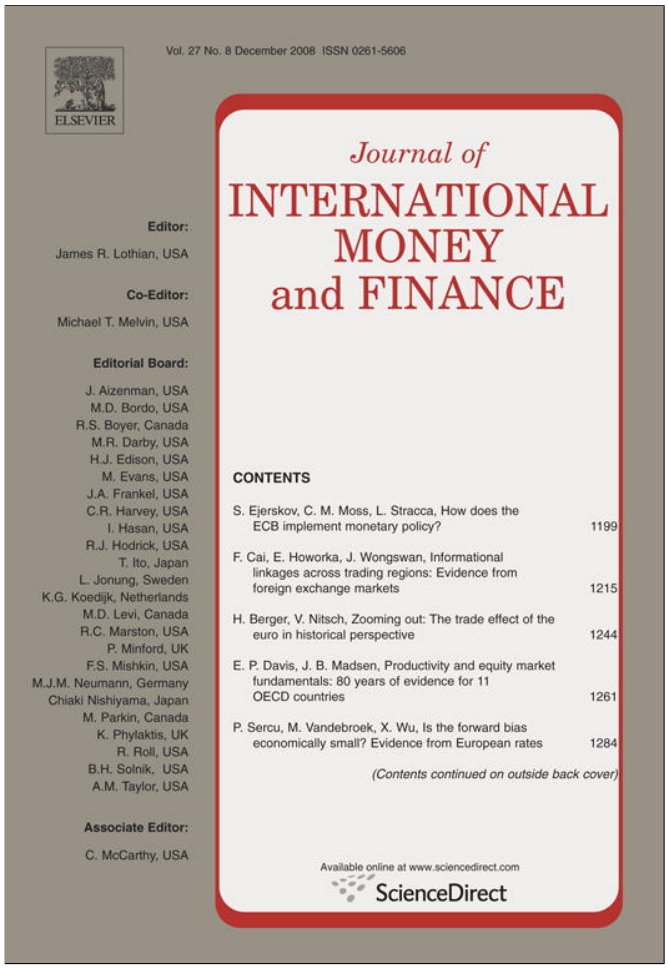

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# Does openness to trade make countries more vulnerable to sudden stops, or less? Using gravity to establish causality

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**Abstract**

Openness to trade is one factor that has been identified as determining whether a country is prone to sudden stops in capital inflows. Several authors have offered empirical evidence that having a large tradable sector reduces the contraction necessary to adjust to a given cut-off in funding. Such studies may, however, be subject to the problem that trade is endogenous. We use the gravity instrument for trade openness, which is constructed from geographical determinants of bilateral trade. We find that openness indeed makes countries *less* vulnerable to crises, and that the relationship is even stronger when correcting for the endogeneity of trade.


*JEL classification:* F32; F36; F41

*Keywords:* Sudden stops; Current account adjustment; Trade; Gravity model

## 1. Introduction

A ‘‘sudden stop’’ – an abrupt cut-off in capital inflows – entails a resource transfer to creditors, from the debtor country. Often it also entails a financial or currency crisis in the latter, accompanied by a sharp fall in output.[1] Broadly speaking, there are two opposing views on

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[1] The expression ‘‘sudden stops’’ was first popularized by Dornbusch et al. (1995). Calvo (1998) provided the first analytic approach to the problem of sudden stops.

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the relationship between a country's openness to trade and whether it is prone to these external crises. The first view is that trade openness makes a country more vulnerable to crises. A country highly integrated into world markets is more exposed to shocks coming from abroad. The second view is that countries that are open to international trade are *less* vulnerable to shocks generating in foreign markets. If the ratio of trade to GDP is structurally high, it is easier to adjust to a cut-off in international financing of a given magnitude. This paper tests the relationship between trade openness and vulnerability to sudden stops to help choose between the two hypotheses. Such tests have been done before, but without fully taking into account the possible endogeneity of trade. Our incremental contribution here is to use the gravity instrument for trade openness – which aggregates geographically determined bilateral trade across a country's partners – to correct for the possible endogeneity of trade.

The view that openness makes countries more vulnerable to crises comes in a number of forms. One variant is that a weakening in a country's export markets is sometimes the trigger for a sudden stop in capital flows, so that a high-trade country is more vulnerable. Another variant notes that sudden stops in finance often extend to a loss in trade credit – especially for imports, but sometimes also even for exports – and that the resulting shrinkage in trade is more painful if trade was a larger share of the economy. A third variant says that openness to trade in practice goes hand in hand with openness to financial flows, for example, because much trade needs multinational corporations, who in turn need to be able to move money across national borders; or because it is harder to enforce capital controls if trade is free.[2] In the limiting case, a country that is in autarky with respect to trade must have a net capital account of zero due to the balance of payments adding up constraint. Regardless the specific reasoning, the notion that globalization leads to crises is a generalization that appeals to many.

The view that openness to trade makes countries *less* vulnerable also comes with a number of different specific mechanisms that have been proposed. Rose (2005) argues that the threatened penalty of lost trade is precisely the answer to the riddle "why do countries so seldom default on their international debts?" and offers empirical evidence that strong trade links are correlated with low default probabilities.[3] International investors will be less likely to pull out of a country with a high trade/GDP ratio, because they know the country is less likely to default. A higher ratio of trade is a form of "giving hostages" that makes a cut-off of lending less likely. In an early contribution, Sachs (1985) suggested that Asian countries had been less vulnerable to debt crises than Latin American countries in the early 1980s – despite similar debt/GDP ratios – because they had higher export/GDP ratios which enabled them to accommodate the shocks better.[4] More recently, Martin and Rey (2006) show in the setting of a general equilibrium model that when emerging markets start opening their financial account but are closed to trade in goods, they are more prone to financial crises because profits and dividends depend on volatile domestic demand. Therefore, a policy implication of the model is that trade openness reduces the vulnerability to financial crises.

[2] Aizenman (2008), and Aizenman and Noy (2004).

[3] Rose's argument has been contested. Martinez and Sandleris (2006) argue that in the aftermath of defaults, there seems to be no evidence of a larger decline in bilateral trade with creditor countries affected by the default. This would imply that the declines in trade are not due to punishments imposed by these creditor countries.

[4] Countries that are more open to trade can *export away* part of the external shock that entails a given cut-off in financing. This is true if it must adjust either with or without nominal or real exchange rate flexibility. See Cavallo and Frankel (2007) for a discussion. Also, Guidotti et al. (2004) provide evidence that economies that trade more recover fairly quickly from the output contraction that usually comes with the sudden stop, while countries that are more closed suffer sharper output contraction and a slower recovery.

These opposing views suggest that the relationship between openness to trade and probability of sudden stops is an empirical question. Despite these specific mechanisms, the hypothesis that openness to trade reduces a country's vulnerability to sudden stops transcends any one formal model, causal link, or country example. The same is true of the hypothesis that trade openness raises a country's vulnerability. This paper seeks to choose empirically between the two competing hypotheses.[5]

What do we mean by "vulnerability to sudden stops?" A long taxonomy of definitions of financial crises has developed in the literature in recent years.[6] Here we focus on two popular definitions: our first criterion will be a probit model measuring the probability of a sudden reduction in the magnitude of net capital inflows, following closely the definition of Calvo et al. (2003b). Secondly, we also look at the definition of crisis episodes in Frankel and Rose (1996) and Frankel and Wei (2004). It is based on the exchange market pressure variable defined as percentage currency depreciation plus percentage loss in foreign exchange reserves.

We are not the first ones to test the relationship between trade openness and vulnerability to some form of financial crises. Edwards (2004a,b) is among the empirical papers that find that openness to trade is associated with fewer sudden stops. On the other hand, Milesi-Ferretti and Razin (1998, 2000) find conflicting evidence in their analysis of current account reversals and currency crises.[7]

These papers measure trade openness using the trade/GDP ratio. But a critic might argue that the trade/GDP ratio is endogenous. One way in which trade openness could be endogenous is via income: richer countries tend to liberalize trade barriers – in part because their mode of public finance shifts from tariff revenue to income or value added taxes. A second way is that trade liberalization could be part of a more general reform strategy driven by pro-globalization philosophy or "Washington Consensus" forces. Other aspects of such a reform program, such as privatization, financial liberalization, or macroeconomic stabilization might affect the probability of crises, and yet an OLS regression analysis might inappropriately attribute the effect to trade. A third way that trade openness could be endogenous is that experience with crises – the dependent variable – may itself cause liberalization, via an IMF program. Or it might have the opposite effect, if a country's response to a crash is disenchantment with globalization and the Washington Consensus. A fourth way in which trade openness could be endogenous is through the feedbacks between trade and financial openness. Aizenman (2008) shows how more commercial openness can increase the effective cost of enforcing financial repression, rendering financial openness a by-product of greater trade integration. Similarly, one could potentially think of a reverse causality process, whereby for example, greater financial openness may reduce the cost of trade credit and encourage FDI, and both adjustments may facilitate more commercial trade.[8]

[5] The proposition that openness to trade ameliorates the negative effects of sudden stops that occur – need not be linked to the proposition that openness reduces the ex-ante probability of a crisis. One possibility is that in a country where sudden stops are associated with large recessions, they are more likely to occur, because the country will default to avoid the recessions – as in the model of Cavallo (2006). The opposite relationship is also possible, however. Dooley (2000) has suggested that when crises lead to recessions, countries are more likely to take care to avoid them, and so sudden stops are less likely. We found that openness does make the crises that occur less severe, in the working paper version, Cavallo and Frankel (2007).

[6] Cavallo and Frankel (2007).

[7] Easterly et al. (2001) find that trade openness raises output volatility.

[8] Aizenman and Noy (2004) empirically investigate the presence of two-way feedbacks between financial and trade integration.

How can the endogeneity of trade be addressed? We use gravity estimates to construct an instrumental variable for trade openness. This methodology was developed by Frankel and Romer (1999) in the context of the effect of trade on growth, and was later applied to a variety of settings in which trade and some other variable could potentially be jointly determined.[9] It consists of aggregating up across a country's partners the prediction of a gravity equation that explains bilateral trade by means of distance, population, language, land-border, land-area, and landlocked status. Gravity estimates provide a good instrumental variable, because they are based on geographical variables which are plausibly exogenous and yet when aggregated across all bilateral trading partners are highly correlated with a country's overall trade.

We use capital account (also known as financial account) and current account data for 141 countries in total, over the period 1970–2002, to identify sudden stops in capital flows statistically. Using instrumental variables' techniques and controls for other plausible determinants of external crises, we find that openness indeed reduces vulnerability to sudden stops and their effects.

In the next section we elaborate on the empirical strategy and discuss the estimation method. Next, we present standard probit results using sudden stop episodes as the dependent variable and confirm the negative correlation between trade openness and the probability of sudden stops that has already been noted in the literature. We then present instrumental variable probit results to show that the direction of causality goes from trade openness to reduced vulnerability to sudden stops. Next, we repeat the exercise using the Frankel and Rose (1996) definition of crisis episodes and confirm the previous results. Finally, we perform several robustness checks.

## 2. Empirical strategy

We begin by testing whether countries that trade more are (all else equal) more or less prone to sudden stops in capital flows. We estimate variants of the following equation:

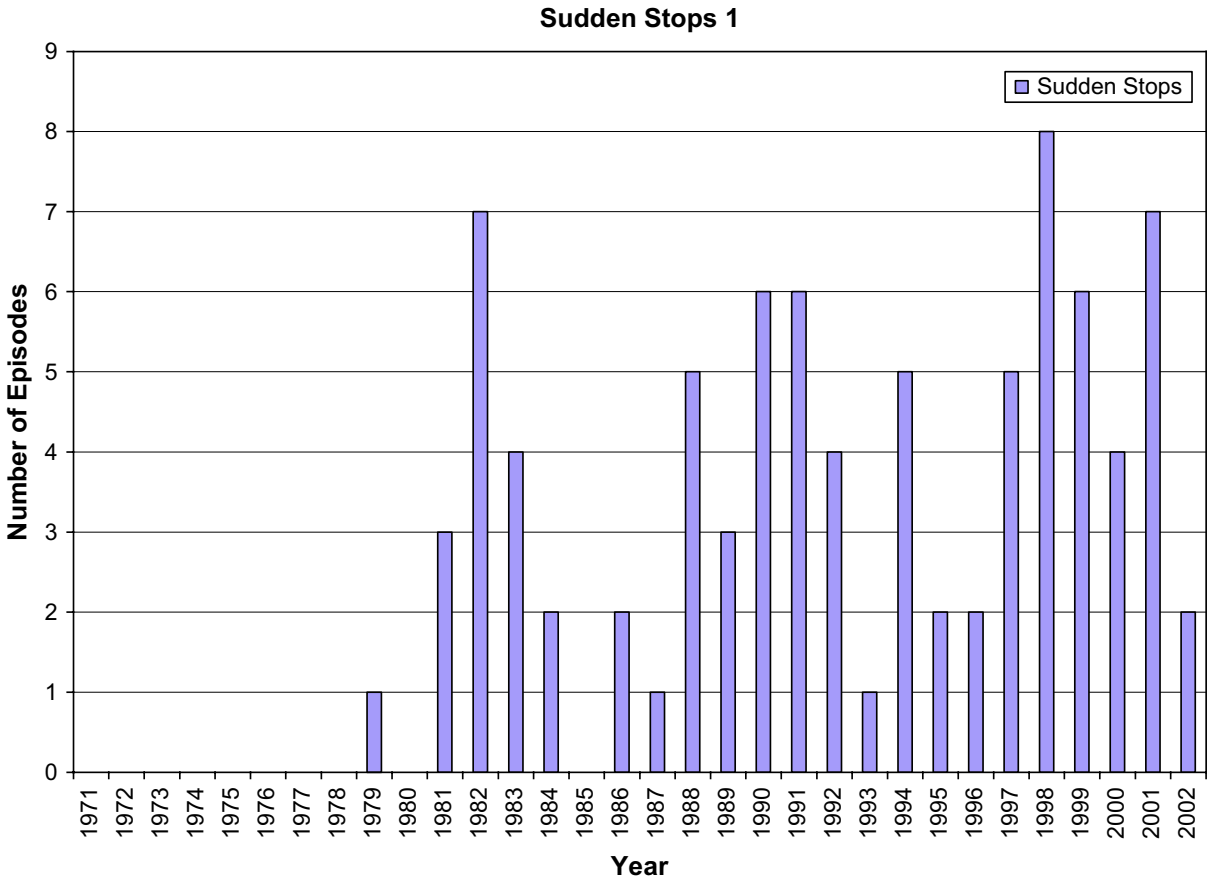
$$SS_{i,t} = c + \varphi(\text{trade openness})_{i,t-1} + \omega Z + \mu_{i,t} \quad (1)$$

where "$SS_{i,t}$" takes value 1 if a sudden stop hits country "$i$" at year "$t$" and 0 otherwise, and "$Z$" is a set of lagged regressors included for robustness check purposes.

Let us begin with the dependent variable. In order to construct $SS_{i,t}$, we follow the Calvo et al. (2003b) criterion for a sudden cut in foreign capital inflows (i.e., worsening of the financial account surplus, FA) that is not the consequence of a positive trade shock. Using a data set containing annual observations for all the countries in the world with available data in the IMF International Financial Statistics Database (IFS) for the period 1970–2002, we compute sudden stop episodes as a reduction in the CA deficit during the same year as a reduction in FA surplus. To guarantee that this reduction in the CA deficit is not the result of a boom – rising exports and income – the episode has to be *disruptive*, i.e., accompanied by a simultaneous reduction in real output or international reserves. In other words, a sudden stop occurs during the year in which there is a noticeable reduction in the current account deficit that is accompanied by a recessionary reduction in foreign capital inflows.[10] Based on alternative definitions of what is

[9] For example, Frankel and Rose (2002) show that currency unions can raise output via trade. For a survey of the gravity model in general, and applications and extensions, see chapters 4 and 6 of Frankel (1997).

[10] Technical details are left to Appendix and are carefully developed in the working paper version of this paper, Cavallo and Frankel (2007).




Source: Author's computations

Fig. 1. Sudden stop 1.

"noticeable" and "disruptive" we compute five classifications of sudden stops to be used as robustness checks for the results.

The preferred definition is SS1. This algorithm classifies as a sudden stop a situation in which at a year "$t$," the financial account surplus of country "$i$" (prevailing at year "$t-1$") has fallen at least two standard deviations below the sample mean for that country; the current account deficit falls by any amount either in "$t$" or in "$t+1$"; and GDP per capita falls by any amount either in "$t$" or in "$t+1$." The overall global pattern of sudden stops under this criterion is summarized in Fig. 1.

The total number of episodes captured using this methodology is 86, which is 2.4% of total available country/year observations in the data set.[11] As Fig. 1 shows, these events often take place around well-known crisis periods: the early 1980's debt crises in Latin America; the 1992–1993 European Monetary System crises; the new waves of crises in Asia and other developing countries in the late 1990s and 2001. In terms of regional distribution, 16% of all sudden stops occurred in the Asia-Pacific region; 13% in Europe; 33% in Latin America; 15% in the Middle East; 21% in Africa; and 1% each in South Asia and North America. Alternative definitions show similar patterns of temporal/spatial distribution.[12]

On the regressors' side of the equation, trade openness is typically measured as a country's ratio of total trade to GDP: $([X+M]/Y)$. All these data are readily available from the standard

[11] The complete list of crisis episodes per country, plus data availability, is in Table 6 in Appendix.
[12] Cavallo and Frankel (2007).

sources for almost all countries. But, as argued in Section 1, the problem of using this measure of trade openness is that it might be correlated with other unobserved country characteristics, creating identification problems and potentially biased estimators. As already noted, the contribution we seek to make to the literature is to avoid these problems by using instrumental variables' regression techniques. We instrument trade openness by the predicted ratio of trade to GDP based on gravity equations. In its most basic form, the gravity equation captures the intuitive notion that bilateral trade flows are proportional to the product of each country GDP level, and inversely related to the distance between them. Therefore, the ''predicted'' trade to GDP ratio can be computed from data on countries' geographic characteristics, bilateral trade flows, and GDP. The gravity model has become popular. We used the data set at Andrew Rose's webpage, which is perhaps the most complete one available and has been widely used for empirical research.[13] Details on the methodology are left to an Appendix. The important point is that, to the extent that the ''predicted'' trade to GDP ratio is highly correlated with the actual trade to GDP ratio,[14] one can argue that it is a good instrument on the grounds that it is less likely that geography is related to economic outcomes through any channels other than trade. In other words, geography is quite plausibly exogenous.[15]

As for the control variables, ''$Z$'' is a set of lagged regressors included for robustness check purposes and to minimize potential omitted variable bias. These are:

- ''Liability dollarization.'' This variable represents ''balance sheet'' effects. According to the emerging markets' crises literature, the mismatch between the currency denomination of assets and the liabilities in the private and public balance sheets of these countries increases the output costs of external shocks that trigger real exchange rate depreciations, such as sudden stops in capital flows.[16] Indeed, something like a balance sheet mismatch is required to explain why real depreciations are sometimes contractionary, because one would otherwise expect an expansionary effect via trade.[17] The impact of this variable on the probability of a crisis is a priori ambiguous: there is no reason why something (i.e., liability dollarization) that makes the consequences of a crisis worse (i.e., more recessionary) should also necessarily make them more likely. Indeed, the effect could go the other way.[18]

  We use two alternative measures of ''liability dollarization:'' (i) One is the ratio of foreign liabilities of the financial sector to money (IFS). This is not a direct measure of the extent to which a country's balance sheets present a mismatch in the currency denomination of assets and liabilities. Nevertheless this variable has been used in the literature as a proxy,[19] primarily because it is available for almost all countries since 1970 and because it should be correlated with actual balance sheet mismatches. (ii) Our alternative proxy is a measure of

[13] See http://faculty.haas.berkeley.edu/arose/RecRes.htm. The data set consists of 41,678 bilateral trade observations spanning six different years (1970, 1975, 1980, 1985, 1990, and 1995). All 186 countries, dependencies, territories, overseas departments, colonies, and so forth for which the United Nations Statistical Office collects international trade data are included in the data set. The trade data are taken from the World Trade Database, a consistent recompilation of the UN trade data presented in Feenstra et al. (1997), augmented with data from UN's International Trade Statistics Yearbook. This data set is estimated to cover at least 98% of all trade.

[14] The actual correlation between the ''trade openness'' and the instrument used in this paper is 0.52.

[15] To the best of our knowledge, the only other paper that addressed the problem of endogeneity explicitly is Calvo et al. (2003b). They use instruments in a probit context along the lines suggested by Rivers and Vuong (1988).

[16] Krugman (1999) was one of the earliest and most influential contributions.

[17] See Céspedes et al. (2003) and Frankel (2005) for a thorough discussion.

[18] Calvo et al. (2003a,b) argue that ''domestic liability dollarization'' increases the probability of sudden stops.

[19] E.g., Alesina and Wagner (2003) and Guidotti et al. (2004).

deposit dollarization from Arteta (2005a,b). This is "dollar deposits/total deposits" in the financial system. Countries with a high percentage of deposit dollarization, but whose domestic currency is not the US dollar, are (most likely) countries that tend to borrow heavily in a currency different from their own.[20]

- "Foreign debt/GDP" is included to control for the level of de-facto financial openness. Without debt to service, there are no sudden stops to worry about. Data for "foreign debt/GDP" come from the IFS database.
- "CA/GDP" is "current account balance/GDP." It controls for the "quantity" of the resource transfer required in the aftermath of a sudden stop in inflows. It is expected that, other things equal, countries with larger current account deficits ex-ante will have a higher probability of experiencing a crisis.
- "The log of reserves in months of imports" is included because foreign exchange reserves could be used as self-insurance against sudden stops, making crises less likely.
- "The log of GDP per capita" is included to control for the stage of economic development.
- "FDI/GDP" is included because it is often thought that FDI flows are stable and reduce the likelihood of sudden stops. But the reverse effect is also possible: Guimaraes and Morris (2003) develop a model where foreign direct investment (illiquid investments in the target currency) makes crises more likely.
- "Institutional quality" is included to make sure that "trade openness," whether or not instrumented, is not incorrectly appropriating effects on sudden stops that really go through institutions.
- "The ratio of short-term debt to total debt" controls for the effect of the maturity structure of the debt on the likelihood of a crisis.
- "Index of exchange rate rigidity," is a measure of the nominal exchange rate regime.

Most of these variables come from World Development Indicators CD ROM, with the exception of the "institutional quality" data, which come from Kaufmann et al. (2002) and Marshall and Jaggers (2002)'s Polity IV Project, and data on "index of exchange rate rigidity," which come from Levy-Yeyati and Sturzenegger (2003) based on their "de-facto" exchange rate regime classification.

We first present results without instrumental variables, to confirm the existence of a negative correlation between sudden stops and trade openness. Our specification is probit. Then, we present the results based on instrumental variables for probit (IV probit). We report fixed-effects' results only for ordinary probit regressions because the instrument used in the IV regressions has, by construction, almost no time series variation. This is not a serious limitation because most of the variation in trade openness is *across* countries, not over time. Nevertheless, without country fixed-effects we cannot be sure that the estimated coefficient is not biased by some omitted variable.[21] To minimize this problem, we include controls in all the regressions for various possible determinants of sudden stops. Reassuringly, the results we obtain are consistent across all the alternative specifications, suggesting that omitted variable bias is not driving our results. But even at the risk of some persistent omitted

[20] In Arteta's database, data on the aggregate volume of foreign-currency-denominated ("dollar") deposits of residents are available for 92 developing and transition economies. The time span varies, with some countries having data from as early as 1975 and some having data only from about 1995 onwards.

[21] We are aware of the incidental parameter problem with fixed effect probit. However, it is an asymptotic problem, and the number of countries is finite.

variable bias, the methodology used here at least properly controls for endogeneity so that reverse causality cannot be blamed for the estimated effect of trade openness on the probability of sudden stops. Summary statistics for all the variables are found in Table 7 in Appendix.

We then run similar regressions where the dependent variable is currency crises, from the Frankel and Rose (1996) and Frankel and Wei (2004) definition, in place of the sudden stop measure.[22] They define crisis episodes based on the foreign market pressure index. This index is defined as the percentage fall in reserves plus the percentage fall in the foreign exchange value of the currency. The idea is that this index measures the fall in demand for the country's currency (which is arguably another form of sudden stops); it is then up to the monetary authorities to determine whether to accommodate, by letting the money supply fall, or to depreciate. To avoid treating every year of a multi-year high-inflation period as a separate crisis, the approach followed by the authors requires that the increase in exchange market pressure represents an acceleration of at least an additional 10% over the preceding period in order to be considered a crisis episode; and they also adopt an exclusion window of 3 years. The total number of episodes captured using this methodology is 419, which is 13% of total available country/year observations in the data set. This means that the alternative way of computing crisis episodes is much more comprehensive than the sudden stop criterion. The overall global pattern of crises' events under this criterion is illustrated in Fig. 2.

## 3. Results

We begin by estimating non-instrumental variables variants of Eq. (1). We compute standard errors robust to clustered heteroskedasticity.[23] All independent variables are lagged one period to ameliorate endogeneity.[24] Estimation includes year fixed-effects and regional dummies, but these coefficients are not reported. The results reported here are based on "SS1," but all estimates are robust to the use of alternative definitions of sudden stops.[25] We do not exclude contiguous crisis episodes, but all the results reported here are robust to the inclusion of a 1-year, two-sided omission window around crisis episodes.

The explanatory power of the regressions is not high. This is not surprising; it is consistent with the performance of standard models of crises and the usual inability of leading-indicator exercises to predict events.[26] Table 1 summarizes the results for some variants of (1) using ordinary probit specification.

These results confirm the existence of a negative correlation between trade openness and the likelihood of a sudden stop, as previously documented in Edwards (2004a,b). Column (1) includes only lagged trade openness alongside with regional and time dummies, while columns (2)–(9) sequentially include some of the control variables discussed in the previous section, to check that these results are not biased by omitted variables. The effect of trade openness on the probability of sudden stops increases in absolute value and in statistical significance as

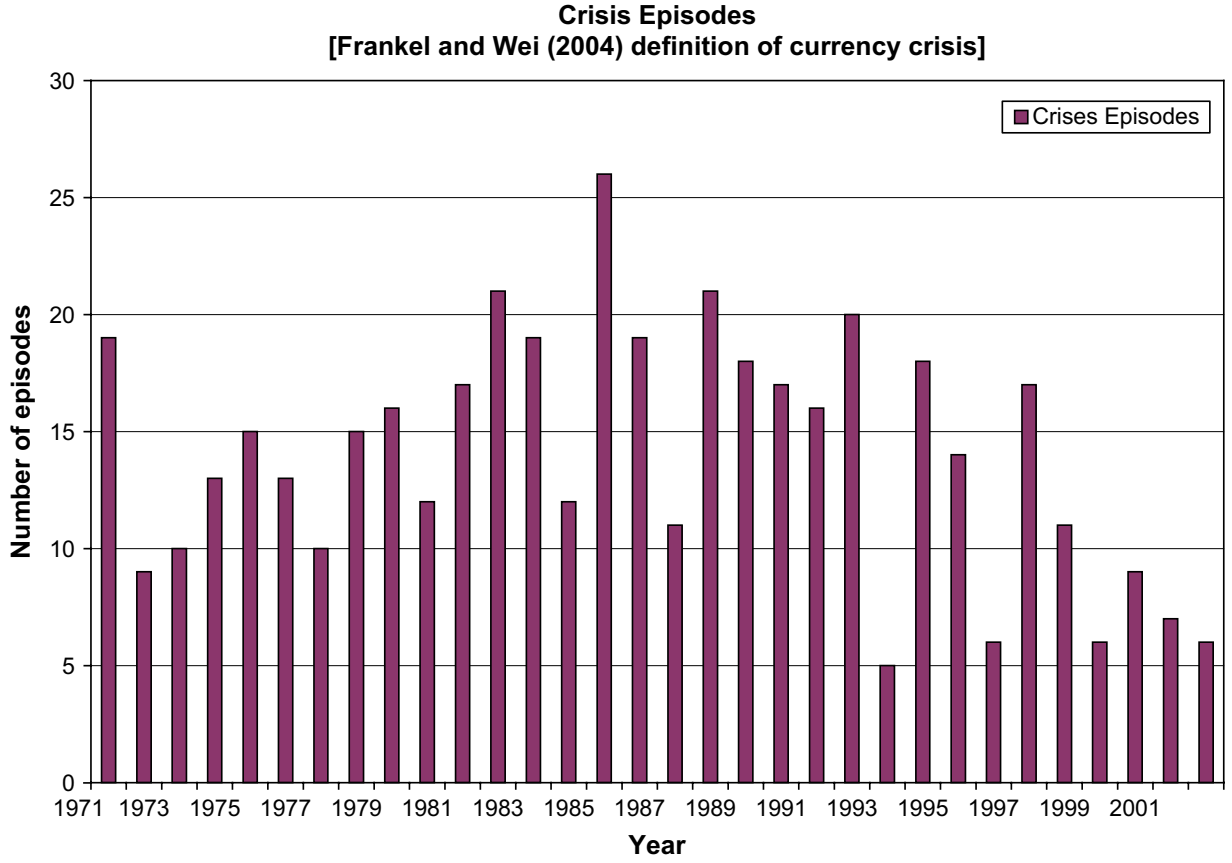
[22] Summary statistics are in Appendix.

[23] Clusters by country.

[24] Trade openness is lagged 1 year, in spite of the fact that it is ultimately instrumented, to avoid spurious correlation that could be due to the cases in which a sudden stop occurred in the beginning of the year and a decline in trade resulted from it. Introducing contemporaneous rather than lagged variables does not affect the results.

[25] As noted we use five alternative definitions. The details are in the Appendix and in Section 4.

[26] See, for example, Arteta (2005a,b).




Source: Author's computations

Fig. 2. Crises episodes based on "foreign exchange market pressure index" (Frankel and Wei, 2004).

additional control variables are included in the regressions, suggesting that, if anything, possible omitted variable bias is working in the direction of dampening the effect that we identify.[27]

As a first control, in column (2) we include the lagged current account balance as a share of GDP, to account for the size of the transfer that is required in the aftermath of the sudden stop. The estimated coefficient is negative and statistically significant. The implication is as conjectured: sudden stops are more likely when a larger resource transfer is expected in its aftermath (i.e., when the initial CA deficit is high).

The coefficient on lagged "foreign debt/GDP," although positive, does not appear statistically significant in column (3).[28] This is consistent with the hypothesis that different countries are able to tolerate different levels of debts.[29]

Similarly, the coefficient that seeks to capture the "balance sheet" effects – the lagged liability dollarization – is positive but only marginally statistically significant in column (4). We

[27] We included controls for all the possible determinants of sudden stops as discussed in the previous section, but as the results remain unchanged, for brevity we only report a subset of all the regressions. Additional results are reported in the working paper version, Cavallo and Frankel (2007).

[28] Similarly, Calvo et al. (2003b) don't find a significant effect of total public debt in their probit regressions for sudden stops, nor do Frankel and Rose (1996) in their probit regressions of currency crashes.

[29] Reinhart et al. (2003). We try "foreign debt/exports" as a solution to concerns about how foreign debt and GDP are measured in domestic currency. It fails to change any results.

Table 1
Ordinary probit regressions

| | Dependent variable: sudden stop 1 | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
| Trade openness$_{t-1}$ | **−0.32** | **−0.52** | **−0.52** | **−0.58** | **−0.60** | **−0.81** | **−0.90** | **−0.86** | **−0.73** | **−0.87** | **−1.44** |
| | (0.22) | (0.25)** | (0.25)** | (0.26)** | (0.26)** | (0.35)** | (0.34)*** | (0.34)** | (0.34)** | (0.41)** | (0.51)*** |
| Current account/GDP$_{t-1}$ | | **−3.59** | **−3.58** | **−3.65** | **−3.57** | **−4.90** | **−5.25** | **−5.76** | **−5.61** | | **−5.67** |
| | | (1.16)*** | (1.19)*** | (1.22)*** | (1.25)*** | (1.61)*** | (1.77)*** | (1.96)*** | (1.92)*** | | (1.23)*** |
| Foreign debt/GDP$_{t-1}$ | | | **0.01** | **0.02** | **0.00** | **0.03** | **0.09** | **0.08** | **0.11** | | |
| | | | (0.2) | (0.21) | (0.3) | (0.25) | (0.25) | (0.27) | (0.24) | | |
| Liability dollarization$_{t-1}$ | | | | **0.35** | **0.32** | **0.32** | **0.31** | **0.31** | **0.23** | | |
| | | | | (0.18)* | (0.22) | (0.24) | (0.23) | (0.23) | (0.24) | | |
| Short-term debt/total debt$_{t-1}$ | | | | | **0.63** | **1.03** | **0.67** | **0.53** | **0.54** | | |
| | | | | | (0.65) | (0.74) | (0.76) | (0.75) | (0.76) | | |
| Effectiveness of government | | | | | | **0.16** | **0.08** | **0.07** | **0.13** | | |
| | | | | | | (0.2) | (0.24) | (0.25) | (0.23) | | |
| Ln GDP per capita$_{t-1}$ | | | | | | | **0.15** | **0.17** | **0.10** | | |
| | | | | | | | (0.18) | (0.18) | (0.15) | | |
| FDI/GDP$_{t-1}$ | | | | | | | | **−0.02** | **−0.02** | | |
| | | | | | | | | (0.03) | (0.03) | | |
| Regional dummies? | YES | YES | YES | YES | YES | YES | YES | YES | NO | NO | NO |
| Year fixed-effects? | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Country fixed-effects? | NO | NO | NO | NO | NO | NO | NO | NO | NO | YES | YES |
| Constant | **−2.49** | **−2.58** | **−2.58** | **−2.74** | **−2.77** | **−2.23** | **−3.19** | **−3.33** | **−3.02** | **−2.66** | **−2.69** |
| | (0.32)*** | (0.36)*** | (0.36)*** | (0.37)*** | (0.55)*** | (0.42)*** | (1.26)*** | (1.29)*** | (1.14)*** | (0.34)*** | (0.37)*** |
| Observations | **1124** | **1124** | **1124** | **1124** | **905** | **773** | **773** | **773** | **773** | **1124** | **1124** |

Robust standard errors reported in parenthesis: *significant at 10%; **significant at 5%; and ***significant at 1%.

obtain similar results when we use Arteta's measure of liability dollarization.[30] These measures of dollarization appear not to have significant detrimental effects in terms of increased vulnerability to sudden stops.

As for the other controls included in columns (4)–(9): the coefficient on "short-term debt to total debt" appears as small and positive, but not statistically significant.[31] The institutional quality proxy is not statistically significant.[32] Neither are the level of GDP per capita nor the level of FDI flows. Regional dummies (not reported) are always insignificant. Column (9) presents the results of the same regression as column (8) but without regional dummies. The results remain unchanged. Importantly, the inclusion of all the control variables does not change significantly the estimate of the effect of trade openness on the probability of a sudden stop, suggesting that the identified stabilizing effect of trade openness is not simply spurious. In order to probe this hypothesis further, in column (10) we replace the control variables and the regional dummies with country fixed-effects. The results are reassuringly similar to those in column (8), both qualitatively and also quantitatively: trade openness significantly reduces the probability of sudden stops. Finally, in column (11) we report the country fixed-effects regression including the current account variable, which is systematically significant in the other regressions. Once again the results are consistent with the previous estimates. All the results reported in Table 1 are robust to the inclusion of additional variables in the regressions.[33]

The methodology employed thus far cannot guarantee the exogeneity of trade openness and therefore falls short of establishing causality. Now we come to what we hope is our contribution to the state of the art. Table 2 presents instrumental variable estimates for probit.[34]

The results are qualitatively very similar to those in Table 1, although the point estimates of the coefficient on trade openness are quantitatively different. Interestingly, when we use gravity estimates as instrumental variables for trade openness, the point estimates are noticeably bigger in absolute value. Correcting for the potential sources of endogeneity, the effect of trade openness on the probability of sudden stop is even stronger than what one would be led to conclude from the OLS regressions.[35]

[30] Results reported in the working paper version, Cavallo and Frankel (2007).

[31] The insignificance of this variable might also be explained by high collinearity between "short-term debt to total debt" ratio and "liability dollarization." When the latter is excluded, the former is typically significant with the correct sign. The correlation between these two explanatory variables is almost 0.40.

[32] As a measure of institutional quality we report the coefficient on "effectiveness of government" which is one of the six proxies of institutional quality in Kaufmann et al. (2002). This institutional quality data are not in panel form, so every country in the sample is assigned a single (time-invariant) value. As additional robustness checks, we also use Marshall and Jaggers (2002)'s Polity IV Project data, whose panel is (country/year). Using this, alternative measure does not change the results, so we don't report them.

[33] For details and additional results see Cavallo and Frankel (2007).

[34] The method of estimation is maximum likelihood, and standard errors are corrected to account for clustered heteroskedasticity. The results are robust when a two-step estimator is implemented using the method of Whitney Newey, "Efficient estimation of limited dependent variable models with endogenous explanatory variables," *Journal of Econometrics* (1987). These results are available from the authors upon request.

[35] One possible reason why the IV coefficients are bigger in absolute value than the ordinary probit is our instrumental variable might be correcting for measurement error that creates attenuation bias. If the actual recorded trade share measures the true "openness" variable (the relevant measure of openness for the question how severely the economy must contract to generate quickly a given quantity of foreign exchange) with error, and if the measurement error is more important than reverse causality and omitted variable bias, then we can expect to get bigger IV coefficients. A similar point is made in Acemoglu et al. (2003). We thank Sebnem Kalemi-Ozcan for suggesting this possibility to us.

Table 2a
Instrumental variables probit regressions

| | Dependent variable: sudden stop 1 | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Trade openness$_{t-1}$ | **−1.63** | **−1.84** | **−1.82** | **−2.00** | **−2.16** | **−2.89** | **−2.97** | **−3.07** |
| | (0.52)*** | (0.68)*** | (0.63)*** | (0.59)*** | (0.60)*** | (0.52)*** | (0.53)*** | (0.55)*** |
| Current account/GDP$_{t-1}$ | | **−6.50** | **−6.01** | **−6.28** | **−5.99** | **−5.88** | **−7.02** | **−6.53** |
| | | (1.33)*** | (1.39)*** | (1.41)*** | (1.31)*** | (1.40)*** | (1.61)*** | (1.65)*** |
| Foreign debt/GDP$_{t-1}$ | | | **0.24** | **0.27** | **0.37** | **0.70** | **0.80** | **0.69** |
| | | | (0.25) | (0.24) | (0.28) | (0.30)** | (0.30)*** | (0.31)** |
| Liability dollarization$_{t-1}$ | | | | **0.51** | **0.35** | **0.11** | **0.09** | **0.13** |
| | | | | (0.20)** | (0.26) | (0.28) | (0.27) | (0.27) |
| Short-term debt/total debt$_{t-1}$ | | | | | **1.53** | **1.65** | **0.71** | **0.74** |
| | | | | | (0.78)* | (0.89)* | (1.05) | (1.03) |
| Effectiveness of government | | | | | | **0.37** | **0.14** | **0.12** |
| | | | | | | (0.19)** | (0.22) | (0.21) |
| Ln GDP per capita$_{t-1}$ | | | | | | | **0.37** | **0.35** |
| | | | | | | | (0.20)* | (0.20)* |
| FDI/GDP$_{t-1}$ | | | | | | | | **0.06** |
| | | | | | | | | (0.04) |
| Regional dummies? | YES | YES | YES | YES | YES | YES | YES | YES |
| Year fixed-effects? | YES | YES | YES | YES | YES | YES | YES | YES |
| Constant | **−1.71** | **−1.85** | **−1.88** | **−2.03** | **−1.81** | **−0.82** | **−3.28** | **−3.06** |
| | (0.32)*** | (0.36)*** | (0.36)*** | (0.37)*** | (0.55)*** | (0.67) | (1.26)** | (1.29)** |
| Observations | **1039** | **1039** | **1039** | **1039** | **827** | **732** | **732** | **732** |

Robust standard errors reported in parenthesis: *significant at 10%; **significant at 5%; and ***significant at 1%.

Table 2b
Marginal effects (for trade openness) after IV probit

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Marginal effects (d*y*/d*x*) are for discrete change of dummy variable from 0 to 1 | | | | | | | | |
| Trade openness$_{t-1}$ | −0.098 | −0.08 | −0.08 | −0.09 | −0.12 | −0.21 | −0.21 | −0.22 |
| Probability of a positive outcome (%) | 2.6 | 1.76 | 1.8 | 1.8 | 2.4 | 3.4 | 3.1 | 3.2 |
| Estimated effects of a 10 percentage point change in trade openness | | | | | | | | |
| Δ(PSS) in percentage points | **−0.98** | **−0.8** | **−0.8** | **−0.9** | **−1.2** | **−2.1** | **−2.1** | **−2.2** |
| % Δ(PSS) | **−37** | **−45** | **−44** | **−48** | **50** | **−63** | **−66** | **−68** |

Δ(PSS) and % Δ(PSS) calculated for a 10 percentage point increase in trade openness (i.e., an increase of 0.10 in the independent variable).
Δ(PSS) is the estimated change in the probability of a positive outcome (line 2), in percentage points, given by a 10 percentage point increase in trade openness. The calculations are based on the marginal effects reported in line 1.
% Δ(PSS) is the estimated change in the probability of a sudden stop, calculated as a percentage of the unconditional probability of a crisis (line 2), given by 10 percentage point increase in trade openness. It is computed by multiplying the marginal effect (line 1) by 0.10 and dividing by the probability of a positive outcome (line 2).
Marginal effects estimated at the mean of the independent variables.

Table 3
Ordinary probit regressions

| | Dependent variable: crisis episodes | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Trade openness$_{t-1}$ | **−0.32** | **−0.43** | **−0.47** | **−0.54** | **−0.58** |
| | (1.45) | (2.07)** | (2.22)** | (2.54)** | (2.72)*** |
| Ln reserves in months of imports$_{t-1}$ | | **−0.28** | **−0.28** | **−0.27** | **−0.24** |
| | | (4.45)*** | (4.30)*** | (4.24)*** | (3.57)*** |
| Exchange rate rigidity index$_{t-1}$ | | | **0.14** | **0.15** | **0.10** |
| | | | (1.66)* | (1.80)* | (1.17) |
| Foreign debt/GDP$_{t-1}$ | | | | **0.27** | **0.25** |
| | | | | (1.37) | (1.29) |
| Effectiveness of government | | | | | **−0.22** |
| | | | | | (2.05)** |
| Regional dummies? | YES | YES | YES | YES | YES |
| Year fixed-effects? | YES | YES | YES | YES | YES |
| Constant | **−1.54** | **−1.15** | **−1.36** | **−1.48** | **−1.29** |
| | (4.83)*** | (4.56)*** | (5.28)*** | (4.87)*** | (4.24)*** |
| Observations | **611** | **611** | **611** | **611** | **573** |

Robust standard errors reported in parenthesis: *significant at 10%; **significant at 5%; and ***significant at 1%.

Table 2b reports the implied marginal effects for trade openness estimated from the IV probit regressions at the mean of the independent variables (first row).[36] It also reports the predicted change in the probability of a sudden stop for a 10 percentage point increase in openness: it combines the marginal effects with the estimated probability of sudden stops (second row).[37] To make the experiment tangible, this is the equivalent of moving from Argentina's current trade share (approximately 20% of GDP) to Australia's average trade share (approximately 30% of GDP). The results indicate that an increase in trade openness of this sort reduces the probability of a crisis by between 1 and 2 percentage points (third row). Given that sudden stops are, by construction, low-probability events, these seemingly small changes constitute a large share of the unconditional probability of a crisis.[38] The shares range from 37% to 68%. That is, a country that trades 10% less of GDP (i.e., Argentina vis-à-vis Australia) is, ceteris paribus, at least 37% more likely to be hit by a sudden stop. Some may find this result counterintuitive: trade protectionism does not "shield" countries from the volatility of world markets as proponents might hope. On the contrary, less trade openness leads to greater vulnerability to sudden stops and currency crises.

The rest of the point estimates are qualitatively similar to those found in Table 1. The methodology here only promises the exogeneity of trade openness, so no causal relationship can be derived from the other estimates.

Next, we redo the exercise using the Frankel–Rose and Frankel–Wei definition of crises as the dependent variable. In Table 3 we report ordinary probit results, and in Table 4 we present IV probit results with gravity estimates as the instrumental variable for trade. The sample size is

[36] Note that the magnitudes of the effects at the tail of the distribution will be lower than that at the mean because a normal CDF is fitted to the data. Thus, the results reported below should be taken as an approximation.

[37] A 10 percentage point increase in the independent variable "trade openness" is, for example, an increase from the mean value of this variable in the sample, which is 0.73, to 0.83 (see Table 7 in Appendix for summary statistics). It is also an increase equal to approximately 0.25 standard deviations.

[38] Recall that the 2.4% probability of a crisis in the whole sample.

Table 4
Instrumental variables' probit regressions

| | Dependent variable: crisis episodes | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Trade openness$_{t-1}$ | **−0.88** | **−1.40** | **−1.48** | **−1.37** | **−1.31** |
| | (0.52)* | (0.52)*** | (0.54) *** | (0.51) *** | (0.51) *** |
| Ln reserves in months of imports$_{t-1}$ | | **−0.3** | **−0.3** | **−0.3** | **−0.3** |
| | | (0.07)*** | (0.07)*** | (0.07)*** | (0.07)*** |
| Exchange rate rigidity index$_{t-1}$ | | | **0.15** | **0.16** | **0.11** |
| | | | (0.08)* | (0.08)* | (0.09) |
| Foreign debt/GDP$_{t-1}$ | | | | **0.43** | **0.41** |
| | | | | (0.25) | (0.25) |
| Effectiveness of government | | | | | **−0.14** |
| | | | | | (0.1) |
| Regional dummies? | YES | YES | YES | YES | YES |
| Year fixed-effects? | YES | YES | YES | YES | YES |
| Constant | **−1.07** | **−0.23** | **−0.40** | **−0.84** | **−0.79** |
| | (0.65) | (0.73) | (0.78) | (0.67) | (0.63) |
| Observations | **583** | **583** | **583** | **583** | **548** |

Robust standard errors reported in parenthesis: *significant at 10%; **significant at 5%; and ***significant at 1%.

smaller as, for comparability purposes, it is limited to countries for which we also have sudden stop data. Given that crisis episodes are related to currency crashes, in the reported regressions we include as control variables the set of variables that are typically identified as determinants of currency crises in the literature (see Frankel and Rose, 1996).[39]

The main highlights are:

- Openness reduces the probability of a currency crisis. The point estimates are not as large in absolute value as those obtained when using "SS1," but the new coefficients are always statistically significant at standard confidence levels and the instrumental variables results are still stronger than the ordinary probit results. This parallels the earlier finding: that correcting for the potential sources of endogeneity, the effect of trade openness on the probability of an external crisis is even stronger than what one would be led to conclude from the OLS regressions.
- The coefficient on "foreign debt/GDP" is positive and (weakly) statistically significant in the IV probit regressions, suggesting that the presence of a large stock of foreign debt as a percentage of GDP increases the probability of crisis. The result is not robust in the ordinary probit regressions and is idiosyncratic to this particular definition of crisis episodes.
- The coefficient on the "log of reserves in months of imports" is systematically negative and statistically significant, across both, standard and IV probit regressions. Having a large stockpile of reserves evidently reduces the probability of being hit by a crisis. This result is interesting because this variable is always insignificant in the regressions that use "SS1" as the dependent variable.[40] The most likely reason for the difference is the way

[39] Cavallo and Frankel (2007) report the results for all the additional control variables discussed in the previous section.

[40] Variables not reported in Tables 1 and 2, but reported in Cavallo and Frankel (2007).

in which crises are defined in both cases. Frankel–Rose definition of crisis episodes uses the foreign exchange market pressure index which itself includes changes in reserves in the definition, while "SS1" does not.[41]

- The coefficient on the "index of exchange rate rigidity" is positive and statistically significant across many of the regressions in both tables. This suggests that having a peg increases the chances of being hit by a crisis. This result is also idiosyncratic to this definition of crises.
- The variable "effectiveness of government" enters the regressions with the expected negative sign and is statistically significant (in the ordinary probit regressions) at standard confidence levels. This suggests that having better institutions reduces the likelihood of crises.

The rest of the controls never appear as statistically significant, but all the results are robust to the inclusion of these variables from the regressions. Regional dummies (not reported) are always insignificant.

We find it reassuring that we get very similar results using two very different definitions of crises. We also have found other variables that increase or reduce the probability of a crisis, but we choose not to emphasize these so strongly because the methodology we propose here only promises the exogeneity of openness.

## 4. Robustness checks

Finally, we perform a variety of robustness checks. First, we look at alternative definitions of sudden stops. Based on variants of what is "noticeable" and "disruptive" we compute five classifications of sudden stops to be used as robustness checks: our preferred definition "SS1," and four alternative: "SS2," "SS3," "SS4," and "SS5." "SS2" and "SS3" are conceptually equivalent to "SS1," but are more restrictive in that they capture fewer episodes because they have more stringent definitions of financial flows volatility. "SS4" is, instead, less restrictive in that it classifies as sudden stops events that don't necessarily trigger output contractions. "SS5" is equivalent to "SS1" but uses the criterion that the sudden stop be accompanied by a loss of reserves rather than a fall in output. Alternatively, we also use the definition of sudden stops from Calvo et al. (2006), which we call "systemic." The reason is that in order to isolate episodes of capital account reversals related to systemic events of an external origin, these authors defined crises as periods of net capital inflows collapse that are accompanied with sharply higher emerging markets' bond spreads. The new definition necessarily restricts the sample to those emerging market economies that are integrated into world capital markets (i.e., that are included in the EMBI index). Table 5 summarizes the IV probit results for these alternative definitions. As shown in the table, trade openness always enters the regressions with a negative and statistically significant point estimate. This suggests that the result that openness reduces the vulnerability to sudden stops is not idiosyncratic to a particular definition.

[41] Although we don't report it here, the coefficient on "CA/GDP" is systematically insignificant across all regressions that use Frankel–Rose's crisis episodes as the dependent variable. This is also different from the case in which the dependent variable is "SS1." Once again the most likely reason is the definition of the crisis variable itself. Recall that "SS1" is built upon the assumption that there is an outstanding current account deficit that has to be abruptly reduced in the presence of a crisis; while in the alternative definition of crises, an episode can occur independently of what happens to the current account if the government is willing to give up reserves to finance an outstanding deficit.

Table 5
Instrumental variables probit regressions (alternative sudden stop definitions)

| | SS1 | SS2 | SS3 | SS4 | SS5 | Systemic |
|---|---|---|---|---|---|---|
| Trade openness$_{t-1}$ | **−1.95** | **−1.45** | **−2.43** | **−0.89** | **−2.48** | **−1.55** |
| | (0.55)*** | (0.53)** | (0.67)*** | (0.48)* | (0.59)*** | (0.54)*** |
| Foreign debt/GDP$_{t-1}$ | **0.20** | **0.28** | **−0.42** | **0.13** | **−1.38** | **0.47** |
| | (0.24) | (0.22) | (0.44) | (0.16) | (0.58)** | (0.46) |
| Liability dollarization$_{t-1}$ | **0.56** | **0.70** | **0.79** | **0.51** | **0.43** | **0.65** |
| | (0.22)** | (0.17)*** | (0.19)*** | (0.19)*** | (0.29) | (0.36)* |
| Current account/GDP$_{t-1}$ | **−5.66** | **−4.79** | **−6.50** | **−5.11** | **−5.67** | **−5.91** |
| | (1.14)*** | (1.27)*** | (1.78)*** | (1.21)*** | (1.54)*** | (2.34)** |
| Regional dummies? | YES | YES | YES | YES | YES | YES |
| Year fixed-effects? | YES | YES | YES | YES | YES | YES |
| Constant | **−1.33** | **−1.89** | **−1.29** | **−2.05** | **−1.87** | **−1.19** |
| | (0.54)** | (0.50)*** | (0.6) | (0.39) | (0.46)*** | (0.45)*** |
| Observations | **1040** | **1040** | **1040** | **1040** | **1024** | **355** |

Robust standard errors reported in parenthesis: *significant at 10%; **significant at 5%; ***significant at 1%.

Next, we check that our results are not determined by outliers by excluding African countries and also the poorest 25% percentile countries, which have limited access to private capital markets. The results do not change. We also re-run the main regressions using alternative estimation methods (i.e., linear regressions, pure cross-section regressions). These, along with some additional robustness checks are reported in Cavallo and Frankel (2007) and Cavallo (2006).

## 5. Conclusion

In summary, the evidence overall appears to be quite robust. Economies that trade less with other countries are more prone to sudden stops and to currency crashes. Controlling for other plausible determinants of these shocks and instrumenting trade openness by gravity estimates to avoid identification problems, we find a causal link between lack of openness to trade and the instability of financial flows. In fact, out of the set of controls we tried, only the size of current account deficit before the shock appears on a par with trade openness as significant predictors of sudden stops. Trade openness, reserves and nominal exchange rate rigidity also appear as significant predictors of the other form of external crises analyzed.

The effect of trade openness on the probability of sudden stop appears not only qualitatively robust, but also quantitatively significant. Our striking result is that, all else equal, increasing the trade to GDP ratio by 10 percentage points − i.e., going from Argentina's current trade share to Australia's average trade share − reduces the probability of a sudden stop by approximately 1 percentage point. Given that sudden stops are low-probability events, this is equivalent to approximately 40% of the unconditional probability of a crisis.

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## Appendix

### *1. Sudden stops*

We use five alternative definitions of sudden stops: our preferred definition "SS1," and four alternative "SS2," "SS3," "SS4" and "SS5." The list of countries with available data for the sudden stops series, along with the years for which each country is in the sample are reported in Table 6.

The variables "SS2" and "SS3" are conceptually equivalent to "SS1," but are more restrictive in that they capture fewer episodes. "SS4" is, instead, equivalent to "SS1" but is less restrictive in that it classifies as sudden stops events that don't necessarily trigger recessions. "SS5" is equivalent to "SS1" but uses the criterion that the sudden stop be accompanied by a loss of reserves rather than a fall in output. For a full description of the algorithms for every variable, see Cavallo and Frankel (2007).

Table 6
Sudden stop 1

| Country | Data availability | Episodes | | |
|---|---|---|---|---|
| Afghanistan, I.S. of | 1980–1989 | 0 | | |
| Algeria | 1978–1991 | 1 | 1990 | |
| Angola | 1986–2001 | 0 | | |
| Antigua and Barbuda | 1978–2001 | 0 | | |
| Argentina | 1977–2002 | 1 | 2001 | |
| Aruba | 1987–2001 | 0 | | |
| Australia | 1971–2002 | 0 | | |
| Austria | 1971–2002 | 0 | | |
| Bahamas, The | 1977–1982 | 0 | | |
| | 1985–2001 | | | |
| Bahrain, Kingdom of | 1976–2002 | 0 | | |
| Bangladesh | 1977–2002 | 0 | | |
| Barbados | 1971–2001 | 1 | 1982 | |
| Belize | 1985–2002 | 0 | | |
| Benin | 1975–2001 | 1 | 1983 | |
| Bolivia | 1977–2002 | 1 | 1982 | |
| Bosnia and Herzegovina | 1999–2002 | 0 | | |
| Botswana | 1976–1999 | 0 | | |
| Brazil | 1976–2002 | 0 | | |
| Bulgaria | 1981–2002 | 0 | | |
| Burkina Faso | 1975–1996 | 1 | 1989 | |
| Burundi | 1986–2002 | 0 | | |
| Cambodia | 1993–2002 | 0 | | |
| Cameroon | 1978–1995 | 2 | 1988 | 1990 |
| Canada | 1971–2002 | 1 | 1982 | |
| Cape Verde | 1978–2002 | 1 | 1990 | |
| Central African Republic | 1978–1994 | 1 | 1988 | |

Table 6 (*continued*)

| Country | Data availability | Episodes | | | |
|---|---|---|---|---|---|
| Chad | 1978–1994 | 0 | | | |
| Chile | 1976–2002 | 3 | 1982 | 1983 | 1998 |
| China, P.R.: Mainland | 1983–2002 | 0 | | | |
| China, P.R.: Hong Kong | 1999–2002 | 0 | | | |
| Colombia | 1971–2002 | 2 | 1998 | 1999 | |
| Comoros | 1981–1995 | 1 | 1988 | | |
| Congo, Republic of | 1979–2002 | 2 | 1984 | 1996 | |
| Costa Rica | 1978–2002 | 2 | 1981 | 1996 | |
| Côte d'Ivoire | 1976–2002 | 0 | | | |
| Croatia | 1994–2002 | 0 | | | |
| Cyprus | 1977–2002 | 0 | | | |
| Czech Republic | 1994–2002 | 0 | | | |
| Denmark | 1976–1978 | 0 | | | |
| | 1982–2002 | | | | |
| Djibouti | 1993–1995 | 0 | | | |
| Dominica | 1977–2001 | 1 | 2001 | | |
| Dominican Republic | 1971–2002 | 0 | | | |
| Ecuador | 1977–2002 | 2 | 1983 | 1999 | |
| Egypt | 1978–2002 | 1 | 1990 | | |
| El Salvador | 1977–2002 | 1 | 1979 | | |
| Equatorial Guinea | 1988–1996 | 0 | | | |
| Ethiopia | 1978–2002 | 2 | 1982 | 1991 | |
| Fiji | 1980–1999 | 1 | 1999 | | |
| Finland | 1976–2002 | 1 | 1991 | | |
| France | 1976–2002 | 0 | | | |
| Gabon | 1979–1999 | 0 | | | |
| Gambia, The | 1979–1997 | 1 | 1982 | | |
| Germany | 1972–2002 | 1 | 2001 | | |
| Ghana | 1976–2002 | 0 | | | |
| Greece | 1977–1997 | 0 | | | |
| | 2000–2002 | | | | |
| Grenada | 1978–2000 | 0 | | | |
| Guatemala | 1978–2002 | 0 | | | |
| Guinea | 1987–2002 | 0 | | | |
| Guinea-Bissau | 1983–1997 | 1 | 1986 | | |
| Guyana | 1978–1985 | 0 | | | |
| | 1993–1997 | | | | |
| Haiti | 1972–1998 | 0 | | | |
| Honduras | 1975–2002 | 0 | | | |
| Hungary | 1983–2002 | 0 | | | |
| Iceland | 1977–2002 | 1 | 2001 | | |
| India | 1976–2002 | 0 | | | |
| Indonesia | 1982–2002 | 1 | 1997 | | |
| Islamic Republic of Iran | 1977–2000 | 0 | | | |
| Iraq | 1977–1977 | 0 | | | |
| Ireland | 1975–2002 | 0 | | | |
| Israel | 1971–2002 | 2 | 1988 | 1998 | |
| Italy | 1971–2002 | 0 | | | |
| Jamaica | 1977–2002 | 0 | | | |
| Japan | 1978–2002 | 0 | | | |
| Jordan | 1973–2002 | 2 | 1992 | 1993 | |
| Kenya | 1976–2001 | 0 | | | |

(*continued on next page*)

Table 6 (*continued*)

| Country | Data availability | Episodes | | | | |
|---|---|---|---|---|---|---|
| Kiribati | 1980–1994 | 0 | | | | |
| Korea | 1977–2002 | 1 | 1997 | | | |
| Kuwait | 1976–2002 | 0 | | | | |
| Kyrgyz Republic | 1994–2002 | 0 | | | | |
| Lao People's Dem. Repub. | 1985–2001 | 0 | | | | |
| Lesotho | 1976–2002 | 0 | | | | |
| Liberia | 1980–1987 | 0 | | | | |
| Libya | 1978–1999 | 0 | | | | |
| Macedonia, FYR | 1976–2001 | 0 | | | | |
| Madagascar | 1997–2002 | 0 | | | | |
| Malawi | 1975–2002 | 1 | 1981 | | | |
| Malaysia | 1978–2002 | 1 | 1997 | | | |
| Maldives | 1975–2002 | 0 | | | | |
| Mali | 1978–2002 | 0 | | | | |
| Malta | 1976–2001 | 1 | 2000 | | | |
| Mauritania | 1972–2002 | 0 | | | | |
| Mauritius | 1976–1998 | 0 | | | | |
| Mexico | 1977–2002 | 3 | 1982 | 1994 | 1995 | |
| Mongolia | 1980–2002 | 2 | 1990 | 1991 | | |
| Montserrat | 1982–2002 | 0 | | | | |
| Morocco | 1976–2002 | 1 | 1995 | | | |
| Mozambique | 1981–2001 | 0 | | | | |
| Myanmar | 1977–2001 | 0 | | | | |
| Namibia | 1991–2002 | 0 | | | | |
| Nepal | 1977–2000 | 0 | | | | |
| Netherlands | 1971–2002 | 1 | 1981 | | | |
| Netherlands Antilles | 1977–2002 | 0 | | | | |
| New Zealand | 1973–2002 | 2 | 1988 | 1998 | | |
| Nicaragua | 1978–2002 | 1 | 1986 | | | |
| Niger | 1975–1995 | 0 | | | | |
| Nigeria | 1978–1999 | 1 | 1999 | | | |
| Norway | 1976–2002 | 0 | | | | |
| Oman | 1975–2001 | 2 | 1987 | 1999 | | |
| Pakistan | 1977–2002 | 0 | | | | |
| Panama | 1978–2002 | 1 | 2000 | | | |
| Papua New Guinea | 1977–2001 | 0 | | | | |
| Paraguay | 1976–2002 | 1 | 2002 | | | |
| Peru | 1978–2002 | 1 | 1998 | | | |
| Philippines | 1978–2002 | 2 | 1997 | 1998 | | |
| Poland | 1977–2002 | 0 | | | | |
| Portugal | 1976–2002 | 1 | 1992 | | | |
| Romania | 1972–2002 | 0 | | | | |
| Rwanda | 1977–2002 | 1 | 1994 | | | |
| Samoa | 1978–1999 | 0 | | | | |
| Sao Tome and Principe | 1975–1990<br>2000–2002 | 0 | | | | |
| Saudi Arabia | 1972–2002 | 0 | | | | |
| Senegal | 1975–1999 | 0 | | | | |
| Seychelles | 1977–2002 | 1 | 2000 | | | |

Table 6 (*continued*)

| Country | Data availability | Episodes | | | | |
|---|---|---|---|---|---|---|
| Sierra Leone | 1978–1995 | 0 | | | | |
| Singapore | 1973–2002 | 0 | | | | |
| Slovak Republic | 1994–2000 | 0 | | | | |
| Slovenia | 1993–2002 | 0 | | | | |
| Solomon Islands | 1976–1998 | 1 | 1998 | | | |
| Somalia | 1978–1989 | 0 | | | | |
| South Africa | 1986–2002 | 0 | | | | |
| Spain | 1976–2002 | 1 | 1992 | | | |
| Sri Lanka | 1976–2002 | 1 | 2001 | | | |
| St. Kitts and Nevis | 1981–2001 | 0 | | | | |
| St. Lucia | 1977–2001 | 1 | 2001 | | | |
| Saint Vincent and the Grenadines | 1979–2001 | 1 | 2000 | | | |
| Sudan | 1978–2002 | 0 | | | | |
| Suriname | 1978–2002 | 1 | 1992 | | | |
| Swaziland | 1975–2002 | 1 | 1999 | | | |
| Sweden | 1971–2002 | 1 | 1991 | | | |
| Switzerland | 1978–2002 | 0 | | | | |
| Syrian Arab Republic | 1978–2000 | 1 | 1989 | | | |
| Tanzania | 1977–2002 | 0 | | | | |
| Thailand | 1976–2002 | 1 | 1997 | | | |
| Togo | 1975–2001 | 0 | | | | |
| Tonga | 1972–1993 | 1 | 1989 | | | |
| Trinidad and Tobago | 1976–2001 | 1 | 1984 | | | |
| Tunisia | 1977–2002 | 0 | | | | |
| Turkey | 1975–2002 | 4 | 1991 | 1994 | 1998 | 2001 |
| Uganda | 1981–2002 | 0 | | | | |
| United Kingdom | 1971–2002 | 0 | | | | |
| United States | 1971–2002 | 0 | | | | |
| Uruguay | 1979–2002 | 1 | 2002 | | | |
| Vanuatu | 1983–2001 | 1 | 1991 | | | |
| Venezuela, Rep. Bol. | 1971–2002 | 1 | 1994 | | | |
| Vietnam | 1997–2002 | 0 | | | | |
| Republic of Yemen | 1991–2002 | 1 | 1994 | | | |
| Zambia | 1979–1991 | | | | | |
| | 1998–2000 | 1 | 1990 | | | |
| Zimbabwe | 1978–1993 | 1 | 1983 | | | |

## 2. Gravity estimates

To compute the gravity estimates we use Frankel and Rose (2002) data set. It consists of 41,678 bilateral trade observations spanning six different years (1970, 1975, 1980, 1985, 1990, and 1995). All 186 countries, dependencies, territories, overseas departments, colonies, and so forth for which the United Nations Statistical Office collects international trade data are included in the data set. The trade data are taken from the World Trade Database, a consistent recompilation of the UN trade data presented in Feenstra et al. (1997), augmented with data from UN's International Trade Statistics Yearbook. This data set is estimated to cover at least 98% of all trade.

For each of the six different years for which we have data we compute OLS regressions of the following form:

$$\mathrm{Log}\left(T_{i,j}/Y_i\right) = c + \alpha \log \mathrm{dist}_{i,j} + \beta \log \mathrm{pop}_j + \gamma \mathrm{comlang}_{i,j} + \delta \mathrm{border}_{i,j} + \theta \mathrm{areap}_{i,j} + \rho \mathrm{landlock} + \mu$$

where "$T_{i,j}$" is the bilateral trade value between countries "$i$" and "$j$"; "$Y_i$" is the real GDP of country "$i$"; "$c$" is a constant term; "log $\mathrm{dist}_{i,j}$" is the log of the distance between the economic centers of countries "$i$" and "$j$"; "comlang" is a dummy variable that takes value one if "$i$" and "$j$" share a common language and is zero otherwise; "border" is a dummy variable that takes value one if "$i$" and "$j$" share a border and is zero otherwise; "$\mathrm{areap}_{i,j}$" is the log of the product of the areas (in $\mathrm{km}^2$) of countries "$i$" and "$j$"; and "landlock" takes values two if "$i$" and "$j$" are both landlocked, one if either "$i$" or "$j$" are landlocked, and zero otherwise; and "$\mu$" is the error term.

As an example, we report the results obtained for the equation estimated for 1990:

Instrumental variable (first stage) generation

$$\mathrm{Log}\left(T_{i,j}/Y_i\right) = \underset{(0.05)}{-0.94} \log \mathrm{dist}_{i,j} + \underset{(0.02)}{0.82} \log \mathrm{pop}_j + \underset{(0.11)}{0.53} \mathrm{comlang}_{i,j} + \underset{(0.21)}{0.64} \mathrm{border}_{i,j} - \underset{(0.01)}{0.27} \mathrm{areap}_{i,j} - \underset{(0.08)}{0.47} \mathrm{landlock}$$

Equation estimated for 1990 using OLS. $R^2 = 0.28$ and number of observations = 4052.

Robust standard errors in parentheses; intercept not reported. The correlation between trade ratio and generated IV for the entire panel is 0.52.

The gravity estimates (or predicted trade to GDP ratios used in the regressions) are generated by taking the exponent of fitted values and summing across bilateral partners $j$. This yields estimates for six different years: 1970, 1975, 1980, 1985, 1990 and 1995. The missing values of the panel are generated by taking the observation corresponding to the closest year with data.

## 3. Summary statistics and data sources

Table 7

| Variable | Obs. | Mean | Std. dev. | Min. | Max. |
|---|---|---|---|---|---|
| SS1 | 3596 | 0.02 | 0.15 | 0 | 1 |
| SS2 | 3599 | 0.02 | 0.14 | 0 | 1 |
| SS3 | 3599 | 0.01 | 0.11 | 0 | 1 |
| SS4 | 3595 | 0.04 | 0.20 | 0 | 1 |
| SS5 | 3398 | 0.02 | 0.13 | 0 | 1 |
| Trade openness (A) | 4247 | 0.73 | 0.43 | 0.02 | 2.96 |
| Fitted openness (B) | 4261 | 0.15 | 0.15 | 0.00 | 1.36 |
| Liability dollarization (1) (C) | 3454 | 0.32 | 0.39 | 0 | 2 |
| Liability dollarization (2) (D) | 897 | 0.27 | 0.28 | 0 | 1 |
| CA/GDP (F) | 3630 | −0.04 | 0.10 | −2.40 | 0.59 |
| Foreign debt/GDP (G) | 1791 | 0.28 | 0.44 | 0.00 | 5.84 |

Table 7 (*continued*)

| Variable | Obs. | Mean | Std. dev. | Min. | Max. |
|---|---|---|---|---|---|
| Index of exchange rate rigidity (H) | 3059 | 2.41 | 0.81 | 1 | 3 |
| Voice and accountability (I) | 3255 | 0.35 | 0.90 | −1.62 | 1.69 |
| Political stability/lack of violence (I) | 3038 | 0.23 | 0.83 | −1.69 | 1.69 |
| Effectiveness of government (I) | 3038 | 0.31 | 0.84 | −1.32 | 2.08 |
| Regulatory framework (I) | 3224 | 0.36 | 0.59 | −1.50 | 1.24 |
| Rule of law (I) | 3224 | 0.29 | 0.87 | −1.20 | 2 |
| Control of corruption (I) | 3038 | 0.30 | 0.92 | −1.10 | 2.13 |
| FDI/GDP (J) | 3963 | 1.90 | 4.58 | −83 | 145 |
| Reserves in month of imports (K) | 3795 | 3.42 | 2.96 | −0.09 | 32 |
| GDP per capita (L) | 2799 | 6840 | 9583 | 84 | 52675 |
| Short-term debt/total external debt (M) | 3430 | 12.40 | 12.86 | 0 | 99 |
| Polity 2 (O) | 4102 | 0.42 | 7.57 | −10 | 10 |
| Crisis episodes (P) | 3039 | 0.14 | 0.34 | 0 | 1 |
| Systemic sudden Stop (Q) | 1440 | 0.12 | 0.33 | 0 | 1 |

(A) The negative of the trade to GDP ratio over 100. *Source*: WDI-CD ROM. (B) See for an explanation of the methodology employed and data used. (C) The ratio of foreign liabilities of the financial sector to money. *Source*: IFS (line 26C/line 34). (D) The ratio of ''total dollar deposits/total deposits''. *Source*: Arteta (2005a,b). (F) Ratio over 100. *Source*: WDI-CD ROM. (G) *Source*: IFS line 89c. (H) index = 1 is (de-facto) flexible exchange rate; index = 2 is (de-facto) intermediate arrangement; and index = 3 is (de-facto) peg. *Source*: Levy-Yeyati and Sturzenegger (2003). (I) *Source*: Kaufmann et al. (2002). (J) *Source*: WDI-CR ROM. (K) *Source*: WDI-CD ROM. (L) *Source*: WDI-CD ROM. (M) Ratio over 100. *Source*: WDI-CD Rom. (O) Range = −10 to 10 (−10 = high autocracy; 10 = high democracy). Combined polity score. *Source*: Marshall and Jaggers (2002). (P) *Source*: Frankel and Wei (2004). The approach in Frankel and Wei (2004) is to use the foreign exchange market pressure index. This index is defined as the percentage fall in reserves plus the percentage fall in the foreign exchange value of the currency. The idea is that this index measures the fall in demand for the country's currency; it is then up to the monetary authorities to determine whether to accommodate, by letting the money supply fall, or to depreciate. To avoid treating every year of a multi-year high-inflation period as a separate crisis, the approach followed by the authors requires that for an event to be considered a crisis episode, the increase in exchange market pressure must represent an acceleration of at least an additional 10% over the preceding period; and they also adopt an exclusion window of 3 years. (Q) *Source*: Calvo et al. (2006).

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