Title

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Abstract

Are countries with unregulated capital flows more vulnerable to currency crises? Efforts to answer this question properly must control for "self selection" bias since countries with liberalized capital accounts may also have more sound economic policies and institutions that make them less likely to experience crises. We employ a matching and propensity score methodology to address this issue in a panel analysis of developing countries. Our results suggest that, after controlling for sample selection bias, countries with liberalized capital accounts experience a lower likelihood of currency crises. That is, when two countries have the same likelihood of allowing free movement of capital (based on historical evidence and a very similar set of economic and political characteristics)—and one country imposes controls and the other does not-- the country without controls has a lower likelihood of experiencing a currency crisis. This result is at odds with the conventional wisdom and suggests that the benefits of capital market liberalization for external stability are substantial.

The views presented in this paper are those of the authors alone and do not necessarily reflect those of the Federal Reserve Bank of San Francisco or the Board of Governors of the Federal Reserve System. Please address correspondence to Michael Hutchison at hutch@ucsc.edu.

1. Introduction

The benefits and costs of liberalizing administrative and legal controls on international capital flows have been the subject of renewed debate in recent years. Some studies suggest that eliminating or reducing the extent of these types of controls and restrictions can lower the cost of capital, promote portfolio diversification and risk sharing, and/or reduce microeconomic distortions, thereby improving investment, productivity, and growth.[1] Nonetheless, supporters of capital controls argue that they can yield benefits by reducing country vulnerability to volatile capital flows and currency crises. Recent examples of emerging markets that liberalized their capital accounts and subsequently experienced currency crises in the 1990s are often cited to support this view. For example, the crises of Mexico (1994-95) and of Asia (1997-98) are often attributed to premature liberalization of international capital flows.[2]

While there is an extensive empirical literature measuring the effects of capital account liberalization on particular economic variables—e.g. capital flows, interest differentials, inflation, and output—surprisingly little systemic work has been undertaken regarding its impact on exchange rate stability in developing countries. Several papers have investigated the relationship of exchange rates and capital controls and/or capital account liberalization for a few selected countries (e.g. Edison and Reinhart, 2001a, 2001b; Edwards, 1999; Gregorio et al, 2000), while Glick and Hutchison (forthcoming) have done so for a broad set of developing and emerging market economies.

In general, these studies find little effect of capital controls in averting currency crises, at least not without supporting economic policies. They typically have found capital controls to be ineffective, distortionary, and/or counterproductive in the sense of signaling inconsistent and poorly designed government policies that may induce capital flight (see Bartolini and Drazen, 1997a). Glick and Hutchison (forthcoming) in fact find a significant positive correlation between capital controls and the occurrence of currency cries. Specific examples supporting these findings are commonplace—Malaysia experienced a currency crisis in late 1997, despite having re-imposed capital controls a year earlier; El Salvador experienced crises in 1986 and again in 1990 despite having controls, while not having a crisis when controls were liberalized in 1996-

[1] While there is agreement that capital market integration is qualitatively beneficial over the long run, there is much debate about the magnitude of these benefits; e.g., see Gourinchis and Jeanne (2003).
[2] The appropriate pace of deregulation of domestic financial markets also has been of concern, even in many industrial countries. The United States, Japan, and Sweden, among others, all have experienced some domestic financial instability following deregulation of domestic financial institutions.

97; Kenya has had six currency crises since 1975 despite having controls over most of this period; and so on. Dooley (1996), summarizing the literature, concludes: "Capital controls or dual exchange rate systems have been effective in generating yield differentials, covered for exchange rate risk, for short periods of time, but they have little power to stop speculative attacks on regimes that were seen by the market as inconsistent" (p. 677).

One possible explanation of why capital controls may be associated, not with a lower vulnerability, but in fact with a greater vulnerability to currency crises concerns the special characteristics and "self selection" of countries that choose to liberalize their capital accounts. Countries with macroeconomic imbalances, financial weaknesses, political instability, and/or institutional problems may choose to retain capital controls in order to avoid difficult economic reforms or to avoid capital outflows that may trigger a crisis. Conversely, countries with sound macroeconomic and political environments and more robust financial systems and institutions are not only less likely to experience crises, but also may be less likely to enact capital controls and forego the benefits of free capital flows. Consequently, countries with open capital accounts may be less prone to financial crises, both domestic and international in origin. Although capital controls may reduce country vulnerability to crises in some cases, capital account liberalization can still be associated with a lower overall likelihood of financial crises.

A particular source of concern for empirical analysis arises when the policy choice to establish or maintain an environment with a liberalized capital account is correlated with macroeconomic, financial, and institutional policy variables that in turn lower the likelihood of currency crises. Specifically, estimation of the likelihood of crises may yield a biased measure of the effect of capital controls because of sample selection bias, i.e., systematic differences between countries that do and do not liberalize the capital account.[3] In light of possible sample selection bias for the group of countries that maintain a liberalized capital account-- and the fact that studies to date have not dealt with this issue -- can we put much faith in prior empirical findings that free movement of capital reduces a country's vulnerability to currency crises?

In this paper we address the sample selection problem by employing the matching and propensity score methodology that was developed precisely for the bias associated with this type

[3] Glick and Hutchison (forthcoming) control for a host of economic, political, and institutional factors usually associated with currency instability and capital controls. They also develop an empirical model of the factors explaining governments' decisions to maintain capital controls, jointly explaining this decision with the onset of currency attacks through bivariate probit estimation. However, they do *not* formally address the issue of sample selection bias.

of estimation problem. In particular, we apply the matching methodology developed to help account for the estimation bias arising from the “selection on observables” problem, which to date has mainly been applied in the medical and labor economics literature.[4] The basic idea is straightforward. Each participation observation is matched to a non-participation observation that has the same observed values of a vector of other characteristics that determine participation. Under certain standard assumptions, the difference in the observed outcome between the two matched observations is thus the program’s effect. As Heckman et al. (1997) state: “…simple balancing of observables in the participant and comparison group samples goes a long way toward producing a more effective evaluation strategy” (p. 607).

This paper evaluates the effect of an environment with liberalized capital flows on the likelihood of currency crises using several recently developed matching methods designed to deal with sample selection bias. In particular, we use “nearest neighbor”, “radius,” and “stratification” matching methods, as well as a “regression-adjusted” matching estimator suggested by Heckman et al. (1997)—all methods designed to account for selection on observables bias. Our analysis suggests that, even after controlling for sample selection bias (and obtaining unbiased estimates), a liberalized capital account is associated with a lower likelihood of currency crises. That is, when two countries have the same likelihood of allowing free movement of capital (based on historical evidence and a very similar set of identical economic and political characteristics at a point in time)—and one country imposes controls and the other does not-- the country without controls has a lower likelihood of experiencing a currency crisis.

The plan of the paper is as follows. Section 2 discusses the matching methodology in more detail and its application to the problem at hand. Section 3 discusses construction of the key variables in our analysis – measures of currency crises and of capital account liberalization – and gives descriptive statistics. Section 4 presents empirical results concerning calculation of the propensity scores used in creating the matched samples as well as estimation of the probability of currency crisis equations used in regression-adjusted matching methods. Section 5 presents the

[4] The selection bias problem typically addressed in the medical and health care literature arises when the patients with worse health problems seek out the better doctors and facilities. In assessing treatment effectiveness, matching methods are employed to control for the downward bias associated with the lower survival rates of these patients. Persson (2001) and Hutchison (2004) are exceptions in the macroeconomics literature by applying the matching methodology to investigations of, respectively, the effect of currency unions on trade growth and the effect of IMF program participation on output growth.

main results of the paper measuring the effect of capital account liberalization on currency crises while controlling for selection bias. Section 6 concludes the paper and draws policy implications.

2. Matching Methodology

The advantage of matching methods is that they address the problem of non-random sample selection and, as a non-parametric statistical method, avoid strong assumptions about functional form[5]. To examine the effect of capital account liberalization on the occurrence of currency crises we employ three matching algorithms—nearest neighbor, stratification, and radius matching—These different approaches all match observations with *similar characteristics,* excepting that one group of countries liberalizes capital controls (the "treatment group") and the other does not (the "control group"). Following the matching of observations, we assess the "treatment effect" by measuring the difference in the frequency of currency crises between the two groups.

In order to assess similarity among countries and construct the samples of countries *with* and *without* liberalized capital accounts (the "participation" and "non-participation" observations, respectively), we consider a set of observable country characteristics. One approach is to match each participation observation with a non-participation observation that has exactly the same observed values of a vector of other characteristics that determine participation (X). In macroeconomic studies, where the size of the sample is typically limited, this matching method is difficult or impossible to implement. Fortunately, Rosenbaum and Rubin (1983 and 1985) have shown that, if the probability of participation – $P(X)$ – is known, then matching by $P(X)$ instead of X is sufficient. This collapses the multidimensional problem of matching to one dimension based on the estimated probabilities or *propensity scores* and greatly simplifies the procedure. Rubin and Thomas (1992) show that using an estimated probability of participation $\tilde{P}(X)$ based on the set of observable characteristics, instead of $P(X)$, still reduces selection-on-observables bias. When two countries have a similar propensity score, they are paired according to one of the following three matching criteria.

The *nearest neighbor* approach matches each participation observation to the non-participation observation that has the nearest propensity score. After each non-participation observation is used, it is "returned" to the pool of non-participation observations. The treatment

[5] See Persson (2001) for an excellent review of matching methodology and an application with macroeconomic data.

effect is computed as a simple average of the differences in outcomes across the paired matches. The *radius* approach matches each participation observation to the average of all the non-participation observations with propensity scores falling within a pre-specified radius from the propensity score of the participation observation[6]. In this case, the treatment effect is again computed as an average of the difference in outcomes, but with weighting according to the number of non-participation observations used in the construction of each matched pair. The *stratification* approach divides the sample into several groups, or strata, based on their propensity scores. Within each stratum, the average of the participation observations is matched with the average of the non-participation observations. An average of the difference in outcomes of the strata, weighted by the number of participation observations in each one, is then calculated to create the treatment effect. In all three cases, weighted standard errors are constructed as described in the appendix of Persson (2001).[7]

Following Rubin (1979) and Heckman et al. (1997, 1998), we also implement *regression-adjusted* variants of our matching estimators. While the aforementioned matching methodologies do not impose any structure on the currency crisis equation, biases can result from omitted variables that are correlated with both the outcome (the occurrence of currency crises) and the treatment (liberalization of capital controls). Both consistency and efficiency may be improved by implementing a regression-adjusted estimator. Rubin (1979) suggests that the outcome regression should contain all available observations, while Heckman et al. (1997) conclude that estimation using only the non-participation observations (i.e., those with capital controls, in our case) is preferable. We employ the method of Heckman et al. (1997). The residuals from this regression are then used in our three matching methods.

[6] More specifically, for radius of magnitude r, each participation observation with an estimated propensity score $\tilde{n}$ is matched with all the non-participation observations whose propensity scores (q) satisfy the condition $\tilde{n}-r<q<\tilde{n}+r$. Following Persson (2001), we use a value of r=.05 as our benchmark value.

[7] The nearest neighbor and radius approaches are each implemented in Dehejia and Wahba (2002), who also employ a version of the stratification method to estimate propensity scores. All three methods are implemented in Persson (2001).

3. Data Construction and Descriptive Statistics

3a. Defining Currency Crises

The objective of this study is to evaluate the effect of capital account liberalization on the incidence of currency crises for a panel of developing countries. We include developing countries that both did and did not experience a severe currency crisis/speculative attack during the 1975-97 sample period. Using such a broad control group allows us to make inferences about the conditions and characteristics distinguishing countries encountering crises and others managing to avoid crises. The minimum data requirements to be included in our study are that GDP are available for a minimum of 10 consecutive years over the period 1975-97. This requirement results in a sample of 69 developing countries.

To identify currency crises we construct a measure of monthly exchange rate pressure and date each by the year in which it occurs. Specifically, currency crises are defined as "large" changes in a monthly index of currency pressure, measured as a weighted average of monthly real exchange rate changes[8] and monthly (percent) reserve losses.[9] Following convention (e.g. Kaminsky and Reinhart, 1999), the weights attached to the exchange rate and reservation components of the currency pressure index are inversely related to the variance of changes of each component over the sample for each country.[10] The exchange rate and reserve data are drawn from the International Monetary Fund's *International Financial Statistics* CD-ROM (lines ae and 11.d, respectively).

Our measure presumes that any nominal currency changes associated with the exchange rate pressure should affect the purchasing power of the domestic currency, i.e. result in a change in the real exchange rate (at least in the short run). This condition excludes some large

[8] Real exchange rate changes are defined in terms of the trade-weighted sum of bilateral real exchange rates (constructed in terms of CPI indices, line 64 of the *IFS*) against the U.S. dollar, the German mark, and the Japanese yen, where the trade-weights are based on the average of bilateral trade with the United States, the European Union, and Japan in 1980 and 1990 (from the IMF's *Direction of Trade*). Most panel studies of currency crises define the currency pressure measure in terms of the bilateral exchange rate against a single foreign currency. For example, Kaminsky, Lizondo, and Reinhart (1998) and Kaminsky and Reinhart (1999) measure the real exchange rate for all of the developing countries in their sample against the U.S. dollar. In defining the effective rate in terms of the three major nations likely to be main trading partners of most developing countries, our approach provides a broader measure than these other studies and is computationally easier to construct than a multilateral exchange rate measure defined in terms of all of a country's trading partners.

[9] Ideally, reserve changes should be scaled by the level of the monetary base or some other money aggregate, but such data is not generally available on a monthly basis for most countries.

[10] Our currency pressure measure of crises does not include episodes of defense involving sharp rises in interest rates. Data for market-determined interest rates are not available for much of the sample period in many of the developing countries in our dataset.

depreciations that occur during high inflation episodes, but it avoids screening out sizable depreciation events in more moderate inflation periods for countries that have occasionally experienced periods of hyperinflation and extreme devaluation.[11] Large changes in exchange rate pressure are defined as changes in our pressure index that exceed the mean plus 2 times the country-specific standard deviation, provided that it also exceeds 5 percent.[12] The first condition insures that any large (real) depreciation is counted as a currency crisis, while the second condition attempts to screen out changes that are insufficiently large in an economic sense relative to the country-specific monthly change of the exchange rate.

For each country-year in our sample, we construct a binary measure of currency crises, as defined above (1 = crisis, 0. = no crisis). A currency crisis is deemed to have occurred for a given year if the change in currency pressure for any month of that year satisfies our criteria (i.e. two standard deviations above the mean as well as greater than five percent in magnitude). To reduce the chances of capturing the continuation of the same currency crisis episode, we impose windows on our data. In particular, after identifying each "large" monthly change in currency pressure, we treat any large changes in the following 24-month window as part of the same currency episode and skip the years of that change before continuing the identification of new crises. With this methodology, we identify 160 currency crises over the 1975-97 period. Appendix C lists the countries included in the sample and corresponding currency crisis dates, if any.

3b. Measuring Liberalization of Restrictions on International Payments

The underlying source for our measures of capital account liberalization are data on external restrictions in the IMF's *Annual Report on Exchange Arrangements and Exchange Restrictions* (*EAER*). A country is classified as either "liberalized (value of unity) or "restricted (value of zero) depending on the existence of controls on the capital account at year-end. Specifically, for the 1975–94 period the *EAER* coded countries (published in the reports through

[11] This approach differs from that of Kaminsky and Reinhart (1999), for example, who deal with episodes of hyperinflation by separating the nominal exchange rate depreciation observations for each country according to whether or not inflation in the previous 6 months was greater than 150 percent, and they calculate for each sub-sample separate standard deviation and mean estimates with which to define exchange rate crisis episodes.
[12] Kaminsky and Reinhart (1999) use a three standard deviation cut-off. While the choice of cut-off point is somewhat arbitrary, Frankel and Rose (1996) suggest that the results are not very sensitive to the precise cut-off chosen in selecting crisis episodes.

1995) for the existence (or not) of "restrictions on payments for capital transactions." From 1996, the *EAER* (starting with the 1997 Annual Report) reported 10 separate categories for controls on capital transactions (11 categories in the 1998 Annual Report). We defined the capital account to be restricted for the 1996-97 observations (i.e. not liberalized) if controls were in place in 5 or more of the *EAER* sub-categories of capital account restrictions *and* "financial credit" was one of the categories restricted.[13]

We are aware of concerns about the quality of the IMF data on capital account liberalization. By providing only a dichotomous indication of the existence of administrative controls, they are limited in their ability to measure the extent to which restrictions are applied and enforced over time and across countries. Nor do they clearly distinguish between restrictions on capital inflows and outflows. However, the IMF measures are the only source of data available that can be collected with some consistency across a broad group of developing countries and over a reasonably long period of time. This is a constraint faced by any panel study in this literature.[14] Glick and Hutchison (forthcoming) consider alternative balance of payment restriction indicators, including controls on export receipts or current account transactions, as well as domestic financial controls. They find that while these alternative measures differ somewhat in indicating the presence of controls for individual countries, their results were not sensitive to the particular measure used: countries without restrictions, however measured, were always less prone to currency crises.

3c. Descriptive Statistics on Currency Crises and Capital Account Liberalization

Table 1 shows the frequency of country-years with currency crises and capital account liberalization over the 1975–97 period, and by 5-year intervals (except for the 1995–97 sub-sample). The table reports the unconditional frequency of currency crises and liberalization observations (i.e., the number of "crisis" or "liberalization in place" observations, divided by the total number of observations).

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[13] The 11 classifications under capital restrictions reported in the 1998 *EAER* were controls on: (1) capital market securities, (2) money market instruments, (3) collective investment securities, (4) derivatives and other instruments, (5) commercial credits, (6) financial credits, (7) guarantees, sureties, and financial backup facilities, (8) direct investment, (9) liquidation of direct investment, (10) real estate transactions, and (11) personal capital movements.
[14] See Edison *et al* (2002) for a comparison of different measures of capital controls in the context of an analysis of the effects of capital account liberalization on long-run economic growth.

The 69 developing countries in our dataset experienced 160 currency crises over the 1975–97 period, implying a frequency of 11.7 percent of the available country-year observations. Crises were least frequent during the 1975–79 period (9.9 percent average frequency) and most frequent during the 1985–89 period (14.3 percent frequency). The frequency of crises in the most period of our sample, 1995-97, was only 9.7 percent. Thus, in our sample, the spate of currency crises around the world in the latter half of the 1990s does not indicate a rise in the frequency of currency crises over time.[15]

Table 1 also reports the frequency with which liberalized capital accounts were in place during the period. Liberalized capital flows were relatively infrequent, accounting for only 16.2 percent of the observations. Although this frequency was always low during the sample period, it fell noticeably from 1975 through 1989, before rising n the 1990s. The low point was an average frequency of 11.0 percent during 1985–89, and the high point was 23.8 percent during 1995-97.

3d. Currency Crisis Frequencies Conditional on Capital Account Liberalization

Table 2 shows the frequency of currency crises conditional upon a country's having liberalized capital flows. This table sheds light directly upon the main question of interest: whether liberalization of capital flows affects the probability of a currency crisis. To take account of the possibility that controls are implemented in response to a crisis, we report results conditional on the absence of controls at the end of the year *prior* to a crisis as well as at the end of the year in which a crisis occurs. χ^2 statistics for tests of the null hypothesis of independence between the frequency of crises and whether liberalization was in place are also presented.

The most striking result from Table 2 is that the country-year observations associated with fewer restrictions on capital flows have substantially lower frequencies of currency crises than those observations where controls were in place. Specifically, countries with liberalized capital flows had crises contemporaneously 6.8 percent of the time, compared to 12.7 percent for those with restrictions. The χ^2 statistics reject the null of independence and indicate that this difference is significant (at better than 5 percent). The difference in currency crisis frequency according to whether the capital account was liberalized or not in the preceding year is smaller

[15] Currency crises were most frequent in Africa (16.2 percent frequency), and least frequent in Asia (9.6 percent). Despite recent high profile currency crises in Thailand, Malaysia, Indonesia, and Korea, the developing economies in Asia have been less frequently affected by currency instability.

(8.0 percent versus 12.5 percent), but is still significant at the 10 percent level. This is suggestive *prima facie* evidence that controls may not be effective and, indeed, may increase the likelihood of a currency crisis (e.g. Bartolini and Drazen, 1997a). It suggests that the presence of capital controls does not reduce a country's exposure to currency instability.

4. Preliminaries: Estimating Propensity Score and Currency Crisis Equations

4a. Propensity Scores

In controlling for sample selection bias, a benchmark probit equation explaining the likelihood of a country having a liberalized capital account is estimated to calculate propensity scores. We consider a number of potential structural, political, and economic determinants of capital account liberalization. The selection of these potential variables is guided by previous literature in this area. Alesina, Grilli, Milesi-Ferretti (1994), Bartolini and Drazen, (1997a, b), Glick and Hutchison (forthcoming) and Grilli and Milesi-Ferretti (1995), for example, present empirical results on a number of possible determinants of capital controls (and/or capital account liberalization). They find countries with a higher level of government expenditure, more closed to international trade, and with larger current account deficits are more likely to restrict capital flows. Grilli and Milesi-Ferretti (1995) also report evidence that political instability is associated with fewer capital account restrictions in developing economies. Bartolini and Drazen (1997b) link a high degree of restrictions on international payments in developing economies with high world real interest rates—measured as the weighted real interest rate in the G-7 industrial countries—in a yearly time-series regression. They view the causality as running from world interest rates to capital restrictions: restrictions are removed when the cost of doing so is low, i.e. only a small outflow of capital is expected when world interest are low. Edwards (1989), investigating the experiences of twenty countries over the 1961-82 period, finds that capital controls are frequently intensified in the year prior to the onset of a currency crisis. This suggests that a common set of factors may contribute both to the onset of a currency crisis and lead governments to impose or maintain capital account restrictions, or conversely liberalize their capital accounts.

Following these studies, we include two macroeconomic variables, two economic structure variables, and two political variables in our benchmark probit selection equation. The macroeconomic variables are the current account (as a percent of GDP) and the level of

"Northern" real interest rates (proxied by the level of the U.S. real long-term interest rate). The economic structure factors considered are the relative size of government spending and openness to world trade (measured by the sum of exports and imports as a percentage of GDP). These macroeconomic data series are taken from the International Monetary Fund's IFS CD-ROM. The two political explanatory variables included in our model are the total changes in government and a measure of political freedom.[16]

In addition to these variables, we also estimate an augmented probit selection equation that includes two additional variables – the previous history (i.e., the lagged occurrence) of currency crises and of capital account liberalization. We choose to be selective in not adding more variables, however, following the observation of Heckman and Navarro-Lozano (2004) that adding more observables other than the determinants of the selection equation may not help reduce bias of the treatment effect estimator, especially when information is lacking on the correlation between the unobservables from the selection equation and those from the currency crisis equation.

Appendix A presents our two probit models used to predict the likelihood of capital account liberalization. In the benchmark specification reported in column (1), larger current account surpluses, greater trade openness, higher world interest rates, frequent changes in governments, and more economic freedom are associated with a higher likelihood that capital account liberalization is in place. Higher levels of government spending are associated with a lower likelihood of liberalization. All coefficient signs, except for that on the interest rate, are consistent with priors.

Column (2) reports the augmented specification, with dummies for the occurrence of a currency crisis in the preceding year and for the presence of capital account liberalization in the preceding year included as explanatory variables. A currency crisis in the previous year has a negative effect on the likelihood of liberalization in the current year, though the effect is insignificant. The presence of liberalized capital controls in the preceding year significantly raises the likelihood of liberalization in the current year. The inclusion of lagged capital liberalization in the equation reduces the significance level of some explanatory variables

[16] The total number of democratic and undemocratic (e.g. coups) changes in government over the period 1970-97 was determined from Zarate's Political Collections website (www.terra.es/personal2/monolith), supplemented by information from the Encarta Encyclopedia website (www.encarta.msn.com). The political freedom measure is taken from the Freedom House website (www.freedomhoouse.com); the variable is measured on a 0-3 scale, with "0" indicating the highest level of freedom.

(current account, U.S. real interest rate, changes in government, freedom). This suggests that capital liberalization in place is the best indicator of future capital liberalization, with the other explanatory variables playing a secondary role. The importance of lagged dependent variables in models of this nature, however, begs the question of what are the *fundamental* factors that lead governments to impose or remove capital controls.

Both probit specifications predict the presence of capital controls very well, calling 98 percent of these observations correctly. However, the augmented model is much better at predicting observations with liberalization in place (90 percent are called correctly, compared to 11 percent for the benchmark model. Correspondingly, the pseudo-R2 of the augmented model is better as well (.77, compared to 0.20 for the benchmark model). It should not be surprising that lagged liberalization is a very good predictor of future liberalization, since the switch from a liberalized capital flows to a regime with capital controls and vice versa is not a common occurrence in the sample.

Table 3 shows summary statistics (mean values and standard errors) for economic and political variables in the treatment group (171 country-year observations with capital account liberalization in place) and the unmatched control group (822 observations with capital controls). We also present summary statistics for two alternative control groups—observations matched (using propensity scores derived from the probit equation explaining capital controls) by either the nearest neighbor method or the radius measure (with a radius magnitude of 0.05).

It is noteworthy that the mean values of the current account balance and government spending are lower, and trade openness is larger for the treatment group than in any of the control groups, implying economic fundamentals are better on average in countries with liberalized capital accounts. The U.S. interest rate is lower for the treatment group (with the exception of the nearest neighbor control group), suggesting that these countries benefited from the lesser attractiveness of investment opportunities in industrial economies. There are meaningful differences as well in the mean values of the political variables: governments change more often and the degree of political freedom is greater (since a lower index value implies greater freedom) in the treatment group.

Table 3 also reports that there is almost a 10 percentage point difference between the predicted likelihood (i.e., mean propensity) of the treatment group having liberalized capital accounts and that of the unmatched control group (0.295 versus 0.196). This is not surprising since by construction all observations in the treatment group have liberalized their capital accounts, while none of the observations in the control group have done so. Compared to the unmatched control group, the predicted likelihood of liberalized capital accounts is slightly higher for the two matched control groups -- 0.227 for the nearest neighbor procedure and 0.196 for the radius procedure, but still well below the mean of the treatment group (0.295).

Some examples of country/time observations with similar propensity scores, but different treatments and outcomes, may be informative in pointing out both the strengths and weaknesses of the matching methodology. These examples were “matches” using the nearest neighbor approach: (1) Bolivia had no capital controls in 1977 and an estimated probability (propensity score) of capital account liberalization of 0.400. Bolivia did not experience a crisis in that year. Thailand had a similar propensity score (0.396) in 1981, but did have capital controls in place and did experience a currency crisis. (2) Malaysia had a liberalized capital account in 1986, and a propensity score of 0.235. Malaysia experienced a currency crisis. India did have capital controls in 1995, but had an identical score (0.235)—and also experienced a currency crisis in that year. (3) Guatemala had a liberalized capital account in 1989 and a propensity score of 0.414, but experienced a currency crisis. Swaziland in 1991 had capital controls and an identical propensity score, but did not experience a currency crisis at that time.

These examples illustrate the fact that country experiences vary greatly across time, and the matching (nearest neighbor) procedure will pick out the observations with the closest likelihood of a liberalized capital account. As we shall show, the model when estimated across time and countries has good explanatory power and predictive characteristics. Nonetheless, at each point in time the conditions associated with (or without) a currency crisis in a particular country may differ greatly. (This is addressed in the regression-adjusted matching procedures below where a model of currency crises is employed). Moreover, there are many examples of matched observations of countries with and without capital controls associated with “low” as well as “high” propensity scores. For example, Indonesia had a liberalized capital account in 1983 but a relatively low propensity score (0.120); Ethiopia had capital controls in 1983 and an identical propensity score in 1983. But Ethiopia avoided a currency crisis, while Indonesia did not.

4b. Currency Crisis Equations

In order to generate Heckman et al.'s (1997, 1998) regression-adjusted matching estimators, it is necessary to specify an equation that controls for the factors—other than capital account liberalization—that may influence the occurrence of currency crises. We follow Glick and Hutchison (forthcoming) in identifying the variables for inclusion in the currency crisis equation. Their basic model includes five macroeconomic control variables (all are lagged to limit simultaneity problems). These variables are the log ratio of broad money to foreign reserves, domestic credit growth, the current account to GDP ratio, real GDP growth, and real exchange rate overvaluation[17]. Unlike their work (based on a locally linear regression technique), however, we use a linear probability model since Heckman et al. is essentially a two-stage estimation procedure where the first stage generates residuals to be used as the "outcome variable" in the second-stage matching test. The residuals from the linear probability model of currency crises may be interpreted in an intuitive way, with positive (negative) residuals associated with currency crises (periods of tranquility) that were unexpected based on the set of explanatory variables that are statistically good predictors of currency crises (periods of tranquility).

The first stage of the Heckman et al. method is based on a regression using the sample restricted to the control group, i.e. the (751) observations with capital controls. These results are reported in Appendix B. As expected, the M2/foreign reserves ratio and domestic credit growth are positively associated with currency crises. Current account surpluses, real overvaluation, and strong real GDP growth are associated with a lower frequency of currency crises.

5. Impact of Capital Account Liberalization on Currency Crises

5a. Unconditional Matching Results

We first estimate propensity scores from the benchmark selection equation and then employ nearest neighbor, radius, and strata matching methods to evaluate the impact of capital account liberalization on the frequency of currency crises. We term these results "unconditional

[17] The data are drawn from the IMF IFS CD-ROM: log ratio of broad money to foreign reserves (lines 34 plus 35 divided by 1ld times ae), domestic credit growth (line 32), the current account to GDP ratio (line 78ald times xrrf divided by 99b) real GDP growth (line 99b.r or 99b.p), and real exchange rate overvaluation. The latter variable is constructed as the degree of real exchange rate overvaluation from deviations from a fitted trend in the real trade-weighted exchange rate index, where the exchange rate index we fit is the annual average of the monthly series used in constructing the exchange rate component of our currency pressure index.

matching" since we compare the unconditional frequency of currency crises for observations without capital controls imposed with the matched set of observations with capital controls in place. Table 4 shows that the frequency of currency crises is significantly lower in countries with liberalized capital accounts than in the matched samples with capital controls; this result is invariant to the matching method employed. Specifically, the frequency of currency crises in countries with liberalized capital accounts, compared to those with capital controls, ranges from 4.84 percent lower with the radius method to 7.60 percent lower with the nearest neighbor method. These results are statistically (at the 5 percent level) and economically significant.

Table 5 undertakes two robustness checks using the unconditional matching procedure. The results from the analysis reported in Table 4 do not impose any restrictions that preclude matches between different year observations for the same country. In Table 5 we consider the possibility of correlation among observations from the same country—a potential source of estimation bias-- and impose the restriction that the match(es) for each observation in the treatment group are always drawn from a different country in the control group. We report the results of unconditional matching with this restriction for both the nearest neighbor and radius measures. We also consider as a robustness check the "tighter" radius parameter of 0.01 (as compared with the benchmark value of 0.05). This is a potentially important robustness check since the propensity scores are grouped rather compactly in the sample. A tighter radius parameter generates a control group that narrows differences —in terms of similarity of observed characteristics—with the matched treatment observations.

The results are generally unaffected in terms of point estimates, with the frequency of currency crises ranging from 5 to 7 percent lower for countries without capital controls; however, the significance levels are noticeably higher for the radius measure results, particularly with the tighter radius parameter, with both significant at better than 1 percent.

Overall, the negative treatment effects of liberalized capital accounts reported in both Table 4 and Table 5 suggest that countries with liberalized capital accounts are much less likely to experience a currency crisis. This impact is statistically significant for all matching methods. These results support previous work finding a negative (positive) link between capital account liberalization (control) and the onset of currency crises. In particular, using probit model estimates of the likelihood of a currency crisis, Glick and Hutchison (forthcoming) find that the marginal probability effect of contemporaneous capital controls is 11 percent in a simple bivariate equation and 8 percent when other explanatory variables are included. Their estimates fall to 9 percent and 5 percent, respectively, when capital controls are entered as lagged explanatory variables in the probit regression. Thus our matching methodology with the baseline model gives results of the same order of magnitude.

5b. Regression-adjusted Matching Results

The regression-adjusted matching method controls for factors other than capital account liberalization that may affect the likelihood of currency crises. As described in Section 4, we implement this approach by estimating currency crisis prediction equations (reported in Appendix A) for the sample of observations with capital controls, in accordance with the approach of Heckman et al. (1997). We then take the residuals ("unexplained currency crises") from the currency crisis equation and compare them for observations without and with capital restrictions, where the latter are constructed using our three matching method.

The results in Table 6 confirm the implications from our other matching methods: countries with less restrictive capital controls tend to be less vulnerable to speculative attacks. In fact, the results indicate that the estimated effect of liberalized capital accounts is dramatically stronger in terms of magnitude (i.e. more negative) and significance (all the results are significant at better than 1 percent). The frequency of currency crises is 15 percent lower in countries without capital controls than those with controls for the radius and strata matching methods, and 22 percent lower for the nearest-neighbor method. Thus conditioning our matches on the determinants of currency crises gives larger and more precise estimates of the effects of capital account liberalization.

5c. Robustness to Alternative Propensity Score Equations

Table 7 presents robustness tests using alternative propensity scores derived from our augmented probit model of capital account liberalization that includes lagged currency crises as well as lagged liberalization (shown in column 2 of Appendix Table A). As noted earlier, inclusion of the lagged capital liberalization measure increases the explanatory power of the selection model markedly—increasing the pseudo R2 from 0.20 to 0.77—because the switch from a regime with controls to one with free movement of capital (and vice versa) is relatively infrequent, i.e. being in a regime of liberalized capital flows is a very good predictor continuing in that regime in the following year. This alternative estimation equation for propensity scores gives much higher predictive power and better captures the dynamics of the system.

The results reported in Table 7 are much stronger than the benchmark unconditional matching results reported in Table 4, and similar in magnitude to the regression-adjusted matching results reported in Table 6. Specifically, the frequency of currency crises is 18 to 26 percent less in countries that have liberalized their capital accounts, much larger in magnitude than the 5 to 7 percent difference for the benchmark cases (and are comparable to the regression-adjusted results in Table 6). In addition, the results are more precisely estimated, with the statistical significance exceeding 1 percent with the nearest neighbor method (with a t-statistic of 6.1) and 5 percent for both the radius and strata methods (with t statistics of 2.25 and 2.14, respectively).

7. Concluding Remarks

Whether countries that allow international capital to flow freely— without substantial administrative controls on international payments—subject themselves to greater risk of currency and balance of payments turmoil is an important but unresolved policy issue. A theoretical literature and countless policy experiences suggests that countries with liberalized capital accounts may or may not be more susceptible to crises depending on a host of economic, administrative, and political factors. The empirical literature in this area is similarly mixed in terms of providing evidence both for and against the vulnerability of countries with liberalized capital flows. Unfortunately, this literature is not very helpful to economic policymakers seeking practical benchmarks in guiding their decisions on whether to liberalize capital accounts.

We argue that more attention needs to focus on the environment in which countries liberalize their capital accounts— freedom of international capital movements may be associated with less vulnerability to currency crises in large part due to the special characteristics and “self selection” of countries that choose to liberalize. Countries with relatively balanced macroeconomic policies, strong financial sectors, political stability, and/or institutional stability may choose to liberalize their capital accounts because they want to take advantage of long-run efficiency gains in the allocation of capital and are not overly concerned with external crises. By contrast, countries with capital controls may hope to avoid difficult economic reforms or to avoid capital outflows that may trigger a crisis. This implies that countries with sound macroeconomic and political environments and more robust financial systems and institutions may not only be less likely to experience crises, but also less likely to enact capital controls and forego the benefits of free capital flows. Consequently, countries with closed capital accounts may be more prone to financial crises, both domestic and international in origin. Although capital account liberalization may increase country vulnerability to crises in some cases, capital controls can still be associated with a greater overall likelihood of financial crises.

We address this issue by employing the matching and propensity score methodology that was developed precisely for this type of sample selection bias. Methods of matching were developed to help account for the estimation bias arising from the "selection on observables" problem. We use "nearest neighbor", "radius," and "stratification" matching methods, as well as a "regression-adjusted" matching estimator—all methods designed to account for selection on observables bias. To our knowledge, none of these methods have been applied to the problem at hand.

All of our results suggest that, even after controlling for sample selection bias (and obtaining unbiased estimates), capital restrictions are associated with a greater likelihood of currency crises. That is, when two countries have the same likelihood of maintaining a liberalized capital account (based on historical evidence and a very similar set of identical economic and political characteristics at a point in time)—and one country imposes controls and the other does not-- the country without controls has a lower likelihood of experiencing a currency crisis. These results are robust to changes in the type of methodology or changes in the equations that predict the likelihood of capital account liberalization. The point estimates suggest that countries without capital controls are from 5-7 percent to as much as 18 -26 percent less likely to experience a currency crisis in any given year.

The link between crises and capital controls is robust to our methodological correction for possible sample selection bias, a clear problem in estimating models concerning the application of capital controls and whether they are useful "medicine" for warding off currency crises. This improvement in methodological approach provides a much stronger empirical foundation for earlier empirical findings. Moreover, the upper end of our range of magnitudes obtained with matching methodology suggests that earlier work may have underestimated the positive benefits of liberalization for macroeconomic stability. Indeed, these results support previous work finding a negative (positive) link between capital account liberalization (controls) and the onset of currency crises but are much larger in magnitude. The estimates reported here, using matching methods, suggest that previous studies may have greatly underestimated— perhaps as much as a factor of five-- the reduced likelihood of a currency crisis in an environment with liberalized capital flows. Rather than weakening the observed negative correlation between liberalized international payments and the likelihood of currency instability—the premise that motivated this article—the application of a rigorous methodology for sample selection bias has made it even clearer that countries with liberalized capital flows are more likely to avoid currency crises.

Table 1

Currency Crises and Capital Account Liberalization, Unconditional Frequency (in percent)

	1975-1997	1975-1979	1980-1984	1985-1989	1990-1994	1995-1997
Currency crises[a]	11.7	9.9	12	14.3	11.8	9.7
(Number of crises)	(160)	(26)	(34)	(43)	(38)	(19)
Liberalization[b]	16.2	20.6	15.8	11.0	13.4	23.8

[a] Number of crises divided by total country-years with available data. Number of crises is in parentheses.
[b] Number of country-years with capital account liberalization divided by total country-years with available data.

Table 2

Currency Crises, Frequency Conditional on Capital Account Liberalization (in percent)

	Yes[a]	No[b]	χ^2 [c]
Liberalization in place during current year?	6.8	12.7	6.11**
Liberalization in place during previous year?	8.0	12.5	3.50*

[a] Number of currency crises for which capital account liberalization in place at end of current or previous year, divided by total number of country-years with liberalization in place.
[b] Number of currency crises for which capital account liberalization in place at end of current or previous year, divided by total number of country-years with liberalization in place.
[c] Null hypothesis of independence between frequency of currency crises and capital account liberalization is distributed as $\chi^2(1)$. Note: ** and * indicate rejection of null at 5 and 10 percent significance levels, respectively.

Table 3

Sample Characteristics of Treatment and Control Groups

Variable	Treatment Group	Unmatched Control Group	Matched Control Group (Nearest Neighbor)	Matched Control Group (Radius <0.05)
Current account/GDP	-3.0123 (7.07)	-3.9265 (7.24)	-3.0202 (7.84)	-3.9496 (7.22)
Govt. spending/GDP	12.4968 (3.89)	13.9190 (5.70)	12.7519 (4.75)	13.9210 (5.70)
Trade openness	79.1126 (85.52)	51.7559 (33.68)	53.5127 (38.16)	51.6132 (33.45)
U.S. real interest rate	3.8042 (2.13)	4.1391 (2.08)	3.6092 (2.16)	4.1458 (2.07)
Change of govt.	4.3450 (3.12)	3.8114 (2.57)	4.2031 (2.70)	3.8124 (2.57)
Freedom	1.7251 (0.54)	1.8674 (0.70)	1.8984 (0.74)	1.8684 (0.70)
Mean propensity scores	0.295 (0.22)	0.1965 (0.11)	0.2274 (0.12)	0.1960 (0.11)
No. of observations	171	822	128	821

Note: the table reports the sample means of variables (with associated standard errors in parenthesis) of the treatment group, i.e. country-years with liberalized capital accounts, and control groups, i.e. country-years with capital controls

Table 4

Unconditional Matching, including Within-Country Observations

	Nearest-Neighbor	Radius (<0.05)	by Strata
Estimated effect of liberalization (percent)	-7.6023	-4.8410	-5.2343
t-statistic	-2.2210	-1.9358	-2.0721
No. of observations in the treatment group (No. of observations in the control group)	171 (822)	171 (822)	171 (822)

Table 5

Robustness

Unconditional Matching: Across-Country Observations and Tighter Radius Parameter

	Nearest-Neighbor	Radius (<0.05)	Radius(<0.01)
Estimated effect of liberalization (percent)	-7.0175	-4.7754	-5.1470
t-statistic	-2.0717	-2.9168	-3.8217
No. of observations in the treatment group (No. of observations in the control group)	171 (822)	171 (822)	171 (822)

Table 6

Regression-adjusted Matching

	Nearest-Neighbor	Radius (<0.05)	by Strata
Estimated effect of liberalization (percent)	-22.2783	-15.4827	-14.7987
t-statistic	-6.4192	-9.2622	-5.9428
No. of observations in the treatment group (No. of observations in the control group)	171 (751)	171 (751)	171 (751)

Note: Based on residuals from the currency crisis probit equation reported in Appendix B

Table 7

Robustness

Unconditional Matching: Alternative Propensity Score Estimation

	Nearest-Neighbor	Radius (<0.05)	By Strata
Estimated effect of liberalization (percent)	-26.7974	-18.1544	-18.0193
t-statistic	-6.1235	-2.2552	-2.1409
No. of observations in the treatment group (No. of observations in the control group)	153 (695)	153 (695)	153 (695)

Note: Propensity score equation augmented to include dummy variables for currency crisis and capital account liberalization in place in the previous year. See column (2) in Appendix Table A.

Appendix A

Probit Equations for Estimating Capital Account Liberalization Propensity Scores

Explanatory Variable	**(1)**	**(2)**
Current account/GDP t-1	0.72*** (3.68)	-0.16 (0.48)
Govt. spending/GDP t-1	-1.46*** (4.81)	-1.27*** (2.72)
Openness t	0.23** (9.81)	0.12*** (2.79)
U.S. real interest rate t-1	0.15** (3.13)	0.01 (0.13)
Total changes of government	2.75*** (4.26)	-0.19 (0.18)
Freedom t-1	2.84 (1.30)	-5.72 (1.61)
Currency crisis t-1		-4.96 (0.71)
Capital Account Liberalization t-1		9.05*** (48.01)
No. of observations	1177	975
Percent of liberalization observations correctly predicted (threshold=0.5)	10.7%	89.7%
Percent of capital control observations correctly predicted (threshold=0.5)	98.2%	97.9%
Log likelihood	-547.28	-137.45
Pseudo-R2	0.20	0.77

Note: The table reports the change in the probability of capital account liberalization in response to a 1 unit change in the variable evaluated at the mean of all variables (x 100, to convert into percentages) with associated z-statistic (for hypothesis of no effect) in parentheses below. Results significant at 1, 5, and 10 percent levels are indicated by ***, **, and *, respectively. Constant included, but not reported. Observations are weighted by real GDP per capita (in dollars).

Appendix B

Currency Crisis Equations for Generating Residuals Used in Regression-Adjusted Matching Procedure

Explanatory Variable	
Log(M2/Reserves) t-1	1.6233 (1.22)
Credit growth t-1	0.0291 (1.06)
Current account/GDP t-1	-0.2976 (1.48)
Real overvaluation t-1	0.2145 (4.07)
Real GDP growth t-1	-0.4817 (1.88)
No. of observations	751
Adjusted R-square	0.05

Note: Table reports the results (in percent) from a linear probit model, with associated standard errors in parenthesis. Only observations from the control group (with capital controls) are included, as in Heckman, Ichimura and Todd (1997).

Appendix C

Currency Crisis and Capital Account Liberalization Episodes

Country	Currency Crisis Episodes	Capital Account Liberalization Episodes
Argentina	1975, 1982, 1989	1993–
Bangladesh	1975	
Belize		1981-85
Bolivia	1981, 1983, 1988, 1991	1975-80, 1986-95
Botswana	1984, 1996	
Brazil	1982, 1987, 1990, 1995	
Burundi	1976, 1983,1986, 1989, 1997	
Cameroon	1982, 1984, 1994	
Chile	1985	
China, P.R.: Hong Kong		1975–
Columbia	1985	
Costa Rica	1981	1980-81, 1995–
Cyprus		
Dominican Republic	1985, 1987, 1990	
Ecuador	1982, 1985, 1988	1975-85, 1988-92, 1995
Egypt	1979, 1989	
El Salvador	1986, 1990	1996–
Equatorial Guinea	1991, 1994	
Ethiopia	1992	
Fiji	1986	
Ghana	1978, 1983, 1986	
Grenada	1978	
Guatemala	1986, 1989	1975-79, 1989–
Guinea-Bissau	1991, 1996	
Guyana	1987, 1989	
Haiti	1977, 1991	
Honduras	1990	1975-79, 1993-95
Hungary	1989, 1994	
India	1976, 1991, 1995	
Indonesia	1978, 1983, 1986, 1997	1975-95
Jamaica	1978, 1983, 1990	1996–
Jordan	1983, 1987, 1989, 1992	
Kenya	1975, 1981, 1985, 1993, 1995, 1997	1996–

Korea	1980, 1997	
Lao People’s D. R.	1995	
Madagascar	1984, 1986, 1991, 1994	
Malawi	1982, 1985, 1992, 1994	
Malaysia	1986, 1997	1975-95
Mali	1993	
Malta	1992, 1997	
Mauritius	1979	1996–
Mexico	1976, 1982, 1985, 1994	1975-81
Morocco	1983, 1990	
Mozambique	1993, 1995	
Myanmar	1975, 1977	
Nepal	1975, 1981, 1984, 1991, 1995	
Nicaragua	1993	1975-77, 1996–
Nigeria	1986, 1989, 1992	
Pakistan		
Panama		1975–
Paraguay	1984, 1986, 1988, 1992	1982-83, 1996–
Peru	1976, 1979, 1987	1978-83, 1993–
Philippines	1983, 1986, 1997	
Romania	1990	
Sierra Leone	1988, 1990, 1997	
Singapore	1975	1978–
South Africa	1975, 1978, 1984, 1996	
Sri Lanka	1977	
Swaziland	1975, 1979, 1982, 1984	
Syrian Arab Republic	1977, 1982, 1988	
Thailand	1981, 1984, 1997	
Trinidad & Tobago	1985, 1988, 1993	1994–
Tunisia	1993	
Turkey	1978, 1994	1997–
Uganda	1981, 1987, 1989	1997–
Uruguay	1982	1978-92, 1996–
-Venezuela	1984, 1986, 1989, 1994	1975-83, 1996–
Zambia	1985, 1994	1996–
Zimbabwe	1982, 1991, 1994, 1997	

Notes: Currency crises defined by criteria described in text, with 24-month exclusion windows imposed. Blank cell indicates currency crisis never occurred or capital controls never implemented.

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