Do Currency Crises Cause Capital Account Liberalization?*

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Do economic crises facilitate or hamper neoliberal policy reforms? Despite the plausibility of claims linking crises to reforms, this question has faced strikingly little systematic empirical scrutiny. This paper focuses on the argument that currency crises cause capital account liberalization, making two contributions. First, I outline a common theoretical counterargument, that currency crises lead governments to restrict capital flows as a form of self-help. I show, moreover, that testing the competing claims requires a strategy for addressing the argument that the direction of causality is reversed: that capital account liberalization itself affects the likelihood of currency crises. Second, I propose an identification strategy that corrects for this possibility of simultaneous causation. Using country-specific debt-weighted Northern interest rates as an instrument for currency crises, I show that in a broad sample of developing countries, crises are robustly associated with capital account closure. Moreover, these effects are durable, and hold as well in an out-of-sample test. The findings refocus the debate on currency crises and liberalization, and speak to larger debates about the role of critical junctures in prompting economic reform.

1 Introduction

Political economists have long argued that economic crises are the engine of neoliberal policy reform. Evidence of this is normally drawn from expert readings of economic crises and policy trajectories. A prominent example is Mexico, where economic hardship in the mid-1980s appears to have prompted a new round of reform into the early 1990s. Similar other examples include economic reforms in Zambia in the early 1980s, Philippines in the last years of the Marcos regime, and many others, each held to show how regimes respond to economic crises with neoliberal economic reforms (see e.g. Nelson 1990). The literature on such cases and others is vast, but the general lesson drawn from them has been that crises prompt neoliberal reform.

Two central problems plague this literature. The first is the possibility of selection bias. By examining primarily confirmatory cases—and discounting examples of resistance or retrenchment—researchers cannot assess the average impact of crises on reforms across countries (Drazen 2000: 449-54). Zimbabwe, for example, has faced near continual economic

crisis since 1995, yet Robert Mugabe's government has yet to embrace anything approaching economic reform (Clemens and Moss 2005). Is Zimbabwe or Zambia more emblematic of policy responses to economic crises in the developing world? The second problem is endogeneity. The relatively small cross-national quantitative literature that tests the link between crises and adjustment (Biglaiser and DeRouen 2004; Campos et al. 2006; Drazen and Easterly 2001) downplays the possibility that neoliberal policy reforms themselves drive economic crises. This, however, is another key argument in the political economy of structural adjustment. Experiments with neoliberal economic policies in Latin America during the late 1970s, by many accounts, were decisive in causing economic crises in the early 1980s (see e.g. Green 1995: 20-23). This possibility makes it incumbent upon researchers not only to chart the average association between crises and policy reforms, but to find strategies for disentangling the direction of causality between them.

Studies of international monetary relations face these very problems. In an influential article, Stephan Haggard and Sylvia Maxfield (1996) argue that when facing severe financial hardship and plummeting exchange rates, governments adopt liberal capital account policies to encourage foreign capital owners to resume investment. Such qualitative arguments linking currency crises to financial liberalization are common in the large literature in the 1990s that linked economic crises to liberal economic reform in the developing world (Drazen and Grilli 1993; Haggard 2000; Haggard and Kaufman 1992; Little et al. 1993; Nelson 1990; Rodrik 1996). Still, there are reasons to suspect that the link from currency crises to financial liberalization is far from straightforward. Heterodox responses to currency crises appear common as well: President José López Portillo famously closed Mexico's capital account in response to the Latin American debt crisis in 1982. Moreover, the direction of causality is unclear, for policy makers

and scholars alike frequently charge that capital account liberalization itself causes currency crises. As a consequence, studies of the link between crises and liberalization must account for both the possibility of heterodox adjustment and the possibility of endogeneity. The few cross-national studies that test the hypothesis find no consistent evidence that crises drive reform, but their empirical models cannot identify causality, which questions the validity of their findings (Brooks 2004; Brune and Guisinger 2007; Chwieroth 2007).

This paper uses new quantitative methods to test the links between currency crises and capital account liberalization. It derives a proper cross-national test of the argument that crises lead to policy reform, using instrumental variables to identify the strength of the relationship between the two. Economic theory suggests that interest rates in industrial economies are an ideal instrument for currency crises in the developing world. A substantial proportion of currency crises in the developing world in the 1970s, 1980s, and early 1990s are directly attributable to the slowdown of foreign capital inflows during episodes of high interest rates in the developed world. The average of large lending country interest rates, weighted for each borrowing country by the proportion of the borrower's debt denominated in each lender's currency, provides a country-specific measure of "Northern interest rates." As I argue, Northern interest rates have no other unobserved relationship to capital account policy than through this mechanism. This makes them an ideal instrument for currency crises, allowing for the exact identification of the relationship between currency crises and subsequent financial liberalization. Instrumenting for currency crises with Northern interest rates, I find that contrary to dominant arguments, on average in the developing world, currency crises cause capital account closure. I therefore find that crises drive *change*, but not in the direction suggested by the qualitative literature. Currency crises hamper capital account liberalization rather than facilitating it.

Currency crises and capital account policy provide an ideal domain to test the argument that crises drive policy reforms. First, currency crises are common in the developing world. Between 1970 and 2005, Leblang (2005) identifies 949 distinct currency crises throughout the world, with 807 occurring in developing countries. Hicken et al. (2005) argue that this prevalence makes them particularly important for the understanding the politics of economic adjustment. Second, consistent and reliable datasets on capital account policies allow for direct measurement of the dependent variable, in marked contrast to other adjustment policies such as enterprise privatization and interest rate liberalization that might result from other types of economic crises. And finally, capital account liberalization is a policy response that neatly falls onto a dimension of orthodoxy versus heterodoxy. The fact that there exist prominent arguments in favor of discretionary capital account restrictions in the context of currency crises (see e.g. Rodrik 1998) make capital account liberalization a uniquely salient adjustment policy decision.

In the next section I outline two competing political logics that link crises to capital account policies, and show why a simple correlation in any direction between balance-of-payments crises and liberalization of international capital movements may either mask or overstate the link between the two. I then describe the instrumental variables estimator that overcomes this identification problem, and summarize the data and methods. I pay particular attention to the identification problem, describing a variety of tests that allow me to confirm that Northern interest rates are a relevant and valid instrument for currency crises. The subsequent section presents the results. I show from both baseline models and a battery of robustness tests that there are strong links between currency crises and capital account closure. Turning to a broader question of the durability of this relationship, I show that far from a temporary policy shock, the effect of currency crises on capital account openness persists for at least five years. In

the following section, I present an out-of-sample test using data from the global currency crises of 1998, showing that countries that experienced currency crises are more likely to have closed their capital accounts than their counterparts who did not. A final section concludes by outlining the consequences of this argument for future research on capital account liberalization, and by stressing the importance of identification strategies in the study of policy reform.

2 Linking Crises to Liberalization

There are four possible relationships between currency crises and financial liberalization, each of which has received some attention in the literature. The path diagram below summarizes possible arguments.

--- Figure 1 about here ---

Paths A and B lead from currency crises to capital account liberalization, but suggest different average effects—either currency crises lead to increased liberalization, or decreased liberalization. Likewise, Paths C and D lead from liberalization back to currency crises, but differ as to whether liberalization increases or decreases the likelihood of currency crises.

Haggard and Maxfield focus on Path A, arguing the currency crises prompt financial internationalization, which they operationalize through measures of capital account liberalization. Since countries experiencing currency crises need ready access to foreign capital, they ease restrictions on capital flows in order to signal to foreign lenders their country's creditworthiness. When capital inflows subsequently resume, exchange rates recover and financial difficulties ease. Using qualitative data and a rough proxy for capital account policies, the authors find that currency crises in Mexico, Chile, Indonesia, and South Korea prompted subsequent liberalization. Focusing on a more general problem of a potentially creditworthy government facing a need for foreign capital, Bartolini and Drazen (1997) independently

proposed that capital account liberalization signals government resolve and capability to foreign creditors. These arguments each suggest that currency crises prompt capital account liberalization, but also that the absence of liberalization may prompt currency crises by signaling a lack of government capacity.

The logic of this argument rests on two political forces. The first is some domestic pressure for access to foreign investment, arising either through interest group lobbying from domestic constituents or through the ideological pressures of technocrats occupying influential roles in the policy making establishment. The second is the international constraint of government credibility to foreign investors, who require that the government adopt favorable policies to prompt investment. Together, these political forces create incentives for governments to respond to currency crises with capital account liberalization, the very policy that reassures foreign investors that a government is committed to good policies and that domestic pressure groups demand. We can express this argument as Hypothesis 1.

> **Hypothesis 1 (Path A)**: Currency crises increase the likelihood that a government liberalizes its capital account.

Operating at cross-purposes are common national defenses against currency crises: the imposition of additional capital controls to limit capital outflows, to disrupt transactions such as currency speculation and round-tripping, and to change the composition of capital inflows from speculative hot money to "safer" foreign direct investment. Each of these policies would correspond to a decrease in capital account openness, represented by Path B. A casual inspection of country evidence would indicate that such policies are quite common. Malaysia in 1998 responded to a sharp currency crisis by closing its capital account. Middle Eastern countries in the late 1980s did much the same. Interestingly, two of the countries that Haggard and Maxfield analyze, Mexico and Chile, made similar retreats from financial openness in 1982 in responding

to the Latin American debt crisis (Lustig 1998: 25; Silva 1996: 151-82), as did Argentina beginning in 1983 with the initial series of policies that culminated in 1985 with the Austral Plan (Manzetti and Dell'Aquila 1988). Each of these examples would suggest that in contrast to the dominant argument that economic crises drive neoliberal reform, currency crises often lead countries to retreat from capital account openness.

The logic behind these responses is similarly political. While some foreign and domestic constituents that might demand capital account liberalization in response to currency crises, the domestic constituency that favors increased capital account restrictions may be just as influential. It may include a broad coalition of wage laborers, industrial enterprises, rural peasants and landowners, and other actors who fear the devastating consequences of a currency crisis and demand the national autonomy to enact expansionary macroeconomic policies. Such populist coalitions were evident following currency crises in countries like Malaysia, Mexico, and Argentina, cited above. Rather than incentivizing foreign capital to return through capital account liberalization, the preferred nationalist response may be simply to prevent it from leaving through capital account closure. We can express this argument as Hypothesis 2.

> **Hypothesis 2 (Path B)**: Currency crises increase the likelihood that a government closes its capital account.

If there were no possibility of endogeneity, then it would be a simple matter to test empirically whether on average Hypothesis 1 or Hypothesis 2 best describes the effect of currency crises on capital account policies. But it has long been suspected that capital account openness puts developing countries at further risk for currency crises. This is represented by Path C, and the logic behind this relationship is straightforward. The more freely that capital can flow in and out of a country, the more possible are risky international lending, speculative attacks, and sudden capital account reversals that lead to currency crises. This line of reasoning

forms the basis of many prominent arguments—frequently issued in the wake of global currency crises—which hold that domestic financial liberalization should be undertaken well before capital account liberalization to minimize the risks of devastating subsequent crises (McKinnon 1993; Tobin 1978; Tornell et al. 2003).

Even so, recent research has challenged this conventional wisdom and suggested that capital controls may actually *cause* currency crises, as represented in Path D. Arguments citing on this relationship focus on the policy independence that capital controls provide, and the incentives for financial malfeasance that correspondingly arise. Several authors have argued that distortionary financial policies undertaken with the shield of a relatively closed capital account increase the likelihood of a currency crisis (Glick et al. 2006; Glick and Hutchison 2005; Kitano 2007). Alternatively, as noted above, Bartolini and Drazen (1997) suggest that governments that do not liberalize capital markets signal their lack of resolve or capacity to foreign creditors, which increases the likelihood that these creditors launch speculative attacks.

That each of these links is at least theoretically plausible means that a simple correlation (or lack thereof) between currency crises and capital account liberalization is itself uninformative. A finding that currency crises are associated with more capital account liberalization may indicate that A is true, but it may also indicate that C is true. Even assuming that A is actually true, the estimated size of this effect may be biased upward (if C is true) or downward (if D is true). In reverse, a finding that currency crises are associated with less capital account liberalization may indicate that B is true, or that D is true, or assuming B is true, the estimated effect may be inflated (if D is true) or masked (if C is true). A finding of no effect could indicate that there is no average effect, or that both A and D are true, or both B and C are true. To ascertain whether Hypothesis 1 or Hypothesis 2 better describes the relationship

between currency crises and capital account liberalization, we need a strategy to untangle the direction of causality.

3 Research Design

With such good reasons to suspect that simultaneity may be at play, the few studies that have included currency crises as control variables explaining subsequent capital account liberalization cannot have identified the magnitude or direction of this relationship. The simplest representation of the relationship between crises and liberalization is represented by the empirical model in (1).

(1) $$Y_{it} = \mathbf{X}_{it}'\boldsymbol{\beta} + u_{it}$$

where Y designates capital account liberalization, $\mathbf{X}$ designates a matrix of independent variables (in this case, currency crises and others), u designates an error term, $\boldsymbol{\beta}$ are parameters to be estimated, and subscripts i and t designate countries and time, respectively. If there is a simultaneous relationship between some $\mathbf{X}$ and Y, then $Cov[\mathbf{X}_{it}, u_{it}] \neq 0$, rendering the ordinary least squares (OLS) estimate of $\boldsymbol{\beta}$ both biased and inconsistent.

I employ instrumental variables (IV) to overcome this problem. Dropping the subscripts for convenience, consider a partition among the independent variables such that $\mathbf{X_1}$ are endogenous and $\mathbf{X_2}$ are exogenous, and an instrument $\mathbf{Z}$ that is an exogenous predictor of $\mathbf{X_1}$. In this case, writing $\mathbf{P_Z} = \mathbf{Z}(\mathbf{Z}'\mathbf{Z})^{-1}\mathbf{Z}'$, a consistent estimate of $\boldsymbol{\beta}$ is

(2) $$\tilde{\boldsymbol{\beta}}_{IV} = (\mathbf{X}'\mathbf{P_Z}\mathbf{X})^{-1}\mathbf{X}'\mathbf{P_Z}Y$$

The technical requirements of such an instrument $\mathbf{Z}$ are that it is conditionally independent from the error term of the true regression model ($E[\mathbf{Z}_{it}\, u_{it}] = 0$), and correlated with $\mathbf{X_1}$.[1] The first requirement is that the instrument is "valid," and such instruments that are conditionally independent of the error term are deemed to have met the exclusion restriction. The second requirement is that the instrument is "relevant." Beyond simple correlation, though, since $\tilde{\boldsymbol{\beta}}_{IV}$ is biased in finite samples, in empirical applications $\mathbf{Z}$ must explain a substantial proportion of $\mathbf{X_1}$ in order to avoid the "weak instruments" problem. If we write

$$\mathbf{X_1} = \mathbf{Z}'\boldsymbol{\gamma} + \mathbf{X_2}'\boldsymbol{\delta} + v \tag{3}$$

then we can express a rough measure of bias of the IV estimate in finite samples as a ratio of the OLS bias in (1), measured as the correlation between u and v, to the product of the sample size and the amount of the total variation ($\tilde{R}^2$) of $\mathbf{X_1}$ explained by $\mathbf{Z}$ in (3) (Murray 2006: 122-23). Accordingly, as $\tilde{R}^2$ declines—as instruments become weaker—finite-sample IV bias increases.

In the subsequent section and in the empirical analysis, I test rigorously both the relevance and the validity of the instruments, ensuring that I do not suffer from weak instruments and that the instrument does meet the exclusion restriction.

3.1 Identification Strategy: Northern Interest Rates

Good instruments rarely suggest themselves, but Northern interest rates are an ideal instrument for currency crises in the developing world. They have a clear relationship with currency crises, as uncovered in a number of studies (Eichengreen and Rose 1998; Frankel and Rose 1996). As a theoretical matter, then, Northern interest rates should be relevant instruments.

[1] The model makes no distributional assumptions about the endogenous variable(s), meaning that parameter estimates from an instrumental variables regression are consistent with an endogenous binary variable. See Wooldridge (2002: 83-84).

However, there is a plausible case that Northern interest rates do not pass the exclusion restriction. An alternative pathway linking Northern interest rates to capital account policy lies through capital flows alone, independently of whether or not capital flows cause currency crises. Governments may tighten capital controls when capital flows out of their country regardless of whether or not this causes a currency crisis, and Northern interest rates have substantial impacts on capital flows, as cited above. The task is to show that given this plausible argument, that Northern interest rates do meet the assumption of validity in their relationship to capital account policy. A variety of strategies can check whether or not there are other relationships between Northern interest rates and capital account policies, which would violate this assumption, and can correct for this relationship if it does exist.

The simplest strategy relies on the existing literature's findings to assess whether or not Northern interest rates are related capital account policy when controlling for other possible determinants of capital controls. A number of authors have tested this link, finding *no* evidence that developing countries change capital account policies in direct response to lending country interest rate changes (see Chwieroth 2007; Glick et al. 2006; Glick and Hutchison 2005). The failure of existing approaches to find a direct link between Northern interest rates and capital account policies is a first step in maintaining the instrument's validity.

Still, misspecification bias in earlier research may mean that a relationship between Northern interest rates and capital account policies exists, but researchers have incorrectly failed to reject the null of no relationship. A more rigorous strategy focuses on the necessity of conditional independence between $\mathbf{Z}$ and u. The exclusion restriction is violated in the case of other indirect links between Northern interest rates and capital account policies, such as the possibility that credit stocks in developing countries will decrease as a result of higher Northern

interest rates. This affects capital account policy in the event that governments respond to evaporating capital stocks by tightening capital account restrictions. As a consequence, Northern interest rates have a second pathway that affects capital openness. Yet the requirement of *conditional* independence means that accounting for this second pathway can recover the independence between **Z** and u. Including measures of the change in capital stocks as control variables in both stages assures that the assumption of validity is still met. So even if misspecification bias explains the failure of existing work to find a link between Northern interest rates and capital controls, controlling for alternative pathways achieves the conditional independence required to meet the exclusion restriction.[2]

Despite these arguments, and the theoretical attractiveness of Northern interest rates as instruments for currency crises, it is still important to emphasize that failure to meet the exclusion restriction is still possible. Validity is an assumption that cannot be properly tested. One can, however, test for relevance by simply examining whether an observed **Z** is associated with observed $\mathbf{X_1}$. One can also test for weak instruments by specifying a level of confidence and finite IV sample bias beyond which one is willing to accept instruments as sufficiently strong. For all models, I report LM test statistics that confirm that the instrument is relevant, and Cragg-Donald Wald F statistics to check that they exceed the Stock-Yogo critical values for strong instruments (Stock and Yogo 2005). These reassure us that the estimates do not suffer from bias arising from either underidentification or weak instruments.

I bring different tools to bear to test the assumption of validity. I begin with the weakest test, simply regressing capital account openness on Northern interest rates and other control variables, and find no significant relationship between the two. I also include control variables

[2] This point is subtle, but too frequently overlooked. See Wooldridge (2002: 88).

for changes in private capital stocks, changes in foreign reserves, and the exchange rate regime, each of which captures potential alternate pathways through which the assumption of conditional independence might be violated. It is possible to test for the validity of a subset of instruments with an overidentified model, where there are more relevant instruments than endogenous regressors. This is not the case in the baseline specification. Nevertheless, I experiment with artificial methods to achieve overidentification in order to test the assumption of validity. I base these on the hypothesis that the effect of Northern interest rates on currency crises is nonlinear, so that the square of Northern interest rates is another instrument for currency crises. I find strong evidence that with an overidentified model, both instruments meet the assumption of validity, even though the two instruments together are notably weaker than the single instrument that exactly identifies the model. I report more on the results of these tests in section 4 below.

3.2 Data and Methods

The data for currency crises (*CRISIS*) and debt-weighted Northern interest rates (I^*) come from Andrew Berg and Catherine Pattillo (1999).[3] These data comprise the broadest available coverage of developing country currency crises between 1971 and 1996, with data available for 103 countries.[4] A country is coded as having experienced a currency crisis when its nominal exchange rate depreciates by at least twenty-five percent within one year, and when this depreciation exceeds the previous year's depreciation by at least ten percent. This definition provides a country-specific measure of whether or not a country has experienced a currency crisis, and ensures that the coding picks up only depreciation which is substantially different than the country's normal exchange rate trajectory.

[3] The data update the measures in Frankel and Rose (1996).
[4] Following Berg and Pattillo (1999), Frankel and Rose (1996), and others on the causes and consequences of currency crises, I implement three-year windows around crisis onsets to ensure that all measured crises are independent events. See also Milesi-Ferretti and Razin (1998).

The instrumental variable *I** is a weighted average of interest rates in six large industrial economies that provide a substantial proportion of loans to the developing world. Specifically, the variable averages short-term interest rates in the United States, Germany, Japan, France, the United Kingdom, and Switzerland, weighted for each country according to the proportion of its foreign debt denominated in each lender's currency. Weighting by debt compositions ensures that this variable's values are specific to each borrower. The dependent variable is *KAOPEN*, as measured by Menzie Chinn and Hiro Ito (2006). The variable is an index of four indicators of regulations on international transactions which appear in the International Monetary Fund's *Annual Report on Exchange Arrangements and Exchange Restrictions*. Higher values correspond to higher levels of capital account openness.

As control variables, I use a standard set of economic and political variables held to influence capital account liberalization in the cross-national literature. From the World Development Indicators I calculate the natural logarithm of gross domestic product at purchasing power parity in constant 2000 dollars (*LNGDP*); the natural logarithm of GDP per capita at purchasing power parity in constant 2000 dollars (*LNGDPPC)*; annual percent GDP growth (*GROWTH*); the natural logarithm of 100 plus consumer price inflation (*INFLATION*); national reserves as months of imports (*RESERVES*); and government final consumption expenditure (*EXPEND*), national savings (*SAVINGS*), current account balance (*BALANCE*), private credit (*PRIVCRED*), and international trade (*TRADE*), each as a percentage of GDP. This is the sample for which full data are available. I also include Klein and Shambaugh's (2006) measure of fixed exchange rates (*PEG*) and the data on regime type from Polity IV (*POLITY*) (Polity IV Project 2006). A variable *AVERAGE* captures the average value of *KAOPEN* around the world. A year

index variable (*COUNTER*) increases by 1 for every year after 1970 to account for unobserved temporal factors. Summary statistics for these variables are presented in Table 1.

--- Table 1 about here ---

The control variables each capture key potential determinants of capital account liberalization. *LNGDP*, *LNGDPPC*, and *GROWTH* test the hypotheses that larger, more developed, and faster growing economies are more likely to liberalize their capital accounts due to the lower costs of doing so. *INFLATION* and *RESERVES* test whether countries impose capital controls when they suffer from poor macroeconomic fundamentals that place them at high risk of speculative attacks. *EXPEND* and *SAVINGS* test similar arguments, that poor fiscal policy fundamentals or low levels of national savings increase the likelihood of crises, and prompt governments to restrict cross-border capital flows. *TRADE* and *BALANCE* test whether or not capital account liberalization follows trade policy liberalization, so that more open economies are more likely to permit cross-border capital flows. *PRIVCRED* tests whether countries with developed financial sectors are more likely to embrace capital account liberalization. *PEG* tests whether countries with fixed exchange rates are more likely to restrict capital account convertibility in order to achieve macroeconomic policy autonomy. *AVERAGE* tests the hypothesis that capital account liberalization proceeds through international contagion and policy emulation (Simmons and Elkins 2004). And finally, *POLITY* tests the link between political regime and capital account liberalization, an argument often increasingly made in research on financial liberalization (Dailami 2000).

The variables capturing private credit to the domestic financial sector, current account balance, reserves, and exchange rate regime all play dual roles. They all are standard determinants of capital account policies, but they also capture the potential alternative pathways

through which Northern interest rates might affect capital account policies. Since the model is estimated with first-differences (discussed below), private credit to GDP and reserves variables capture changes in domestic credit stocks and changes in official reserve stocks, each of which are alternative pathways through which Northern interest rates might affect capital account policies. Including the *PEG* variable ensures that the model does not pick up the possibility that countries change capital account policies when they have maintain fixed exchange rates and wish to raise their own interest rates in response to deteriorating terms of credit with the developed world. Controlling for these alternative theoretical pathways ensures that the model satisfies the key requirement of conditional independence between **Z** and *u*—there are no unobserved channels through which the instrument affects the dependent variable.

Despite the large number of potential alternative influences on capital account policy that I capture with these variables, we might still worry about unobserved factors that affect countries' decisions to impose capital controls. Hausman's test of exogeneity on a pooled baseline OLS model overwhelmingly rejects the null hypothesis that the pooled model is consistent but more efficient than a fixed effects OLS model.[5] With evidence that unobserved heterogeneity is indeed a problem, in all subsequent analyses I estimate IV models with fixed effects. I also test for the possibility heteroskedasticity, which could bias the estimated standard errors reported below and accordingly mislead our inferences. Pagan-Hall tests performed on the residuals of an unconstrained fixed effects IV model find no evidence to reject the null hypothesis of homoskedasticity, so I report default standard errors in most models.[6]

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[5] Hausman (1978). χ^2 (15) = 42.75.

[6] χ^2 (90) = 66.908 (p = 0.967) for a test using all independent variables and their squares and cross-products as indicators of heteroskedasticity; χ^2 (1) = 1.757 (p = 0.185) for a test using the IV-predicted value of the dependent variable as an indicator of heteroskedasticity. The Pagan-Hall test is a strict test making no assumptions of homoskedastic errors in the first stage

Finally, the theories outlined above hold that currency crises lead to changes in capital account policies. I therefore test whether countries that have experienced currency crises subsequently *change* their capital account policies. I do this by taking first-differences of the dependent variable, as well as all independent variables (except for the binary currency peg measure). In addition to ensuring that the results that are not driven by non-stationarity in the regressors, this strategy allows me to directly test the hypotheses that crises drive change. Interestingly, in separate results, I also find that currency crises also affect levels of capital account openness as well—all models and all tests presented below yield substantively identical results, as do results from the Hausman and Pagan-Hall tests discussed above. I return to this issue below when I investigate to the durability of crises' effects on capital account openness.

4 Analysis

I begin by analyzing the potential for simultaneity with two simple bivariate regressions. The first estimates the effect of previous levels of capital account openness on the probability of a crisis. The second estimates the impact of a crisis on subsequent levels of capital account openness. The results appear in Table 2 as Models 1 and 2.

--- Table 2 about here ---

Without controlling for alternative factors, it appears that countries that have experienced currency crises are subsequently less likely to have open capital accounts —but at the same time, countries with more open capital accounts are less likely to experience currency crises. This is

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regression, as do more commonly used tests derived by Koenker and Cook and Weisberg; see Baum et al. (2003) and Pesaran and Taylor (1999). A model with a binary endogenous variable is highly likely to be heteroskedastic in the first stage.

very rough evidence that both Paths B and D are at work, suggesting that simultaneity may be a serious issue.

The baseline OLS regression in Model 3 examines whether currency crises are correlated with capital account openness when controlling for covariates but not accounting for simultaneity. The coefficient estimate is negative, suggesting that currency crises are associated with capital account closure, but the estimate is not significant at conventional levels. Based on this model, we find no evidence in favor of either Hypothesis 1 or Hypothesis 2—consistent with existing results. Note also that there is the estimate of I^* is insignificant, implying no direct relationship between Northern interest rates and capital account liberalization. But since simultaneity may either mask or inflate the effect of currency crises on capital account liberalization, Model 4 presents the baseline IV regression, using Northern interest rates to instrument for currency crises. The results support Hypothesis 2, that currency crises lead countries to close their capital accounts. In the first stage of the regression, we see that Northern interest rates are a statistically significant predictor of currency crises. The partial R^2 of the excluded instrument is .018, which is also highly statistically significant and reassures us that Northern interest rates are a relevant instrument for currency crises (Shea 1997).[7] Also, the LM test statistic confirms that the Northern interest rates are a relevant instrument for currency crises, and the significance of the Cragg-Donald test statistic indicates that weak instruments are unlikely to be a problem.

From these first regressions, we have the first piece of evidence that currency crises cause capital account closure, and that existing models have failed to uncover this relationship because of the problem of endogeneity. Comparing the size of the coefficients on *CRISIS* in Models 3

[7] $F(1,480) = 8.62$ ($p = .004$).

and 4, we see that OLS severely underestimates the effect of crises on liberalization, with the parameter in Model 3 only a thirtieth as large as that of Model 4. This dramatic difference in coefficient sizes is likely due to high levels of endogeneity, which underscores the need for an identification strategy. Also note that the variables measuring domestic savings, foreign reserves, and average openness are no longer significant in Model 4. One possible interpretation is that instrumental variables have identified a quantity known as the "local average treatment effect" (Imbens and Angrist 1994), or the effect of crises on capital account policies for countries that might be affected by changing terms of credit with Northern lenders. Within this population, savings, reserves, and global openness do not influence capital account policies.

To check that the results in Model 4 do not depend on model specification choices, the models in Table 3 include different control variables and show results from different estimators.

--- Table 3 about here ---

In a search for overidentification, Model 5 includes the square of I^* as an additional instrument for *CRISIS*. Results remain unchanged, and Hansen's J statistic fails to reject the null of overidentification (Hansen 1982), but together the two instruments are weak. I therefore estimate a Limited-Information Maximum Likelihood model (Model 6). The results are again consistent, and there is still no evidence to reject the null of overidentification. This reassures us that the instrument I^* is indeed valid. Model 7 removes the lagged dependent variable, for by construction a fixed effects model with a lagged dependent variable is biased (although panels are long enough that this bias is likely to be small (Beck and Katz 2004)). Model 8 removes the variable for political regime. The Polity dataset does not contain data for nine small countries included in the dataset of currency crises: Barbados, Belize, Cape Verde, Grenada, Maldives, Malta, Sao Tomé and Principe, Vanuatu, and Western Samoa. The sample size accordingly

rises, and this model ensures that results are not driven by the exclusion of a handful of very small countries that may be both more vulnerable to currency crises and more willing to open their capital accounts. Finally, while Pagan-Hall tests find no evidence of heteroskedastic errors, Model 9 presents results with country-clustered robust standard errors. In all models, the effect of crises, instrumented by Northern interest rates, on capital account openness is significant and negative. Even more encouragingly, the estimated size of the effect of currency crises on liberalization remains nearly the same across these different specifications.

Before proceeding to analyze the durability of these effects, I summarize findings thus far. Bivariate regressions suggest that simultaneity may be a key concern (Models 1 and 2). Nonetheless, simple OLS regressions find no evidence that currency crises lead to changes in capital account openness (Model 3). However, instrumental variables regression finds strong evidence that crises lead to decreases in capital account openness (Model 4). These findings are consistent across a wide variety of alternative specifications (Models 5-9). Rigorous tests of the relevance of Northern interest rates as an instrument for currency crises show that they explain an acceptable amount of variation in the first stage, so the estimates are unlikely to suffer from the bias present in all finite-sample IV models (Model 4-9). Simple tests of the validity of Northern interest rates as an instrument for currency crises find relationship between them and capital account policy when controlling for alternatives in a multivariate OLS regression (Model 3). Formal tests of the validity of Northern interest rates are only possible through an artificial overidentification strategy, but they yield no evidence to reject the null hypothesis that the exclusion restriction is met (Models 5 and 6). And finally, exhaustive inclusion of potential alternative pathways linking Northern interest rates to capital account policy further ensures that

the exclusion restriction is met (Models 4-9). Together, these results provide powerful evidence that currency crises cause capital account closure.

A possible response is that qualitative literature's findings come from reading the long-term effect of currency crises on subsequent policy choices. Accordingly, scholars may be more interested in the long-term effects of crises than in their immediate effects. To investigate this, I estimate the durability of the effect of crises on capital controls by implementing leads of the second stage of the model for up to five years. An important difference, though, is that I am no longer interested in testing whether countries *change* capital account policies up to five years after a currency crisis, but rather whether or not the *level* of capital account openness remains lower in the wake of these crises.[8] For this reason, the models estimated here include levels rather than first-differences of independent and dependent variables.

Table 4 contains the results of this investigation. The models test whether a currency crisis in year 1 affects capital account liberalization in future years, up to year 6. Included in these models are all economic control variables.

--- Table 4 about here ---

The models show that currency crises' impacts on capital account openness are quite durable, and are still evident after five years. Moreover, these models display better fit and parameter estimates on predicted crises are tighter, suggesting that currency crises' greatest impact on capital openness happens several years after the crisis' onset. One interesting result is an apparent U-shaped pattern, as the effect of currency crises on capital account policies is substantively larger five years out than four years out. This is perhaps an artifact of the increased

[8] In separate results (not reported), I find no evidence that the effect of crises on changes in capital account openness lasts beyond one year.

sample size associated with longer leads, which would include the currency crises of 1998. As I show now, these were also associated with capital account closure.

5 Out-of-Sample Evidence: 1998

The preceding evidence has shown that on average, currency crises cause capital account closure rather than capital account liberalization. But the data employed end at 1996. Currency crises that have occurred in the past decade are arguably different. Some analysts contend that economic conditions in industrial economies play less of a role in causing currency crises, which are more likely today to be caused by self-fulfilling investor panics, international financial contagion, and domestic political weaknesses. This raises the possibility that the causal effect of currency crises on capital account policy may no longer hold in a world that is more financially integrated than ever before. If this is the case, then one of the heretofore unmentioned assumptions of instrumental variables regressions may be violated. This is the assumption that "variation in the endogenous regressor related to the instrumental variable must have the same causal effect as variation unrelated to the instrument" (Dunning forthcoming: 2). If currency crises clearly caused by factors other than increases in Northern interest rates do not have the same effect on capital account policy, this impugns not only the temporal bounds of the effects that I identify, but more broadly the entire identification strategy.

To check if this is possible, I examine cross-sectional data from the 1998 currency crises that swept through the developing world in the wake of Thailand's surprise 1997 devaluation of the baht. These crises had no relationship to changing terms of credit with developed country lenders (see e.g. Furman and Stiglitz 1998 on crisis onsets in Asia), and accordingly present an ideal out-of-sample test. I employ Leblang's (2005) currency crisis data to measure crises, with a variable I call *CRISIS98*. It codes a country has having experienced a currency crisis if in any

month of 1998, an index of exchange market pressure exceeds the country-specific mean of that index plus twice its country-specific standard deviation (see Eichengreen et al. 1995 for a precise definition). All other variables are measured from the same sources, and capture the same hypotheses. Summary statistics are presented in Table 5.

--- Table 5 about here ---

Because there is no evidence that Northern interest rate changes caused the currency crises in the wake of the baht's devaluation, I cannot use them as an instrument. I therefore proceed by estimating simple cross-sectional OLS regressions. Of course, endogeneity may still be a concern, but I attempt to correct for this by including lagged capital account openness as a predictor of subsequent change. In the absence of a good instrument for these crises that is unrelated to subsequent policy changes, this is the best available strategy.

The results of three OLS regressions appear in Table 6. Model 15 estimates the effect of currency crises on changes in capital account openness. Model 16 estimates the effect of a currency crisis on the level of capital account openness in the subsequent year. Model 17 estimates the effect of a crisis on the level of capital account openness five years later (with all other control variables measured five years in the future as well).

--- Table 6 about here ---

Once again, in all models currency crises have a significant negative effect on subsequent capital account policies. On average, countries that experienced currency crises in 1998 were more likely to close their capital accounts than their counterparts that did not. Moreover, controlling for covariates and past openness, as late as 2003 countries that had experienced currency crises in 1998 still maintained more restrictions on capital account transactions than their counterparts.

The results of these out-of-sample tests are doubly reassuring for the reasons outlined above. They provide good evidence that developing countries close their capital accounts in response to currency crises in another empirical domain. This confirms that my findings extend beyond the period of 1971-1996 to further rounds of currency crises across the developing world. Even more importantly, though, these findings underline the validity of my instrumental variables identification strategy. Currency crises caused by factors other than increases in Northern interest rates still produce the same policy responses.

6 Conclusion

This paper started with a simple question: do currency crises prompt capital account liberalization, or impede it? I have shown that answering this question is no easy task. The literature on the politics of international monetary relations has proposed a number of different links between crises and liberalization, so any observational strategy must acknowledge the possibility that reforms themselves cause crises, and take steps to purge parameter estimates of this endogeneity. I identify the relationship from crises to liberalization using instrumental variables, employing country-specific debt-weighted Northern interest rates as an instrument for currency crises. My findings show that the standard accounts linking currency crises to capital account liberalization are incorrect. Cross-nationally, currency crises in the developing world lead to capital account closure. The findings hold up to a variety of specification tests, and are quite durable over time. Turning to an out-of-sample test from the spate of currency crises in 1998, I find additional evidence that the predominant response to currency crises in the developing world is capital account closure.

Because identification is a key concern in any application of instrumental variables, I devote considerable effort to checking that Northern interest rates are relevant and valid

instruments for currency crises in a model of capital account policy. I test directly for relevance and weak instruments, and find good evidence that Northern interest rates pass conventional benchmarks for each. I also test both directly and indirectly whether the instrument passes the exclusion restriction. I show that existing work finds no direct relationship between Northern interest rates and capital account policies, that my own data show no direct relationship between Northern interest rates and capital account policies, that controlling for potential alternative pathways allows me to meet the assumption of conditional independence even if all of these models are misspecified, and that direct tests for overidentification based on the square of Northern interest rates cannot reject the null hypothesis of validity. This battery of tests is the strongest available, and I find consistent results across all of them. In turn, evidence from the currency crises of 1998 confirms that the relationship holds up quite well in different empirical domains.

These results cast doubt on the simple argument that crises drive neoliberal reform. As such, they are of interest not only for the study of capital account liberalization, but also for the political economy of crises and economic reform in general. While the old literature on crises and liberalization emphasized how crises create openings for neoliberal economic reform, assessing such links requires close attention to the variety of contrary experiences across the world. The results presented here also underline the importance of disentangling policy changes as the result of crises from policy changes as the cause of crises. Findings in one direction, or the lack of findings in any direction, communicate very little without a clear strategy for assessing the direction of causality.

More recent studies have suggested that the link between crises and reforms is conditional on factors such as political institutions, government capacity, and economic

preconditions (Adams 2000; Alesina et al. 2006; Spolaore 2004; Tommasi 2005; Tommasi and Velasco 1996). In most of these studies, though, there is a presumption that given the opportunity and the capacity, governments react to crises with liberal macroeconomic reform, trade liberalization, labor market liberalization, financial liberalization, or other forms of orthodox policies. Still, heterodox policies have a powerful pull for governments facing economic crises—not because governments are incompetent or their policies incoherent, but because broad social coalitions may support heterodoxy. Capital account restrictions in the face of economic crises are clearly one such policy: their supporters may include wage laborers, industrial enterprises, rural peasants and landowners, and others. The findings here suggest that such pressures are quite influential. The question that political scientists must ask is why, in some cases, crises *do* lead to liberalization, rather than the opposite.

Studies of the politics of international monetary relations have yet fully to grasp the impact of such critical junctures as currency crises in prompting capital account policy change. The notion that currency crises cause liberalization has become so prevalent that scholars increasingly treat it almost as an afterthought. Current political explanations for capital account policy focus more on slow-moving variables such as regime type, macroeconomic conditions, global ideological trends, and competition for capital (see, recently, Brooks 2004; Chwieroth 2007; Li and Smith 2002; Quinn and Toyoda 2007; Simmons and Elkins 2004). These doubtless have a role to play, but governments facing the immediate costs of international capital mobility retain substantial leverage to enact self-protecting policies. Countries that have faced currency crises in the past, moreover, are more hesitant to embrace capital account liberalization than their counterparts that have not. Only by fully accounting for the simultaneous relationship between policy reforms and economic crises can we uncover the power of economic crises to affect future

policy trajectories in the international economy. Critical junctures such as economic crises can present regimes with unprecedented opportunities to implement policies that might otherwise face overwhelming domestic opposition. Nevertheless, currency crises impede capital account liberalization, they do not foster it.

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Table 1: *Summary Statistics*

Variable	Mean	Standard Deviation	Minimum	Maximum
CRISIS	0.092	0.289	0	1
*I**	7.464	2.950	0.660	16.378
KAOPEN	-0.536	1.199	-1.753	2.623
BALANCE	-4.745	8.866	-132.796	31.982
LNGDP	23.535	1.914	19.206	28.918
LNGDPPC	7.867	0.803	6.166	9.693
EXPENDITURE	14.907	6.513	2.900	64.392
GROWTH	1.392	6.116	-50.486	37.492
INFLATION	4.798	0.394	4.411	10.081
PRIVCRED	50.160	598.180	0	15231.810
RESERVES	3.263	3.043	-0.092	24.781
SAVINGS	16.170	11.121	-42.165	70.515
TRADE	64.998	38.076	2.576	280.361
PEG	0.508	0.500	0	1
POLITY	-1.989	6.808	-10	10
AVERAGE	-1.170	0.192	-1.382	-0.592

Table 2: *Baseline Models*[a]

	Bivariate Models			Multivariate Models		
	Model 1		**Model 2**	**Model 3**	**Model 4**	
Method	*Logit*		*OLS*	*OLS*	*IV*	
					First Stage	*Second Stage*
KAOPEN	-0.715***	*CRISIS*	-0.333***	*CRISIS*	-0.052	--	-1.510**
	(0.156)		(0.060)		(0.068)		(0.705)
		Constant	-0.366***	*BALANCE*	-0.007	-.003	-0.010
			(0.018)		(0.004)	.003	(0.007)
				LNGDP	2.605	-6.941*	-5.682
					(4.168)	(3.731)	(9.138)
				LNGDPPPC	-2.322	4.701	2.065
					(4.199)	(3.774)	(8.459)
				EXPENDITURE	0.015	-.013	-0.011
					(0.011)	(.008)	(0.019)
				GROWTH	0.000	.003	0.007
					(0.004)	(.003)	(0.007)
				INFLATION	-0.133	.032	-0.040
					(0.088)	(.051)	(0.109)
				PRIVCRED	-0.005*	.000	-0.004
					(0.003)	(.002)	(0.004)
				RESERVES	0.041**	-.014	0.015
					(0.017)	(.013)	(0.031)
				SAVINGS	0.005	.006*	0.015**
					(0.004)	(.003)	(0.007)
				TRADE	0.002	.001	0.006
					(0.003)	(.002)	(0.004)
				PEG	-0.091*	.014	-0.147*
					(0.052)	(.040)	(0.085)
				POLITY	0.005	.005	0.017
					(0.008)	(.006)	(0.014)
				AVERAGE	-0.404*	.421	-0.191
					(0.210)	(.308)	(0.560)
				COUNTER	0.006	.002	0.013
					(0.004)	(.004)	(0.008)
				KAOPEN(LAG)	-0.172***	-.019	-0.169**
					(0.042)	(.032)	(0.070)
				*I**	-0.015	.021***	--
					(0.011)	(.007)	
				constant	-0.094	--	--
					(0.145)		
				LM Test Statistic	--		8.745***
				Cragg-Donald Wald F Statistic	--		8.615**

[a] All models include country fixed effects, and parameter estimates for all variables except for *COUNTER* and *PEG* reflect first-differences in Models 3 and 4. Cells contain parameter estimates and standard errors. For parameter estimates and the LM test, * = statistically significant at $\alpha < .1$, ** = statistically significant at $\alpha < .5$, and *** = statistically significant at $\alpha < .01$. For Cragg-Donald Wald F test, ** equal 5% confidence of less than 20% IV bias.

Table 3: *Robustness Tests*[b]

	Model 5	**Model 6**	**Model 7**	**Model 8**	**Model 9**
Method	*IV*	*IV, LIML*	*IV*	*IV*	*IV, Clustered Errors*
CRISIS	-1.226**	-1.552**	-1.326**	-1.575**	-1.510**
	(0.614)	(0.772)	(0.650)	(0.725)	(0.723)
BALANCE	-0.010	-0.011	-0.010	-0.009	-0.010
	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
LNGDP	-3.711	-5.968	-4.164	-1.735	-5.682
	(8.215)	(9.481)	(8.574)	(6.906)	(9.108)
LNGPDPPC	0.756	2.255	0.960	-1.493	2.065
	(7.651)	(8.679)	(7.968)	(6.567)	(8.214)
EXPENDITURE	-0.007	-0.012	-0.010	-0.012	-0.011
	(0.017)	(0.020)	(0.018)	(0.018)	(0.020)
GROWTH	0.006	0.007	0.006	0.005	0.007
	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)
INFLATION	-0.050	-0.038	-0.026	-0.032	-0.040
	(0.099)	(0.111)	(0.104)	(0.109)	(0.105)
PRIVCRED	-0.004	-0.004	-0.004	-0.003	-0.004
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
RESERVES	0.021	0.014	0.025	0.017	0.015
	(0.0280	(0.032)	(0.029)	(0.029)	(0.035)
SAVINGS	0.014**	0.016**	0.013*	0.012*	0.015**
	(0.007)	(0.008)	(0.007)	(0.007)	(0.007)
TRADE	0.005	0.006	0.005	0.005	0.006
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
PEG	-0.142*	-0.147*	-0.126	-0.140*	-0.147*
	(0.077)	(0.086)	(0.080)	(0.079)	(0.080)
POLITY	0.016	0.018	0.018	--	0.017
	(0.013)	(0.014)	(0.013)		(0.011)
AVERAGE	-0.188	-0.192	-0.286	-0.154	-0.191
	(0.510)	(0.568)	(0.529)	(0.526)	(0.616)
COUNTER	0.013*	0.013	0.011	0.014*	0.013
	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)
KAOPEN	-0.161**	-0.170**	--	-0.176**	-0.169*
	(0.063)	(0.071)		(0.068)	(0.097)
Instrument(s)	*I*, (I*)*^{2}	*I*, (I*)*^{2}	*I**	*I**	*I**
LM Test Statistic	9.561**	9.561**	9.211***	8.895***	7.036**
Cragg-Donald Wald F Statistic	4.707	4.707**	9.102***	8.794**	--
Hansen's J Statistic	2.306	2.086	--	--	--
N	566	566	566	627	566

[b] All models include country fixed effects, and parameter estimates for all variables except for *COUNTER* and *PEG* reflect first-differences. Cells contain parameter estimates and standard errors. For parameter estimates and the LM test, * = statistically significant at $\alpha < .1$, ** = statistically significant at $\alpha < .5$, and *** = statistically significant at $\alpha < .01$. For Cragg-Donald Wald F test, ** equal 5% confidence of less than 20% IV bias, *** equals 5% confidence of less than 15% IV bias. Hansen's J statistic undefined for models 7-9 with a single exogenous instrument. Stock-Yogo critical values for Wald F test statistic not valid for Model 9 with heteroskedastic errors.

Table 4: *Durable Effects of Crises*[c]

	Model 10	**Model 11**	**Model 12**	**Model 13**	**Model 14**
Lead	1	2	3	4	5
CRISIS	-3.035*	-1.565***	-1.513***	-1.621***	-2.091***
	(1.691)	(0.574)	(0.537)	(0.476)	(0.576)
BALANCE	-0.005	-0.005	-0.001	0.009	-0.007
	(0.008)	(0.005)	(0.005)	(0.006)	(0.006)
LNGDP	-2.477***	-2.967***	-3.266***	-3.192***	-3.064***
	(0.922)	(0.541)	(0.535)	(0.528)	(0.589)
LNGDPPC	0.802	1.876***	1.906***	1.966***	1.763***
	(1.266)	(0.584)	(0.545)	(0.524)	(0.581)
EXPENDITURE	-0.046	-0.002	0.003	-0.020	-0.010
	(0.039)	(0.017)	(0.013)	(0.015)	(0.014)
GROWTH	-0.029	0.004	0.002	0.014**	0.025**
	(0.023)	(0.006)	(0.007)	(0.007)	(0.008)
INFLATION	0.197	-0.169	-0.367	0.074	0.122
	(0.406)	(0.229)	(0.270)	(0.190)	(0.258)
PRIVCRED	-0.005	-0.008***	-0.010***	-0.011***	-0.014***
	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
RESERVES	-0.008	0.014	0.025	0.030**	0.032*
	(0.033)	(0.018)	(0.016)	(0.015)	(0.017)
SAVINGS	0.017**	0.018***	0.023***	0.018***	0.027***
	(0.008)	(0.005)	(0.006)	(0.005)	(0.006)
TRADE	0.009**	0.007***	0.008***	0.008***	0.006**
	(0.004)	(0.002)	(0.002)	(0.002)	(0.003)
PEG	-0.523	-0.087	-0.073	-0.055	0.061
	(0.332)	(0.081)	(0.090)	(0.078)	(0.078)
COUNTER	0.106***	0.111***	0.128	0.129	0.138***
	(0.021)	(0.014)	(0.015)	(0.014)	(0.015)
LM-Test	4.406**	20.105***	26.186***	36.374***	30.739***
Cragg-Donald Wald F	4.349	20.309***	26.641***	37.421***	31.375***
N	746	800	868	930	971

[c] All models include country fixed effects. *Lead* denotes the lead length in the second stage equation. Cells contain parameter estimates and standard errors. For parameter estimates and the LM test, * = statistically significant at $\alpha < .1$, ** = statistically significant at $\alpha < .5$, and *** = statistically significant at $\alpha < .01$. For Cragg-Donald Wald F test, ** equal 5% confidence of less than 20% IV bias, *** equals 5% confidence of less than 10% IV bias.

Table 5: *Summary Statistics*

Variable	Mean	Standard Deviation	Minimum	Maximum
CRISIS98	0.230	0.422	0	1
KAOPEN	-0.003	1.447	-1.753	2.623
BALANCE	-5.779	8.785	-39.267	22.192
LNGDP	23.731	1.890	19.764	29.083
LNGDPPC	8.104	0.949	6.219	10.024
EXPENDITURE	15.774	7.305	4.402	52.332
GROWTH	1.825	5.796	-30.003	30.842
INFLATION	33.911	310.142	-24.076	3789.209
PRIVCRED	32.359	32.204	1.739	206.088
RESERVES	3.534	3.023	0.028	23.694
SAVINGS	15.979	9.619	-7.946	45.481
TRADE	81.374	41.954	1.531	275.232
PEG	0.458	0.500	0	1
POLITY	1.301	6.570	-10	10

Table 6: *Effects of Crises on Openness, 1998*[d]

	Model 15	Model 16	Model 17
CRISIS98	-0.163*	-0.566**	-0.231**
	(0.089)	(0.244)	(0.105)
BALANCE	0.008	0.006	0.004
	(0.007)	(0.011)	(0.007)
LNGDP	0.003	0.044	-0.039
	(0.032)	(0.057)	(0.038)
LNGDPPC	0.009	0.110	0.094
	(0.059)	(0.105)	(0.063)
EXPENDITURE	0.010	0.033*	-0.008
	(0.008)	(0.019)	(0.008)
GROWTH	0.005	0.000	0.000
	(0.008)	(0.013)	(0.015)
INFLATION	-0.009**	0.001	0.008
	(0.004)	(0.007)	(0.006)
PRIVCRED	-0.003**	-0.004	0.000
	(0.001)	(0.002)	(0.002)
RESERVES	0.052***	0.061***	-0.011
	(0.014)	(0.019)	(0.014)
SAVINGS	-0.009	-0.017	0.007
	(0.007)	(0.011)	(0.006)
TRADE	0.002*	0.004*	0.001
	(0.001)	(0.002)	(0.001)
PEG	-0.135	-0.234	-0.060
	(0.107)	(0.166)	(0.092)
POLITY	0.007	0.017	0.022**
	(0.009)	(0.013)	(0.010)
KAOPEN(LAG)	-0.009	0.920***	0.902***
	(0.034)	(0.072)	(0.044)
constant	-0.116	-2.067	0.189
	(0.744)	(1.498)	(0.736)

[d] Cells contain parameter estimates and robust standard errors. Dependent variable in Model 15 is change in *KAOPEN* between 1997 and 1998. Dependent variable in Model 16 is *KAOPEN* in 1999. All variables in Model 17 measures in 2003, except *CRISIS98*. * = statistically significant at $\alpha < .1$, ** = statistically significant at $\alpha < .5$, and *** = statistically significant at $\alpha < .01$

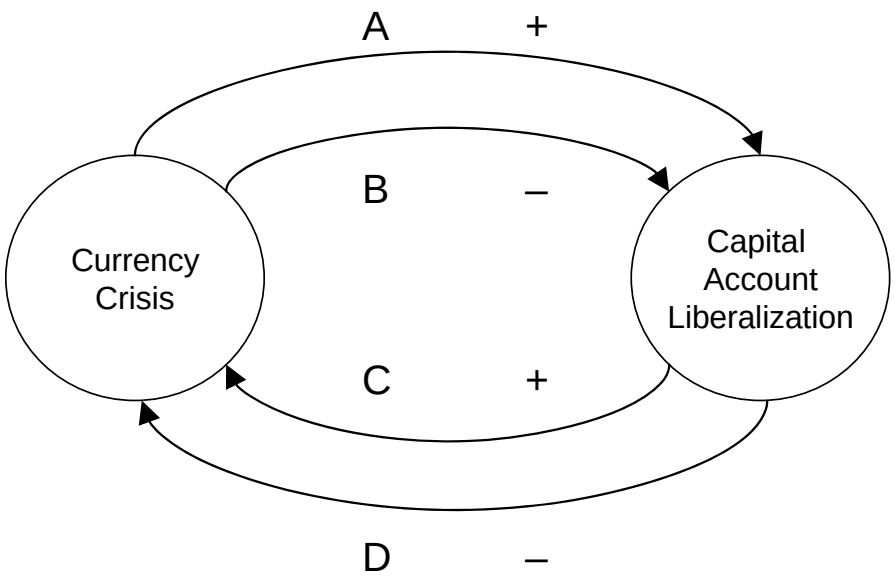

Figure 1: *From Currency Crises to Capital Account Liberalization*