Shifting from the Salsa to the Waltz:

The Consequences of Policy Responses During Global Economic Booms

Kristin J. Forbes –MIT-Sloan School of Management and NBER

Michael W. Klein—Tufts-Fletcher School and NBER

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In 2007, Chuck Prince (the CEO of Citigroup at the time) famously remarked, "When the music stops…things will be complicated. But as long as the music is plying, you've got to get up and dance"….. This quote summarizes the challenge facing emerging markets during boom times—periods of large net capital inflows from abroad, strong global growth, and low levels of risk aversion. They can share in the global boom and benefit from large inflows of inexpensive capital. If the capital is invested inefficiently, asset bubbles develop, and/or the economy is not positioned to handle a sudden stop in capital inflows, however, the inevitable end to the boom period can lead to sharp recession and financial crises. Are there any policies that emerging markets can adopt during the boom period so that they can still partake in the "dance" but avoid the "complications" when the dance ends? Is there a safer way to dance—a shift from a salsa to a waltz?

This paper evaluates different policy responses to the surge in capital inflows during the most recent boom period of 2002-2007. It focuses on five different strategies—raising interest rates (to reduce inflation, bubbles and credit growth), accumulating reserves (to reduce exchange rate appreciation and support export competitiveness), allowing exchange rate appreciation (to simply absorb the inflows), increasing capital controls (focusing on temporary controls or "gates" targeting short-term inflows), and increasing macroprudential measures (focusing on reducing certain aspects of credit growth and aggregate financial vulnerabilities). The first section of the paper documents the incidence of these different types of policy responses across countries to better understand the extent to which each policy was used as well as which combinations (if any) of these policies were popular. It will build on new datasets on capital controls and prudential measures constructed in Klein (2012) and Forbes, Fratzscher and Straub (2013)—which will both be extended for this project.

The second section of the paper will then examine which factors affect the probability that each country adopts each of the four policy responses to crises. More specifically, it will estimate a

logit model of the probability that a country increases interest rates, accumulates reserves, allows exchange rate appreciation, increases capital controls, or increases macroprudential measures as a function of a variety of global characteristics (global risk, interest rates in major economies, commodity prices, etc.), domestic variables (gdp growth, credit growth, inflation, capital inflows, fiscal position, institutional strength, etc.), and other recent policy changes in the country (such as changes in interest rates, reserves, the exchange rate, etc). These estimates will be useful to better understand why different countries adopt such different responses during booms, as well as to use in the next section of the paper in order to better understand the effect of these different policy choices.

The next section of the paper then uses a propensity-score methodology to estimate the impact of these five different policy responses during booms on key economic variables during the recent global financial crisis. Propensity-score methodology has been widely used in the labor literature, but is only recently starting to be applied to international macroeconomic questions.[1] This methodology uses the estimates from the logit models (discussed above) to estimate the "propensity score" showing the probability that each country adopts each type of policy during the boom period. Then it uses the estimated propensity scores to match each type of policy response (each treatment) to a similar control group of countries. This matching is important in order to create a counterfactual of what would have happened in different countries if they had not adopted the specific policy (or treatment). This methodology is particularly useful to address problems of selection bias that limits standard studies estimating the effect of different policies. Countries that adopt specific policies (such as capital controls) are fundamentally different than other countries that do not adopt these policies—and therefore would be expected to be differentially affected during the global financial crisis. Propensity-score matching allows us to better control for this selection bias than is possible in standard regression analysis.

The paper uses several different techniques to "match" countries that adopt different policies to a control group—nearest-neighbor matching, fiver-nearest neighbor matching, local-linear matching, and possibly kernel matching or radius matching. Each technique has different advantages and disadvantages and using several methodologies is useful to check the robustness of the results. Then we use the matched control groups to estimate the impact of the different policy responses on variables such as GDP growth, unemployment, inflation, number of banking collapses, and other variables during the global financial crisis. These estimates should allow us to evaluate if countries which increased interest rates, accumulated reserves, allowed exchange rate appreciation, increased temporary capital controls and/or increased macroprudential measures during the crisis had better (or worse) economic performance during the Global Financial Crisis.

[1] See Angrist and Pischke (2008), chp. 3 for an excellent discussion of this methodology. Also see Glick, Guo and Hutchison (2006), Das and Bergstrom (2012), Forbes, Fratzscher and Straub (2013), and Forbes and Klein (2013) for applications in the international macroeconomic literature.

The structure and framework of this paper will closely follow the structure of Forbes and Klein's (2013) draft of "Pick Your Poison: The Choices and Consequences of Policy Responses to Crises"—which is also included with this proposal. In fact, these two papers are intended to be compliments to each other. In "Pick Your Poison", the authors examine the immediate effects of different policy responses during crises—when capital flows dry up—such as of depleting reserves, allowing currency depreciation, and reducing controls on capital inflows (or increasing them on capital outflows). In this proposal for a new paper, we hope to examine the longer-term effects of different policy responses during booms—when capital flows are abundant—to see if these different responses can lead to better economic outcomes when the "music stops" and the boom ends.

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Pick Your Poison:

The Choices and Consequences of Policy Responses to Crises*

Kristin J. Forbes –MIT-Sloan School of Management and NBER

Michael W. Klein—Tufts-Fletcher School and NBER

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VERY PRELIMINARY DRAFT—COMMENTS APPRECIATED

Abstract: Countries chose different strategies to respond to crises. These choices have consequences. To assess the impact of these choices on key variables, however, it is necessary to control for selection bias and the factors determining the policy choices. This paper uses propensity-score matching to estimate the effects of major reserve sales, large currency depreciations, substantial changes in policy interest rates, and adjustments of capital controls on real GDP growth, unemployment, and inflation during two periods marked by crises, 1997 to 2001 and 2007 to 2011. We find that large currency depreciations significantly raise GDP growth, but the effect is lagged and significantly increases inflation. The other policies do not generate inflation, but also do not significantly improve growth. Substantial interest rate increases significantly raise unemployment. During the recent Global Financial Crisis, countries were more likely to chose currency depreciations and less likely to raise interest rates or adjust capital controls than during the earlier crises in the late 1990s'.

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I. Introduction

In the mid- and late-1990's a series of emerging markets were faced with a sudden stop of capital inflows and severe financial crises. The standard prescribed policy response involved sharp increases in interest rates and large currency depreciations. Countries that followed this prescription generally had severe recessions and increased unemployment, at least in the immediate wake of the crisis. This experience caused many countries subsequently to take steps to attempt to avoid such severe policies during the next crisis. These included accumulating international reserves, experimenting with capital controls, and developing currency swap arrangements. When that next crisis hit in 2008, there were a range of policy responses and, in many cases, countries avoided substantial interest rate increases. Any policy response to a crisis, however, likely includes some negative effects, and, consequently countries must "pick their poison".

This paper analyzes the causes and consequences of different policy responses to contractions in international capital flows. It focuses on two crisis windows: from 1997 through 2001 (which includes the crises in Asia, Russia, Brazil, Argentina and Turkey) and from 2007 through 2011 (which includes the Global Financial Crisis (GFC) and the Euro crisis). It concentrates on four policy responses: major reserve sales, large currency depreciations, substantial interest rate increases, and capital control adjustments. These are nimble policies that can quickly be adopted in response to sudden stops in capital flows.[1] We document patterns in the use of these four policy responses during the two crisis periods and show that countries were more likely to sharply raise interest rates and adjust capital controls during the 1990's crisis

[1] This roughly corresponds to the tradeoffs countries face in the policy trilemma, as discussed in Obstfeld, Shambaugh and Taylor (2010), Dominguez (2013) and Klein and Shambaugh (2013). As with the policy trilemma, we do not consider fiscal policy responses since these are less nimble and generally take longer to implement. Also, fiscal policy responses to crises are in the focus of other papers prepared for this conference, such as Chari and Henry (2013).

period, and more likely to have large depreciations during the 2007 to 2011 period. Then we estimate what factors determine a country's choice of policies and test the efficacy of these policies in terms of their impact on key outcome variables: GDP growth, inflation, and unemployment.

The analysis in this paper uses a propensity-score matching methodology. This technique is common in some areas of economic analysis (such as the labor literature), but used only rarely in international macroeconomics.[2] It is well-suited for our analysis because it allows us to construct a counterfactual using a single propensity score to select relevant observations to compare countries which use a certain policy (a treatment group) with countries that do not (a control group), as developed in Rosenbaum and Rubin (1985).[3]

In the first stage of this propensity-score matching methodology, we estimate the probability that a country undertakes each of the four policy responses as a function of global variables, domestic vulnerabilities, other country characteristics, and other recent policy decisions. The model has a high degree of explanatory power and most coefficients have the expected signs. The estimates also indicate that after controlling for this range of global and country-specific variables, countries were significantly more likely to increase interest rates, adjust capital controls, and sell reserves during the 1990's crises than the 2000s crises.

In the next stage of the analysis, we use these estimates of the determinants of a country's policy responses to estimate propensity scores and match countries which adopted each policy to a control group. We focus on three matching algorithms (nearest neighbor without replacement, five-nearest neighbors, and local-linear) and present a series of tests showing the efficacy of

[2] Three exceptions are: Glick, Guo and Hutchinson, 2006, Das and Bergstrom, 2012, and Forbes, Fratzscher and Straub, 2013.

[3] In Section IV.A, we discuss the differences between the propensity score matching methodology to the standard multivariate regression analysis.

these matching algorithms. Then we use the matched sets of countries to estimate the average-treatment-effect-on-the-treated, the statistic that shows the effect of the policy while controlling for sample selection bias.

The estimates suggest that a large currency depreciation is the only policy response which generates a significant increase in GDP growth relative to the counterfactual–although the positive effect does not occur until the 3rd quarter after the depreciation and the initial effect may be a contraction in growth. A sharp currency depreciation, however, is also the only policy that generates a significant increase in inflation. Reserve sales do not generate any increase in inflation (and may decrease inflation, although the effect is insignificant), but have no significant benefits in terms of increasing growth or reducing unemployment, especially over several quarters. Substantial increases in interest rates appears to have particularly negative effects, as they generate a significant and economically meaningful increase in unemployment, a negative (although insignificant) effect on growth, and no significant reduction in inflation. [Results from adjustments in capital controls are still in process.]

The analysis in this paper suggests that none of these policy responses offer unalloyed positive outcomes for all three of these key measures of economic performance. Countries must, therefore, respond to crises by "picking their poison". The different patterns in policy responses during the more recent crises of the late-2000s relative to the 1990's window, however, suggests that countries have recently preferred currency depreciations in order to support growth, despite the "poison" of a higher risk of inflation.

II. Policy Responses During Crises

Countries can, and do, deal with economic crises using a variety of policy tools.

In this section we document the policy choices undertaken in our sample of 85 countries for two four-year periods which include worldwide crises, 1997 – 2001 and 2007 – 2011. To simplify terminology, we will refer to the earlier period as the 1990's crises and the later window as the 2000's crises. We focus on policies involving major sales in foreign exchange reserves, sharp depreciations of the nominal exchange rate, substantial increases in interest rates, and adjustments in capital controls.

These are four of the primary policy responses that a government might quickly undertake when faced with different constellations of exogenous shocks. For example, as described in Blanchard, Das and Faruqee (2010), an increase in perceived risk that reduces capital flows leads to a depreciation and a likely decrease in output through balance sheet effects, and the policy efforts to forestall that depreciation could include selling reserves, raising interest rates, or imposing controls on capital outflows. Likewise, the response to a decrease in foreign output, which also could otherwise lead to a depreciation and a decrease in domestic exports, could be an increase in the policy interest rate, selling reserves, or imposing capital controls to forestall a depreciation. The cost of these policies, however, might be lower output than would otherwise occur with the depreciation, although this depends to some extent on the relative importance of balance sheet effects. In any case, as Blanchard *et al.* write, "If the policy implications seem complicated, it is because they are." (p. 276)

We focus on episodes of relatively large changes in our four policy responses. More specifically, we define a instance of a large depreciation as occurring in quarters in which there is at least a 25 percent depreciation over the previous year in the country's exchange rate versus the US dollar. To avoid episodes in which this depreciation just reflects a response to inflation, we only count those quarters in which the 25 percent depreciation was not preceded by a quarter

in which annual inflation was 20 percent or higher. We define major reserve sales as quarters in which international reserves (excluding gold) fall by at least 20 percent (compared to the previous year. To ensure that this is economically meaningful, we only include cases where the reserve to GDP ratio is at least 5 percent. We define substantial increases in interest rates as those quarters in which there has been an increase in the policy rate of at least 5 percentage points over the past year. To avoid a spurious change due to rising inflation, we only count quarters in which the annual rate of inflation is less than 20 percent. Finally, we define adjustments in capital controls as years in which there has been any increase in controls on capital inflows or a decrease in controls on capital outflows. We only consider changes in controls on five asset categories (money market instruments, bonds, financial credits, equities and collective investments) and do not include changes related to foreign direct investment.[4] For each of these four policies, we impose the condition that after a major policy change of the type described above, there cannot be another policy change of that type for the next three quarters, so that there can be at most one major policy change in each of the four categories per year for each country.

The cutoff values used for these policy changes to qualify as "major" are restrictive. Over the period 1995 – 2011, a time that includes both of these crisis periods and a more quiescent intervening period, these cutoffs are at about the 2nd percentile for the changes in interest rates, exchange rates, and reserves. The criteria to qualify as an adjustment in capital controls is less stringent (representing 4% of the sample for the same period), but unfortunately there are no good measures of the intensity of different capital controls by which to create a

[4] We include years in which there has been any increase in controls on any of the five categories of outflows (or any decrease in controls on any of the categories of inflows). To make the frequency of these policy responses match that of our other policy variables, we count a control as being put in place in the third quarter of a year.

higher threshold. During just the crisis windows from 1997-2001 and 2007-2011, the cutoffs are still quite stringent, although include a greater percentage of observations as would be expected.[5]

Tables 1a and 1b present the numbers of instances of these major changes in reserves, exchange rates, interest rates and capital controls for the two crisis periods. The numbers on the diagonal represent changes in one policy that did not occur within one quarter of a change in another of the three policies. The numbers in the upper triangular part of the matrix represent pairs of policies that occurred either in the same quarter or within one quarter of each other; for example, element (2,3) in Table 1a shows that there were five instances in which a substantial increase in interest rates occurred contemporaneously, or within one quarter of, a large depreciation. The numbers in the lower triangular part of the matrix represent triplets of policies that occurred contemporaneously or within one quarter; for example, there were 3 examples in the 1997 – 2001 period, when a country had a major reserve loss, a large depreciation and a substantial increase in interest rates all within one quarter of each other. The numbers in the last column of each table show the number of instances of each policy, and the percentage of the total number of large policy changes, for the respective period.

The first thing to note about the policies in both periods is that about two thirds of the policy changes represent a change in a single policy (represented by the diagonal elements) rather than changes in two or more policies within one quarter of each other; this happened 68 percent of the time (97 of 142) in the 1997 – 2001 period and 63 percent of the time (60 of 95) in the 2007 – 2011 period. Depreciations and the adjustments in capital controls were undertaken more often than reserve sales and interest rate increases. Of course, these relative rankings could

[5] The cutoff for counting a depreciation as large marks the upper 15th percentile for the 1997 – 2001 period, and the upper 6th percentile for the 2007 – 2011 period. The reserve loss cutoff reflects the 8th percentile for the 1997 – 2001 period and the 4th percentile for the 2007 – 2011 period. The interest rate increase represents the top 5th percentile of changes in the 1997 – 2011 period, and the top 2nd percentile of changes in the 2007 – 2011 period.

shift with different thresholds to qualify as a major policy change, and the more frequent use of capital controls undoubtedly reflects the less stringent criteria to qualify. Comparing the two periods, however, allows us to assess relative changes in the use of the different policies over the two crisis windows. The tables indicate greater use of interest rate increases and capital control adjustments in the earlier period as compared to the later period, and greater use of depreciations in the later period as compared to the earlier period.

Figures 1a and 1b present the time series of the use of these policies for the 1997 – 2001 and the 2007 – 2011 periods, respectively. Figure 1a shows that during 1997-1998, a similar number of countries had major reserve sales, currency depreciations, and interest rate hikes. Use of all of these policies declined over the window. In contrast, Figure 1b shows that countries had more major currency depreciations during 2008-2009 relative to any of the other policies, and fewer large increases in interest rates. Consistent with the statistics in Table 1a, there are more instances of adjustments in capital controls than the other three policies.

III. Explaining Policy Choices

The previous section documented the use of four different policies during crisis periods; selling reserves, currency depreciations, raising interest rates, and adjusting capital controls. But what determines which of these policies, if any, a country chooses? To answer this question, this section estimates the probabilities of each of these policies being employed during the crisis periods in the 1990's and late 2000's. These estimates contribute to our understanding of the determinants of policy choices. They are also instrumental for the analysis in the next section that uses propensity scores to estimate the effects of these policies on key outcome variables such as economic growth.

As discussed in the previous section, we focus on quarterly observations in which there are relatively large changes in three policy variables (reserve losses of at least 20 percent, depreciations of at least 25 percent, and increases in policy interest rates of at least 5 percentage points), subject to some additional conditions and the criteria that a country can have at most one major policy change in any four quarters to qualify as a separate event. We also consider a fourth policy change, years in which there was an decrease in controls on capital inflows or an increase in controls on capital outflows. (The capital control variable is only available at an annual frequency). As discussed in Section II, the imposition of any of these policies is a relatively rare event, occurring in about 2 percent of the observations for the entire 1995 – 2011 period, and at a somewhat higher percentage during the two crisis periods, 1997 – 2001 and 2007 to 2011. In order to avoid using a country that is about to make or has just made one of these policy changes as a country that is not adjusting policy, we also calculate an "exclusion window" for the 3 quarters before and the 3 quarters after a major policy change.

We estimate the likelihood of each of these events using a logit model for our panel of quarterly data for 85 countries over the periods 1997Q1 to 2001Q4 and 2007Q1 to 2011Q4. There are a wide number of potential covariates that we could employ in this analysis. We use those which are available for a large sample of countries at (preferably) a quarterly frequency and that have been highlighted as important in research predicting vulnerability to crises (e.g., Frankel and Saravelos, 2012, Rose and Spiegel, 2010, Claessens, Dell'Ariccia, Igan and Laeven, 2010, and IMF, 2010), sudden stops in capital flows (e.g. Forbes and Warnock, 2012, Calvo, 1998, Calvo, Izquierdo and Meijía, 2008), and the use of capital controls (e.g., Forbes, Fratzscher and Straub, 2013), and Aizenman and Pasricha, 2013).

The covariates can be roughly divided into four categories, recognizing that some variables could be placed in more than one group. One set of covariates controls for changes in the global environment. These include an indicator of global risk and uncertainty (the VXO[6]) and the change in the U.S. interest rate. This category also includes a dummy variable that distinguishes the earlier crisis period from the latter period, and is equal to 1 for observations in the 1997.I – 2001.IV period. A second category includes variables that reflect domestic vulnerabilities. These include changes in real GDP growth, changes in gross capital outflows (relative to GDP), changes in gross capital inflows (relative to GDP), the country's current account balance, changes in CPI inflation, and the percent change in private credit (relative to GDP).[7] A third category represents domestic characteristics that vary more in the cross section than in the time series for any particular country. This set includes the logarithm of income per capita, an indicator of institutional quality[8], openness (measured by the Chinn-Ito KAOPEN variable[9]), the level of reserves relative to GDP, and the whether the exchange rate is pegged[10]. The fourth and final category of covariates includes variables reflecting any recent changes in the four policies on which we are focusing. As with all the other covariates, these variables are

[6] The VXO is the Volatility Index calculated by the Chicago Board Options Exchange and measures implied volatility using prices for a range of options on the S&P 100 index. The measure is similar to the VIX (with a correlation of 99%), except the VIX is only available starting in 1990.

[7] Gross capital inflows and outflows are the sum of direct investment, portfolio investment, and other investment flows. Gross capital outflows also includes changes in reserves and related items. All capital flow data are from the dataset created in Forbes and Warnock (2012), based on IMF Balance of Payments data as of 2011. This does not incorporate the revised capital flow categories currently reported by the IMF which are only available starting in 2005. Private credit is measured by private credit by deposit money banks and other financial institutions to GDP and reported in Beck, Demirguc-Kunt, Levine, Cihak, and Feyen (2013).

[8] Institutions are measured using an index based on the ICRG measures of institutional quality compiled by the World Bank. The index is calculated as the weighted average of the six institutional variables: legal strength, law and order, investment profile, government stability, corruption and bureaucracy quality.

[9] We focus on the KAOPEN measure of capital controls as developed in Chinn and Ito (2008) and updated as of July 2013. KAOPEN is based on the principal components from four binary variables reported by the IMF: (1) capital account openness; (2) current account openness; (3) the stringency of requirements for the repatriation and/or surrender of export proceeds; and (4) the existence of multiple exchange rates for capital account transactions.

[10] See Klein and Shambaugh (2013). A country is defined as having a peg if the country's exchange rate remains within a +/-2% band.

lagged one quarter. The four variables are the lagged: percent change in reserves (as a share of GDP), percent changes in the nominal exchange rate versus the US dollar, changes in the country's policy interest rate (relative to the US rate) and changes in capital controls.

Using these four categories, the logit model that is the base case for our analysis is:

$$Prob(pc_{it} = 1) =$$

$$F\left(\mathbf{\Phi}_{t-1}^{\text{Global}}\mathbf{B}_{\text{G}} + \mathbf{\Phi}_{i,t-1}^{\text{Vulnerabilities}}\mathbf{B}_{\text{V}} + \mathbf{\Phi}_{i,t-1}^{\text{Characteristics}}\mathbf{B}_{\text{C}} + \mathbf{\Phi}_{i,t-1}^{\text{RecentPolicies}}\mathbf{B}_{\text{RP}}\right), \quad (1)$$

where pc_{it} is an episode dummy variable that takes the value of 1 if country *i* is adopting a major policy change (reserve sale, currency depreciation, interest hike, or change in capital controls) in quarter *t*; $\mathbf{\Phi}_{t-1}^{\text{Global}}$ is a vector of global variables lagged by one quarter; $\mathbf{\Phi}_{i,t-1}^{\text{Vulnerabilities}}$, $\mathbf{\Phi}_{i,t-1}^{\text{Characteristics}}$, and $\mathbf{\Phi}_{i,t-1}^{\text{RecentPolicies}}$ are vectors of variables measuring country vulnerabilities, other country characteristics, and recent changes in related policies for country *i*, lagged by one quarter. All standard errors are robustly estimated.

Table 2 shows the estimates of this logit model for each of the four crisis-response policies. In general, the model is better able to explain major reserve sales, large currency depreciations, and significant increases in interest rates than the imposition of capital controls, with the pseudo-R^2 for the first three policies ranging from 0.30 to 0.39, while the pseudo-R^2 for the capital control regression is 0.15. The coefficients on the many of the covariates for the reserve sales, currency depreciations and interest rate increases are significant and have the expected sign. For example, countries are significantly more likely to enact any of these three policies when real GDP growth is weaker. Reserve sales and interest rate increases are significantly more likely when capital outflows increase or capital inflows decrease. Currency

depreciations and reserve sales are significantly more likely when global risk (as measured by the VXO) is higher. Countries with pegged exchange rates (in the previous period) are more likely to have large reserve sales and less likely to have large currency depreciations. The likelihood of major currency depreciation is lower for countries with larger current account surpluses. The likelihood of major interest rate increases is lower for countries with slower private credit growth, higher income per capita, and greater openness. Recent policy changes also affect the likelihood of enacting different policies today. For example, countries with reserve losses or currency appreciations in the previous quarter are more likely to have a major reserve sale or increase in interest rates in the subsequent quarter (although the last effect is only significant at the 10% level).

The coefficients on the dummy variables for the 1990's crisis window are of special interest. These estimated coefficients are positive and significant for three of the four policy changes, and insignificant for currency depreciations. This suggests that countries were significantly more likely to allow major reserve sales, interest rate hikes, and changes in capital controls during the 1990's crisis window than during the more recent global financial crisis when controlling for changes in the global environment, domestic vulnerabilities, country characteristics, and recent policy changes. Moreover, the exponentiated values of the coefficient estimates for the 1990's crisis dummy suggest that the sizes of these differences across the two crisis windows are notable. [11] Conditional on the other variables included in the regression, major reserve sales are 7.7 more likely in the earlier period than in the later period, interest rate increases are 8.6 times more likely, and the imposition of capital controls is 6.0 times more

[11] For a Logit regression, the relevant exponentiated coefficient is the odds ratio (p/1-p), with $p=Pr(y = 1/X)$ the probability of a positive outcome. The exponentiated values reported in the text correspond to the 2.037 coefficient on the 1990's crisis dummy for the model predicting reserve sales, the 2.154 coefficient for interest hikes, and the 1.797 coefficient for capital controls.

likely. These results are consistent with the higher incidence of these interest rate increases and changes in capital controls in the 1990's crisis period as shown in the unconditional statistics in Tables 1a and 1b. This suggests that the greater use of policies such as interest rate increases, capital controls, and possibly reserve sales during the 1990s does not just reflect greater global instability or greater domestic vulnerabilities in the earlier period (insofar as the covariates are capturing these effects).

We have conducted a number of robustness tests for the results in Table 2. One set of these tests includes additional variables, such as the country's debt to GDP, fiscal balance as a share of GDP, stock market capitalization relative to GDP, changes in commodity prices, changes in the real exchange rate, and a dummy for membership in the euro area. The inclusion of many of these additional variables shrinks the sample size (especially the number of countries which adopted one of the four major policy changes), but does not alter the key results. As another robustness check, we estimated the model using a complimentary logarithmic (or cloglog) estimator (instead of the standard logit model) in order to adjust for the fact that major policy adjustments occur irregularly and $F(\cdot)$ is is asymmetric. This also has no significant effect on the results.[12] Likewise, the main results are not much affected when we estimated the model using a stepwise approach that only includes control variables that are significant at least at the 20% level in the original (Table 2) regressions. These second-round regressions slightly increase the significance of several variables, but do not affect the main results.

The estimates in Table 2 do a fairly good job of predicting major reserve sales, large currency depreciations and substantial increases in interest rates, but have more limited success in predicting changes in capital controls during crisis periods. These results show that the

[12] This specification assumes that the $F(\cdot)$ is the cumulative distribution function (cdf) of the extreme value distribution. In other words, this estimation strategy assumes that: $F(z) = 1 - exp[-exp(z)]$. See Forbes and Warnock (2012) for details on this estimation methodology.

likelihood of policy changes is affected by global variables, domestic vulnerabilities, country characteristics and recent policy actions and highlights the importance of controlling for these factors when comparing the outcomes associated with these policies. More specifically, countries with certain characteristics (such as higher income levels, faster credit growth, and greater openness) are less likely to undergo certain policy changes (such as major interest rate increases), but the key outcome variables (such as GDP growth) may be associated with these country characteristics as well as the policy actions. Propensity scoring offers one method of controlling for these country characteristics, vulnerabilities and recent policy changes by matching groups of similar countries in order to assess the impact of the policies. The next section discusses this method and then present results from the propensity scoring.

IV. Propensity Score Methodology and Results

The logit estimates presented in the previous section, while interesting in their own right, can also be used to calculate propensity scores that reflect the probabilities that each country undertakes each policy response in each quarter. These propensity scores can then be used to match country-quarter observations in order to analyze the effects of "treatments" (in this case, the effects of major reserve sales, large currency depreciations, substantial interest rate increases, or adjustments in capital controls) on key outcome variables. This section begins with a brief discussion of this methodology. Then it presents initial results and test statistics from using the propensity scores to match each treatment with a control group of observations. The section ends by using these matched groups to construct counterfactuals and estimate the effects of these four policy responses on GDP growth, unemployment, and inflation.

A. Propensity Score Methodology

Countries that undergo large reserve sales, currency depreciations, or interest rate increases, or that change their capital controls, tend to differ from countries which do not make these policy choices. For example, as shown in Table 2, countries which have a major reserve loss are more likely to be countries with characteristics that could slow growth independent of the reserve loss, such as increased capital outflows, decreased capital inflows, a pegged exchange rate, and recent currency appreciation. As a result, it is difficult to gauge whether differences in key outcome variables between countries that undertake these policies and those that do not are driven by the policy choices or simply by underlying differences in the two sets of countries.

To illustrate this issue using some notation, define the adoption of the treatment by the i^{th} country (such as a major sale of reserves) as $D_i = 1$, and the absence of this action as $D_i = 0$. The outcome variable (say GDP growth) is $Y_{1,i}$ for the i^{th} member of the treated group and $Y_{0,i}$ for the i^{th} member of the untreated (control) group. Summing over members of each group, we are able to observe $E[Y_{1,i}|D_i=1]$ and $E[Y_{0,i}|D_i=0]$, but we are interested in the average effect of the treatment on the treated (ATT), which is $E[Y_{1,i} - Y_{0,i}|D_i=1]$. The difference in the two observable statistics reflects both the average effect of the treatment on the treated and sampling bias. Sampling bias, represented by $E[Y_{0,i}|D_i=1] - E[Y_{0,i}|D_i=0]$, reflects the difference in outcomes that is attributable to differences in the treated and control group (such as different country characteristics) rather than any effect of the treatment itself.[13]

The effect of sampling bias could be easily minimized if countries differed along only one or two discrete relevant dimensions with respect to the likelihood of undertaking a policy (such as selling reserves). If the set of countries could be readily apportioned to a small number of "cells" reflecting all differences along these dimensions, and there were enough instances of both treated

[13] See Angrist and Pischke (2008, Chapter 3) for an excellent, intuitive presentation of this topic.

and control cases in each cell, it would be straightforward to calculate the differences between the treated and the untreated in each cell, and take a weighted average of those differences in order to estimate the effect of different treatments (i.e., policy changes).

In practice, however, the differences across countries are manifold and multidimensional, and it is impossible to match two countries which share identical macroeconomic characteristics. Propensity score matching offers a means to address this challenge. This methodology matches countries that undertake "treatments" (i.e., the four policy responses we focus on) to a subset of countries that do not, based on a set of observable country characteristics and global variables, represented by the vector X_i for the i^{th} country.[14] This matching controls for the differences in the treated and untreated groups that affect outcomes, such that the sampling bias disappears (at least any bias that is captured in the vector X_i). In other words, the underlying assumption is: $E[Y_{0,i}|X_i,D_i=1] = E[Y_{0,i}|X_i,D_i=0]$. While this still leaves a multidimensional problem, Rosenbaum and Rubin (1985) show that it is sufficient to match treated and control observations based on a "propensity score," $P(X_i)$, which is the probability that country *i* receives the treatment. This single propensity score reduces the number of dimensions over which observations must be matched. Rubin and Thomas (1992) further show that it is possible to estimate these propensity scores based on the vector of observable characteristics.[15]

[14] This methodology has been widely used in medical studies and labor economics, but has only recently been employed in international and macroeconomic research. See Angrist and Pischke (2008) for an excellent summary of this methodology and examples from the labor literature. Four papers that have used this methodology in the international/macroeconomic literature are: Ehrmann and Fratzscher (2006) for the effect of monetary policy shocks on firms, Forbes, Fratzscher and Straub (2013) on the effects of capital controls and macroprudential measures, and Glick, Guo, and Hutchison (2006) and Das and Bergstrom (2012) on the link between openness and currency crises and growth, respectively.

[15] One key assumption which must be satisfied to use this methodology is the conditional independence assumption. This requires that conditional on the vector of observable characteristics (X), the outcome variable is independent of the choice of treatment.

The propensity score is the conditional probability of adopting the treatment (in our case, the policy response), given pre-treatment characteristics, X_i. Continuing to define the adoption of the treatment as $D_i = 1$ (and not adopting the treatment as $D_i = 0$), the propensity score, $p(X_i)$ is:

$$p(X_i) = \Pr[D_i=1|X_i] \; . \qquad (2)$$

In the context of our model, propensity scores are the likelihoods that a country undergoes a major reserves sale, large currency depreciation, substantial interest rate increase or an adjustment in capital controls, controlling for the set of country and global characteristics, as specified in equation (1). In practice, the propensity scores can be generated using logit regressions, such as those reported in Table 2.

After the propensity scores have been calculated, there are several algorithms that can be used to match each treated observation with one or more untreated observations (i.e., controls). We focus on three matching algorithms: nearest-neighbor without replacement, five-nearest neighbors with replacement, and local-linear matching. Nearest-neighbor matching pairs each treated observation with the one observation from the control group that has the closest propensity score. We use the method "without replacement," which means that any observation from the untreated group can be matched to only one treated observation. We also use a variant of this method, focusing on the five nearest neighbors (rather than one) as a control group, and allow "replacement" (so that untreated observation can be used more than once as a match). A third algorithm is local-linear matching, which calculates a weighted average of all observations in the control group using a nonparametric estimator which uses a generalized weighting

function to assign a higher weight to control observations closer to the treated observation.[16] Thus, the two nearest-neighbor algorithms are basically extreme forms of local-linear matching, with all the weight given to the closest propensity score(s).[17]

Each of these matching methodologies has advantages and disadvantages. Nearest-neighbor is straightforward, easy to implement, and minimizes "bad" matches with control observations that have little in common with the treated observation. It is also straightforward to check which country is "matched" as the nearest neighbor in a control group. Nearest neighbor matching, however, ignores useful information from other countries in the control group. Local-linear matching use more information and therefore tends to have lower variances—but at the risk of including bad matches, which can increase the mean standard deviation relative to the treated group. For each of the three matching algorithms, it is possible to calculate several statistics to assess the accuracy of the matching and it removes any significant differences between the treated and control groups. These tests and statistics are discussed in more detail in the next section.

To conclude this section on the propensity-score matching methodology, it is useful to mention how this approach compares to the more familiar regression analysis. Multivariate regressions estimate the partial correlation of the treatment with the outcome, and, like the matching method, controls for the other variables included in the equation. The main difference between the standard regression approach and the propensity-score methodology, however, is the

[16] See Heckman, Ichimura, and Todd (1997, 1998) for a detailed description of the local-linear matching method.
[17] Two other algorithms that we have used in our analysis are radius and kernel matching. Both of these are similar to local linear matching. Radius matching sets a pre-determined radius for observations to be included in the control group, thereby excluding bad matches, but a disadvantage of this method is that it does not place greater weight on better matches (as done in kernel and local-linear matching). Fan (1992a, b) shows that local-linear matching has several important advantages over kernel matching, such as a faster rate of convergence near boundary points and greater robustness to different data design densities. For all of these reasons, we focus on the other three strategies.

weighting of the covariate-specific differences between the treated and the untreated.[18] More specifically, consider cells constructed from discrete values of *X*. In both approaches, it is necessary to construct weights for the difference between treated and untreated values across different cells in order to calculate the average effect for the whole sample. In propensity-score methodology, the weights are based on the distribution of covariates among the treated, with the greatest weights put on cells representing the highest likelihood of being treated. In contrast, in regression analysis, the greatest weights are placed on cells where the conditional variance of treatment status is larger; roughly speaking, those cells with equal likelihood of its elements being treated or untreated.

These two different weighting approaches can significantly affect the estimated average effects of the treatments (i.e., policy changes) if the differences between the treated and the untreated observations vary across cells. For example, when estimating whether reserve sales significantly affect growth, a propensity-matching approach could put more weight on the cells representing a pegged exchange rate regime than would be the case with a regression approach. This could occur because pegged exchange rates are more likely to be associated with large reserve sales (as shown in the estimates in Table 2), so the propensity score methodology could put more weight on controls that are similar according to this dimension of the vector *X*.

B. Results of Propensity Score Matching

[18] Angrist and Pischke (2008, Chapter 3) present an excellent discussion of the similarities and differences between regression analysis and propensity-score matching approach, and their relative advantages. Also, although propensity-score matching can reduce asymptotic efficiency relative to a regression framework, Angrist and Hahn (2004) show that there can be efficiency gains in a finite sample, even if there is no asymptotic efficiency gains from the use of propensity-scoring estimators. Given the small size of our sample—this suggests that the one potential drawback of the propensity score methodology is less likely to be an issue.

We use the estimates from the logit model in Table 2 to calculate separate propensity scores for each of the four policies (reserve sales, currency depreciations, interest rate increases and adjustments in capital controls) for all quarters during our crisis periods (1997-2001 and 2007-2011). We then use these estimated propensity scores to match treated observations with control observations through the three matching algorithms discussed above; nearest neighbor without replacement, five-nearest neighbors with replacement, and local linear. In order to avoid using a country that is about to make or has just made one of these policy changes as a control variable for that same policy change, we continue to include an "exclusion window" for the 3 quarters before and the 3 quarters after a major policy change, during which a country cannot be included as a control observation for that same type of policy (although it could serve as a control for a country that undertook a different policy change). A country can also serve as its own control, albeit not within the period of the exclusion window.

One additional consideration in propensity matching is whether to exclude treated observations that are "off support". A treated observation is "on support" when its propensity score is between that of the minimum and maximum propensity scores for the untreated observations. If this is not the case, then the treated observation is "off support" and comparisons with the untreated observations are less accurate. To be conservative, in the analysis that follows we impose a common-support condition which drops all observations with a propensity score higher than the maximum or lower than the minimum propensity score of the controls in order to reduce the effect of any "bad" matches. The literature suggests that this condition can be important, especially for local-linear matching.

Fortunately, the logit models we use perform well in generating matches that are on-support for most of the major policy responses. More specifically, for each of the three matching

algorithms the treated observations are on-support for 20 of the 22 treatments of major reserve sales, 37 of the 38 treatments of large currency depreciations, 17 of the 20 treatments of sharp interest rate hikes, and all 50 of the treatments of capital control adjustments.[19] The treatments which are off support for reserve sales are: Thailand (1997q3) and Russia (2009q1). The treatment which is off support for currency depreciations is Poland (2009q1) and those off support for interest rate increases are Thailand (1997q3), Philippines (1997q4) and India (1998q1).

In order to obtain a more intuitive understanding of the how the matching methodology functions, it is useful to check the results from matching between treatment and control groups using the "nearest neighbor" algorithm for some well-know examples of crisis responses. For example, in 1997Q4 Korea's major currency depreciation is matched with Turkey in 1998Q1. Argentina's adjustment in capital controls in 2001Q3 is matched with Mexico in 2000Q2. Latvia's major reserve sales and interest rate increase in 2009 in order to defend its peg are matched with Portugal (2000Q1) and Poland (2009Q1), respectively. During the recent GFC, Mexico's large depreciation is matched with Croatia in 2009Q2 and Malaysia's major reserve sales are matched with Slovenia in 2007Q2. These examples suggest that the countries identified as "nearest neighbors" generally make intuitive sense with respect to the control observation sharing similar country characteristics as the treated country, and also with the control observations generally being relatively proximate in time to the treatment event.

While these matches obtained using the nearest neighbor algorithm seem intuitive, it is important to more formally test whether the different matching algorithms remove significant differences in observables between the treatment and control groups. Table 3 reports results of

[19] We apply these matching algorithms with the Stata module PSMATCH2, developed by Leuven and Sianesi (2003). The number of treated observations is lower than reported in Table 1 because data are not available for the covariates needed to estimate propensity scores for all observations.

these tests for large interest rate increases (which was the treatment least successful in matching in the sense that it had the greatest number of off-support observations). The first two columns present the mean values for the treated group (μ_T) and control group (μ_C) for the unmatched samples for each of the covariates used to estimate propensity scores. The third column reports *t*-statistics for tests of the hypothesis that the mean of each variable in the treatment group is equal to the mean in the control group (H0: $\mu_T = \mu_C$). There are significant differences (at the 95 percent confidence level) between the treatment group and the unmatched control group for six of these variables. Countries were more likely to have a major increase in interest rates during the 1990's crisis window and if they had slower real GDP growth, lower income per capita, worse institutions, lower measures of openness, and had sold fewer reserves in the last period. These significant differences across the treatment and unmatched control groups highlight that selection bias is important; countries which had a major increase in interest rates had significantly different characteristics than countries which did not.

The remaining columns of Table 3, however, indicate that the matching algorithms are able to remove the selection bias (at least as measured by the included variables). These columns show mean values for each of the covariates in the matched control groups using the three matching algorithms: nearest neighbor, five-nearest neighbors, and local-linear. It also reports the same t-statistics of tests for significant differences between the treatment and matched control groups for each of the variables. In contrast to the results for the unmatched control group, there are no longer significant differences for any of the variables between the treatment and any of the three matched control groups.

This pattern of results in Table 3 showing the ability of the propensity score matching to remove significant differences between the variables in the treatment and the matched control

groups also applies when this analysis is performed for major reserve sales, large currency depreciations, and adjustments in capital controls. In each of these cases, as with large increases in interest rates, there are significant differences in the means of several variables between the treatment and unmatched control groups, but after using each of the matching algorithms, there are no longer any significant differences between the variables in the treatment and matched control groups.

As a final analysis of the performance of the different matching algorithms, Table 4 reports several statistics from propensity-score matching for each of the four different policy responses. The statistics include the mean propensity scores for the treated group, unmatched control group, and matched control group using each of the three algorithms. In each case, the mean propensity scores after matching are much closer to that of the treatment group than for the unmatched control group. The table also reports the mean absolute bias of the treatment group relative to the unmatched control group and control group using each of the matching algorithms (with standard deviations in parentheses). In each case, the matching reduces the mean absolute bias by a substantial amount. The table also reports the number of treatment observations that are successfully matched and "on-support" using each algorithm (as discussed above). In order to simplify the discussion that follows, we will focus on results obtained using the five-nearest neighbor and local-linear matching algorithm, as these tend to yield control groups with mean propensity scores closest to the treatment group and with the lowest mean absolute bias.

C. Effect of policy choices: Average Treatment Effects on the Treated (ATT)

This section concludes with an analysis of the effects of each of the four policies on three outcome variables: real GDP growth, inflation and unemployment. These effects are calculated

by matching the treated observations with the selected control groups. The results presented in the previous section show that the matching algorithms removed significant differences between the treatment and control samples, at least with respect to the variables included in the logit analysis presented above.

The effect of these policies is calculated as the average-treatment effect on the treated or ATT (as described in Section IV.A). We present two separate ATTs, one based on the five-nearest neighbor matching algorithm and the other based on the local-linear matching algorithm. When calculating the average outcome for the control group, the five-nearest neighbor approach uses equal weights for the five control observations with the closest propensity score to the treated observation, while the local-linear approach uses the full set of control observations but places higher weights on observations with propensity scores closer to that of the treated observation. The standard errors of the ATT, which are required to gauge whether there is a statistically significant difference between the treated and untreated groups, must be bootstrapped because the propensity scores are themselves estimates.[20]

We test for the cumulative ATT on outcome variables at for each of four quarters, beginning with the quarter that the major policy response (the treatment) occurs. This enables us to consider both immediate and longer-run effects. We do not consider effects beyond one year because the matching algorithms become less accurate over longer time periods.[21] We calculate the change in each of the outcome variables (real GDP growth, CPI inflation, and

[20] See Lechner (2002) for the appropriate methodology. We use 200 repetitions for the bootstrap.
[21] Moreover, countries included in the control group may adopt a major policy change after four quarters and would therefore not be accurate controls.

unemployment) relative to the average over the four quarters before the policy change occurred.[22]

To illustrate our approach, consider the estimated ATT measuring the effect of reserve sales on real GDP growth. We calculate the average difference between the change in real GDP growth between the treated and control groups. For the first post-treatment period, this is the difference between real GDP growth from the quarter of the reserve sales (t=0) to the next quarter (t=1), expressed as an annual rate, relative to average GDP growth over the previous four quarters (t-1, t-2, t-3, and t-4). For the second, third and fourth post-treatment periods, this would be the change from period 0 to period 2, period 0 to 3, and period 0 to 4, respectively, all expressed in terms of an annual rate, and all relative to the average over the same four quarters before the major reserve sales (periods t-1, t-2, t-3, and t-4). One advantage of this approach is that it allows us to capture any differential effects of major policy responses over different time periods rather than choosing, *a priori*, the time period on which to focus. This is a *ceterus paribus* approach in that it does not incorporate any adjustment for changes in post-treatment covariates.

The most straightforward way to characterize the results of this ATT analysis is bar graphs of the estimated cumulative effects in each of the four quarters. Figures 2 through 4 present these graphs for the estimated effects of each of the four policies on each of the three outcomes (GDP growth, unemployment, and CPI inflation, respectively). Each figure also presents results using the two preferred matching methods: five-nearest neighbors and local-linear. The bars in these graphs show the cumulative magnitude of the estimated ATT since the policy change occurred. A bar shaded in dark black indicates that the ATT for that quarter is

[22] We use the average over the previous four quarters in order to minimize seasonal affects and also to smooth the baseline value that we use as a comparison to the post-policy change values.

significant at the 5% level or better, and a bar shaded in medium-blue indicates that the ATT is significant at the 5% to 10% level. The black line is the fitted line for the average treatment effect.

[NOTE: GRAPHS FOR ADJUSTMENTS IN CAPITAL CONTROLS WILL BE INCLUDED IN THE NEXT DRAFT.]

Figure 2 presents the ATT for real GDP growth for each of the major policy responses and the two matching methods. The results indicate that the one policy which appears to generate a significant increase in real GDP growth is large currency depreciations. More specifically, the middle panels indicates that major depreciations may initially lead to a decline in GDP growth (although this effect is not significant using five-nearest neighbor matching), but that by the third quarter after the depreciation the cumulative effect is significant and positive—and this expansionary affect more than outweighs any initial contractionary effect. This is consistent with both a standard J-curve effect and a balance sheet effect, whereby the depreciation increases liabilities denominated in foreign currency relative to assets denominated in domestic currency. In contrast, the top panels show that major reserve sales may increase real GDP growth over the first three quarters, but these effects are not statistically significant and fall over time, possibly becoming negative by the 4th quarter after the reserve sales. The third set of panels shows that a major rise in interest rates is associated with a decrease in GDP growth over each of the four quarters, but, again, these effects are not statistically significant. Therefore, for countries hoping to pursue policies to support GDP growth during international crises, depreciations appear to be the most likely to accomplish this goal.

Figure 3 presents a similar exercise for the average treatment effects on unemployment. In this case, none of the policy responses is able to generate a significant reduction in

unemployment. Reserve sales appear to be more likely to increase unemployment—especially over 3-4 quarters, and currency depreciations appear more likely to decrease unemployment—but neither of these estimates is significant. In sharp contrast, large increases in interest rates appear to lead to significant increases in unemployment. This effect is large in magnitude and appears to increase over time, with the ATT on unemployment rising from about two percentage points in the first two quarters to about three percentage points after four quarters since the interest rate increase. This suggests that choosing the policy of raising interest rates to forestall a currency depreciation or capital outflow involves a substantial cost.

Finally, Figure 4 shows the estimated ATT on inflation. In this case, a substantial currency depreciation is estimated to raise inflation by about 3 to 4 percentage points on impact, and cumulatively by 6 to 7 percentage points after one year. These effects are statistically significant by the 3rd and 4th quarters (but only significant at the 10% level or insignificant in the 1st and 2nd quarter). These results, like those for the effect of an interest rate increase on unemployment, suggest a cost of a major policy response to a crisis.

The results of this analysis presented in Figures 2, 3 and 4 show that different policy responses to crises can yield very different outcomes. The policy that appears to be most beneficial in terms of supporting GDP growth is a major currency depreciation, but currency depreciations also generate a significant and economically important increase in inflation. Reserve sales do not generate any increase in inflation, but have no significant benefits in terms of increasing growth or reducing unemployment, especially over several quarters. Sharp increases in interest rates appear to yield the least benefit, as they correspond to a significant and economically meaningful increase in unemployment, while yielding no benefits in terms of stronger growth or reduced inflation.

Countries are therefore faced with no policy response that yields positive outcomes for all three of these key measures of economic performance and must therefore respond to crises by "picking their poison". The different patterns in policy responses during the more recent crises of the late-2000s relative to the 1990's window, however, suggests that countries have recently preferred currency depreciations in order to support growth, despite the "poison" of a higher risk of inflation.

V. Conclusions

In a period of crisis, policy makers are faced with difficult choices. The analysis in this paper illustrates some of the factors associated with the policy choices to respond to contractions in capital flows. Understanding what determines these choices is interesting in its own right, and these results are also important because they show that these policy choices are not independent of factors that could affect economic outcomes like growth and inflation. Therefore, an analysis of the effects of these policies must take this sample selection seriously. We do this by employing a technique that is relatively novel in empirical international macroeconomic analysis, propensity score matching. We find that certain policies, like allowing a large currency depreciation, have both beneficial effects, such as growth after about two quarters, but also effects that are harmful, such as higher inflation and possibly slower growth immediately after the depreciation. Ultimately, policy makers must pick their poison and face challenging trade-offs during crisis periods.

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Table 1a: Policy Changes, 1997 - 2001					
	Reserve Loss	Depreciation	Raise Interest Rate	Impose Capital Control	Total
Reserve Loss	19	3	1	1	27 (19%)
Depreciation	3	19	5	2	34 (24%)
Int. Rate		2	15	3	29 (20%)
Cap. Cntrl.				44	52 (37%)
					142

Table 1b: Policy Changes, 2007 – 2011					
	Reserve Loss	Depreciation	Raise Interest Rate	Impose Capital Control	Total
Reserve Loss	9	6	2	1	19 (20%)
Depreciation	1	18	3	4	32 (34%)
Int. Rate			8	0	14 (15%)
Cap. Cntrl.				25	30 (31%)
					95

Notes: Instances of reserve loss of at least 20% between one quarter and that quarter in the previous year, depreciation of at least 25 percent between one quarter and that quarter in the previous year, interest rate increase of at least 5 percentage points between one quarter and that quarter in the previous year, and an increase in controls on capital outflows or a decrease in controls on capital inflows in a particular year, all subject to some other conditions described in the text. Diagonal elements represent policy changes that occurred with no other policy change in that quarter, in the preceding quarter, or in the following quarter. Upper triangular elements represent instances of pairs of policies, lower triangular elements represent instances of triplets of policies, with pairs and triplets representing polices that occurred in the same quarter, or in adjoining quarters.

Table 2: Probability of Adopting Policies During Crises

		Reserve Sales	Currency Depreciations	Interest Rate Increases	Capital Controls
Global Measures	Global risk	0.081**	0.071***	-0.013	-0.115***
		(0.034)	(0.026)	(0.034)	(0.039)
	US interest rate (ch)	0.081	-0.313**	-0.137	-0.285**
		(0.137)	(0.134)	(0.181)	(0.144)
	1990's crisis dummy	2.037***	0.729	2.154***	1.797***
		(0.779)	(0.500)	(0.664)	(0.488)
Domestic Vulnerabilities	Real GDP growth (ch)	-0.100***	-0.080**	-0.054**	-0.036*
		(0.031)	(0.036)	(0.024)	(0.020)
	Capital outflows, % of GDP (ch)	10.655***	-6.957*	10.739***	-2.318
		(3.147)	(4.212)	(3.859)	(2.023)
	Capital inflows, % of GDP (ch)	-10.702***	4.921	-13.463***	1.055
		(3.402)	(4.006)	(4.201)	(2.044)
	Current account % of GDP	-0.286	-2.922***	-0.994	-0.999
		(1.096)	(1.132)	(2.093)	(0.678)
	Inflation (ch)	0.024	0.043	0.042	-0.004
		(0.029)	(0.041)	(0.031)	(0.017)
	Private credit, % of GDP (pch)	-0.001	0.001	0.043***	0.004
		(0.012)	(0.022)	(0.010)	(0.010)
Other Country Characteristics	Income per capita (log)	-0.374	-0.538*	-1.702***	0.377
		(0.397)	(0.283)	(0.380)	(0.244)
	Institutions index	3.407	0.123	0.214	-2.418**
		(2.200)	(1.636)	(1.865)	(1.125)
	Reserves as % of GDP	1.673	-7.268**	-2.999	2.383**
		(1.023)	(3.531)	(4.721)	(0.946)
	Peg dummy	1.359**	-3.649***	-0.699	-0.547
		(0.588)	(1.411)	(0.916)	(0.487)
	Openness	-0.465*	-0.391	-0.597**	-0.305**
		(0.241)	(0.243)	(0.232)	(0.153)
Recent Policy Changes	Reserves as % of GDP (pch)	-0.054***	-0.011	-0.053***	0.002
		(0.014)	(0.022)	(0.016)	(0.003)
	ER vs. US $ (pch)	-0.042**	-0.010	-0.048*	-0.011
		(0.020)	(0.013)	(0.028)	(0.008)
	Interest rate vs. U.S. rate (ch)	0.065*	-0.009	0.032	-0.002
		(0.034)	(0.022)	(0.035)	(0.014)
	Capital controls (ch)	-0.335	0.347	1.172*	0.337
		(0.805)	(0.549)	(0.684)	(0.396)
Observations		***1,238***	***1,158***	***1,263***	***1,116***
Pseudo R-squared		***0.299***	***0.362***	***0.390***	***0.148***

Notes: Results of logit regressions predicting probability of each policy variable listed at top and as defined in Section II. "Ch" denotes change and "Pch" denotes percentage change. Constant is not reported. * is significant at the 10% level, ** at the 5% level and *** at the 1% level. All regressions include robust standard errors.

Table 3
Major Interest Rate Hikes: Means for Treatment and Control Groups using Different Matching Algorithms

	Mean: Treatment Group (μ_T)	Mean: Unmatched Control (μ_C)	t-Statistics (H0: $\mu_T = \mu_C$)	Nearest Neighbor (no replacement)		5 Nearest Neighbors		Local-linear	
				Mean: Matched Control	t-stat	Mean: Matched Control	t-stat	Mean Matched Control	t-stat
Global Risk	27.928	25.620	1.24	27.936	0.23	30.144	-0.52	26.964	0.59
U.S. interest rate (ch)	-0.927	-0.685	-0.72	-0.844	-0.51	-1.259	0.21	-0.873	-0.46
1990's crisis dummy	0.700	0.452	2.21**	0.765	-0.74	0.682	-0.21	0.765	-0.74
Real GDP growth (ch)	8.137	9.608	-6.19***	8.524	-0.56	8.402	-0.20	8.640	-0.95
Capital outflows/GDP (ch)	-0.007	-0.012	0.10	0.004	-0.62	0.009	-0.60	0.007	-0.82
Capital inflows/GDP (ch)	-0.038	-0.011	-0.61	-0.016	-0.65	-0.027	-0.24	-0.011	-0.80
Current account/GDP	-0.119	-0.017	-1.62	-0.068	-0.50	-0.109	0.01	-0.092	-0.20
Inflation (ch)	-1.401	-0.614	-0.37	-1.061	-0.08	-0.192	-0.61	-1.384	0.12
Private Credit/GDP (%ch)	8.420	4.997	1.15	18.826	-0.92	12.253	-0.49	19.535	-0.97
Income per capita	8.137	9.608	-6.19***	8.524	-0.56	8.402	-0.20	8.640	-0.95
Institutions index	-0.484	-0.320	-3.74***	-0.454	-0.75	-0.471	-0.50	-0.439	-1.02
Reserves/GDP	0.122	0.141	-0.45	0.125	-0.05	0.120	0.15	0.120	0.11
Peg dummy	0.100	0.298	-1.93	0.176	-1.05	0.153	-0.88	0.176	-1.05
Openness	0.086	1.458	-4.62***	-0.106	0.59	0.311	-0.29	-0.106	0.59
Reserves/GDP (%ch)	-8.455	9.247	-2.2**	-5.880	-0.50	-3.752	-0.77	-6.286	-0.43
ER/US$ (%ch)	14.252	5.344	1.55	12.461	0.23	12.190	0.29	12.877	0.18
Interest rate vs. US (ch)	2.719	-0.223	1.95*	2.979	-0.01	2.241	0.26	2.821	0.03
Capital controls (ch)	0.200	0.113	1.20	0.176	0.41	0.318	-0.52	0.235	0.00
Mean Propensity Score	***629.7***	***370.5***		***436.9***		***494.7***		***514.6***	
Observations	***20***	***1243***		***1243***		***1243***		***1243***	

Notes: Reports difference in means between treatment and control groups, with control group created based on regression results reported in Table **** and matching performed using algorithms listed at top of table. See ******for detailed variable definitions. * indicates significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table 4
Summary Statistics for Different Matching Algorithms

	Treatment Group	Unmatched Control Group	Matched Control Group Based on Matching Algorithm: Nearest Neighbor (no replacement)	5 Nearest Neighbors	Local-linear
Major Reserve Sales					
Mean Propensity Score	505.2	380.4	423.9	461.2	450.0
Mean Absolute Bias		29.28	11.87	5.25	11.70
(Standard Deviation)		(27.01)	(11.40)	(6.15)	(9.68)
Observations	22	1216	20	20	20
Large Currency Depreciations					
Mean Propensity Score	448.7	382.9	593.8	572.0	596.4
Mean Absolute Bias		46.98	18.96	11.99	9.41
(Standard Deviation)		(37.65)	(21.45)	(8.91)	(7.31)
Observations	38	1158	37	37	37
Large Interest Rate Increases					
Mean Propensity Score	629.7	370.5	436.9	494.7	514.6
Mean Absolute Bias		43.41	21.25	12.90	19.04
(Standard Deviation)		(64.44)	(15.47)	(9.24)	(23.59)
Observations	20	1263	17	17	17
Adjustments in Capital Controls					
Mean Propensity Score	544.4	348.9	448.8	446.7	434.6
Mean Absolute Bias		23.56	14.12	9.94	16.24
(Standard Deviation)		(17.36)	(10.50)	(5.99)	(13.27)
Observations	50	1066	50	50	50

Notes: Reports difference in means between treatment and control groups, with control group created based on regression results reported in Table 2 and matching performed using local-linear and five-nearest neighbors algorithms. * indicates significant at the 10% level, ** at the 5% level and *** at the 1% level

Figure 1a
Number of Countries
Adopting Each Policy, 1997 - 2001

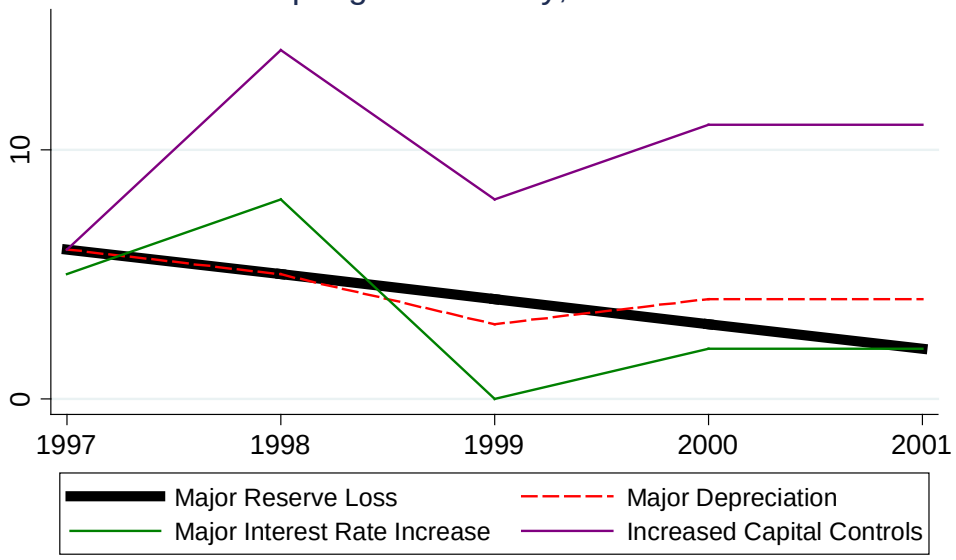


Figure 1b
Number of Countries
Adopting Each Policy, 2007 - 2011

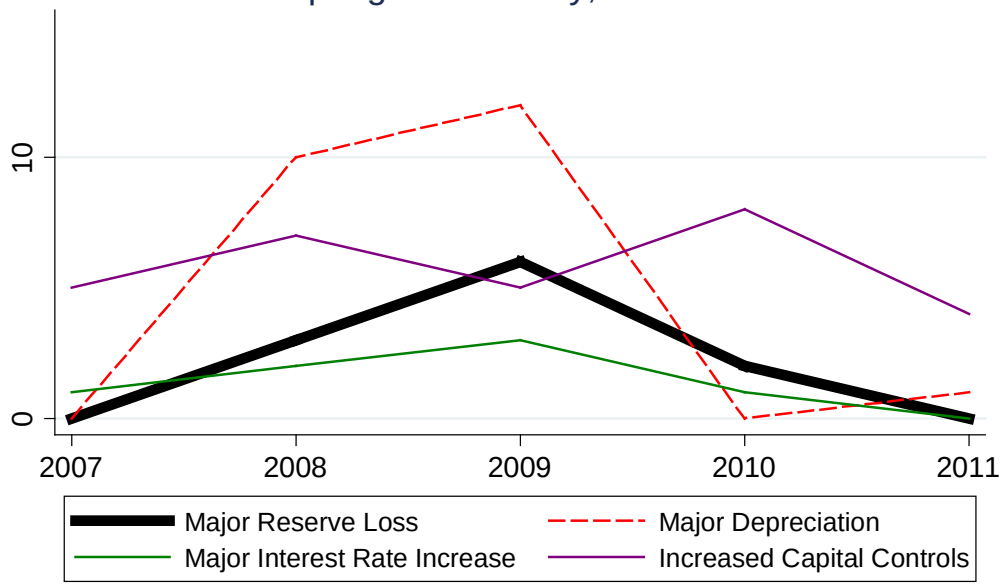
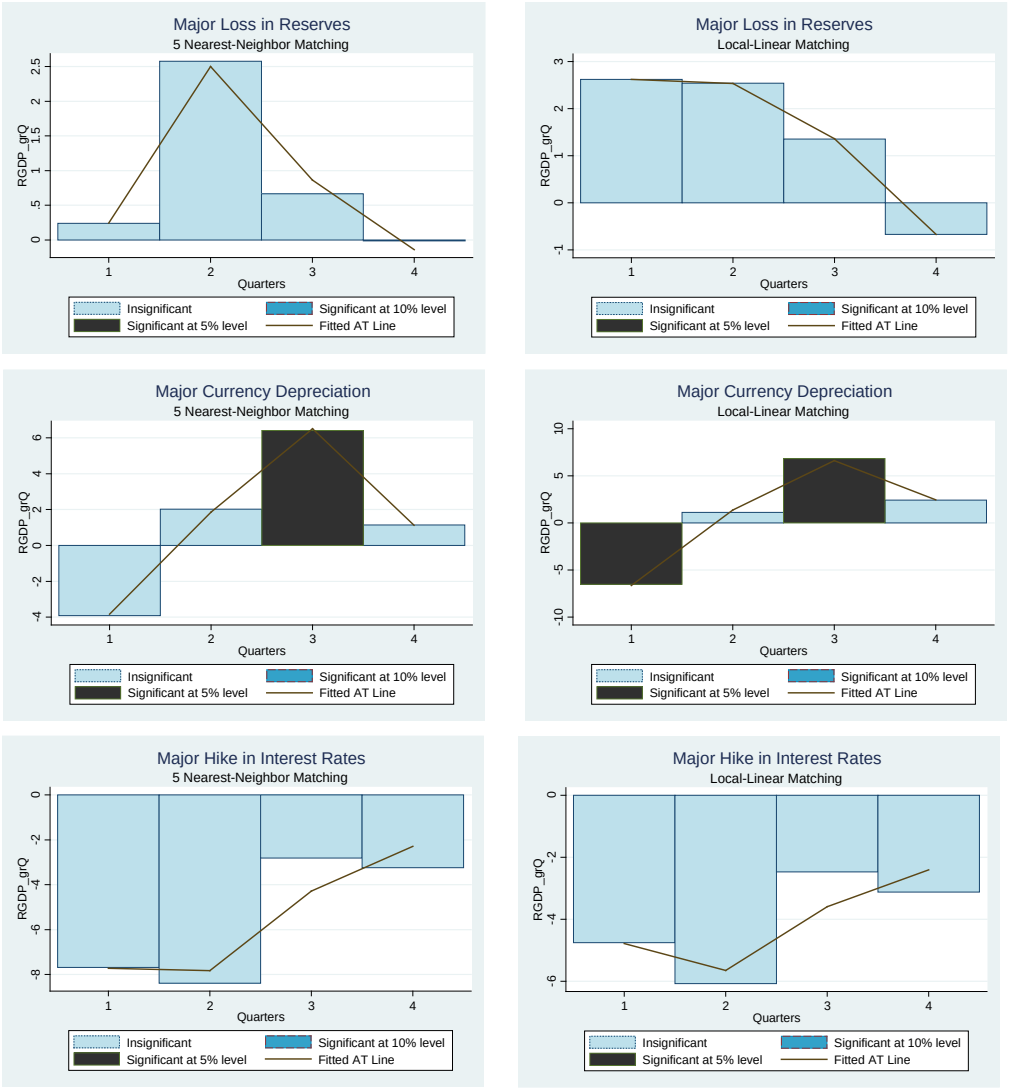


Figure 2:
Average Treatment Effects on Real GDP Growth

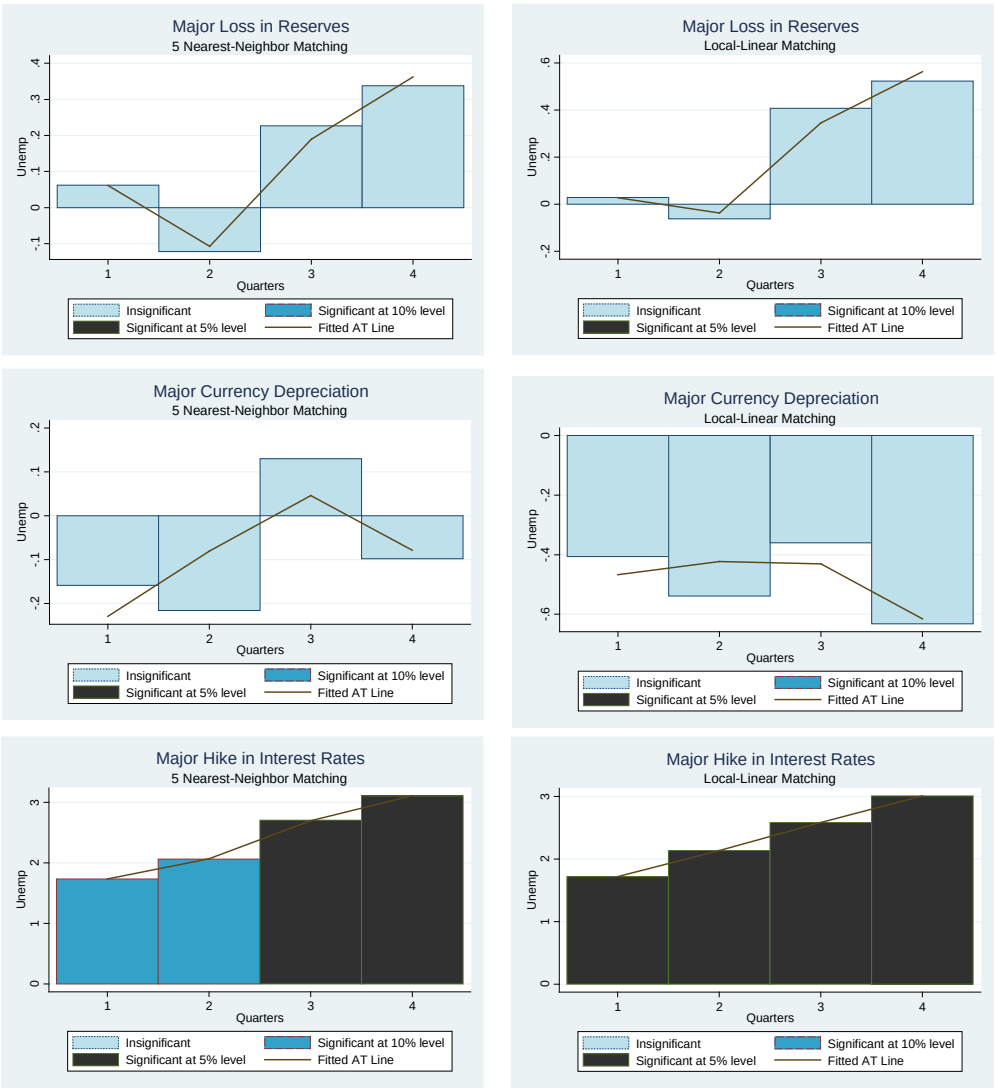
Five-Nearest Neighbors Matching | **Local-Linear Matching**



Notes: Average treatment effect measured as the change in the relevant quarter relative to the four-quarter average before the treatment occurred.

Figure 3:
Average Treatment Effects on Unemployment

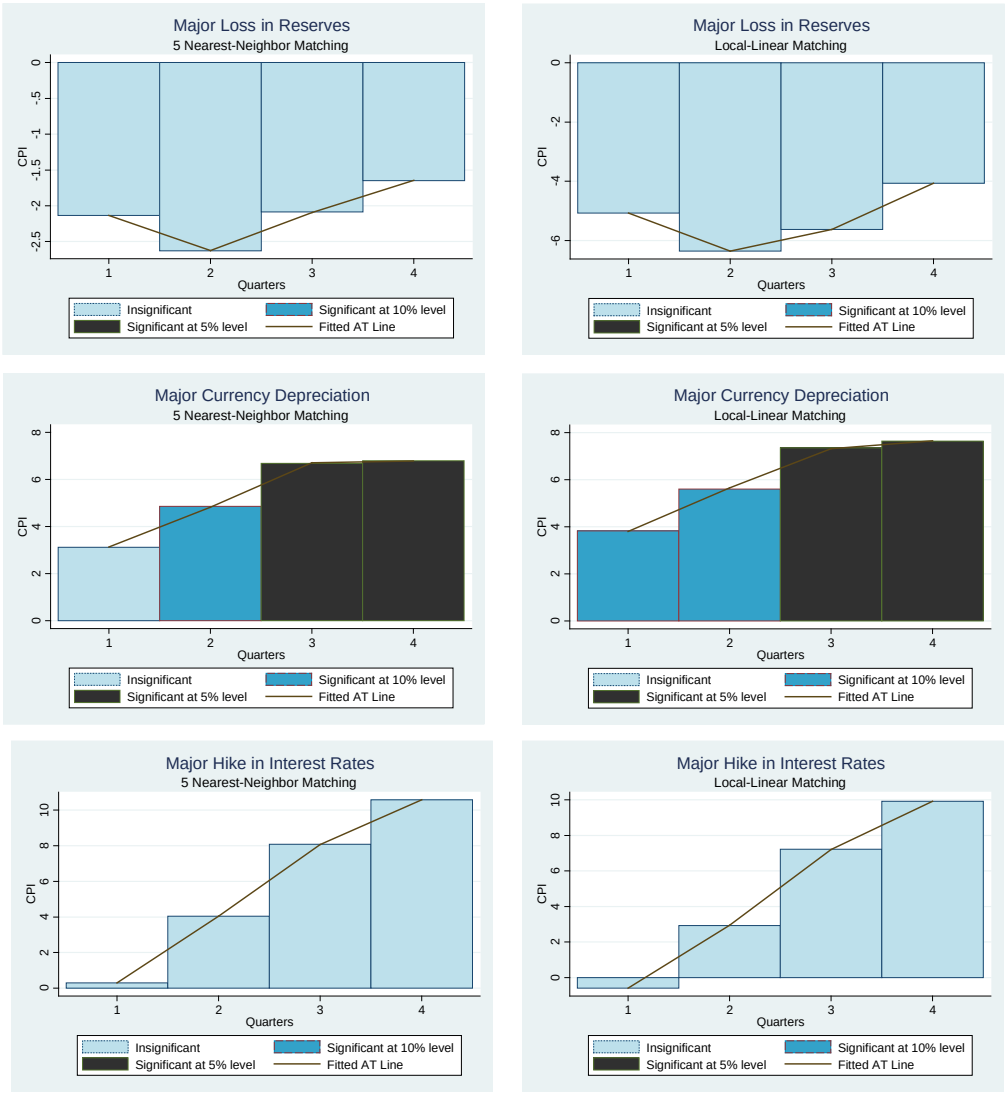
Five-Nearest Neighbors Matching | **Local-Linear Matching**



Notes: Average treatment effect measured as the change in the relevant quarter relative to the four-quarter average before the treatment occurred.

Figure 4:
Average Treatment Effects on Inflation

Five-Nearest Neighbors Matching | **Local-Linear Matching**



Notes: Average treatment effect measured as the change in the relevant quarter relative to the four-quarter average before the treatment occurred.

Data Appendix:

Countries in the Sample

Argentina	Ecuador	Jamaica	Namibia	Slovenia
Australia	Egypt	Japan	Netherlands	South Africa
Austria	Estonia	Jordan	New Zealand	Spain
Bahrain	Finland	Kazakhstan	Nigeria	Sri Lanka
Bangladesh	France	Kenya	Norway	Sweden
Belgium	Germany	Korea	Oman	Switzerland
Botswana	Ghana	Kuwait	Pakistan	Taiwan
Brazil	Greece	Latvia	Panama	Thailand
Bulgaria	Hong Kong	Lebanon	Peru	Trinidad & Tobago
Canada	Hungary	Lithuania	Philippines	Tunisia
Chile	Iceland	Luxembourg	Poland	Turkey
China	India	Malaysia	Portugal	Ukraine
Colombia	Indonesia	Mauritania	Qatar	United Arab Emirates
Cote d'Ivoire	Iran	Mauritius	Romania	United Kingdom
Croatia	Ireland	Mexico	Russia	United States
Czech Republic	Israel	Moldova	Singapore	Vietnam
Denmark	Italy	Morocco	Slovak Republic	Zambia

Variable Name	Definition	Data Source and Frequency
res	International Reserves, includes gold, IMF loans, SDRs, some SWFs, and drawn swap lines. In millions of US$.	IMF, IFS CD-ROM, June 2013 (code ..1..DZF), 1990-2012 (Q), data for Vietnam from Haver
resXg	International Reserves (see above) excluding gold; In millions of US$.	IMF, IFS CD-ROM, June 2013 (code ..1L..DZF), 1990-2012 (Q), data for Taiwan is NA so instead use res (reserves including gold)
res_gdp	International reserves (res) as a share of gdp (NGDP_D), all in millions of US$	Calculated
res_actCh	Change in foreign exchange reserves resulting from active management; excludes any changes in reserves from valuation changes, interest income, Gold, IMF loans, SDRs, SWGs and swap lines. In mn $US, monthly 2000-2010(q2)	Dominguez, Hashimoto and Ito (2012), based on IMF SDDS data
ER	Units of local currency per dollar, end-of-period	IMF, IFS, accessed 07/09/13
kaopen	Measure of capital account openness, constructed as the principal components from IMF AREARs data, with higher value indicating greater openness , annual from 1970-2011	Chinn and Ito (2013), with data updated through end-2011 from their website
fci, fco	Inflow (i) and outflow (o) controls on commercial credits, including banks, annual 1995-2011	Klein (2012)

dii, dio	Inflow (i) and outflow (o) controls on direct investment, annual 1995-2011	Klein (2012)
eqi, eqo	Inflow (i) and outflow (o) controls on equities (ex. FDI), annual 1995-2011	Klein (2012)
boi, boo	Inflow (i) and outflow (o) controls on debt securities of maturity >1 year, annual 1995-2011	Klein (2012)
mmi, mmo	Inflow (i) and outflow (o) controls on debt securities, maturity <=1 year, annual 1995-2011	Klein (2012)
cio, cio	Inflow (i) and outflow (o) controls on collateralized investments (eg, mutual funds, investment trusts), annual 1995-2011	Klein (2012)
peg	Dummy variable equal to 1 if country has a +/- 2% peg, annual from 1970-2011	Klein and Shambaugh (2013), updating Shambaugh (2004)
vxo	S&P 100 OEX Volatility Index, end-of quarter from 1990-2012	Global Financial Data, accessed 07/11/13
RGDP_pcA	Real GDP per capita, annual basis, constant 2005$	World Bank, World Development Indicators, accessed 07/13
RGDP_grA	Real GDP growth, annual basis, y-o-y growth in total GDP, measured in constant 2005$	World Bank, World Development Indicators, accessed 07/13
RGDP_pcQ	Real GDP per capita, quarterly basis, from 1995-2011, constant 2005$; if quarterly data is not available than data reported at an annual basis is substituted	IMF, IFS, accessed 07/12/13
RGDP_grQ	Real GDP growth, quarterly, from 1995-2011 Q-o-q growth in total GDP; constant 2005$; if quarterly data is not available than data reported at an annual basis is substituted	IMF, IFS, accessed 07/12/13
NGDP_D	Nominal GDP in US$ (in millions); if quarterly data is not available than data reported at an annual basis is substituted	IMF, IFS, accessed 07/09/13
Unemp	Unemployment rate	IMF, IFS, accessed 07/09/13
CPI	% change in consumer price index relative to the previous year, quarterly basis from 1995-2011, quarterly; data for Hong Kong and Chile is annual	IMF, IFS, accessed 07/09/13
CPI_US	% change in consumer price index relative to the previous year for the United States, quarterly basis from 1995-2011	IMF, IFS, accessed 07/09/13
intrate	Policy interest rate related to monetary policy for each country (line 60a); if policy rate is not available, we use the Treasury bill rate (line 60c); if neither of these is available, we use the money market rate (line 60b); for euro area countries, the euro area rate is used if a country rate is not available; quarterly basis from 1995-2011	IMF, IFS, accessed 07/16/13
intrate_US, intrate_Jap,	The policy interest rate (intrate) for either the United States, Japan or Euro area	IMF, IFS, accessed 07/16/13

intrate_Euro		
outflows, outflows_gdp	Total Capital Outflows (the sum of outflows of direct investment abroad, portfolio investment assets, other investment assets, and reserves and related items), in million of US$; data from older BOP series as of 2011 and not revised definitions , reported with a positive sign (unlike BOP accounting); also expressed as share of gdp	Forbes and Warnock (2012), originally from IMF, BOP as of 2011, with additional country information added from original sources
inflows, inflows_gdp	Total Capital Inflows (the sum of outflows of direct investment in the reported economy, portfolio investment liabilities, and other investment liabilities), in million of US$; data from older BOP series as of 2011 and not revised definitions, also expressed as share of gdp	Forbes and Warnock (2012), originally from IMF, BOP as of 2011, with additional country information added from original sources
netflows, netflows_gdp	Net capital flows; the sum of capital inflows (inflows) and capital outflows (outflows), also expressed as share of gdp	Forbes and Warnock (2012), originally from IMF, BOP as of 2011, with additional country information added from original sources
CA, CA_gdp	Current account balance (also expressed as share of GDP), in millions of US$, annual basis	IMF, WEO database, accessed 07/17/13
LStrength, LawOrder, InvProfile, GovStab, Corrupt BurQual LegComp	Measures of institutional quality; with lower values indicating weaker institutions; variables represent legal strength, law & order, investment profile, government stability, corruption and bureaucracy quality; Legal compliance is an index = sum of LawOrder, BurQual, and LStrength (with a value ranging from 0 to 14)	ICRG compiled by the World Bank; Entire list of variables available (including definitions): http://www.prsgroup.com/VariableHelp.aspx ICRG Methodology: http://www.prsgroup.com/ICRG_Methodology.aspx
instit, ln_instit, ln_instit_sq	Institutional variable index (including log and log squared terms); calculated as the average of each of the 6 ICRG institutional variables (described above), with each weighted by the maximum value of the variable	