the ATT of selecting a floating exchange rate in 1996. If I have properly matched, then I have controlled for relevant pre-crisis differences between countries so any post-crisis differences represent the treatment effect.

As a first pass, I inspect the distributions of the propensity scores. Figures 2.1-2.2 illustrate, in both the R&R and LYS cases, the considerable overlap of the two distributions. The area of overlap represents the set of control and treatment countries that are identical, on average, on the observables, except for treatment status. Specifically, the area of overlap represents the subsample of paired countries where each has the same, or nearly the same, probability of adopting the policy but only one actually adopts the policy. Therefore, between the two countries in each treatment-control pair treatment assignment is random. This large area of overlap means I have many comparable pairs from which to draw inference.

As a formal check that the control and treatment samples are on average identical, I consider tables 2.7 and 2.8. These tables show the differences of the average of the matched and unmatched treatment-control samples for each regressor for the two de-facto exchange rate regimes. If the average of the differences of each of the covariates is not statistically different from zero, then we have evidence that treatment assignment is strongly ignorable (Dehejia & Wahba, 2002). In this case, we say the covariates are balanced.

For the R&R sample (table 2.7), when I match the groups via inverse propensity score weighting, I fail to reject the null hypothesis of no mean difference for all regressors at the 91% level. For the LY&S sample (table 2.8), I fail to reject the null hypothesis of no mean difference for each of the right-hand side covariates at the 76% level, and in most cases at even lower levels. Therefore, I conclude by matching on these covariates, I have accounted for this endogenous selection on these observables and have made these groups, on average, identical on the relevant, observable factors.

More importantly, this means endogenous selection on these observables into the treatment group will not bias my estimates of the ATT.

Overall, I find in the immediate aftermath of the Asian Financial Crisis (i.e. 1997-2002), countries with de-facto flexible exchange rate regimes before the crisis fared no better than countries with pre-crisis fixed exchange rate regimes in terms of level RGDP per-capita but did suffered slower post-crisis RGDP per-capita growth. Furthermore, countries with pre-crisis flexible exchange rates suffered slightly higher post-crisis inflation in the five years immediately following the crisis, though this result differs across exchange rate regime classifications.

As a way of seeing the importance of accounting for endogenous selection, I present both the matched and unmatched results in table 2.9. The matched results are consistently insignificant in all years and across both de-facto exchange rate regimes. However, for the R&R sample, the unmatched ATT estimates are statistically significant at the 99% level and positive for all years. This is an example of the bias one can encounter from failing to account for endogenous selection into the treatment group.

As for post-crisis RGDP per-capita growth rates (table 2.10), a country's pre-crisis exchange rate regime has a statistically significant and mostly negative effect on post-crisis growth. This result is robust to the type of de-facto exchange rate considered. Furthermore, across both exchange rate regime samples, failing to account for endogenous selection biases the estimates of ATT towards zero.

In the matched R&R estimates, the treatment effect is negative and significant at the 99% level in 1999 and 2001. In 2002, the treatment effect is significant and negative at the 90% level. Notice across these three years, countries with flexible exchange rates grew, on average, 2.1 percentage points slower than countries with fixed exchange rates in 1996. The unmatched estimate of the treatment effect is not statistically different from zero across all years.

In the matched LY&S estimates, the treatment effect is negative and significant at the 95% level in 1999, at the 99% level in 2001, and at the 90% level in 1997. Over these three years, countries with flexible exchange rates grew, on average, 2.2 percentage points slower than countries with fixed exchange rates in 1996. In 1998, the treatment effect is positive and significant at the 95% level. The unmatched estimate of ATT is only significant and negative in 2000, a year when the matched estimate of ATT is not statistically different from zero.

Finally, we consider inflation (see table 2.11). These results are not robust across exchange rate regime classification schemes in terms of the direction and size of the ATT of pre-crisis exchange rate regime selection on post-crisis inflation. Additionally, the direction of the bias varies across exchange rate regime classifications.

In the R&R de-facto exchange rate regime case, countries with pre-crisis flexible exchange rates experience statistically significant higher inflation in the fourth year after the crisis. The matched estimate of the inflation differential (7.43%) between the control and treatment countries is higher than the dummy variable estimate (3.27%), which is not statistically different from zero.

In the LYS de-facto estimates, I find no corresponding statistically significant inflation differential in the fourth year. In fact, the estimated treatment effect of having a flexible exchange rate in 1996 on post-crisis inflation rates is not statistically significant at the 95% level in any year. In the unmatched estimates, however, we see that countries with flexible exchange rates suffer higher inflation, about 8.6 percentage points, than pre-crisis fixed exchange rate countries in 1998, the year immediately following the crisis. Here again we see an attenuation bias from failing to account for endogenous selection into the treatment group.

Overall, I conclude that for this particular crisis and for the countries in this sample, having a de-facto flexible exchange rate regime in 1996 did not affect the magnitude of the shock from the Asian Financial on post-crisis RGDP per-capita or post-crisis inflation, but did cause treatment countries to grow more slowly relative to their 1996 fixed exchange rate counterparts.

Conclusion

In this paper, I have shown having a flexible exchange rate before the crisis did not insulate RGDP per-capita from the effects of the crisis. Moreover, countries with de-facto, floating exchange rate regimes the year before the crisis, recovered more slowly. Perhaps equally important, I find that in 1996 de-facto flexible and fixed exchange rate regime countries are, on average, statistically different from one another on variables that affect the outcomes of interest. Moreover, failing to account for these differences produces biased results that are, in some cases, significantly different from matching results that account for selection of exchange rate regime on observable factors. The sign of the bias is not, however, consistent in direction across outcomes of interest.

These findings have two implications. First, the results of the previous empirical research on this topic are not robust to relaxing the assumption that a country's de-jure or de-facto exchange rate regime is exogenous. Second, my results argue against making the policy prescription for flexible exchange rate regimes based on the justification that countries with flexible exchange rates recover more quickly from economic crises. Of course, these results are only generalizable to the countries in my sample and for crises identical to the 1997 Asian Financial Crisis.

In my continuing research on this topic, I would like to consider other crisis of various types to investigate if these results are generalizable to all crisis types. We know that the causes, dynamics, and economic impacts of crises are variable depending on their type (i.e. liquidity, currency, banking, balance sheet, etc.) (Claessens & Kose 2013). Therefore, while we might expect exchange rate regime selection to affect post-crisis economic growth in the case of a financial crisis marked by speculative currency attacks and sudden current account reversals, such as the Asian Financial Crisis. We may find that exchange rate regimes do not have statistically significant impact on post-crisis recover after banking crisis, unless the trigger is a rise in the value of foreign currency denominated debt, or in the case of a balance sheet recession initiated by the bursting of a housing bubble, a spike in non-performing loans, and high consumer indebtedness.

Tables

Table 2.1: Reinhart & Rogoff Sample Countries by 1996 De-Facto Exchange Rate

#	Treatment	#	Control	#	Control	#	Control
1	Australia	1	Algeria	26	Ethiopia	51	Nepal
2	Chile	2	Argentina	27	Finland	52	Netherlands
3	Colombia	3	Bahrain	28	Gabon	53	Nicaragua
4	Ecuador	4	Bangladesh	29	Gambia, The	54	Niger
5	Haiti	5	Barbados	30	Guatemala	55	Pakistan
6	Honduras	6	Belize	31	Guinea	56	Panama
7	Israel	7	Benin	32	Guinea-Bissau	57	Papua New Guinea
8	Japan	8	Bolivia	33	Guyana	58	Paraguay
9	Madagascar	9	Botswana	34	Hungary	59	Peru
10	Mexico	10	Brazil	35	Iceland	60	Philippines
11	New Zealand	11	Burkina Faso	36	India	61	Saudi Arabia
12	Nigeria	12	Cameroon	37	Indonesia	62	Senegal
13	Norway	13	Canada	38	Italy	63	Sri Lanka
14	Poland	14	Central African Republic	39	Jordan	64	St. Lucia
15	Singapore	15	Chad	40	Kenya	65	St. Vin. the Grenadines
16	South Africa	16	China	41	Korea, Rep.	66	Sudan
17	Suriname	17	Congo, Rep.	42	Kuwait	67	Swaziland
18	Sweden	18	Costa Rica	43	Lebanon	68	Thailand
19	Syrian Arab Republic	19	Cyprus	44	Lesotho	69	Togo
20	Uganda	20	Denmark	45	Malawi	70	Tunisia
21	United Kingdom	21	Dominica	46	Malaysia	71	Uruguay
22	United States	22	Dominican Republic	47	Mali		
23	Zimbabwe	23	Egypt, Arab Rep.	48	Mauritius		
		24	El Salvador	49	Morocco		
		25	Equatorial Guinea	50	Mozambique		

Table 2.2: Levy-Yeyati & Sturzenegger Sample Countries by 1996 De-Facto Exchange Rate

#	Treatment	#	Control	#	Control	#	Control
1	Algeria	1	Argentina	28	Guinea-Bissau	55	Philippines
2	Australia	2	Bahrain	29	Guyana	56	Rwanda
3	Bulgaria	3	Barbados	30	Iceland	57	Saudi Arabia
4	Canada	4	Belize	31	Indonesia	58	Senegal
5	Colombia	5	Benin	32	Italy	59	Sierra Leone
6	Dominican Republic	6	Bolivia	33	Jordan	60	Singapore
7	Ecuador	7	Botswana	34	Kenya	61	Sri Lanka
8	Guatemala	8	Brazil	35	Korea, Rep.	62	St. Lucia
9	Guinea	9	Burkina Faso	36	Kuwait	63	St. Vin. & the Grenadines
10	Haiti	10	Cameroon	37	Lebanon	64	Swaziland
11	Honduras	11	Central African Republic	38	Lesotho	65	Syrian Arab Republic
12	India	12	Chad	39	Malawi	66	Togo
13	Israel	13	Chile	40	Malaysia	67	Tunisia
14	Japan	14	China	41	Mali	68	Uganda
15	Madagascar	15	Comoros	42	Mongolia	69	Venezuela, RB
16	Mauritius	16	Congo, Rep.	43	Mozambique	70	Zimbabwe
17	Mexico	17	Costa Rica	44	Namibia		
18	Pakistan	18	Cyprus	45	Netherlands		
19	Peru	19	Denmark	46	New Zealand		
20	Poland	20	Dominica	47	Nicaragua		
21	South Africa	21	El Salvador	48	Niger		
22	Sudan	22	Equatorial Guinea	49	Nigeria		
23	Sweden	23	Ethiopia	50	Norway		
24	Turkey	24	Finland	51	Oman		
25	United Kingdom	25	Gabon	52	Panama		
26	United States	26	Gambia, The	53	Papua New Guinea		
27	Uruguay	27	Ghana	54	Paraguay		

Table 2.3: Exchange Rate Regime Selection Equation Variables

Variable	Description	Data Source
CA	Current account to GDP	WDI
FINDEV	Broad money (M2) to GDP	WDI
CI	Chinn-Ito Index	Portland State Univ.
OPEN	Exports + Imports to GDP	WDI
SIZE	Log of NGDP	WDI
LEVEL	log of NGDP per capita	WDI
RESERVE	Foreign reserves to M2	WDI
NOMSHK	Absolute deviation of M2 growth from trailing 4 year average growth rate of M2	WDI and author's calculations
INF	GDP deflator	WDI

Table 2.4: PROBIT Regression Results:
Adoption of Floating Exchange Rate Regime

	R&R: De-Facto	LYS: De-Facto
	(1)	(2)
	Float	Float
CA_{t-1}	0.008	-0.0133***
	(0.006)	(0.0052)
CI_{t-1}	-0.298***	-0.134***
	(0.033)	(0.031)
$FINDEV_{t-1}$	0.005***	0.008***
	(0.002)	(0.002)
$OPEN_{t-1}$	0.003***	-0.004***
	(0.001)	(0.001)
$SIZE_{t-1}$	0.228***	0.277***
	(0.026)	(0.028)
$LEVEL_{t-1}$	-0.2060	-0.59**
	(0.266)	(0.285)
$RESERVE_{t-1}$	0.034	0.102
	(0.1)	(0.106)
$NOMSHK_{t-1}$	-0.0003	0.000
	(0.0003)	(0.0001)
INF_{t-1}	-0.001	0.000
	(0.001)	(0.0001)
N	1481	1565
pseudo R^2	.1166	.1721
Log Likelihood	-792.13	-798.05

S.E. statistics in parentheses
*p<.1, **p<.05, ***p<.01

Table 2.5:
Characteristics of R&R De-Facto Flexible and Fixed Exchange Rate Countries in 1996

	A	B	(A-B)	H_0: A-B = 0	
Variable	Treated Mean (n=23)	Control Mean (n=71)	Difference	T-stat	Pr(|T| > |t|)
CA_{t-1}	-1.32	-5.42	4.10	1.46	0.15
CI_{t-1}	0.46	0.30	0.15	0.48	0.63
$FINDEV_{t-1}$	50.07	44.67	5.40	0.71	0.48
$OPEN_{t-1}$	66.78	78.42	-11.64	0.99	0.32
$SIZE_{t-1}$	4.37	2.58	1.79	3.44	0.00
$LEVEL_{t-1}$	2.12	2.00	0.12	2.40	0.02
$RESERVE_{t-1}$	0.30	0.38	-0.08	0.64	0.52
$NOMSHK_{t-1}$	10.24	28.47	-18.23	0.49	0.63
INF_{t-1}	25.40	13.71	11.69	1.51	0.13

Table 2.6:
Characteristics of LY&S De-Facto Flexible and Fixed Exchange Rate Countries in 1996

	A	B	(A-B)	H_0: A-B = 0	
Variable	Treated Mean (n=27)	Control Mean (n=70)	Difference	T-stat	Pr(|T| > |t|)
CA_{t-1}	-2.32	-5.24	2.92	1.14	0.26
CI_{t-1}	0.30	0.34	-0.04	0.14	0.89
$FINDEV_{t-1}$	46.65	42.50	4.15	0.58	0.56
$OPEN_{t-1}$	51.36	84.95	-33.59	3.21	0.00
$SIZE_{t-1}$	4.39	2.32	2.07	4.25	0.00
$LEVEL_{t-1}$	2.08	2.01	0.07	1.61	0.11
$RESERVE_{t-1}$	0.20	0.43	-0.23	1.94	0.06
$NOMSHK_{t-1}$	7.28	30.51	-23.23	0.67	0.50
INF_{t-1}	22.48	14.51	7.97	1.67	0.10

Table 2.7: Matched Results:
Characteristics of R&R De-Facto Flexible and Fixed Exchange Rate Countries in 1996

Variable	A Treated Mean (n=23)	B Control Mean (n=71)	(A-B) Difference	H_0: A-B = 0 T-stat	Pr(|T| > |t|)
CA_{t-1}	-1.32	-4.61	3.29	1.13	0.26
CI_{t-1}	0.46	0.01	0.44	1.04	0.30
$FINDEV_{t-1}$	50.07	61.99	-11.92	0.98	0.33
$OPEN_{t-1}$	66.78	84.96	-18.18	1.04	0.30
$SIZE_{t-1}$	4.37	3.25	1.11	1.60	0.11
$LEVEL_{t-1}$	2.12	2.02	0.10	1.63	0.11
$RESERVE_{t-1}$	0.30	0.52	-0.22	0.79	0.43
$NOMSHK_{t-1}$	10.24	4.99	5.25	1.64	0.10
INF_{t-1}	25.40	12.14	13.27	1.02	0.31

Legend: In this table I compare the inverse propensity score matched mean differences of each regressor using the propensity scores calculated using equation 1 and the R&R exchange rate classification assignments.

Inverse propensity score matching matches each treatment country j to every control country using a Gaussian weighting matrix where the weight placed on the data from a control country k is inversely related to the difference of the propensity scores between the treated country j and the control country k.

Table 2.8: Matched Results:
Characteristics of LY&S De-Facto Flexible and Fixed Exchange Rate Countries in 1996

	A	B	(A-B)	H_0: A-B = 0	
Variable	Treated Mean (n=26)	Control Mean (n=70)	Difference	T-stat	Pr(|T| > |t|)
CA_{t-1}	-2.32	-4.25	1.93	0.88	0.38
CI_{t-1}	0.30	0.11	0.19	0.33	0.74
$FINDEV_{t-1}$	46.65	43.91	2.74	0.22	0.83
$OPEN_{t-1}$	51.36	59.20	-7.84	0.93	0.35
$SIZE_{t-1}$	4.39	2.06	0.10	0.13	0.90
$LEVEL_{t-1}$	2.08	2.06	0.03	0.31	0.76
$RESERVE_{t-1}$	0.20	0.26	-0.06	0.97	0.33
$NOMSHK_{t-1}$	7.28	63.02	-55.75	0.50	0.62
INF_{t-1}	22.48	17.82	4.66	0.52	0.60

Legend: In this table I compare the inverse propensity score matched mean differences of each regressor using the propensity scores calculated using equation 1 and the LY&S exchange rate classification assignments.

Inverse propensity score matching matches each treatment country j to every control country using a Gaussian weighting matrix where the weight placed on the data from a control country k is inversely related to the difference of the propensity scores between the treated country j and the control country k.

Table 2.9: ATT of Float in 1996 on RGDP Per-Capita

Outcome: Float 1996: R&R De-Facto	1997	1998	1999	2000	2001	2002
Inverse Propensity Score Matching (Gaussian Kernel)	0.831*	0.851*	0.827*	0.816*	0.801*	0.777
	(0.473)	(0.475)	(0.479)	(0.482)	(0.481)	(0.485)
Unmatched: Dummy Variable	0.932***	0.922***	0.908***	0.915***	0.905***	0.898***
	(0.369)	(0.371)	(0.372)	(0.375)	(0.374)	(0.375)
Outcome: Float 1996: LYS De-Facto	**1997**	**1998**	**1999**	**2000**	**2001**	**2002**
Inverse Propensity Score Matching (Gaussian Kernel)	0.136	0.163	0.138	0.130	0.108	0.095
	(0.662)	(0.665)	(0.667)	(0.671)	(0.67)	(0.673)
Unmatched: Dummy Variable	0.552	0.560	0.557	0.573	0.561	0.560
	(0.357)	(0.359)	(0.36)	(0.362)	(0.362)	(0.362)

Note: * p<.1, ** p<05, p<.01***; SE in parenthesis

Unmatched: Dummy variable is the unmatched difference of means of the outcomes of interest between the fixed and floating countries.

Inverse propensity score matching matches each treatment country j to every control country using a Gaussian weighting matrix where the weight placed on the data from a control country k is inversely related to the difference of the propensity scores between the treated country j and the control country k.

Table 2.10: ATT of Float in 1996 on RGDP Per-Capita Growth

Outcome: Float 1996: R&R De-Facto	1997	1998	1999	2000	2001	2002
Inverse Propensity Score Matching (Gaussian Kernel)	-0.014	0.017	-0.025***	-0.010	-0.016***	-0.023*
	(0.009)	(0.017)	(0.01)	(0.008)	(0.006)	(0.012)
Unmatched: Dummy Variable	-0.004	.000	-0.014	0.006	-0.010	-0.007
	(0.008)	(0.014)	(0.011)	(0.008)	(0.013)	(0.009)

Outcome: Float 1996: LYS De-Facto	1997	1998	1999	2000	2001	2002
Inverse Propensity Score Matching (Gaussian Kernel)	-0.018*	0.026**	-0.025**	-0.008	-0.022***	-0.013
	(0.01)	(0.012)	(0.011)	(0.009)	(0.008)	(0.014)
Unmatched: Dummy Variable	-0.008	0.013	-0.003	0.016**	-0.012	-0.001
	(0.008)	(0.012)	(0.011)	(0.007)	(0.013)	(0.01)

Note: * p<.1, ** p<05, p<.01***; SE in parenthesis

Unmatched: Dummy Variable is the unmatched difference of means of the outcomes of interest between the fixed and floating countries.

Inverse propensity score matching matches each treatment country j to every control country using a Gaussian weighting matrix where the weight placed on the data from a control country k is inversely related to the difference of the propensity scores between the treated country j and the control country k.

Table 2.11: ATT of Float in 1996 on Inflation

Outcome: Float 1996: R&R De-Facto	1997	1998	1999	2000	2001	2002
Inverse Propensity Score Matching (Gaussian Kernel)	4.082	-6.675	5.018	7.429**	2.193	3.503
	(2.748)	(4.492)	(4.885)	(3.292)	(2.19)	(2.516)
Unmatched: Dummy Variable	0.025	-2.178	2.527	3.271	3.357*	1.647
	(2.827)	(2.68)	(3.191)	(2.682)	(1.731)	(2.255)

Outcome: Float 1996: LYS De-Facto	1997	1998	1999	2000	2001	2002
Inverse Propensity Score Matching (Gaussian Kernel)	6.464	10.888*	2.72	-0.875	1.823	0.369
	(5.676)	(5.997)	(2.848)	(3.832)	(3.39)	(3.069)
Unmatched: Dummy Variable	5.047	8.572**	0.233	-0.659	1.701	0.854
	(4.462)	(4.04)	(2.338)	(2.604)	(2.596)	(2.21)

Note: * p<.1, ** p<05, p<.01***; SE in parenthesis

Unmatched: Dummy Variable is the unmatched difference of means of the outcomes of interest between the fixed and floating countries.

Inverse propensity score matching matches each treatment country j to every control country using a Gaussian weighting matrix where the weight placed on the data from a control country k is inversely related to the difference of the propensity scores between the treated country j and the control country k.

Figures

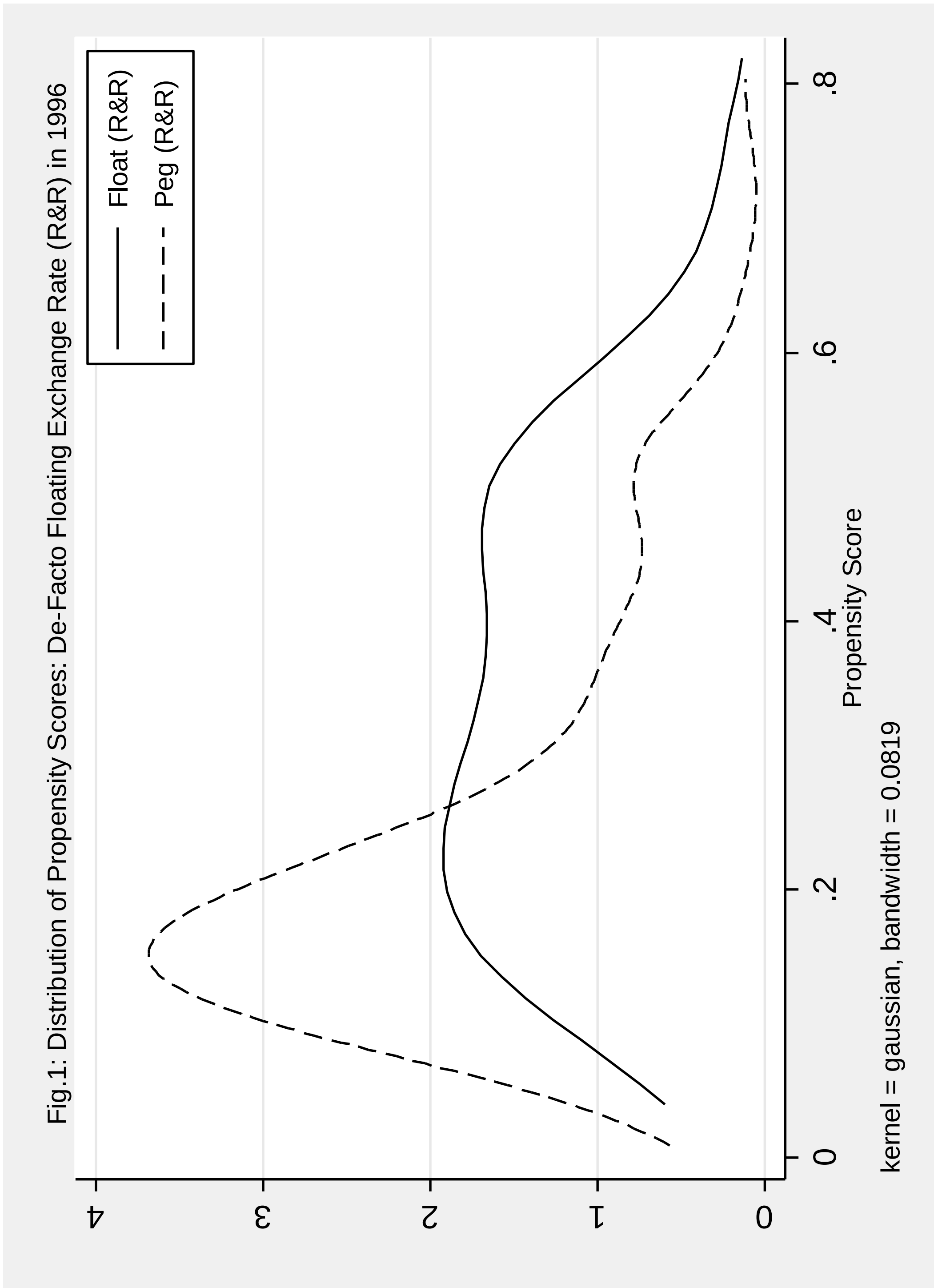
Fig.1: Distribution of Propensity Scores: De-Facto Floating Exchange Rate (R&R) in 1996
Float (R&R)
Peg (R&R)
0
1
2
3
4
0
.2
.4
.6
.8
Propensity Score
kernel = gaussian, bandwidth = 0.0819

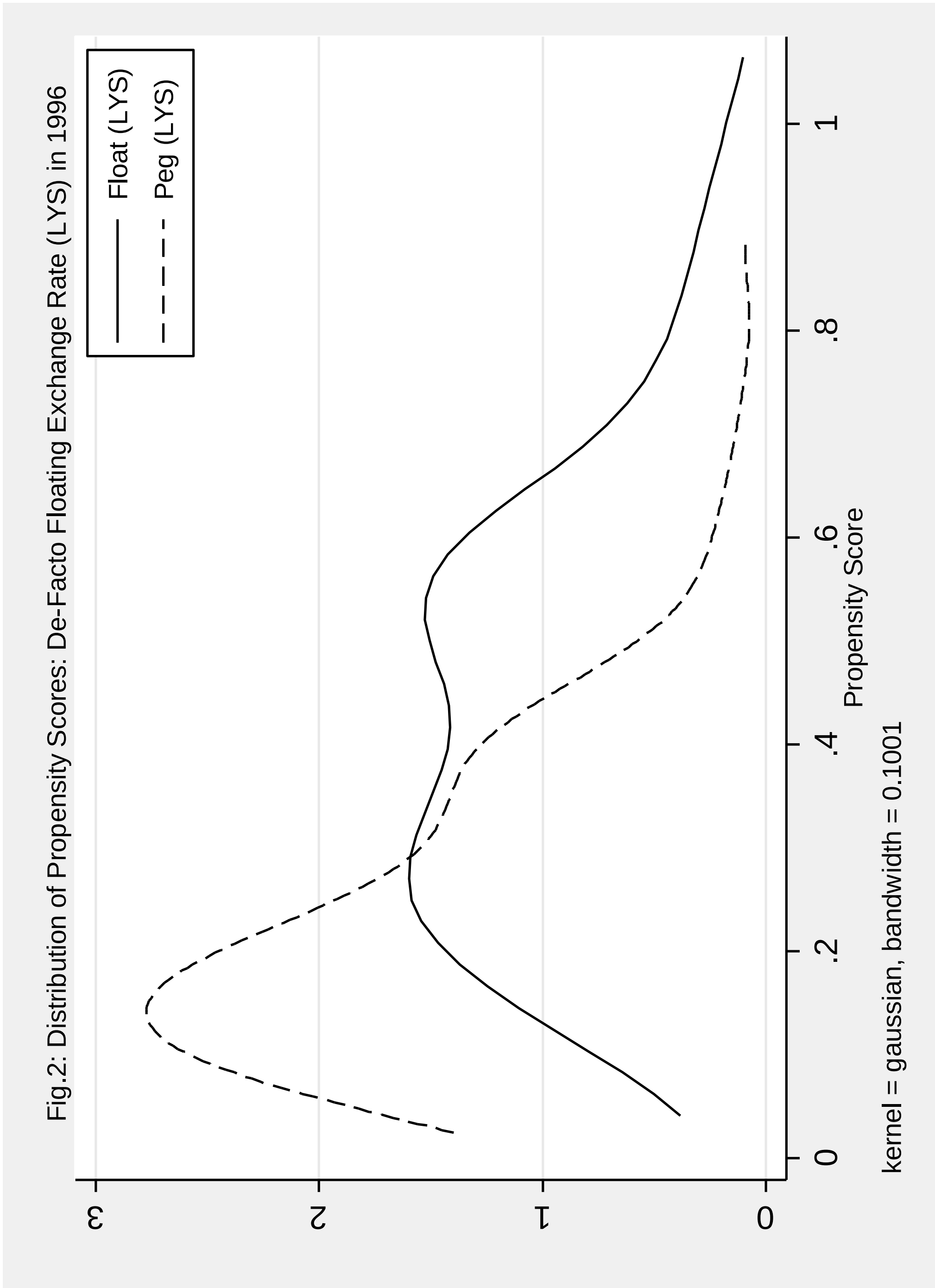

Fig.2: Distribution of Propensity Scores: De-Facto Floating Exchange Rate (LYS) in 1996

Chapter 3:

Breaking the fall: Does Pre-Crisis Exchange Rate Regime Soften the Impact of a Currency Crisis?

Introduction

One major concern for a developing country on a high or steady growth path is a sudden crisis precipitating a growth reversal. While developed countries, such as the United States, tend to return to trend growth, excepting the 2008 financial crisis, most developing countries do not. Therefore, many researchers attempt to explain sudden growth accelerations and post-crisis growth stagnation or decline.[23] With respect to exchange rate regimes as a policy tool, much of the contemporary empirical research on economic development and exchange rate regimes focuses on the effect of exchange rate regimes on long-run trend growth or trend growth volatility.[24,25] However, given that catch-up growth is frequently stymied by economic crises, the relevant question is, "Are there exchange rate policies that help insulates countries from currency crises?"

Research exploring the link between exchange rate regime choice and the likelihood of currency crisis has shown that countries with floating exchange rates or managed floating exchanges are more prone to speculative attacks and currency crises. [26] If fixed exchange rate regimes are associated with a lower frequency or likelihood of a currency crisis, then the next important policy question is, "Conditional on experiencing a currency crisis, does having a fixed exchange rate mitigate the intensity of the crisis?"

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[23] Pritchett (2000); Hausmann, Pritchett, and Rodrik (2005); Jerzmanowski (2006); Cerra & Saxena (2008); Jones & Olken (2008); Eichengreen, Park, and Shin (2011); Berg, Ostry, & Zettelmeyer (2012)

[24] Calvo & Vegh (1992); King & Levine (1993) Svensson (2003); Edwards & Magendzo (2003, 2006); Alesina & Barro (2002); Calvo & Mishkin (2003); Levy-Yeyati & Sturzenegger (2003); Beetsma & Giuliodori (2010); Husain, Mody, and Rogoff (2005); Rose (2011)

[25] Ghosh et al., (1997) find countries with de-jure fixed exchange rates tend to have lower inflation but higher real output volatility, though this result seems only to hold for developing countries with little access to international capital markets. Using de-facto exchange rate regime data, (the Levi-Yeyati & Sturzenegger classification method) Levi-Yeyati & Sturzenegger (2003) conclude, fixed exchange rates are, in emerging market countries, associated with slower growth and higher output volatility. In a review of the literature comparing growth outcomes across multiple de-facto exchange rate classification systems, Andrew Rose (2012) concludes cross country variation in exchange rate regime choices across similar countries does not correspond to statistically significant differences in macroeconomic outcome variables, such as economic growth and inflation.

[26] See Esaka (2010) for specific results and a review of relevant literature.

Despite the importance of this question, only a handful of case-studies have investigated the effect of a country's pre-crisis exchange rate regime on immediate post-crisis outcomes.

These case-studies reveal several common macroeconomic factors influence the intensity of a currency crisis and the speed of recovery. Sachs, *et al.,* (1996) and Tornell (1999) find crisis intensity, defined as the weighted average of the depreciation of the nominal exchange rate with respect to the USD and the percent decrease in foreign reserves, of the Tequila and Asian crises is greater in countries with higher credit expansion and lower foreign reserve levels in the pre-crisis period. Frankel & Rose (1996) show currency crises are more likely to occur when output growth is low, domestic credit growth is high, and foreign interest rates are high. In terms of post-crisis outcomes, Hong & Tornell (2005) examine factors that affect recovery from currency crises in the post-Bretton Woods period and find higher credit expansion before the crisis slows post-crisis GDP growth while greater reserve adequacy before the crisis accelerates recovery. Most of this literature does not explore the role of a country's pre-crisis exchange rate regime.

The few articles that do control for a country's pre-crisis exchange rate regime tend to analyze a specific crisis or crisis period. For example, Grier & Grier (2001) find that having a fixed exchange rate at the onset of the Asian financial crisis failed to limit the extent of devaluation during the 1997 crisis and exacerbated the shock to the stock markets by approximately 40 percentage points. Also investigating the Asian financial crisis, Hallren (2014) concludes that having a fixed exchange rate in 1996 reduced subsequent income growth and weakly increased inflation. Finally, during the recent

2008 financial crisis, countries with flexible exchange rates recovered more quickly but suffered no differential shock to GDP (Tsangarides, 2012).

I contribute to this literature by investigating the effect of a country's pre-crisis exchange rate regime on real income, stock market returns, and inflation in the initial year of and the two subsequent years of the crisis across all currency crises in the post-Bretton Woods period to 2005. Moreover, I only include countries that actually experience a currency crisis, according to an empirical definition. Previous research on this topic tends to analyze a given crisis and investigate the impact across all countries. Therefore, I am investigating factors that affect the intensity of a crisis conditioning on a country being a crisis whereas previous works only condition on being in a general crisis period but include countries in their sample that were unaffected. My approach allows me to isolate the partial effect of pre-crisis exchange rate regime choice on crisis intensity and recovery. Only conditioning on being in a general crisis period means that the counterfactual for countries with pre-crisis pegged exchange rates that actually experience a currency crisis are both countries that did experience the crisis and had a pre-crisis flexible exchange rate, as well as, countries with pre-crisis flexible exchange rates but that did not suffer a currency crisis.[27]

Using data from 1972 to 2005 and a sample of developing countries, I find that countries with fixed exchange rates suffer greater shocks to real income during and immediately following a currency crisis than their counterparts with pre-crisis flexible exchange rates. However, I also find that countries with fixed exchange rates and those with flexible exchange rates the year before a crisis are, on average, different from one

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[27] I am estimating $ATT = E(Y|Treated = 1, Crisis = 1, X) - E(Y|Treated = 0, Crisis = 1, X)$. Previous works estimate $ATT = E(Y|Treated = 1, X) - E(Y|Treated = 0, X)$.

another on factors that affect the outcomes of interest. Once I account for these differences using propensity score matching, I show that having a fixed exchange rate at the onset of a currency crisis does not affect post-crisis real income, stock returns, or inflation relative to comparable counterfactual countries with pre-crisis flexible exchange rates.

The paper proceeds as follows. Section 2 describes the theory, data, estimation, and results from my investigation of the impact of having a fixed exchange rate the year before a crisis on currency depreciation. Section 3 through 5 contains similar sections related to my estimation of the impact of a country's pre-crisis exchange rate regime on stock market returns, real income, and inflation, respectively. Section 6 concludes.

Currency Depreciation & Currency Crises

Theory

The first stage of my analysis is to investigate if having a fixed exchange rate before a currency crisis insulates an economy from major swings in the exchange rate. Since an important goal of establishing a pegged exchange rate is to stabilize the exchange rate at a constant value, it is important to verify a pegged exchange rate is effective at accomplishing this policy goal during a crisis. If countries with pegged exchange rates at the on-set of a crisis experience less currency depreciation during the crisis than floating exchange rate countries with similar economic fundamentals, then the peg is effective at maintaining a stable nominal exchange rate during a crisis. If countries with pegs before a crisis experience no more depreciation than that predicted by fundamentals, then I would conclude that although the peg is not effective at maintaining a constant nominal exchange rate, abandoning the peg does not exacerbate the currency depreciating effect of a currency crisis. If countries with pre-crisis fixed exchange rates suffer greater currency depreciation than countries with pre-crisis floating exchange rates with similar economic fundamentals, then not only is the peg not effective but also breaking the peg compounds the currency depreciating impact of the crisis.

Data/Estimation

I analyze countries that actually experience a currency crisis in the post-Bretton Woods period. To identify if a country suffers a crisis, I utilize three data driven definitions of currency crisis and one for speculative attack. I include in my sample every country-year in which a country experiences a currency crisis by any of the four definitions.[28] To further isolate the sample to developing and emerging market countries, I only include countries with real income of less than $10,000. The crisis data come from Frankel & Rose (1996), Kraay (2003), Hong & Tornell (2005), and Laeven & Valencia (2013). Each author or authors identify crisis years as follows.

Sachs, *et al.* (1996) identify currency crisis via a crisis index that is a weighted average of the percent change in the real exchange rate and the percent change in foreign reserves. To identify a crisis if in two or more quarters of a given year, the crisis index is more than two standard deviations below its meaning. For countries that meet the currency crisis criteria for several continuous years, Hong & Tornell (2005) use the first year of each three-year window to avoid double counting a single crisis.

[28] This pooled approach is necessary to generate a sample size sufficiently large to perform my estimation. Where possible, I did split the sample by definition and found the results to be robust to the type of crisis definition used.

Frankel & Rose (1996) define a currency crisis as a nominal depreciation of the currency vis-a`-vis the U.S. dollar of at least 20 percent that is also at least 10 percentage points higher than the rate of depreciation in the year before. They compute exchange rate depreciation as the percent change of the end-of-period official nominal bilateral dollar exchange rate from the IFS database of the IMF. For countries that meet the currency crisis criteria for several continuous years, Frankel & Rose (1996) use the first year of each three-year window to identify the crisis.

Similar to this definition, Laeven & Valencia (2013) define a currency crisis as a nominal depreciation of the currency vis-a`-vis the U.S. dollar of at least 30 percent that is also at least 10 percentage points higher than the rate of depreciation in the year before. Laeven & Valencia (2013) compute exchange rate depreciation as the percent change of the end-of-period official nominal bilateral dollar exchange rate from the World Economic Outlook (WEO) database of the IMF. For countries that meet the currency crisis criteria for several continuous years, they use the first year of each five-year window to identify the crisis.

Kraay (2003) identifies a successful attack if the monthly devaluation in the nominal exchange rate is higher than 10% (5% for OECD countries) and the average absolute percentage change in the 12 months prior to that month is smaller than 2.5% (1% for OECD countries). He identifies a failed attack as the month in which there is at least a 50% (25% for OECD countries) decrease in non-gold reserves and a 50% (25% for OECD countries) increase in nominal money market interest spreads over the US Federal Funds and is not followed by a large devaluation for at least 3 months. To avoid double-counting prolonged successful (failed) attacks, Kraay eliminates large devaluation events (spikes in reserves and interest spreads) preceded by events in any of the prior 12 months. In my analysis, I include countries that experienced a speculative attack, regardless of the result of the attack.

To sort countries by exchange rate regime, I utilize the Reinhart & Rogoff (2004) (R&R), de-facto classification method. The key parameter of interest in my prediction equation of currency depreciation during the initial year of a currency crisis, in year T, and the two following years is δ. I estimate this equation three times: year T, year T+1, and year T+2.

$$CDep_{i\,T+j} = \alpha_0 + \boldsymbol{X'_{iT-1}\beta} + \delta Peg_{iT-1} + \varepsilon_{it}\ \forall j \in [0,2] \qquad (3.1)$$

In this equation, $\boldsymbol{X_{it-1}}$ are macroeconomic variables that help predict currency depreciation. Previous papers in this literature offer clear guidance on relevant predictors of currency depreciation.[29] Based on these insights, I include one year lagged values of currency depreciation, current account balance to GDP, external debt to GDP, M2 to foreign reserves, the five year percent change of the ratio claims on the private sector of deposit money banks to GDP, and the five year percent change in the real exchange rate. The five year percent change in private lending controls for a boom in private lending in the immediate pre-crisis period. Data for these variables come from the IMF's IFS database, except for the real exchange rate data. The real exchange rate data come from the USDA's trade weighted real exchange rate database.

Peg is a dummy variable that equals 1 if a country has a fixed exchange rate the year before it suffers a currency crisis. Therefore, the parameter δ is the average

[29] Furman & Stiglitz (1998), Kaminsky, *et al*. (1998), Radelet & Sachs (1998), Corsetti, *et al.*(1999), Tornell (1999) , Grier & Grier (2001), Rose & Spiegel (2011), Frankel & Saravelos (2012), Tsangarides (2012)

treatment effect on the treated (ATT) of having a fixed exchange rate immediately preceding the onset of a currency crisis.[30]

I estimate equation (1) via ordinary least squares with robust standard errors. This framework will generate an unbiased estimate of the ATT of having a fixed exchange rate the year before a crisis if, on average, the treated and control group countries are observationally similar across determinants of the outcome of interest. As a method of checking the assumption of endogenous treatment selection, I utilize propensity score matching to identify control group countries that are, on average, observationally equivalent to treated group countries on all determinants of our outcomes of interest, except for treatment status.

To do this I estimate a probit equation using the determinants of currency depreciation with *Peg* as the dependent variable. I then use the propensity scores from each equation and match treated and control group countries that are, on average, similar on factors that affect our outcomes of interest. The average of the differences for each of the outcomes of interest across all matched pairs is an unbiased estimate of the ATT of having a fixed exchange rate regime, conditional on the determinants (Rubin, 1974; Rosenbaum & Rubin, 1983 & 1985; Heckman, *et al.*, 1998; Dehejia, 2005). One must remember we are not formally modelling the decision to adopt a pegged exchange

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[30] Readers may worry about slippage or movement between the treatment and control group after the onset of a crisis. Because this paper investigate the effects of a country's choice of exchange rate regime leading into a crisis it is unnecessary for me to account for treatment assignment slippage during the crisis.

If, for example, a country fixed in 1996 and due to the crisis had to break the peg, which resulted in a higher interest rates or capital flight and thus lower post-crisis period economic growth, vis-à-vis a 1996-pegged country, then my paper would attribute this result to the country's exchange rate regime choice in 1996. From the standpoint of my framework, any actions a floating exchange rate regime country is forced to take after 1996 that affects currency depreciation or stock market returns is part of the ATT of selecting a floating exchange rate in 1996.

rate. Rather we are using the probit model to match countries that are similar on determinants of currency depreciation.

Results

Table 3.1 shows the summary statistics by pre-crisis exchange rate regime type. The table illustrates that fixed and floating exchange rate regime countries are different from one another on M2 to foreign reserves at the 10% significance level and pre-crisis currency depreciation at the 35% p-level. Table 3.2 presents the estimates of the treatment effect. Columns (1), (3), and (5) are the OLS estimates and columns (2), (4), and (6) are the non-parametric, with respect to currency depreciation, matching estimates. The OLS estimates indicate having a fixed exchange rate before a currency crisis has not effect on currency depreciation during or following the crisis, except in the second year following the crisis. In this year, countries with pre-crisis fixed exchange rate countries experience currency appreciation of 6.53% over what is predicted by relevant macroeconomic fundamentals. The propensity score matching (PSM) results, by contrast, are not statistically different from zero in any year. This shows the importance of accounting for average differences in the determinants across the treated and control group. My results indicate that countries with pegged exchange rates before a currency suffer the same amount of currency depreciation as countries with pre-crisis flexible exchange rates and similar economic fundamentals. This null result does not mean that having pegged results has no effect on currency depreciation. Rather these results mean that pegged exchange rates are no better than flexible exchange rates at maintaining stable nominal exchange rates. To the extent that governments adopt pegs to keep nominal exchange rates fixed, these results indicate pegs are not effective.

Currency Depreciation & Stock Market Returns

Theory

I analyze the effect of currency depreciation on stock market returns in the first year of a currency crisis. Following the examples of Gallinger (1994), Poterba & Samwick (1995), Leigh (1997), and Grier & Grier (2001), I utilize stock returns as a leading indicator of real GDP growth. In so far as markets are forward looking and reflect current firm profitability, it is reasonable to assume that equity market returns are a good proxy of future economic performance.

Grier & Grier (2001) offer several insights into how currency depreciation might impact the real economy. Currency depreciation could impact equity returns in one of three ways. First, if firms are fully hedged against swings in the exchange rate, then currency depreciation would have little impact on firms' profitability and thus no significant effect on equity returns. If the Marshall-Lerner conditions are satisfied, then a nominal depreciation of the exchange rate would improve a country's trade balance, increase GDP growth, and thereby positively affect stock market returns.

If a country suffers an unexpected and uncontrolled depreciation of the exchange rate, firms and/or the central government are highly indebted in foreign currency denominated loans, and firms are insufficiently hedged against swings in the exchange rate, then the real increase in the debt burden resulting from the depreciation in the exchange rate will negatively impact government balance sheets and firm's profitability. The latter will directly depress stock market returns. Moreover, if the rise in the real debt burden prompts a decline in investor confidence and capital flight, then the economy further suffers rising interest rates and additional currency depreciation (Dornbusch, *et al.*,1995; Chang & Velasco, 2000; and Aghion, *etal.*, 2001). This scenario is mostly likely when a country has a fixed exchange rate at the onset of the crisis, a real exchange rate that is over-valued, and a high degree of foreign currency denominated external debt (Krugman, 1979; Obsfeld, 1986; Mishkin, 1999).

Data/Estimation

I estimate equation (2) where the parameter δ measures the ATT of pre-crisis pegged exchange rate on stock returns during a currency crisis.

$$RET_{i\,T+j} = \beta_0 + \delta Peg_{iT-1} + \beta_1 RET_{iT-1} + \beta_2 CDep_{iT-1} + \varepsilon_{it}\ \forall j \in [0,2] \quad (3.2)$$

The findings of Grier & Grier (2001) motivate the selection of explanatory variables as their study of the effect of pegged exchange rates on equity returns during the Asian financial crisis is the most comparable work to this general case. The annual stock return data (RET) come from Nicholas Bloom of Stanford University and cover 60 countries (Baker & Bloom (2013)).[31] I augment the Bloom data with stock return data for Singapore and Hong Kong from Yahoo Finance. As before, I use currency depreciation data from the IMF's IFS database, the R&R exchange rate classification method, and the crisis year definitions described previously.

I estimate this equation using OLS with heteroskedastic robust standard. The fixed and floating exchange rate countries are on average similar across both determinants: I

[31] The Bloom data is quarter-on-quarter returns. Through a conversion from the Canadian Statistics Bureau, I change the Bloom data into year-on-year percent change data. Details are available in an online appendix.

failed to reject the null of a mean difference in the determinants between the fixed and floating countries at the .4 p-level (table 3.3). However, I am able to improve covariate balance via matching, and therefore, do perform the robustness check of estimating the treatment effect via propensity score matching.

Results

Overall, the results show having a pegged exchange rate at the onset of a currency crisis has no differential effect on stock returns during the initial year of the crisis or the two subsequent years. (See table 3.4.) The one exception is that the PSM estimate in the second year following the crisis shows having a fixed exchange rate before the crisis caused equity returns to follow 15% more than for similar pre-crisis flexible exchange rate countries. Nevertheless, the results indicate that conditional on equity returns and currency depreciation before the crisis, the peg neither exacerbated nor diminished the effect of the crisis on stock market returns. In so much as equity returns are a leading indicator, these results suggest a pegged exchange rate neither hurts nor helps a country recover from a currency crisis.

This is an interesting result for two reasons. First, although Grier & Grier (2001) show pre-crisis pegged exchange rates exacerbated the impact of the 1997 Asian crisis on equity markets, I find that, in-general, this result does not hold when we consider all currency crises from 1972 to 2005 and only countries that actually experienced a crisis, rather than analyzing all countries during a specific crisis period. Second, whereas papers that analyze the effect of exchange rate regimes on long-term growth in the post-Bretton Woods period tend to find that countries with hard pegs growth mores slowly, my results indicate that having a peg before a currency crisis does not impede a country's economic performance.

Currency Crisis & Real Income

Theory

Previous empirical work on pre-crisis exchange rate regime and recovery of the real economy tend to show that fixed exchange rate countries recover more slowly than countries with flexible exchange rates that suffer the same crisis (Calvo and Miskin, 2003; Domac and Peria, 2003; Edwards and Levy-Yeyati, 2005; Levy-Yeyati and Sturzenegger, 2005; and Tsangarides, 2012). However, the most comparable article to this paper, Hallren (2014), provides evidence that countries with a fixed exchange rate before the Asian Financial Crisis do not suffer a greater shock to real income than comparable countries with a pre-crisis flexible exchange rate. Moreover, he finds pre-crisis pegged countries recover more quickly than pre-crisis floating exchange rate countries. The results deviate from previous research on the subject because Hallren (2014) allows for non-random sorting of countries between exchange rate regimes.

Here I utilize the same model as in Hallren (2014) to test if the results are robust to isolating the sample only to countries that actually experience a currency crisis rather than investigating economic performance of all countries around the time of a given crisis.

Data/Estimation

I estimate equation (3) where the parameter δ measures the ATT of pre-crisis pegged exchange rate on real income during a currency crisis.

$$RealY_{i\,T+j} = \alpha_0 + \boldsymbol{X}'_{\boldsymbol{iT-1}}\boldsymbol{\beta} + \delta Peg_{iT-1} + \varepsilon_{it} \ \forall j \in [0,2] \qquad (3.3)$$

The matrix (***X***) includes standard macroeconomic determinants of real income used in the literature to estimate the effect of exchange rate regime on real income.[32] These variables include: CA, a country's current account balance to GDP; financial development (FINDEV), M2 to GDP; trade openness (OPEN); the natural log of real GDP in billions of USD (SIZE), which captures the size of a country's economy; natural log of real GDP per-capita (LEVEL), a proxy for the country's standard of living; reserves to GDP (RESERVE); inflation (INF); and the deviation of M2 growth from the four year moving average of M2 growth (NOMSHK). I draw the data from the World Bank's WDI and IMF's IFS databases. I sort countries by their pre-crisis R&R exchange rate regime classification and use the crisis-year definitions described in section 2.

Results

I present summary statistics of the determinants of real-income in Table 3.5. As table 3.5 illustrates, the fixed and floating exchange rate countries differ from on another at the 5% level on one determinant and at the 30% level on six determinants. This indicates non-random sorting of countries across exchange rate type.

[32] Mundell (1961), Holden, *et al*. (1979), Krugman (1979), Melvin (1985), Edwards (1996); Alberto and Barro (2002), Edwards and Magendzo (2003) & (2006), Van Hagen and Zhou (2007), and Carmignani, *et al*. (2008)

I estimate equation (3) using OLS with heteroskedastic robust standard (Table 3.6). The estimates indicate the effect of having a pegged exchange rate the year before a currency crisis increased the shock to real-income by almost $500 per year. However, once I account for covariate imbalance via propensity score matching and compare countries that are observationally similar, I find the effect to be no different from zero. Moreover, the OLS and matching estimates of the treatment effect are statistically different from each other.

Currency Crisis & Inflation

Theory

A typical policy objective cited when adopting a fixed exchange rate is price stability. When high inflation is the result of accommodative monetary policy and profligate government spending, a fixed exchange rate serves as a nominal anchor to curb these drivers of inflation, though a review of the literature by Rose (2011) indicates no long-run inflation abatement effect of having a pegged exchange rate. Moreover, there is always concern the fixed exchange rate will become over-valued, lead to an uncontrolled devaluation, and result in a sudden rise in the price of imported goods. If imported goods represent a large portion of the domestic consumption basket, then a collapse of the fixed exchange rate will increase domestic inflation.

Data/Estimation

To estimate the effect of a pre-crisis pegged exchange rate on inflation in the immediate post-crisis period, I estimate equation (4) where the parameter δ measures the ATT of pre-crisis pegged exchange rate on inflation during and after a currency crisis.

$$INF_{i\,T+j} = \alpha_0 + \boldsymbol{X}'_{\boldsymbol{iT-1}}\boldsymbol{\beta} + \delta Peg_{iT-1} + \varepsilon_{it}\ \forall j \in [0,2] \qquad (3.4)$$

The matrix ($\boldsymbol{X}$) includes standard macroeconomic determinants of inflation: financial development (FINDEV), M2 to GDP; trade openness (OPEN); reserves to GDP (RESERVE); lagged inflation (INF); M2 growth; and the deviation of M2 growth from the four year moving average of M2 growth (NOMSHK). I draw the data from the World Bank's WDI and IMF's IFS databases. I sort countries by their pre-crisis R&R exchange rate regime classification and use the crisis-year definitions described in section 2.

Results

Table 3.7 gives summary statistics of the determinants of inflation. Table 3.7 shows the fixed and floating exchange rate countries differ from on another at the 5% level on one determinant and at the 30% level on three determinants. This indicates non-random sorting of countries across exchange rate type.

Despite this non-random sorting, both the OLS (columns (1), (3), and (5)) and PSM estimates of the average treatment, of having a fixed exchange rate before a crisis on inflation during and immediately following a crisis, are not statistically significant at the 5% level. (See table 3.8.) However, the OLS estimates do show that the effect of having an exchange rate before a crisis increased inflation by 6.44% at the on-set of the crisis, though this is significant only at the 10% level. The two sets of estimates, OLS vis-à-vis

PSM, are statistically different from one another, indicating the importance of accounting for non-random sorting of countries between exchange rate regime groups.

Overall, my analysis indicates that having a fixed exchange rate does not help countries better control their prices than a flexible exchange during or immediately following a currency crisis. Given this is often one of the stated benefits of a fixed exchange rate, this section provides evidence against such a policy.

Conclusion

Using data on emerging market and developing countries, I analyze the effect of having a fixed exchange rate at the onset of a currency crisis on currency depreciation, equity returns, real income, and inflation the year of the crisis and the two years following the crisis. This paper contributes to the literature by considering all currency crises from 1972 to 2005, rather than using a case study approach. Moreover, I focus my analysis only on countries that actually experienced a currency crisis or speculative attack. Consequently, my results are generalizable across all crises and better isolate the ATT of having a peg before a crisis on a country's performance during the crisis.

I find a country's pre-crisis exchange rate regime had no impact on currency depreciation during the first year of a crisis. This result is robust to the exchange rate regime classification and crisis definition used. This confirms the Grier & Grier (2001) results that once we control for key macroeconomic fundamentals, a country's exchange rate has no additional effect on currency depreciation. This null result does not mean that having a peg has no effect on currency depreciation. Rather these results mean that pegged exchange rates are no better than flexible exchange rates at maintaining stable nominal exchange rates.

Additionally, I, in general, find no evidence that a pre-crisis fixed exchange rate caused, on average, a greater shock to equity returns than similar pre-crisis flexible exchange rate countries. This result too is mostly robust across crisis definitions and exchange rate regime classifications. This finding is in sharp contrast to the conclusions of Grier & Grier (2001) but is likely driven by differences in the samples of countries analyzed.

The effect on real income is slightly less clear. The parametric estimates indicate that having a fixed exchange rate before a currency crisis exacerbates the shock of the crisis on real income and that this differential impact between pre-crisis fixed and flexible exchange rate countries persists for at least two years. However, once I control for non-random sorting of countries between exchange rate regime types, this effect disappears.

Finally, a fixed exchange rate regime before a crisis seems to do no better of a job at controlling prices during the crisis than a flexible exchange rate, once we control for relevant determinants of inflation.

Ultimately, I conclude that a having a pegged exchange rate before a currency crisis does not insulate the economy from the shock of the crisis or accelerate its recovery. When considered in light of related research that analyzes the effect of exchange rate regime choice and long-run growth, this paper suggests that flexible exchange rates are a better policy option for emerging market and developing countries.

Tables

Table 3.1:
Summary Statistics: Predictors of Currency Depreciation

Variable	(A) Treated	(B) Untreated	(A) - (B)	S.E.	T-stat	Prob(|T|>t)
Currency Depr.$_{T-1}$	5.59	3.11	2.48	2.59	0.96	0.34
CA $_{T-1}$	-4.59	-4.44	-0.15	1.19	-0.13	0.90
Ext. Debt $_{T-1}$	64.44	68.01	-3.57	7.96	-0.45	0.65
M2/RES$_{T-1}$	21.82	7.17	14.64	8.85	1.65	0.10
Lending Boom $_{T-1}$	26.22	31.62	-5.40	11.94	-0.45	0.65
Real Appr. $_{T-1}$	9.31	5.99	3.32	6.75	0.49	0.63
N	112	(A) Treated	67	(B) Untreated	45	

Table 3.2:

Currency Crisis: Effect of Pre-Crisis Exchange Rate Regime on Currency Depreciation

	Crisis (T)		Crisis (T+1)		Crisis (T+2)	
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Peg(R&R)	-3.2	-2.01	-0.77	-2.43	-6.53*	-0.59
	(4.59)	(3.98)	(1.44)	(2.31)	(3.87)	(5.16)
Regressors (T-1)	X	X	X	X	X	X
Constant	X	X	X	X	X	X
N	134	112	134	112	134	112

Notes: OLS is an ordinary least squares regression with robust standard errors. Match is the matching estimate of the treatment effect using a probit with the listed covariates to calculate the propensity score for treatment. (2), (4), and (6) use .01 radius match. In (1) ~ (6) countries are sorted by de-facto R&R exchange rate regime classification**.**

Table 3.3:

Summary Statistics: Predictors of Stock Returns

Variable	(A) Treated	(B) Untreated	(A) - (B)	S.E.	T-stat	Prob(|T|>t)
RET $_{T-1}$	2.52	12.38	-9.86	12.44	-0.79	0.43
CDep $_{T-1}$	4.05	2.30	1.76	3.78	0.46	0.65
N	43	(A) Treated	34	(B) Untreated	9	

Table 3.4:

Currency Crisis: Effect of Pre-Crisis Exchange Rate Regime on Stock Returns

	Crisis (T)		Crisis (T+1)		Crisis (T+2)	
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Peg $(R\&R)_{T-1}$	2.17	-1.06	-15.62	-24.64	-4.96	-15*
	(12.24)	(12.84)	(11.7)	(15.1)	(7.59)	(8.36)
$Regressors_{T-1}$	X	X	X	X	X	X
Constant	X	X	X	X	X	X
N	43	43	177	43	177	43

Notes: OLS is an ordinary least squares regression with robust standard errors. Match is the matching estimate of the treatment effect using a probit with the listed covariates to calculate the propensity score for treatment. (2), (4), and (6) use normal Gaussian kernel. In (1) ~ (6) countries are sorted by de-facto R&R exchange rate regime classification.

Table 3.5:

Summary Statistics: Predictors of Real Income

Variable	(A) Treated	(B) Untreated	(A) - (B)	S.E.	T-stat	Prob(|T|>t)
CA_{T-1}	-0.05	-0.03	-0.01	0.01	-1.12	0.26
$FINDEV_{T-1}$	39.52	38.17	1.35	4.02	0.34	0.73
$OPEN_{T-1}$	66.88	54.08	12.80	5.79	2.21	0.03
$SIZE_{T-1}$	2.41	2.79	-0.37	0.30	-1.26	0.21
$LEVEL_{T-1}$	7.01	6.77	0.24	0.16	1.57	0.12
$RESERVE_{T-1}$	0.32	0.23	0.08	0.08	1.09	0.28
$NOMSHK_{T-1}$	-64.24	0.16	-64.40	56.50	-1.14	0.26
INF_{T-1}	54.43	15.44	38.98	53.60	0.73	0.47
N	177	(A) Treated	115	(B) Untreated	62	

Table 3.6:

Currency Crisis: Effect of Pre-Crisis Exchange Rate Regime on Real Income

	Crisis (T)		Crisis (T+1)		Crisis (T+2)	
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Peg $(R\&R)_{T-1}$	-484.87***	-398.54	-487.04***	-415.14	-453.36**	-410.43
	(176.4)	(462.27)	(181.69)	(470.46)	(189.7)	(491.08)
$Regressors_{T-1}$	X	X	X	X	X	X
Constant	X	X	X	X	X	X
N	177	177	177	177	177	177

Notes: OLS is an ordinary least squares regression with robust standard errors. Match is the matching estimate of the treatment effect using a probit with the listed covariates to calculate the propensity score for treatment. (2), (4), and (6) use normal Gaussian kernel. In (1) ~ (6) countries are sorted by de-facto R&R exchange rate regime classification.

Table 3.7:
Summary Statistics: Predictors of Inflation

Variable	(A) Treated	(B) Untreated	(A) - (B)	S.E.	T-stat	Prob(|T|>t)
$FINDEV_{T-1}$	39.56	37.37	2.19	3.91	0.56	0.58
$OPEN_{T-1}$	65.51	54.31	11.20	5.52	2.03	0.04
$RESERVE_{T-1}$	0.31	0.23	0.08	0.07	1.06	0.29
$NOMSHK_{T-1}$	-60.59	-0.33	-60.26	52.78	-1.14	0.26
M2 $Growth_{T-1}$	33.31	21.97	11.34	17.00	0.67	0.50
INF_{T-1}	52.14	14.92	37.22	50.06	0.74	0.46
N	189	(A) Treated	122	(B) Untreated	67	

Table 3.8:
Currency Crisis: Effect of Pre-Crisis Exchange Rate Regime on Inflation

	Crisis (T)		Crisis (T+1)		Crisis (T+2)	
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Peg (R&R)$_{T-1}$	6.44*	4.71	2.11	1.23	3.25	1.76
	(3.49)	(4.2)	(3.52)	(4.7)	(4.78)	(4.56)
Regressors$_{T-1}$	X	X	X	X	X	X
Constant	X	X	X	X	X	X
N	189	189	189	189	189	189

Notes: OLS is an ordinary least squares regression with robust standard errors. Match is the matching estimate of the treatment effect using a probit with the listed covariates to calculate the propensity score for treatment. (2), (4), and (6) use normal Gaussian kernel. In (1) ~ (6) countries are sorted by de-facto R&R exchange rate regime classification.

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