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Journal of International Money and Finance
18 (1999) 603–617

Journal of International Money and Finance

www.elsevier.com/locate/jimonfin

Contagion and trade
Why are currency crises regional?

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Abstract

Currency crises tend to be regional; they affect countries in geographic proximity. This suggests that patterns of international trade are important in understanding how currency crises spread, above and beyond any macroeconomic phenomena. We provide empirical support for this hypothesis. Using data for five different currency crises (in 1971, 1973, 1992, 1994 and 1997) we show that currency crises affect clusters of countries tied together by international trade. By way of contrast, macroeconomic and financial influences are not closely associated with the cross-country incidence of speculative attacks.

JEL classification: F32.

Keywords: Speculative; Attack; Exchange rate; International; Macroeconomic; Empirical

1. Introduction

Currency crises tend to be regional. In this paper, we attempt to document this fact, and to understand its implications.

Most economists think about currency crises using one of two standard models of speculative attacks. The “first generation” models of, for example, Krugman

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0261-5606/99/$ - see front matter
PII: S0261-5606(99)00023-6

(1979) direct attention to inconsistencies between an exchange rate commitment and domestic economic fundamentals such as an underlying excess creation of domestic credit, typically prompted by a fiscal imbalance. The "second generation" model of, for example, Obstfeld (1986) views currency crises as shifts between different monetary policy equilibria in response to self-fulfilling speculative attacks. There are many variants of both models, and a number of empirical issues associated with both classes of models, as discussed in Eichengreen et al. (1995). What is common to both classes of models is their emphasis on macroeconomic and financial fundamentals as determinants of currency crises, but macroeconomic phenomena do not tend to be regional. Thus, from the perspective of most speculative attack models, it is hard to understand why currency crises tend to be regional, at least without an extra ingredient explaining why the relevant macro fundamentals are intra-regionally correlated.[1]

On the other hand, trade patterns *are* regional; countries tend to export and import with countries in geographic proximity.[2] Prima facie then, trade linkages seem like an obvious place to look for a regional explanation of currency crises. It is easy to imagine why the trade channel might potentially be important. If prices tend to be sticky, a nominal devaluation delivers a real exchange rate pricing advantage, at least in the short run. That is, countries lose competitiveness when their trading partners devalue. They are therefore more likely to be attacked—and to devalue—themselves.[3]

Of course, this channel may not be important in practice. Nominal devaluations need not result in real exchange rate changes for any long period of time. Devaluations are costly and can be resisted. Making the case for the trade channel is primarily an empirical exercise.

This paper is intended to contribute a single point to the growing literature on currency contagion. We argue that trade is an important channel for contagion, above and beyond macroeconomic influences. Countries who trade and compete with the targets of speculative attacks are themselves likely to be attacked.

Our point is modest and intuitive. We ignore a number of related issues. For instance, in trying to model "contagion" in currency crises, we do not rule out the possibility of (regional) shocks common to a number of countries. Moreover, we do not attempt to study the timing or intensity of currency crises.[4] We *do* intend to show that, given the occurrence of a currency crisis, the incidence of speculative attacks across countries is linked to the importance of international trade linkages. That is, currency crises spread along the lines of trade linkages, after accounting for

[1] Rigobon (1998) provides an alternate theoretical framework which argues that the regional nature of currency crises is due to investors learning about a given model of development (assuming that such models tend to be regional).

[2] The evidence is overwhelming: Leamer and Levinsohn (1995) provide a recent survey.

[3] This reasoning is strengthened if devaluing countries tend to experience contractions, as seems to be the historic norm. For instance, if devaluing countries tend to have un-hedged external liabilities, devaluation may cause bankruptcies in the financial sector, a domestic credit crunch and hence a recession. Since imports are highly cyclic, this puts even more pressure on neighboring countries.

[4] We study the intensity of currency crises in the working paper version of this paper.

the effects of macroeconomic and financial factors.[5] This linkage is intuitive, statistically robust, and important in understanding the regional nature of speculative attacks.

Section 2 motivates the analysis by discussing the regional nature of three recent waves of speculative attacks. This is followed by a section that provides a framework for our analysis. Our methodology and data are discussed in Section 4; the actual empirical results follow. The paper ends with a brief conclusion.

2. Have currency crises been regional?

The answer to this question is substantially, but not exclusively.

The last decade has witnessed three important currency crises. In the autumn of 1992, a wave of speculative attacks hit the European Monetary System and its periphery. Before the end of the year, five countries (Finland, the UK, Italy, Sweden and Norway) had floated their currencies. Despite attempts by a number of countries to remain in the EMS with the assistance of devaluations (by Spain, Portugal and Ireland), the system was unsalvageable. The bands of the EMS were widened to ± 15% in August 1993. Eichengreen and Wyplosz (1993) provide a well-known review of the EMS crisis.

The Mexican peso was attacked in late 1994 and floated shortly after an unsuccessful devaluation. Speculative attacks on other Latin American countries occurred immediately. The most prominent targets of the "Tequila Hangover" were Latin American countries, especially Argentina and Brazil, but also including Peru and Venezuela. Not all Latin American countries were attacked—Chile being the most visible exception—and not all economies attacked were in Latin America (Thailand, Hong Kong, the Philippines and Hungary also suffered speculative attacks). While there were few devaluations, the attacks were not without effect. Argentine macroeconomic policy in particular tightened dramatically, precipitating a sharp recession. Sachs et al. (1996) provide one of many summaries of the Mexican crisis and its aftermath.

The "Asian Flu" began with continued attacks on Thailand in the late spring of 1997 and continued with flotation of the baht in early July 1997. Within days, speculators had attacked Malaysia, the Philippines and Indonesia. Hong Kong and Korea were attacked somewhat later on; the crisis then spread across the Pacific to Chile and Brazil. The effects of "Bhatulism" linger on as this paper is being written; Corsetti et al. (1998a) provide an exhaustive survey.

All three waves of attacks were largely regional phenomena.[6] Once a country had suffered a speculative attack—Thailand in 1997, Mexico in 1994, Finland in 1992—

[5] Of course, currency crises may spread through other channels as well, such as international asset and debt relationships. However, these non-trade linkages tend to be correlated with trade flows. Data constraints prevent us from explicitly comparing these channels to our trade and macro channels for contagion.

[6] Trade patterns have had important effects in spreading currency crises before the 1990s, as we document below.

its trading partners and competitors were disproportionately likely to be attacked themselves. Not all major trading partners devalued—indeed, not all major trading partners were even attacked. Macroeconomic and financial influences are certainly not irrelevant, but neither, as we shall see, is the trade channel irrelevant as a means of transmitting speculative pressures across international borders.

3. The framework

Contagion in currency crises has come to be studied by economists only recently. Eichengreen et al. (1996) provide a critical survey and some early evidence.

For the purposes of this study, we think of a currency crisis as being contagious if it spreads from the initial target(s), for whatever reason. As is well known, it is difficult to distinguish empirically between common shocks and contagion. The evidence in favor of contagion is indirect at best. Still, we believe that the preponderance of evidence favors the existence of contagion effects; Eichengreen and Rose (1998) provide evidence.

There are at least two different types of explanations for why contagion spreads, transmission mechanisms that are not mutually exclusive. The first relies on macroeconomic or financial similarity. A crisis may spread from the initial target to another if the two countries share various economic features. The work of Sachs et al. (1996) can be viewed in this light. They focus on three intuitively reasonable fundamentals: real exchange rate over-valuation; weakness in the banking system; and low international reserves (relative to broad money). They find that these three variables can explain half the cross-country variation in a crisis index, itself a weighted average of exchange rate depreciation and reserve losses. They use data from 20 developing countries in late 1994 and early 1995. Along the same lines, similarity in terms of structural characteristics of the economy is analyzed in Rigobon (1998). Currency crises may be regional if macroeconomic features of economies tend to be regional.

The alternative view is that a devaluation gives a country a temporary boost in its competitiveness, in the presence of nominal rigidities. Its trade competitors are then at a competitive disadvantage; those most adversely affected by the devaluation are likely to be attacked next. Gerlach and Smets (1994) formalize this reasoning; Huh and Kasa (1997) provide related analysis. In this way, a currency crisis that hits one country (for whatever reason) may be expected to spread to its trading partners. Since trade patterns are strongly negatively affected by distance, currency crises will tend to be regional.

Eichengreen and Rose (1998) found both “macroeconomic” and “trade” channels of transmission to be empirically relevant in a large quarterly panel of post-1959 industrial country data; trade effects dominated. Thus it is not clear a priori which of the mechanisms for contagion, if any, might be present in the data we examine. For this reason, we try to account for both in our empirical work.

4. Methodology

Our objective in this paper is to demonstrate that trade provides an important channel for contagion above and beyond macroeconomic and financial similarities. As a result, we focus on the incidence of currency crises *across countries*. We ask why some countries are hit during certain episodes of currency instability, while others are not.

4.1. Empirical strategy

Our strategy keys off the "first victim" of a speculative attack. A country is attacked for some reason. We do not take a stance one way or another on whether this initial attack is warranted by bad fundamentals (as would be true in a first-generation model) or is the result of a self-fulfilling attack (consistent with a second-generation model). Instead, we ask: Given the incidence of the initial attack, how does the crisis spread from "ground zero"? Are the subsequent targets closely linked by international trade to the first victim? Do they share common macroeconomic similarities? We interpret evidence in favor of the first hypothesis as indicating the importance of the trade channel of contagion.

Clearly we do not deal with a number of related and important issues. We assume that there is contagion, and do not test for its presence. We do not attempt to explain the timing, intensity, or ordering of currency crises. Finally, we do not ask why some crises become contagious and spread while others do not.

Our basic regression is:

$$Crisis_i = \varphi Trade_i + \lambda M_i + \epsilon_i$$

where: $Crisis_i$ is an indicator variable which is defined as unity if country i was attacked in a given episode, and zero if the country was not attacked; $Trade_i$ is a measure of trade linkage between country i and ground zero; M_i is a set of macroeconomic control regressors; λ is the corresponding vector of nuisance coefficients; and ϵ is a normally distributed disturbance representing a host of omitted influences which affect the probability of a currency crisis.

We estimate this binary probit equation across countries via maximum likelihood. The null hypothesis of interest is H_0: $\varphi = 0$. We interpret evidence against the null as being consistent with a trade contagion effect.

4.2. The data set

We use cross-sectional data from five different episodes of important and widespread currency instability. These are: (1) the breakdown of the Bretton Woods system in the spring of 1971; (2) the collapse of the Smithsonian Agreement in the late winter of 1973; (3) the EMS Crisis of 1992–93; (4) the Mexican meltdown and the Tequila Effect of 1994–95; and (5) the Asian Flu of 1997–98. Our data set includes

data from 161 countries, many of which were directly involved in *none* of the five episodes.[7]

Making our work operational entails: (a) measuring currency crises; (b) measuring the importance of trade between the "first victim" and country *i*; and (c) measuring the relevant macroeconomic and financial control variables. We now deal with these tasks in order.

4.3. Currency crises

To construct our simple binary indicator regressand, it is relatively easy to determine crisis victims from journalistic and academic histories of the various episodes (we rely on *The Financial Times* in particular). We have five different dummy variables, one for each episode, with crisis countries entered as one, non-crisis countries as zero.[8] Our list of crisis countries is tabulated in Appendix A. All five waves of currency crises we examine have a strongly regional nature.[9]

The table in Appendix A also shows the "first victim" or "ground zero" countries first attacked. For some periods the "first victim" is relatively straightforward (Mexico in 1994, Thailand in 1997). For others, it is more arguable. In 1971 and 1973 we consider Germany to be ground zero. A case can be made that the US should be ground zero for the 1971 and 1973 episodes. However, since the US dollar was the key currency of the international monetary system, the change in the value of the dollar during these periods can be interpreted more as a common shock. A priori, we choose to rule out such a common shock when testing for contagion effects transmitted through the trade channel. The 1992 crisis is more complex still. We think of the Finnish flotation as being the first important incident (making Finland "ground zero"), but one can make a case for Italy (which began to depreciate immediately following the Danish Referendum) or Germany because of the aftermath of unification (though as the center of the EMS, German shocks are common). As we shall see, our results do not appear to be very sensitive to the exact choice of "first victim" country.

4.4. Trade linkages

Once our "ground zero" country has been chosen, we need to be able to quantify the importance of international trade links between the first victim and other coun-

[7] The set consists of economies with bilateral exports of $5 million or more to at least one trade partner in 1971. Not all countries exist for all episodes, and not all countries with trade relations have sovereign currencies. The exact list of countries is contained in the working paper version of this paper.

[8] Countries that were not attacked during any of our five episodes are not included in Table 7, though they are included in our empirical analysis depending on trade and macroeconomic data availability. Since we are interested in linking crisis incidence to trade ties between the "first victim" and other countries, we necessarily exclude the former from our statistical analysis.

[9] Chi-squared tests of independence are included in the working paper, and confirm what the eye can see, namely that currency crises appear to be regional.

tries. We focus on the degree to which ground zero competes with other countries in foreign (third country) export markets. Our default measure of trade linkage is

$$Trade_i \equiv \sum_k \{[(x_{0k} + x_{ik})/(x_{0.} + x_{i.})] \cdot [1 - |(x_{ik} - x_{0k})|/(x_{ik} + x_{0k})]\}$$

where x_{ik} denotes aggregate bilateral exports from country i to country k ($k \neq i,0$) and $x_{i.}$ denotes aggregate bilateral exports from country i. This index is a weighted average of the importance of exports to country k for countries 0 and i. The importance of country k is greatest when it is an export market of equal importance to both 0 and i. The weights are proportional to the importance of country k in the aggregate trade of countries 0 and i. Higher values of $Trade_i$ denote greater trade competition between 0 and i in foreign export markets.

Our trade measures are computed using annual data for the relevant crisis year taken from the IMF's *Direction of Trade* data set.[10] The rankings which result are, for the most part, intuitive and sensible.[11]

Still, our default measure is clearly an imperfect measure of the importance of trade linkages between country i and "ground zero". It relies on actual rather than potential trade, and aggregate data. It ignores direct trade between the two countries. Imports are ignored. Countries of vastly different size are a potential problem. Cascading effects are ignored.[12]

We have computed a number of different perturbations to our benchmark measure, and found that our trade measures are relatively insensitive to the exact way we measure the trade linkage. For instance, we have calculated a "direct" measure of trade and a "total" measure of trade. Our direct trade measure is defined analogously to our benchmark measure as

$$DirectTrade_i = 1 - |x_{i0} - x_{0i}|/(x_{i0} + x_{0i})$$

This index is higher as the bilateral exports between countries 0 and i become more equal. A measure of total trade, $TotalTrade_i$, is the weighted sum of $Trade_i$ and $DirectTrade_i$, where the latter is weighted by $(x_{i0} + x_{0i})/(x_{0.} + x_{i.})$. We have also used a measure of trade linkages which uses trade shares as our measure of competition in foreign export markets, so as to adjust for the varying size of countries:

[10] The timing of our data is as follows: the 1971 episode uses control data for both macroeconomic and trade linkages from 1970; the 1973 episode uses 1972 data; 1992 uses 1992; 1994 uses 1994; and 1997 uses 1996.

This data set was supplemented with Taiwan trade data from *Monthly Statistics of Exports and Imports, Taiwan Area*, Department of Statistics, Ministry of Finance, Taiwan, and macro data from *Financial Statistics, Taiwan District*, Central Bank of China, Taiwan, (various issues).

[11] The top 20 trade partners linked to "ground zero" are tabulated in the working paper version. Our measure has an obvious similarity to the Grubel and Lloyd (1971) measure of cross-country intra-industry trade.

[12] After Finland floated the markka in 1992, Sweden was immediately attacked. One might then ask how the crisis should spill over from both Finland and Sweden.

$$TradeShare_i \equiv$$

$$\sum_k \{[(x_{0k} + x_{ik})/(x_{0.} + x_{i.})] \cdot [1 - |\{(x_{0k}/x_{0.}) - (x_{ik}/x_{i.})\}|/\{(x_{0k}/x_{0.}) + (x_{ik}/x_{i.})\}]\}$$

We check extensively for the sensitivity of our results to ensure that our results do not depend on the exact measure of trade linkage.

4.5. Macroeconomic controls

Our objective is to use a variety of different macroeconomic controls to account for the standard determinants of currency crises dictated by first- and second-generation models. We do this so that our trade linkage variable picks up the effects of currency crises abroad that spill over because of trade. That is, we are interested in the partial effect of trade *after* taking account of macroeconomic and financial imbalances that might lead to a currency crisis.

Our most important macro controls are: the annual growth rate of domestic credit (IFS line 32; we also use private credit [IFS line 32d] which excludes credit to governments, public enterprises and so forth); the government budget as a percentage of GDP (a surplus being positive; IFS line 80 over line 99b); the current account as a percentage of GDP (IFS line 78ald multiplied by line rf in the numerator); the growth rate of real GDP (IFS line 99b.r); the ratio of M2 to international reserves (IFS lines 34 + 35 multiplied by line rf over line1l.d); and domestic CPI inflation (IFS line 64); and the degree of currency under-valuation.[13]

Our data set is annual, and was extracted from the IMF's *International Financial Statistics*.[14] It has been checked for outliers via both visual and statistical filters.

5. Some results

5.1. Univariate evidence on trade and macroeconomic linkages

Table 1 shows a series of t-tests that test for equality of cross-country means for countries affected and unaffected by currency crises. These are computed under the null hypothesis of equality of means between crisis and non-crisis countries (assuming equal but unknown variances). Thus, a significant difference in the

[13] We measure the last by constructing an annual real exchange rate index as a weighted sum of bilateral real exchange rates (using domestic and real CPIs) in relation to the currencies of all trading partners with available data. The weights sum to one and are proportional to the bilateral export shares with each partner. The degree of currency under-valuation is defined as the percentage change in the real exchange rate index between the average of the previous 3 years and the episode year. A positive value indicates that the real exchange rate is depreciated relative to the average of the previous 3 years.

[14] Limited availability of macroeconomic data generally reduces the number of usable observations in our regression analysis far below the set of 161 countries for which we have trade data.

Table 1
T-tests for equality by crisis incidence

	1971	1973	1992	1994	1997
Trade	−9.5	−10.9	−4.7	−6.9	−7.5
%ΔM1	0.8	1.1	1.2	−0.9	−0.1
%ΔM2	1.6	0.8	1.1	−0.6	0.0
%ΔCredit	0.8	1.3	0.4	−0.2	−0.4
%ΔPrivate credit	1.2	0.1	0.7	−0.5	0.3
M2/Reserves	−3.5	−2.6	0.3	0.5	−0.3
%ΔReserves	−1.8	0.7	1.3	1.4	2.1
%ΔExports	−1.0	−0.9	0.1	−0.5	0.1
%ΔImports	−1.5	−1.1	0.8	−1.1	−0.6
Current account/GDP	−2.0	−2.1	−0.8	0.2	−0.8
Budget/GDP	−1.6	−1.9	1.4	−0.9	−0.4
Real growth	0.7	0.5	1.1	−1.6	−2.7
Investment/GDP	−3.2	−2.8	1.0	−0.2	−2.7
Inflation	−0.3	0.7	1.5	−1.0	0.6
Under-valuation	−0.5	−0.9	0.6	1.5	−0.6

Values tabulated are t-statistics, calculated under the null hypothesis of equal means and variances. A significant negative statistic indicates that the variable was significantly higher for crisis countries than for non-crisis countries.

behavior of the variable across crisis and non-crisis countries—for instance, consistently higher money growth for crisis countries—would show up as a large (negative) t-statistic.

There are two important messages from Table 1. First, the strength of trade linkage to "ground zero" varies systematically between crisis and non-crisis countries. In particular, it is systematically higher for crisis countries at reasonable levels of statistical significance. Second, macroeconomic variables do *not* typically vary systematically across crisis and non-crisis countries. While some variables sometimes have significantly different means, these results are not consistent across episodes, and they are never as striking as the trade results. These findings are consistent with the importance of the trade channel in contagion.

5.2. Multivariate probit results

Table 1 is not completely persuasive. One problem is that it consists of a set of univariate tests. We remedy that problem in Tables 2 and 3. Table 2 is a multivariate equivalent of Table 1, including a host of macroeconomic variables simultaneous with the trade variable. It reports probit estimates of cross-country crisis incidence on trade linkage and macroeconomic controls. The latter variables are dictated by a variety of different models of speculative attacks (as discussed in Eichengreen et al. (1995)) which can be viewed as primitive determinants of vulnerability to speculative pressure. Table 3 uses a wider range of countries (since many macroeconomic observations are missing in our sample) but restricts attention to the degree of currency

Table 2
Multivariate probit results with macro controls

	1971	1973	1992	1994	1997
Trade	2.09	3.18	0.003	0.50	0.68
	(2.7)	(2.7)	(2.1)	(2.9)	(2.6)
%ΔCredit	−0.01	−0.01	0.00	0.00	NA
	(1.2)	(0.4)	(1.1)	(0.0)	
Budget/GDP	0.01	0.04	−0.00	0.00	NA
	(0.3)	(1.2)	(0.8)	(0.9)	
Current account/GDP	0.00	0.03	0.00	−0.00	0.00
	(0.2)	(1.0)	(0.1)	(1.7)	(0.0)
Real growth	−0.00	0.04	−0.00	0.00	0.04
	(0.2)	(1.2)	(1.6)	(0.1)	(2.2)
M2/Reserves	0.00	0.01	0.00	−0.00	0.00
	(0.2)	(0.4)	(1.0)	(0.5)	(0.8)
Inflation	0.01	0.01	−0.00	0.00	0.00
	(0.4)	(0.5)	(1.3)	(0.7)	(0.3)
Observations	53	60	67	67	50
Slopes (7)	26	36	24	16	17 (5df)
McFadden's R^2	0.38	0.49	0.50	0.36	0.38
P-value: macro = 0	0.89	0.64	0.59	0.68	0.26

Absolute value of *z*-statistics in parentheses. Probit estimated with maximum likelihood.

Table 3
Probit results with currency misalignment

	1971	1973	1992	1994	1997
Trade	2.25	2.88	0.31	0.45	0.54
	(4.5)	(4.2)	(3.2)	(3.8)	(4.5)
Under-valuation	0.00	0.00	−0.00	−0.00	0.00
	(1.3)	(1.8)	(0.5)	(1.4)	(1.1)
Observations	80	85	111	109	107
McFadden's R^2	0.38	0.48	0.21	0.34	0.36

Absolute value of *z*-statistics in parentheses. Probit estimated with maximum likelihood.

under- or over-valuation. This is viewed by some as a summary statistic for macroeconomic misalignment.

Since probit coefficients are not easily interpretable, we report the effects of one-unit (i.e. one percentage point) changes in the regressors on the probability of a crisis (also expressed in probability values so that 0.01 = 1%), evaluated at the mean of the data. We include the associated *z*-statistics in parentheses; these test the null of no effect variable by variable. Diagnostics are reported at the foot of the table. These include a test for the joint significance of all the coefficients ("slopes") which is distributed as chi-squared with seven degrees of freedom under the null hypothesis of no effect. We also include a *p*-value for the hypothesis that none of the macro effects are jointly significant (i.e. all the coefficients except the trade effect).

The results are striking. The trade channel for contagion seems consistently important in both statistical and economic terms. While the economic size of the effect varies significantly across episodes it is consistently different from zero at conventional levels of statistical significance. Its consistently positive sign indicates that a stronger trade linkage is associated with a higher incidence of a currency crisis.

On the other hand, the macroeconomic controls are small economically and rarely of statistical importance. This is true both of individual variables, and of all seven macroeconomic factors taken simultaneously. It is also true of currency under-valuation.

Succinctly, the hypothesis of no significant trade channel for contagion seems wildly inconsistent with the data, while macroeconomic controls do not explain the cross-country incidence of currency crises.

5.3. Robustness

We have checked for the sensitivity of our probit results with respect to a number of perturbations to our basic methodology. A number of robustness checks are exhibited in Tables 4–6.

Table 4 varies the macro control regressors. In place of the macroeconomic regressors of Tables 2 and 3, we substitute: the growth rate of M1 (IFS line 34); the change in the budget/GDP and current account/GDP ratios; and the investment/GDP ratio (IFS 93e over line 99b). We also add the country credit rating

Table 4
Sensitivity analysis: macro controls

	1971	1973	1992	1994	1997
Trade	1.28	1.21	0.002	0.0002	0.23
	(2.6)	(3.1)	(1.6)	(2.1)	(1.6)
%ΔM1	−0.01	−0.00	−0.00	−0.00	−0.00
	(1.3)	(0.6)	(1.1)	(0.6)	(0.9)
Δ (Budget/GDP)	0.03	−0.01	0.00	0.00	−0.01
	(0.7)	(0.9)	(0.4)	(1.0)	(0.8)
Δ (Current account/GDP)	0.01	−0.01	0.00	−0.00	−0.00
	(0.9)	(1.2)	(1.2)	(0.4)	(0.7)
Investment/GDP	0.02	0.02	−0.00	0.00	0.00
	(1.8)	(2.0)	(1.0)	(1.1)	(0.7)
Institutional Investor rating	NA	NA	0.00	−0.000001	−0.00
			(1.4)	(1.8)	(0.8)
Observations	54	60	62	63	27
Slopes (df)	26 (5)	38 (5)	24 (6)	24 (6)	13 (6)
McFadden's R^2	0.41	0.59	0.61	0.62	0.58
P-value: macro = 0	0.25	0.40	0.60	0.71	0.67

Absolute value of *z*-statistics in parentheses. Probit estimated with maximum likelihood.

Table 5
Sensitivity analysis: trade measure. Coefficients on trade variable; macro controls (from Table 4) not reported

	1971	1973	1992	1994	1997
Rank of trade	−0.01	−0.01	−0.00	−0.001	−0.003
	(3.3)	(3.1)	(1.9)	(1.9)	(2.1)
Total trade	2.05	3.15	0.004	0.51	0.68
	(2.7)	(2.7)	(2.2)	(2.9)	(2.7)
Trade share	1.54	2.04	0.000	0.23	0.57
	(3.5)	(3.3)	(1.8)	(2.2)	(2.1)

Absolute value of *z*-statistics in parentheses. Probit estimated with maximum likelihood.

Table 6
Sensitivity analysis: regressand. Coefficients on trade variable; macro controls not reported

"Ground zero"	1971	1973	1992
US	1.39	1.85	NA
	(2.1)	(2.6)	
Germany	NA	NA	0.95
			(3.0)
Italy	NA	NA	0.46
			(3.0)

Absolute *z*-statistics in parentheses. MLE probit.

from *Institutional Investor*.[15] However, our trade linkage variable remains positive and statistically significant despite our substitutions. We have also tried a variety of other sets of macroeconomic controls, without changing the thrust of our results; for the sake of brevity, these experiments are not reported.[16]

Table 5 leaves the macro controls unchanged (and unreported, again for the sake of compactness) and substitutes different measures of trade linkages between each country and "ground zero". We use: the rank rather than the actual continuous measure of $Trade_i$ (with a rank of "1" denoting the most important trading partner, "2" being the second most important trade linkage and so forth), our measure of total trade, and our measure of trade share linkages. Our finding of a positive statistically significant role for trade linkages is not substantially altered.

We have also changed the regressand, that is, the way we measure the actual incidence of crises across countries. Results are reported in Table 6. The first row

[15] These ratings are taken every 6 months, and range potentially from 100 (a perfect score) to 0. We thank Cam Harvey for providing this data set to us.

[16] For instance, when we add the ratio of FDI to total external debt, the variable is usually correctly (negatively) signed. However, it is always statistically insignificant at conventional levels, and most importantly, it never "knocks out" the significance of our trade variable.

shows the effect of treating the United States as "ground zero" in 1971 and 1973; the second and third rows use Germany and Italy respectively as "ground zero" in 1992. Our finding of a significant trade effect is not destroyed by using other (reasonable) starting points for these contagion episodes.[17]

6. Concluding comments

We have found strong evidence that currency crises tend to spread along regional lines. This is true of five recent waves of speculative attacks (in 1971, 1973, 1992, 1994–95 and 1997). Accounting for a variety of different macroeconomic effects does not change this result. Indeed macroeconomic factors do not consistently help much in explaining the cross-country incidence of speculative attacks.

Our evidence is consistent with the hypothesis that currency crises spread because of trade linkages. That is, countries may be attacked because of the actions (or inaction) of their neighbors, who tend to be trading partners merely because of geographic proximity. This externality has important implications for policy. If this effect exists, it is a strong argument for international monitoring. A lower threshold for international and/or regional assistance is also warranted than would be the case if speculative attacks were solely the result of domestic factors.

Acknowledgements

We thank Priya Ghosh and Laura Haworth for research assistance. For comments, we thank the participants of the CEPR/World Bank Conference "Financial Crises: Contagion and Market Volatility", an anonymous referee, Joshua Aizenman, Gabriele Galati, Marcus Miller, Richard Portes, Javier Suarez, Mark Taylor, and especially David Vines. The views expressed do not represent those of the Federal Reserve Bank of San Francisco or the Board of Governors of the Federal Reserve System, or their staff. This is a condensed version of a paper with the same title, which is available as NBER Working Paper #6806 and CEPR DP No. 1947. A current (PDF) version of this paper and the (Excel 97 spreadsheet) data set used in the paper are available at http://haas.berkeley.edu/~arose.

[17] In the working paper version, we show that our result also shows up in the frameworks of both Corsetti et al. (1998b) and Tornell (1998).

We have also performed comparable analysis for the 1982 debt crisis, using Mexico as "ground zero". Our key result—a positive and statistically significant effect of trade linkages on the probability of crisis—characterizes this data set too. For instance, our benchmark regression (from Table 2) estimated on 1982 data delivers a trade coefficient which is positive and has a *z*-statistic of 1.98. This result seems reasonably robust. These positive results are perhaps surprising, given that our framework focuses on currency crises rather than debt crises.

Appendix A

See Table 7.

Table 7
Countries affected by speculative attacks

	1971	1973	1992	1994	1997
USA	1	1			
UK	1	1	1		
Austria	1	1			
Belgium	1	1	1		
Denmark	1	1	1		
France	1	1	1		
Germany	*	*			
Italy	1	1	1		
Netherlands	1	1			
Norway	1	1			
Sweden	1	1	1		
Switzerland	1	1			
Canada				1	
Japan		1			
Finland	1	1	*		
Greece	1	1			
Iceland		1			
Ireland	1		1		
Portugal	1	1	1		
Spain	1		1		
Australia	1	1			
New Zealand	1	1			
South Africa					1
Argentina				1	1
Brazil				1	1
Mexico				*	1
Peru				1	
Venezuela				1	
Taiwan					1
Hong Kong				1	1
Indonesia				1	1
Korea					1
Malaysia					1
Pakistan					1
Philippines				1	1
Singapore					1
Thailand				1	*
Vietnam					1
Czech Republic					1
Hungary				1	1
Poland					1

Here, "*" denotes "first victim"/"ground zero"; "1" denotes target of speculative attack.

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