

# Do Currency Crises Cause Capital Account Liberalization?<sup>1</sup>

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A central argument in the literature on economic crises and policy reform is that currency crises lead governments to liberalize their capital accounts in order to establish their credibility to international markets. I argue that the reverse is true: currency crises lead governments to restrict capital flows as a form of self-help. Using instrumental variables to account for the possibility that capital account liberalization causes currency crises, I show that currency crises are robustly associated with capital account closure across developing countries. The findings refocus the debate on currency crises and capital account liberalization, and contribute to larger debates about the role of critical junctures in prompting neoliberal policy reform.

## Introduction

A central claim in the international political economy of reform is that crises are key drivers of neoliberal policy reform. Studies of capital account liberalization have contributed significantly to this literature. In an early and influential article, Haggard and 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. These arguments linking currency crises to capital account liberalization are common in the large literature in the 1990s that linked economic crises to liberal economic reform in the developing

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world (Drazen and Grilli 1993; Haggard 2000; Haggard and Kaufman 1992; Little, Cooper, Corden, and Rajapatirana 1993; Nelson 1990; Rodrik 1996).

Yet there is strikingly little cross-national evidence that the suggestive results of these early investigations are more broadly applicable. 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—it is impossible to assess if examples of neoliberal reform are representative (Drazen 2000:449-454). Examples of retrenchment are easy to name. Zimbabwe, for example, has faced near continual economic crisis since 1995, yet Robert Mugabe’s government has yet to embrace anything approaching neoliberal economic reform (Clemens and Moss 2005). The second problem is endogeneity. The relatively small cross-national literature that tests the link between crises and adjustment (Abiad and Mody 2005; Biglaiser and DeRouen 2004; Campos, Hsiao, and Nugent 2006; Drazen and Easterly 2001) ignores 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 also to find strategies for disentangling the direction of causality between them.

Indeed, there are reasons to suspect that the link from currency crises to capital account policy may commonly run counter to the neoliberal impulse to respond to crises by eliminating capital account restrictions and embracing foreign capital. Heterodox responses to currency crises are quite common: President José López Portillo famously closed Mexico’s capital

account in response to the Latin American debt crisis in 1982, as did Prime Minister Mahathir Mohamad of Malaysia in 1998. Moreover, the direction of causality is unclear, for policy makers and scholars alike frequently charge that capital account liberalization itself is the cause of currency crises, not the effect of them. 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 (Brooks 2004; Brune and Guisinger 2007; Chwioroth 2007). Abiad and Mody (2005) do find a positive association between currency crises and financial reform. However, their index of “financial reform” includes controls on credit and interest rates and regulations on entry, ownership, and staffing of financial institutions in addition to restrictions on capital account transactions (Abiad and Mody 2005:68), making it difficult to assess just what aspects of financial reform are driving these results. Additionally, the authors do not address the possible endogenous relationship between crises and openness.

This paper studies the links between currency crises and capital account liberalization, using instrumental variables to identify the strength of the relationship between the two. Economic theory suggests that for the period of time in which previous research has held that crises should prompt capital account liberalization—the quarter century following the collapse of the Bretton-Wood arrangement—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 industrial economies. 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 during this period have no other unobserved relationship to capital account policy than through this mechanism. This makes them an ideal instrument for currency crises, allowing us to identify statistically the relationship between currency crises and subsequent financial liberalization.

Using this empirical strategy, I find systematic evidence that contrary to prominent arguments, on average in the developing world, currency crises cause capital account *closure*. I therefore find that crises drive change, but not towards openness. I also show that democratic political institutions and veto players mediate this relationship—but while I find that political constraints can prevent regimes from responding to crises with closure, under no circumstances do these institutions promote subsequent liberalization. My primary finding is therefore that contrary to common arguments linking economic meltdowns to neoliberal reforms, currency crises hamper capital account liberalization rather than facilitating it.

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 causal link between the two. I also draw on the literature on institutional design to suggest how institutional constraints—in particular, regime types and veto players—may affect the ability of regimes to respond to crises with reform. I then describe my empirical strategy for overcoming 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. I also investigate whether political institutions condition the relationship between crises and reforms.

One possible concern is that by ending my instrumental variables analysis at 1995, I am omitting relevant data. I argue in the following section that while it violates the assumption of relevance to employ Northern interest rates as an instrument for currency crises from 1996 on, there is no evidence that more recent currency crises have been caused by short- or medium-term capital account policy changes. Without the threat of endogeneity, it is possible to analyze data from the post-1995 period using standard panel data techniques. Accordingly, I present results from 1997-2005 that demonstrate that countries that experienced currency crises are more likely to have closed their capital accounts than their counterparts that 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 in the study of policy reform.

### **Linking Crises to Liberalization**

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, Satyanath, and Sergenti (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.

Additionally, 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. Even proponents of market-oriented reform question whether capital account liberalization should be included as a policy prescription for developing countries. Most notably, John Williamson (2002) has argued that capital account liberalization was never part of the mix of policy reforms that he identified as the “Washington Consensus.” However, in the same remarks he also states that capital account liberalization is a “quintessentially neoliberal idea.” So while not all market-oriented policy reformers or proponents of the Washington Consensus have favored capital account liberalization, it is clear that such policies are widely understood as neoliberal reform. It is also clear that in studies of neoliberal policy reform, capital account liberalization plays a central role (Drazen and Grilli 1993; Haggard 2000; Haggard and Kaufman 1992; Little et al. 1993; Nelson 1990; Rodrik 1996).

To understand why a currency crisis might lead to changes in capital account policy, it is helpful to review how economic crises generate policy reform in general. One central mechanism by which crises generate reform is by changing the relative strengths of domestic interest groups (Nelson 1990). For example, a group finds status quo policies costly under normal times, but not costly enough to justify lobbying for reform. An economic crisis, though, increases the costs of the status quo policy, and hence increases the group’s incentives to lobby for changes to that policy, resulting in a shift in the relative strength of different pressure groups. Relatedly, crises may change group interests themselves. In the case of currency crises, firms that previously favored capital openness to welcome capital inflows may come to oppose capital openness to discourage capital outflows and currency speculation. Such a change in interests will also result

in a change in the relative strengths of the domestic constituencies for different policies. Because governments are sensitive to changes in pressures from their constituents—resulting from changes in the nature or in the intensity of domestic preferences over policy outcomes—an economic crisis generates new incentives for policymakers to adopt policy reforms.

These accounts focus on the distributive politics of crises and economic reform. However, most scholars recognize that any account of policy reform must also consider the political environment in which distributional struggles take place (Adams 2000; Alesina, Ardagna, and Trebbi 2006; Spolaore 2004; Tommasi 2005; Tommasi and Velasco 1996). As I argue below, institutions shape both the way in which interests are aggregated and the ability of policymakers to respond to shifting political pressures. A complete understanding of how crises generate reform must take into account both the distributional effects of crises and the institutional environments under which policy reform takes place.<sup>2</sup>

Moving from this general account of crises and reform to the specific crises addressed in this paper, there is no theoretical consensus on just how currency crises influence capital account policy. There are four possible relationships between the two, each of which has received some attention in the literature. The path diagram below summarizes possible arguments.

-- Figure 1 here --

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<sup>2</sup> Distributional politics and institutional constraints are by no means the only factors that shape government responses to economic crises. Constructivists, for example, argue that the reforms enacted after crises depend on policymakers beliefs about how reforms will function, on their beliefs about what policies are appropriate, on the advice that they receive from their advisors and subordinates, and even on their understanding of what a crisis “is.” This literature is now substantial; for recent overviews, see Seabrooke (2007:795-801) and Darden (2009:23-50).

Paths A and B lead from currency crises to capital account liberalization, but suggest different effects—either currency crises lead to increased liberalization, or decreased liberalization. As I argue below, this is due to an implicit disagreement on the net distributional consequences of currency crises. 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. These paths reveal why it is difficult to determine just how currency crises result in policy changes.

Haggard and Maxfield focus on Path A, arguing the currency crises prompt financial internationalization, which they operationalize as 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 domestic financial difficulties ease. Using data on capital account policies from the IMF's Annual Report on Exchange Arrangements and Exchange Restrictions, the authors find that currency crises in Mexico, Chile, Indonesia, and South Korea were associated with subsequent liberalization of capital account policies. Focusing on the more general problem of a potentially creditworthy government facing a need for foreign capital, Bartolini and Drazen (1997) independently propose that capital account liberalization signals government resolve and capability to foreign creditors. Alternatively, currency crises may prompt capital account liberalization because of the dynamics that they generate among affected governments. Mukherjee and Singer (2010), for instance, argue that when countries receive IMF loans after crises, so long as they have capacity to compensate the losers from liberalization, they will liberalize their capital accounts to further attract foreign capital. These arguments each suggest that currency crises prompt capital account liberalization.

The logic of these arguments rests on the interaction between domestic interests and the external economic environment. Crises heighten domestic pressures for access to foreign capital from domestic constituents, primarily from firms seeking access to foreign capital but also potentially from individual borrowers eager to access foreign capital as well. Currency crises cause a sharp increase in firms' need for foreign capital along with a decrease in foreign lenders' willingness to provide it. Increased demand for foreign capital increases domestic firms' willingness to lobby for policies that will attract it. Of course, such firms may favor capital account liberalization even absent a currency crisis; the point is that currency crises (or, for Mukherjee and Singer (2010), the IMF loans that follow crises) independently increase the costs of closure and therefore increase their willingness to pressure governments for liberalization. This domestic lobbying interacts with the international constraint of government credibility to foreign investors, who in the context of a currency crisis are more eager for reassurance that a government is committed to favorable policies. Because capital account liberalization entails surrendering some autonomy over interest rates or exchange rates, it is a costly signal of a government's commitment (and hence creditworthiness) to foreign investors. Together, these forces yield a relative strengthening of domestic and international pressure for capital account liberalization. Because governments respond to such pressure, all else equal this change results in increased incentives for governments to respond to currency crises with capital account liberalization, a policy that reassures foreign investors that a government is committed to good policies, and one that domestic pressure groups demand. This yields 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 currency speculation,

and to change the composition of capital inflows from speculative hot money to “safer” and more stable 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 1996:151-182), 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).

The logic behind these responses is similarly political. Whereas the argument that currency crises lead to capital account liberalization rests on the presumption that the primary distributional effect of currency crises is to strengthen borrowers’ demands for foreign capital, the argument the currency crises lead to capital account closure highlights other distributional effects of currency crises which outweigh new demands for foreign capital. Currency crises have contractionary real sector consequences (Calvo and Reinhart 2000; Frankel 2005), and as such, they generate demands for compensatory adjustment policy from the sectors affected by them. The Mundell-Fleming trilemma shows that expansionary macroeconomic policy is only possible by either abandoning discretion over the exchange rate or by restricting cross-border capital movements. Accordingly, to gain the policy autonomy necessary to counteract the real effects of currency crisis with expansionary policies while simultaneously combating the currency crisis itself, governments must impose capital controls.

Who should favor such policies? Workers facing retrenchment or wage cuts due to the contractionary effects of a currency crisis, which Baldacci, Inchauste, and de Mello (2002) show

have particularly negative effects on labor and the poor, should pressure governments to abandon free capital mobility to make expansionary policies feasible. Domestic producers of non-tradables (which may include businesses but will also include peasants and rural small-holders) should favor the same (see Calvo and Reinhart 2000:178-179 for a discussion of the real and financial effects of currency crises on nontradables). Export-oriented firms favor a *depreciated* currency, which a currency crisis will provide, but still may suffer from other effects of currency crises, such as currency volatility, financing constraints, and a weak real sector that results in costlier inputs and deteriorating infrastructure (see Blalock and Roy 2007:42-44). So even though they may favor currency depreciation, exporters will not prefer to obtain depreciation via a currency crisis. Mitigating all these problems requires both currency stability and discretionary macroeconomic policy, and hence capital account restrictions. Even those firms that value access to foreign capital for all of the reasons described in Path A and unaffected by any of the concerns just listed may still favor capital controls in response to a crisis. If these firms hold foreign currency debt (which is common in emerging market economies, see Eichengreen and Hausmann 1999), a currency crisis will result in increased debt service costs, so they will demand expansionary policies along with an stable and appreciated exchange rate to protect their balance sheets—which will only be feasible with capital account restrictions (Pepinsky 2009).

The resulting pressure for capital account closure may accordingly encompass a number of distinct groups, from wage laborers to indebted firms to rural peasants to domestic producers of non-tradables and even exporters; in all, any actor who fears currency volatility and the real consequences of a currency crisis and demands the autonomy to enact compensatory adjustment policies. The point is not that these groups favor capital account closure itself, but rather that they favor the other policies that capital account closure makes feasible. Such broad cross-class

and cross-sector coalitions in favor of macroeconomic policy autonomy and currency stability were evident following currency crises in countries like Malaysia, Mexico, and Argentina, cited above. By this argument, the distributional effects of currency crises are not to empower groups demanding that the government incentivize foreign capital to return through capital account liberalization (as in Path A), but rather to empower those that demand that the government to prevent foreign capital from leaving through capital account closure. This yields 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 better 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 (Prasad, Rogoff, Wei, and Kose 2003). 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, Westermann, and Martinez 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. Such research focuses on the policy autonomy 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 and Hutchison 2005; Glick, Guo, and Hutchison 2006; 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 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.

Many recent studies have suggested that the links between crises and reform are conditional on factors such as political institutions and government capacity (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 reform measures. The arguments above suggest caution in presuming that crises cause liberalization, but it is possible that the nature of the relationship between currency crises and capital account liberalization obtains only under certain political conditions. Might political institutions shape the types of policy responses that countries adopt, thereby mediating the relationships identified in Figure 1?

Taking insights from this literature, there are at least two areas in which we can expect that they do. First, currency crises are particularly threatening to authoritarian regimes, necessitating firm policy responses by regimes that face the prospect of losing power to mass opposition movements (Pepinsky 2009). For similar reasons, numerous authors have argued that economic crises of all types are key determinants of authoritarian breakdowns (Gasiorowski 1995; Haggard and Kaufman 1995). Recent work also suggests that capital mobility is in the long run incompatible with authoritarian rule (Boix 2003), suggesting that forward-thinking authoritarian regimes concerned with regime survival should be keen to restrict capital mobility during periods of currency turmoil. Therefore, while the negative effects of currency crises are broadly felt in both democracies and dictatorships, dictatorships may have a particularly strong incentive to respond to currency crises with capital account closure in order to insulate themselves from international financial volatility.

The opposite may also be the case, however. The credibility problems discussed by Haggard and Maxfield (1996) and Bartolini and Drazen (1997) suggest that regimes that are already considered less credible or transparent by international capital markets are the ones for whom capital account liberalization is most useful. The lower levels of policy transparency in authoritarian regimes should make them the very types of political regimes that most benefit from the credibility-enhancing benefits of capital account liberalization during currency crises. If Hypothesis 1 is true—a testable proposition examined below—authoritarian regimes may therefore have *stronger* incentives to respond to currency crises with capital account liberalization. All four cases that Haggard and Maxfield analyze—Indonesia, South Korea, Mexico, and Chile—were ruled by authoritarian regimes during the crises under consideration,

which may have made them uniquely interested in responding to currency crises with capital account liberalization.

The second way in which political institutions may shape responses to currency crises is by constraining the ability of regimes to respond to them. This argument is straightforward. When policy-making authority is widely shared across a large number of veto players (Tsebelis 2002), all of whom must agree on a policy change in order for it to be implemented, governments are constrained from responding effectively to acute economic crises. In the context of currency crises, MacIntyre (2001) argues that this very problem of a fragmented political system prevented Thailand from adopting any immediate policy response to the Asian Financial Crisis, while its neighbors with centralized political systems—Indonesia and Malaysia—responded quickly (if not always effectively). Following these insights, political systems which contain high numbers of veto players will limit the ability of regimes to respond to currency crises with any capital account policy response, either liberalization or closure. If so, the higher the number of veto gates, the less able a country will be to respond to a currency crisis with any type of policy change.

These possibilities mean that there are a number of additional conditional hypotheses that we might test in which political institutions condition the relationship between currency crises and changes in capital account openness. Note that even though these arguments focus on the conditional effects of political institutions on capital account policy change, to test them we still must disentangle the direction of causality between currency crises and capital account policy.

### **Research Design**

I employ instrumental variables (IV) to identify the relationship from currency crises to capital account policy change. The choice of instrument here is not obvious. Most predictors of

currency crises are domestic factors such as financial sector size and policy variables such as exchange rate regime, all of which themselves also affect capital account policy directly. The ideal instrument should be one that both has a causal relationship with currency crises and which cannot affect a government's capital account policy choices except through currency crises. International variables outside of governments' control are one attractive option; among them, one common predictor of currency crises which has received considerable theoretical attention is the behavior of interest rates in advanced industrial economies, which I refer to as "Northern interest rates."

Instrumental variables regressions require strong substantive assumptions about the instruments' relationship with other variables. Instruments must be (1) conditionally independent from the error term of the "true" (but unobserved) regression model and (2) correlated with the endogenous variable. The first requirement is that the instrument is "valid," and the second requirement is that the instrument is "relevant." Beyond simple correlation, though, an instrument must explain a substantial proportion of the variation in the endogenous variable (Murray 2006:122-123). In what follows, I test as rigorously as possible 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.

#### *Identification Strategy: Northern Interest Rates*

My identification strategy rests on the claim that the behavior of Northern interest rates provides ideal instruments for currency crises in the developing world. They have a clear relationship with currency crises throughout the 1970s, 1980s, and early 1990s, 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.

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, Northern interest rates are nevertheless a valid instrument for currency crises. 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 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 Chwioroth 2007; Glick and Hutchison 2005; Glick et al. 2006).<sup>3</sup> The

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<sup>3</sup> Abiad and Mody (2005) do find a relationship between U.S. interest rates and financial reform, broadly conceived. However, because their dependent variable captures interest rate liberalization, the removal of credit ceilings, and other components of financial liberalization, these results are likely driven by components of the financial reform index other than capital account liberalization. A reanalysis of their data (available upon request) confirms that their dependent variable is stripped of these other components, the relationship between U.S. interest rates and financial reform disappears. Note also that their instrument is the contemporaneous

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 the instrument and the error term of the true regression. 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 if developing country governments respond to evaporating capital stocks by tightening capital account restrictions. As a consequence, Northern interest rates have a second potential pathway that affects capital openness. Yet the requirement of *conditional* independence means that accounting for this second pathway can recover the independence between the instrument and the error term in the true regression. 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.<sup>4</sup>

Despite these arguments, and the theoretical attractiveness of Northern interest rates as instruments for currency crises, it is important to emphasize that failure to meet the exclusion

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U.S. interest rate, while mine include the lag of and change in a weighted average of six lending country interest rates.

<sup>4</sup> This point is subtle, but too frequently overlooked. See Wooldridge (2002:88).

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 instrument is associated with the endogenous variable. One can also test for weak instruments by specifying a level of confidence and finite sample IV 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 instrument strength (Stock and Yogo 2005). These reassure us that the estimates do not suffer from bias arising from either underidentification or weak identification.

To ensure validity, I include control variables for private capital stocks, foreign reserves, and the exchange rate regime, each of which captures potential alternate pathways through which the assumption of conditional independence might be violated. In addition, I present results from overidentification tests, the results of which are consistent with my argument that Northern interest rates are valid instruments in my analysis.

#### *Data and Methods*

The baseline instrumental variables model uses data from 1971 to 1995, and takes the following form:

$$\Delta Capital\ Account\ Policy_{it} = \beta_1 \widehat{Crisis}_{it-1} + \beta_{2\dots k} Controls_{it} + e_{it} \quad (1)$$

$$Crisis_{it-1} = \gamma_1 I_{it-1}^* + \gamma_2 \Delta I_{it}^* + \gamma_{3\dots k} Controls_{t-1} + u_{it} \quad (2)$$

where  $i$  indexes countries and  $t$  indexes five-year averages of all variables. The five year averages help to smooth out year-specific shocks, but substantively similar results can be obtained from models using yearly data.

The dependent variable is the change in capital account policy, which I measure in the baseline specification using Chinn and Ito's (2008) *KAOpen*. *KAOpen* 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. *KAOpen* has a number of strengths: it is measured consistently across countries, and by aggregating information on indicators such as the presence of multiple exchange rates and current account restrictions, it allows for a more nuanced measure of "capital account openness" than does a binary variable coding the presence or absence of capital account restrictions.<sup>5</sup>

However, there are three potential problems with the index. First, *KAOpen* is constructed using five-year moving averages of the IMF's capital account policy indicator, which means that current values of *KAOpen* can reflect policy changes from the previous five years. This raises the possibility that what the index codes as changes in policies *following* a crisis are actually changes *preceding* the crisis (Karcher and Steinberg 2009). Second, *KAOpen* includes the presence of current account restrictions as an indicator of capital account policy. But because the IMF's *Articles of Agreement* explicitly state in Section VII 3(b) that states may restrict current account transactions when facing currency crises, changes in *KAOpen* may reflect the imposition of restrictions on current account transactions which follow currency crises but which are conceptually unrelated to the changes in capital account policy which I seek to measure. Third, the IMF changed how it coded capital account policy between 1996 and 1997. This means that the data from *KAOpen* prior to 1996 are not directly comparable to data from 1997 onward.

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<sup>5</sup> The logic would be as follows: a country that is fully closed to capital account transactions might "intensify" its capital account closure by, for example, choosing to require exporters to surrender export proceeds. *KAOpen* will capture this increase in the intensity of capital controls.

Attuned to these concerns, I discuss below alternative measures of capital account policy changes that confirm that my results do not depend on these potential issues with the *KAOpen* index.

The data for currency crises (*Crisis*) and debt-weighted Northern interest rates ( $I^*$ ) come from Andrew Berg and Catherine Pattillo (1999).<sup>6</sup> These data comprise the broadest available coverage of developing country currency crises between 1971 and 1996, with data available for 103 countries.<sup>7</sup> 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 that is substantially different than the country's normal exchange rate trajectory.  $I^*$  is constructed from the 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 composition ensures that this variable's values are specific to each borrower.

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<sup>6</sup> The data update the measures in Frankel and Rose (1996).

<sup>7</sup> Following Berg and Pattillo (1999), Frankel and Rose (1996), and others on the causes and consequences of currency crises, I implement one-year leads and three-year windows around crisis onsets to ensure that all measured crises are independent events. See also Milesi-Ferretti and Razin (1998). I implement these *prior* to creating five-year averages in the instrumental variables analysis.

I employ a one-period lag ( $I_{it-1}^*$ ) along with the one-period change ( $\Delta I_{it}^*$ ) in Northern interest rates as instruments for the lags currency crises in each five-year period.<sup>8</sup>

The sample period (from 1971-1995) includes dozens of currency crises in Latin America, Africa, the Middle East, and Asia. In principle, it is possible to extend the dataset past 1995 to include more recent crises. But as I detail below in discussing my results from the more recent period, there is no evidence that currency crises since the mid-1990s have resulted from rising Northern interest rates. This fact has an important consequence. It means that using my identification strategy on data from more recent crises would violate the assumption of “homogenous partial effects,” which holds that “variation in the endogenous regressor related to the instrumental variable must have the same causal effect as variation unrelated to the instrument” (Dunning 2008:391). This does not mean that post-1995 crises are irrelevant or uninteresting, only that a different estimation strategy is necessary in order to study them. For this reason, I study these crises below separately, and using different methods. One important benefit, though, of studying the pre-1996 crises separately is that the structural break in how *KAOpen* was coded does not affect the results from these models.

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 Bank’s 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

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<sup>8</sup> The logic is that both the mean of  $I^*$  within each five-year period and the one-period changes in  $I^*$  should predict the frequency of currency crises within that period.

growth (*Growth*); the natural logarithm of 100 plus consumer price inflation (*Inflation*)<sup>9</sup>; national reserves as months of imports (*Reserves*); and government final consumption expenditure (*Expenditure*), national savings (*Savings*), current account balance (*Balance*), private credit (*Private Credit*), 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 regime type from Polity IV (*Polity2*) (Polity IV Project 2006). A period index variable (*Counter*) increases by 1 for every period after 1971-75 to account for unobserved temporal factors. The one-period lag of *KAOpen* accounts for the fact that countries differ in their initial levels of capital account openness, which in turn affects their ability to respond to crises with further capital account liberalization or closure. Summary statistics for these variables are presented in Table 1.

-- Table 1 here --

This list of control variables is extensive, and can be divided into three groups. The first group includes standard economic determinants of capital account policy that capture the baseline propensity of a country to have an open capital account. These include *Lngdp*, *Lngdppc*, and *Growth*, each of which 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. *Polity2* tests the link between political regime and capital account liberalization, an argument often increasingly made in research on financial liberalization (Dailami 2000; Eichengreen and Leblang 2008).

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<sup>9</sup> I add 100 to the annual rate of inflation because some countries experience negative inflation (that is, deflation), the natural log of which is undefined.

The second group of independent variables includes those economic and policy variables which might affect the willingness of a country to open or close its capital account. *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. *Private Credit* 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.

The third group captures the various factors which might be correlated with both the existence of a currency crisis and changes in capital account policy. Omitting any of these would increase the likelihood that I would find a spurious relationship between currency crises and capital account policy changes. Among these, *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. *Expenditure* 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.

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. *Private Credit* and *Reserves* capture domestic credit stocks and official reserve stocks, each of which are alternative pathways through which Northern interest rates might affect capital account policies. Including *Peg* 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—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.<sup>10</sup> With evidence that unobserved heterogeneity is indeed a problem, in all subsequent analyses I estimate IV models with fixed effects. I also test for heteroskedasticity. Pagan-Hall tests performed on the residuals of an unconstrained fixed effects IV model give mixed results, so I report default standard errors in most models but also estimate models with robust standard errors (clustered by country) as robustness tests.<sup>11</sup>

Finally, I check the robustness of these findings to alternative definitions of the dependent variable. Recall that *KAOpen* includes a moving average of capital account policy for the previous five years as an indicator of current capital account policy, and that it also includes other kinds of capital account restrictions as indicators of the intensity of capital controls. To

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<sup>10</sup> Hausman (1978).  $\chi^2(15) = 86.12$ .

<sup>11</sup>  $\chi^2(94) = 84.035$  ( $p = .760$ ) for a test using all independent variables and their squares and cross-products as indicators of heteroskedasticity;  $\chi^2(1) = 4.822$  ( $p = .029$ ) for a test using the IV-predicted value of the dependent variable as an indicator of heteroskedasticity. See Baum, Schaffer, and Stillman (2003) and Pesaran and Taylor (1999).

ensure that these two problems are not driving my results, I create two alternative dependent variables. The first, following Karcher and Steinberg (2009) is an index defined as the first principle component of the four binary indicators of capital account restrictions. This variable, denoted *CKAOpen*, is identical to Chinn and Ito's index, except for all indicators come from a single year, which removes any potential concern about prior changes in capital account policy appearing as the downstream effects of currency crises. The second alternative dependent variable is the binary indicator variable coded as zero in the presence of capital account restrictions, and one otherwise, called *K3*. This variable focuses tightly on capital account policy to the exclusion of other restrictions on international transactions—each of which may indicate the intensity of capital account liberalization or closure, but each of which might also pick up other policy changes that occur during currency crises. As a final check to ensure that measurement of the dependent variable is not driving these results, I also include Quinn and Toyoda's (2008) index of capital account openness. Their index, *Cap100*, ranges from 0 to 100; to facilitate comparison with *KAOpen* and *CKAOpen*, I rescale Quinn and Toyoda's variable and created a variable *S\_Cap* with mean 0 and standard deviation of 1.

## Results

The main results appear in Table 2. The first two columns (Model 1) display the first and second stage regressions of the baseline model with *KAOpen* as a dependent variable. The results support Hypothesis 2, that currency crises lead countries to close their capital accounts.

-- Table 2 here --

In the first stage of Model 1, we see that both lags and changes in Northern interest rates are statistically significant predictors of currency crises. The partial  $R^2$  of the excluded instruments is 0.132, which reassures us that Northern interest rates are a relevant instrument for currency

crises (Shea 1997).<sup>12</sup> The estimate on the currency crisis variable in the second stage is negative, indicating that currency crises lead to *decreases* in capital account openness. The remaining columns display the results for the same regression using different measures of capital account policy: *CKAOpen* (Model 2), *K3* (Model 3), and *S\_Cap* (Model 4). The results for all four models are in accordance with one another: currency crises cause capital account closure, not capital account liberalization.

Looking to the model statistics at the bottom of Table 2, we learn how well the instrumental variables strategy has performed. First, the LM test statistic confirms that both the level and change in Northern interest rates are relevant instruments for lagged currency crises. Second, the significance of the Cragg-Donald test statistic in Models 1-3 indicates that weak instruments are unlikely to be a problem. In Model 4, however, the Cragg-Donald test statistic does not reach conventional levels of significance, suggesting that the model may suffer from weak instruments bias (Bound, Jaeger, and Baker 1995) when measuring capital account policy changes using *S\_Cap*. I return to this issue in the robustness tests. For now, the insignificance of the Hansen's J statistics in all four models is consistent with the null hypothesis that the model's overidentifying restrictions are valid.

In Table 3 I present the results of these robustness tests as well as models that include interaction terms between crises and political institutions. I discuss these two sets of models in turn.

-- Table 3 here --

Models 5-8 in Table 3 include a series of robustness tests to confirm that the inferences drawn from the baseline results in Table 2 are not an artifact of modeling choices. Weak

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<sup>12</sup>  $F(2,147) = 11.22$  ( $p < .001$ ).

instruments make the results in Model 4 questionable. In Model 5, I re-estimate Model 4 using Limited-Information Maximum Likelihood and find that these results are robust to the weak instruments problem. In Model 6 I drop *Polity2* from the analysis. 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 Príncipe, 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. In Model 7 I drop the lag of capital account policy (in this case *KAOpen*) from the analysis. In Model 8, to ensure that heteroskedasticity does not affect the inferences that we draw, I estimate a model with country-clustered robust standard errors. In all models, the effect of crises, instrumented by Northern interest rates, on changes in capital account policy is significant and negative. Substantively identical results, in the replication materials, were obtained in models using the three alternative measures of capital account policy (*CKAOpen*, *K3*, and *S\_Cap*).

The findings presented thus far indicate that across cases, currency crises lead to capital account closure. However, as discussed above, there are good reasons to think that political institutions may mediate the relationship between currency crises and capital account policy. To test whether or not this is the case, I estimate a series of regressions that interact the currency crisis variable with two measures of political institutions. I measure institutions in two ways, reflecting two different kinds of institutional constraints on policy choices identified in Section 2 above. To measure whether regime type conditions policy responsiveness, I use *Polity2*, described previously. To measure institutional constraints through veto players, I use the *Polcon* dataset from Henisz (2000).

Since *Crisis* is endogenous, its interaction with each institutional variable is endogenous as well. I use the interaction of  $I_{it-1}^*$  with each institutional variable to create an additional instrument. I accordingly estimate the following model:

$$\Delta Capital Account Policy_{it} = \beta_1 \widehat{Crisis}_{it-1} + \beta_2 Institutions_{it} + \beta_3 Crisis_{it-1} \times \widehat{Institutions}_{it} + \beta_{4...k} Controls_{it} + e_{it} \quad (3)$$

$$\frac{Crisis_{it-1}}{Crisis_{it-1} \times Institutions_{it}} = \quad (4)$$

$$\gamma_1 I_{it-1}^* + \gamma_2 \Delta I_{it}^* + \gamma_3 Institutions_{it} \times I_{it-1}^* + \gamma_{4...k} Controls_{it} + u_{it}$$

The results from these models appear as Model 9 and Model 10 in Table 3. In baseline specifications (not reported) both models are identified, but weakly: test statistics barely miss critical values needed to reject the null of weak identification. Therefore, Models 9 and 10 are estimated using Limited Information Maximum Likelihood. Moreover, inferential statistics that are robust to weak instrumentation (reported in Table 3 as the Anderson-Rubin F Statistic) always reject the null hypothesis that the coefficients on *Crisis* and its interaction with each measure of political institutions are jointly equal to zero at  $p < .001$ . So while parameter estimates should be treated with caution, we can be confident that the endogenous variables are jointly significantly different than zero.

The point estimates in Models 9 and 10 of the interactive effects of currency crises and institutions are insignificant, suggesting that institutions do not condition the effects of crises on capital account liberalization. But because it is difficult to interpret the substantive results of interactive models from regression tables alone, in Figure 2 I plot the marginal effect of a crisis on capital account policy changes across the observed values of *Polity2* and *Polcon* following the methods in Brambor, Clark, and Golder (2006).

-- Figure 2 here --

The figures confirm that institutions have a weak interactive effect on currency crises and capital account policy changes. Across the range of values for both *Polity2* (Panel A) and *Polcon* (Panel B), the marginal effect of crises on capital account liberalization is negative. These estimates are statistically indistinguishable from zero in the most authoritarian regimes, as well as in regimes with the fewest veto players, but in more democratic regimes and regimes with more veto players, the estimates achieve significance. These results indicate that in some political contexts there is no relationship between crises and liberalization, but also show that there are no conditions under which Hypothesis 1 holds: political institutions never encourage capital account liberalization in response to currency crises. As before, substantively similar results, available in the replication materials, were obtained in models using the three alternative measures of capital account policy (*CKAOpen*, *K3*, and *S\_Cap*).

### **Post-1996 Evidence**

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 1995. Currency crises that have occurred in the past decade are 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 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 2008:391) may be violated. Recall, moreover, that this possibility justified censoring the data at 1995 for the instrumental variable models. Yet post-1996 crises are

important and deserve study for two reasons. First, they shed light on the effects of crises on capital account openness in recent years. Second, 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 true, I examine available yearly data from 1997-2005, the most recent year for which cross-national data are available (2006 is lost due to differencing), using standard panel data techniques.<sup>13</sup> These crises had no relationship to changing terms of credit with developed country lenders. They are also unrelated to short-term changes in capital account policy, which means that simultaneous causality between currency crises and subsequent capital account policy changes does not threaten the inferences that we draw from them (see e.g. Furman and Stiglitz 1998 on crisis onsets in Asia). To reiterate, because we can be confident that these crises after 1996 are not endogenous to capital account policy changes, we need not worry about endogeneity bias (which necessitated the instrumental variables for the 1971-1995 period). I employ Leblang's (2005) currency crisis data to measure crises, with a variable I call *Crisis2*. 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, Rose, and Wyplosz 1995 for a precise definition). All other variables are measured from the same sources, and capture the same hypotheses.

The results appear in Table 4, and are consistent with the instrumental variables models that cover the period from 1971-1995.

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<sup>13</sup> Because of the shorter time period, in these models I investigate the effect of a crisis on contemporaneous changes in capital account policy.

-- Table 4 here --

In Model 11 the coefficient on *Crisis2* is negative but statistically insignificant, perhaps due to the problems inherent in using *KAOpen* on yearly data. When purged of the five-year average of capital account restrictions (*CKAOpen* Model 12) and when measured as a simple binary indicator (*K3*, Model 13), estimates return to conventional levels of significance.<sup>14</sup> Models 14 and 15 include interaction terms to test, as above, whether political institutions mediate the relationship between crises and capital account policy changes. The interactive effects are displayed in Figure 3.

-- Figure 3 here --

As before, there is only weak evidence that institutions mediate the effects of currency crises on capital account policy. Across nearly the entire range of values for both *Polity2* and *Polcon*, the marginal effect of crises on capital account liberalization is negative. As before, estimates are statistically indistinguishable from zero in the most authoritarian regimes, as well as in regimes with the fewest veto players. In this case, however, estimates are also insignificant at conventional levels in the most democratic regimes as well. The point estimate of the interactive effect of *Polity2* and *Crisis2* is positive (see Panel A of Figure 3) but this never generates a statistically significant marginal effect of regime type on crises and capital account policy changes. These results indicate that in some political contexts there is no statistically significant relationship between crises and liberalization. Nevertheless, they also confirm that there are no conditions under which Hypothesis 1 holds: political institutions never encourage capital account liberalization in response to currency crises.

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<sup>14</sup> The Quinn data end at 1999 so I cannot use *S\_Cap* as a robustness test for these models.

The results of these 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 outside of the period during which we can use Northern interest rates to instrument for crises, which suggests that my findings extend beyond the period of 1971-1995 to further rounds of currency crises across the developing world. Even more importantly, though, these findings underline the substantive inferences drawn from my instrumental variables identification strategy. Currency crises caused by factors other than increases in Northern interest rates still produce the same policy responses, both unconditionally and accounting for the interactive effects of political institutions.

### **Conclusion**

This paper started with a simple question: do currency crises prompt capital account liberalization or capital account closure? The literature on the politics of international monetary relations has proposed a number of different links between crises and liberalization, and any observational strategy must acknowledge the possibility that reforms themselves cause crises, and take steps to purge parameter estimates of this endogeneity. This is particularly a problem for the 1971-1995 period. I identify the relationship between crises and policy change using instrumental variables, employing country-specific debt-weighted Northern interest rates as an instrument for currency crises for the 1971-1995 period. My findings show that 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. Turning to data from the post-1996 period, where endogeneity is no longer a concern, I find additional evidence that the predominant response to currency crises in the developing world is capital account closure. Political institutions appear to weaken the

relationship between currency crises and capital account liberalization to some extent, but under no political conditions do we observe that currency crises systematically lead to capital account liberalization.

These results are inconsistent with the 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 early literature on crises and policy reform 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. For example, Abiad and Mody's (2005) finding that balance-of-payments crises are associated with financial reform (broadly construed) may not have the causal interpretation that they suggest that it does, where crises cause reform rather than the reverse. Careful attention to the direction of causality is a critical task for understanding crises and their policy consequences.

Studies of the politics of international monetary relations have yet to grasp fully 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 explanations for capital account liberalization 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.

**Table 1: Summary Statistics**

<b>Variable</b>	<b>Observations</b>	<b>Mean</b>	<b>Std. Dev.</b>	<b>Min</b>	<b>Max</b>
<i>KAOpen</i>	482	-0.540	1.149	-1.753	2.623
<i>CKAOpen</i>	500	-0.548	1.144	-1.763	2.502
<i>K3</i>	500	0.149	0.337	0	1
<i>S_Cap</i>	308	-0.383	0.845	-1.973	1.662
<i>Balance</i>	404	-4.783	7.336	-52.280	16.981
<i>Lngdp</i>	459	23.470	1.908	19.206	28.587
<i>Lngdppc</i>	459	7.847	0.793	6.198	9.632
<i>Expenditure</i>	457	15.156	6.381	3.915	56.400
<i>Growth</i>	470	1.193	3.745	-21.856	15.103
<i>Inflation</i>	428	4.802	0.347	4.557	8.052
<i>Private Credit</i>	462	77.553	814.753	0	15231.810
<i>Reserves</i>	408	3.188	2.849	0.036	21.753
<i>Savings</i>	386	16.124	10.574	-23.155	55.639
<i>Trade</i>	471	65.735	37.741	3.716	241.024
<i>Peg</i>	500	0.523	0.406	0	1
<i>Polity2</i>	467	-2.075	6.509	-10	10
<i>Crisis</i>	515	0.276	0.422	0	1
<i>I*</i>	502	7.579	2.079	3.240	12.621
<i><math>\Delta I^*</math></i>	400	-3.316	2.673	-4.346	5.280

**Table 2: Main Results**

<i>Dependent Variable</i>	<b>Model 1</b>		<b>Model 2</b>	<b>Model 3</b>	<b>Model 4</b>
	<i>KAOPEN</i>		<i>CKAOPEN</i>	<i>K3</i>	<i>S_CAP</i>
	<i>First Stage</i>	<i>Second Stage</i>			
<i>Crisis</i>		-1.194*** (0.369)	-1.299*** (0.397)	-0.229** (0.104)	-1.111** (0.434)
<i>Balance</i>	0.008 (0.008)	0.019 (0.015)	0.014 (0.016)	0.001 (0.004)	0.007 (0.016)
<i>Lngdp</i>	-0.397 (0.613)	-2.654** (1.072)	-3.003*** (1.096)	-0.372 (0.288)	-1.986* (1.030)
<i>Lngdppc</i>	0.005 (0.600)	1.909* (1.045)	2.064* (1.068)	0.032 (0.280)	1.831* (1.044)
<i>Expenditure</i>	-0.008 (0.013)	0.019 (0.023)	0.031 (0.024)	0.010 (0.006)	-0.025 (0.022)
<i>Growth</i>	0.029** (0.012)	0.047** (0.022)	0.053** (0.023)	0.008 (0.006)	0.040* (0.024)
<i>Inflation</i>	0.211* (0.114)	-0.145 (0.218)	-0.240 (0.231)	-0.176*** (0.062)	0.070 (0.213)
<i>Private Credit</i>	-0.012*** (0.003)	-0.021*** (0.007)	-0.023*** (0.007)	-0.001 (0.002)	-0.014* (0.007)
<i>Reserves</i>	-0.013 (0.018)	0.012 (0.032)	0.016 (0.033)	-0.003 (0.009)	-0.020 (0.031)
<i>Savings</i>	0.013* (0.007)	0.016 (0.013)	0.019 (0.013)	0.008** (0.004)	0.012 (0.017)
<i>Trade</i>	0.010*** (0.003)	0.021*** (0.006)	0.022*** (0.006)	0.002 (0.002)	0.017*** (0.006)
<i>Peg</i>	0.078 (0.131)	0.195 (0.223)	0.241 (0.230)	-0.001 (0.061)	-0.042 (0.202)
<i>Polity2</i>	-0.001 (0.008)	-0.016 (0.014)	-0.015 (0.015)	-0.002 (0.004)	-0.019 (0.015)
<i>Counter</i>	0.042** (0.017)	0.105*** (0.029)	0.118*** (0.030)	0.014* (0.008)	0.106*** (0.028)
<i>Lag KA Policy</i>	-0.076 (0.048)	-0.817*** (0.085)	-0.901*** (0.089)	-0.859*** (0.082)	-0.879*** (0.162)
<i>I* (lag)</i>	0.105*** (0.022)				
<i>I* (change)</i>	0.072*** (0.020)				

(continued on next page)

Table 2 continued

N	233	238	238	184
LM Test Statistic	21.589	21.547	21.779	12.395
<i>p</i> -value	0.000	0.000	0.000	0.002
Cragg-Donald Wald F Statistic	11.221*	11.184*	11.323*	6.010
Hansen's J Statistic	0.657	0.619	0.077	0.034
<i>p</i> -value	0.418	0.431	0.781	0.853

(Notes. Cells contain parameter estimates and standard errors. All models include country fixed effects. For parameter estimates, \*\*\*  $p < .01$ , \*\*  $p < .05$ , \*  $p < .1$ . For Cragg-Donald Wald F test, \* equal 5% confidence of less than 20% IV bias.)

**Table 3: Robustness Tests**

	<b>Model 5</b>	<b>Model 6</b>	<b>Model 7</b>	<b>Model 8</b>	<b>Model 9</b>	<b>Model 10</b>
<i>Dependent Variable</i>	<i>S_CAP</i>	<i>KAOPEN</i>	<i>KAOPEN</i>	<i>KAOPEN</i>	<i>KAOPEN</i>	<i>KAOPEN</i>
<i>Method</i>	<i>IV, LIML</i>	<i>IV</i>	<i>IV</i>	<i>IV</i>	<i>IV, LIML</i>	<i>IV, LIML</i>
<i>Crisis</i>	-1.114**	-1.165***	-1.287***	-1.194***	-1.192***	-0.804
	(0.435)	(0.368)	(0.484)	(0.395)	(0.377)	(0.576)
<i>Balance</i>	0.007	0.015	0.006	0.019	0.018	0.019
	(0.016)	(0.014)	(0.019)	(0.015)	(0.015)	(0.016)
<i>Lngdp</i>	-1.986*	-2.873***	-0.935	-2.654***	-3.080***	-2.952**
	(1.032)	(1.022)	(1.369)	(0.964)	(1.150)	(1.156)
<i>Lngdppc</i>	1.829*	2.239**	-0.191	1.909*	2.372**	2.116*
	(1.046)	(0.986)	(1.334)	(1.009)	(1.138)	(1.113)
<i>Expenditure</i>	-0.025	0.015	0.032	0.019	0.012	0.020
	(0.022)	(0.022)	(0.030)	(0.025)	(0.024)	(0.025)
<i>Growth</i>	0.040*	0.037*	0.095***	0.047*	0.051**	0.056**
	(0.024)	(0.020)	(0.029)	(0.027)	(0.023)	(0.025)
<i>Inflation</i>	0.071	-0.169	0.120	-0.145	-0.114	-0.063
	(0.213)	(0.213)	(0.286)	(0.315)	(0.224)	(0.242)
<i>Private Credit</i>	-0.014*	-0.020***	-0.024***	-0.021***	-0.022***	-0.022***
	(0.007)	(0.007)	(0.009)	(0.008)	(0.007)	(0.007)
<i>Reserves</i>	-0.021	0.008	0.049	0.012	0.005	0.001
	(0.031)	(0.031)	(0.041)	(0.035)	(0.033)	(0.035)
<i>Savings</i>	0.012	0.018	0.008	0.016	0.013	0.015
	(0.017)	(0.012)	(0.016)	(0.014)	(0.013)	(0.013)
<i>Trade</i>	0.017***	0.019***	0.024***	0.021***	0.022***	0.022***
	(0.006)	(0.006)	(0.008)	(0.007)	(0.006)	(0.006)
<i>Peg</i>	-0.042	0.198	-0.340	0.195	0.202	0.182
	(0.202)	(0.213)	(0.285)	(0.226)	(0.230)	(0.236)
<i>Counter</i>	0.106***	0.103***	0.091**	0.105***	0.118***	0.118***
	(0.028)	(0.026)	(0.037)	(0.025)	(0.031)	(0.032)
<i>Lag KA Policy</i>	-0.879***	-0.835***		-0.817***	-0.823***	-0.791***
	(0.162)	(0.085)		(0.099)	(0.088)	(0.094)
<i>Polity2</i>	-0.019		-0.031*	-0.016	0.005	-0.021
	(0.016)		(0.019)	(0.016)	(0.022)	(0.019)
<i>Polity2 × Crisis</i>					-0.064	
					(0.051)	
<i>Polcon</i>						0.967
						(0.951)
<i>Polcon × Crisis</i>						-2.370
						(2.321)

(continued on next page)

*Table 3 continued*

N	184	247	233	233	233	233
LM Test Statistic	12.395	21.804	20.951	17.055	16.349	12.387
<i>p</i> -value	0.002	0.000	0.000	0.000	0.000	0.002
Cragg-Donald F Statistic	6.010**	11.399*	10.915*	13.681**	5.425**	3.975**
Hansen's J Statistic	0.034	0.265	0.730	1.021	0.001	0.010
<i>p</i> -value	0.853	0.607	0.393	0.312	0.979	0.922
Anderson-Rubin F Statistic					6.474	5.878
<i>p</i> -value					0.0002	0.0008

(Notes. Cells contain parameter estimates and standard errors. All models include country fixed effects. For parameter estimates, \*\*\*  $p < .01$ , \*\*  $p < .05$ , \*  $p < .1$ . For Cragg-Donald Wald F test, \* equal 5% confidence of less than 20% IV bias, \*\* less than 15% IV bias.)

**Table 4: Post-1996 Tests**

	<b>Model 11</b>	<b>Model 12</b>	<b>Model 13</b>	<b>Model 14</b>	<b>Model 15</b>
<i>Dependent Variable</i>	<i>KAOPEN</i>	<i>CKAOPEN</i>	<i>K3</i>	<i>CKAOPEN</i>	<i>CKAOPEN</i>
<i>Crisis2</i>	-0.098 (0.102)	-0.251** (0.127)	-0.176*** (0.046)	-0.275** (0.138)	0.024 (0.209)
<i>Polity2*Crisis2</i>	0.001 (0.014)	0.001 (0.017)	-0.004 (0.006)	-0.002 (0.018)	0.014 (0.018)
<i>Polity2</i>				0.008 (0.019)	
<i>Polcon*Crisis2</i>					-0.420 (0.283)
<i>Polcon</i>					-0.940* (0.553)
<i>Balance</i>	-0.017** (0.008)	-0.013 (0.010)	0.002 (0.004)	-0.013 (0.010)	-0.013 (0.010)
<i>Lngdp</i>	-1.308 (0.958)	-2.145* (1.193)	-0.732* (0.428)	-2.162* (1.195)	-2.080* (1.184)
<i>Lngdppc</i>	2.032** (0.974)	2.843** (1.211)	0.857** (0.434)	2.916** (1.223)	2.640** (1.206)
<i>Expenditure</i>	-0.007 (0.014)	-0.001 (0.017)	-0.006 (0.006)	-0.001 (0.017)	-0.001 (0.017)
<i>Growth</i>	0.009 (0.007)	0.006 (0.009)	0.003 (0.003)	0.006 (0.009)	0.007 (0.009)
<i>Inflation</i>	0.029 (0.499)	-0.140 (0.625)	-0.316 (0.224)	-0.152 (0.627)	-0.011 (0.622)
<i>Private Credit</i>	0.004 (0.003)	0.006 (0.004)	0.002 (0.001)	0.005 (0.004)	0.006 (0.004)
<i>Reserves</i>	0.011 (0.021)	-0.004 (0.026)	-0.020** (0.009)	-0.003 (0.026)	-0.001 (0.026)
<i>Savings</i>	0.003 (0.009)	0.002 (0.011)	-0.004 (0.004)	0.001 (0.011)	0.005 (0.011)
<i>Trade</i>	0.004 (0.004)	0.007 (0.004)	0.002 (0.002)	0.007 (0.004)	0.006 (0.004)
<i>Peg</i>	0.006 (0.068)	-0.075 (0.085)	-0.036 (0.030)	-0.077 (0.085)	-0.067 (0.084)
<i>Counter</i>	0.014 (0.022)	0.028 (0.027)	0.012 (0.010)	0.027 (0.027)	0.024 (0.027)
<i>Lag of KA Policy</i>	-0.324*** (0.044)	-0.430*** (0.049)	-0.672*** (0.049)	-0.428*** (0.050)	-0.450*** (0.050)
<i>Constant</i>	15.124 (16.828)	29.705 (20.942)	12.738* (7.507)	29.620 (20.974)	29.223 (20.777)

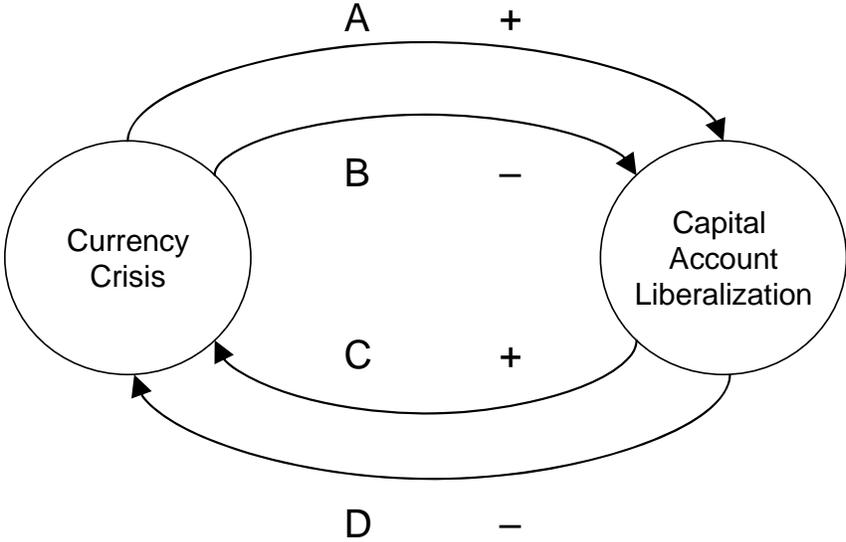
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(Table 4 continued)

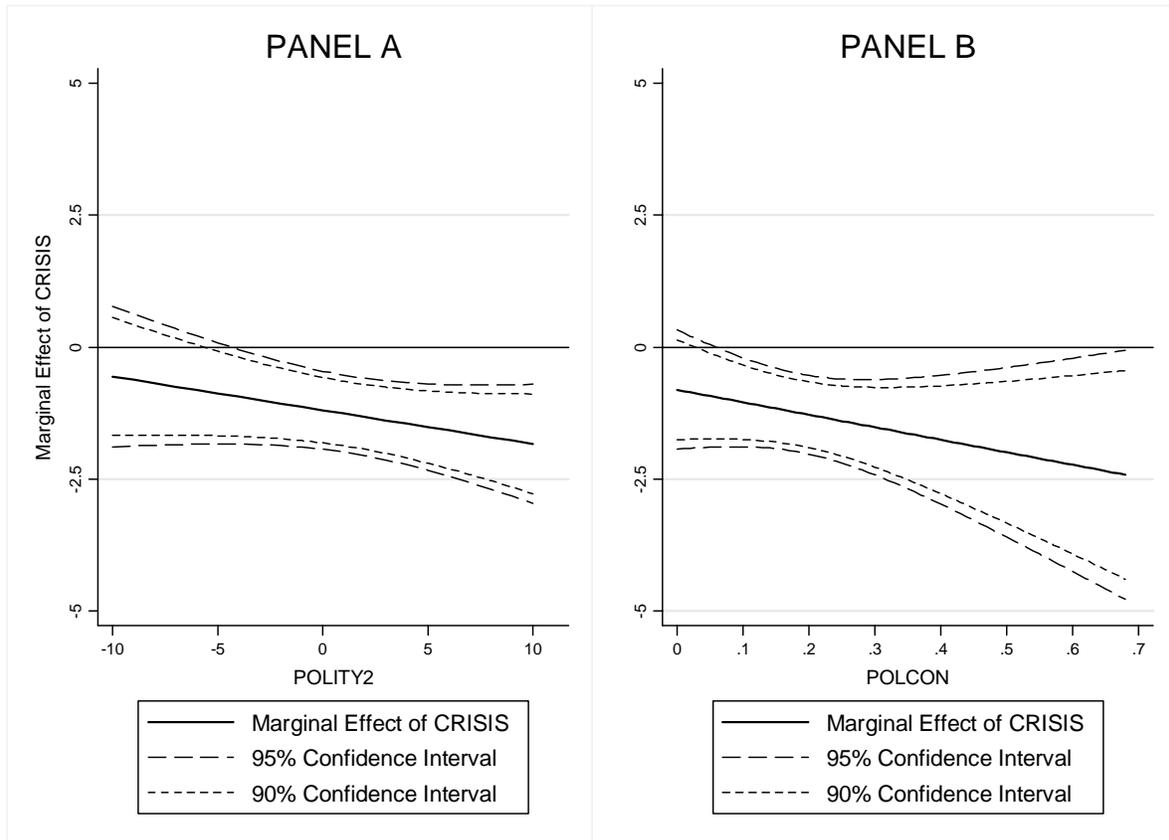
N	358	358	358	358	358
Within R <sup>2</sup>	0.236	0.261	0.443	0.262	0.278
Between R <sup>2</sup>	0.056	0.033	0.008	0.033	0.033
Overall R <sup>2</sup>	0.020	0.007	0.005	0.007	0.007

(Notes. Cells contain parameter estimates and standard errors. The dependent variable in all models is the change in the capital account policy indicator. All models include country fixed effects. \*\*\*  $p < .01$ , \*\*  $p < .05$ , \*  $p < .1$ .)

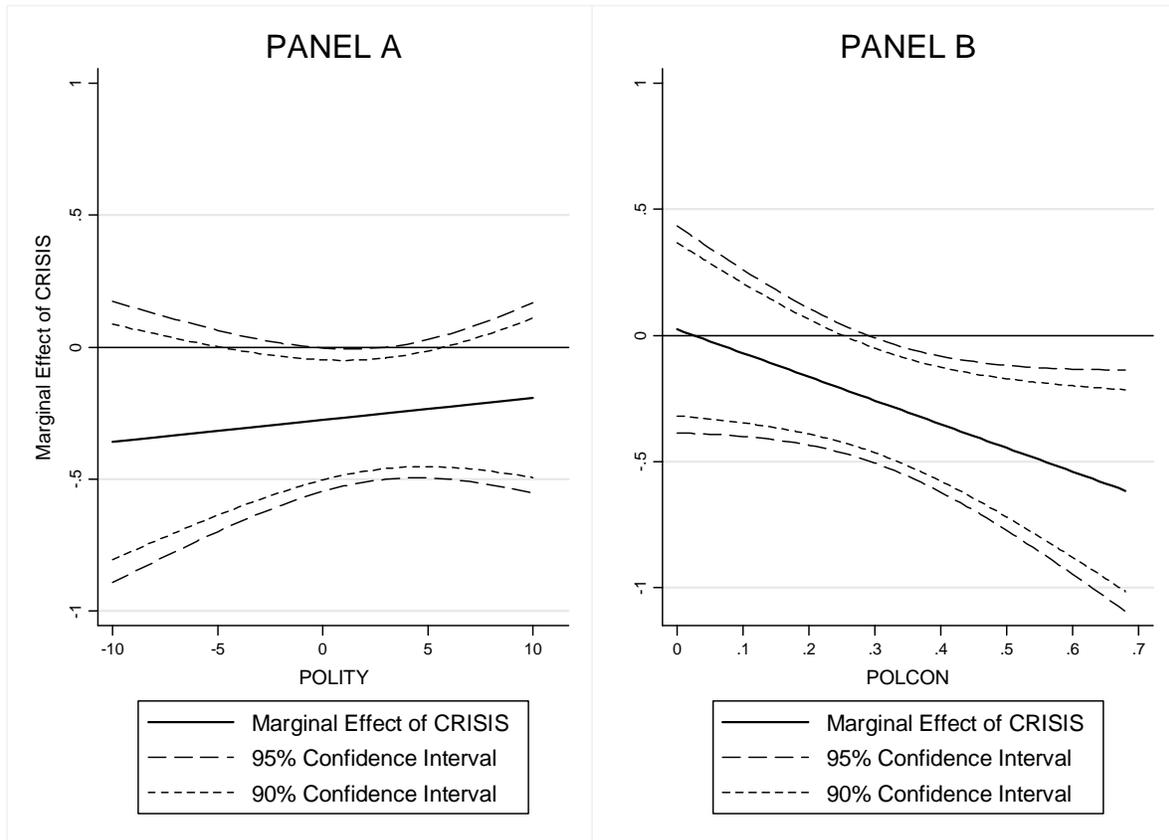
**Figure 1:** *From Currency Crises to Capital Account Liberalization*



**Figure 2:** *Effects of Crises on Capital Account Policy Change, 1971-1995, by Political Institutions*



**Figure 3:** *Effects of Crises on Capital Account Policy Change, 1997-2005, by Political Institutions*



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