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Moral Hazard and International Crisis Lending: A Test

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Abstract

We test for the existence of a moral hazard effect attributable to official crisis lending by analyzing the evolution of sovereign bond spreads in emerging markets before and after the Russian crisis. The nonbailout of Russia in August 1998 is interpreted as an event that decreased the perceived probability of future crisis lending to emerging markets. In the presence of moral hazard, such an event should raise not only the level of spreads, but also the sensitivity with which spreads reflect fundamentals as well as their cross-country dispersion. We find strong evidence for all three effects.

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I. Introduction

No subject in the debate on globalization and international institutions suffers from a greater disconnect between policy debate and empirical literature than the "moral hazard" supposedly caused by international official rescues. Ever since the Mexican (1995) bailout, the possibility that large-scale crisis lending might encourage excessive risk taking by investors and imprudent policies in debtor countries has been a constant charge of some IMF critics, and a source of concern to the official com3munity.[2] "Limiting moral hazard to the extent possible" has been an objective of IMF policies for some time now, as reflected in attempts to better "involve" the private sector in crisis resolution, and most recently in the proposal to establish a Sovereign Debt Restructuring Mechanism (SDRM) as an alternative approach to resolving debt crises.[3] However, no systematic empirical evidence has so far been presented suggesting that moral hazard associated with international crisis lending is, in fact, a problem or has been a problem in the past. Only three studies—by Zhang (1999), Lane and Phillips (2000), and recently Kamin (2002)—study this issue directly, and their conclusions are all negative, either rejecting the presence of moral hazard outright or finding weak and inconsistent effects.[4]

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[2]For a statement and discussion of the critics' view, see Calomiris (1998), Meltzer (1998), and Willett (1999). For an early official reform proposal that explicitly recognizes and is partly motivated by "moral hazard for both creditors and debtors", see Group of Ten (1996). These contributions are not, of course, the first statements of concern about moral hazard related to international official lending, which go back at least to Vaubel (1983).

[3]See "Report of the Managing Director to the International Monetary and Financial Committee on Progress in Strenghtening the Architecture of the International Financial System and Reform of the IMF", September 19, 2000; IMF (2000) and "IMF Executive Board Discusses Involving the Private Sector in the Resolution of Financial Crises", Public Information Notice (PIN) No. 00/80, International Monetary Fund, September 19, 2000 (all documents are available on the IMF's website, www.imf.org). On the SDRM, see Krueger (2001, 2002) and Rogoff and Zettelmeyer (2002).

[4]Although they do not directly test for moral hazard, several additional empirical papers are also worth mentioning in this context. Nunnenkamp (1999) argues that, based on the relatively modest scale of IMF lending, moral hazard is unlikely to be a serious problem. Brealey and Kaplanis (2002) study the impact of IMF-related announcements on a variety of asset prices in the country to which the announcements refers. Their main result is consistent with that of Lane and Phillips in that "good news" announcements tend to have no effect, while "bad news" announcements (of which there are far fewer) tend to have an effect. Eichengreen and Mody (2001) find that IMF programs, ceteris paribus, have a positive impact on the market access and a negative effect on the spreads of the country with the program. This effect is eliminated once a country has been in an IMF program for a number of years; a fact that makes it difficult to give their findings a moral hazard interpretation. Jeanne and Zettelmeyer (2001) do not attempt to test for IMF-related moral hazard, but present evidence showing that international crisis lending does not contain a significant subsidy and thus cannot generate moral hazard through that particular channel. McBrady and Seasholes (2000) examine the impact effect on bond spreads of the January 1999 Paris Club decision to extend its "comparability of treatment" principle to Eurobonds. They find a

These negative results are all the more surprising as the literature in this area does not test for moral hazard directly, but instead the much weaker hypothesis that expectations of IMF intervention reduce investor risk, as reflected by emerging market bond spreads. This is a necessary but not sufficient condition for IMF-led crisis loans to cause moral hazard in the sense that they have a palpable impact on investor behavior. It is even further removed from the IMF critics' claim that official loans cause *excessive* moral hazard, in the sense that the possible beneficial effects of lending to countries in crises are more than offset by adverse incentive effects. The finding that international crisis lending has no impact on bond spreads implies that official crisis loans have *neither* adverse incentive effects through reduced investor risk *nor* beneficial effects in terms of reducing the probability and/or overall economic costs of financial crises, since these should affect a country's ability to repay investors.

The present paper argues that the literature's failure to find any clear-cut effect of IMF bailouts on investor risk is mainly a result of the particular methodologies used. We propose an alternative approach, which focuses on an experiment well suited to studying the effects of a shift in expectations of international bailouts—the IMF's failure to bail out Russia in August of 1998—and presents a range of testable implications which were not exploited in the previous literature. Based on these tests, we find strong evidence for an effect of bailout expectations on investor risk, consistent with the presence of IMF-related moral hazard *prior* to the 1998 Russian crisis (in many emerging market countries, not just in Russia). This does not contradict a recent paper by Kamin (2002), who applies the tests proposed in this paper to argue that moral hazard *after* 1998 has not been a serious problem, but it does conflict with the main conclusions of Zhang (1999) and Lane and Phillips (2000).

Zhang (1999) analyzes the longer-term impact of the 1995 Mexican bailout, which constituted the first large-scale crisis loan of the 1990s. His approach is to regress emerging markets bond spreads on a number of macroeconomic fundamentals and a measure of international liquidity (namely, the spread of high-yield US corporate bonds) in a sample that includes observations before and after the bailout. His main result is that a post-Mexico dummy is insignificant and has a positive sign, contrary to what one would expect in the presence of moral hazard. However, this result is based on an event which arguably is not well suited to test for the existence of moral hazard. Widely viewed as the first of a new type of crises, the Mexican crisis probably led to a general reassessment of risks related to emerging market lending, as investors learned that even a country with a recent track record of reform and relatively sound fundamentals was vulnerable to a sudden capital flow reversal.[5] Consequently, any reduction in spreads due to moral hazard may have been offset by an increase in the perceived riskiness of emerging market debt. Zhang's paper

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large reaction, much as we do for the Russian crisis. However, their results do not directly speak to the issue of moral hazard attributable to official lending in the large capital account crises of the 1990s, none of which involved Paris club restructurings. In addition, their findings could be interpreted as reflecting a change in the perceived seniority of a particular creditor class of private creditors, rather than a general reduction in official guarantees.

[5] See Calvo and Mendoza (1996)

also has the problem that it restricts the regression coefficients before and after the Mexican crisis to be the same, notwithstanding the fact that it is precisely through changes in these coefficients that moral hazard, if it existed, should have effects on the level of spreads (see section II.B).

Lane and Phillips (2000) examine the short-term reactions of bond spreads to 22 events that might have changed expectations of future international crisis lending—namely announcements related to the 1995 Mexican bailout, the crises of 1997-98 and the 1998 IMF quota increase. With some exceptions— notably, the 1998 Russian default—these events fail to produce statistically significant reactions of spreads in the expected direction. The problem is that these findings have ambivalent interpretations, as Lane and Phillips themselves point out. Failure to detect a significant reaction of spreads could be due to the fact that changes in bailout expectations have no effect on investor risk, but it could also mean that the event was anticipated. As to the large reaction of (non-Russian) spreads to the Russian default, this could be attributed to a shift in bailout expectations, but it could also just reflect financial market turbulence caused by investor panic and contagion immediately after the default.

To deal with these problems, we adopt the following strategy. Like Zhang (1999), we examine the *long run*-behavior of emerging market debt spreads in the context of a regression model, controlling for changes in international interest rates as well as most country fundamentals that have been suggested in the literature on bond pricing.[6] This helps us disentangle the structural effects of perceptions regarding official lending from short-term changes in spreads attributable to market turbulence. Second, we concentrate on a highly unanticipated event—the August 1998 Russian "nonbailout"—which we argue below is much better suited to test the impact of changing bailout expectations on spreads than the 1995 Mexican crisis.[7] Third, we not only examine changes in the *levels* of spreads after the event, but also changes in the *sensitivity* with which spreads react to fundamentals[8], as well as changes in the cross-country *variance* of spreads. In the context of a simple model of international lending, these are shown to be testable implications of changes in investor risk attributable to bailout expectations (see section II.B).

Our main result is that the Russian crisis was followed by permanent, significant increases in the levels of spreads in many—but not all—countries studied, in particular in countries with relatively weak fundamentals. This indicates that the Russian non-bailout increased the perceived risk of emerging market debt, particulary for "weak" countries. Moreover, we find a permanent, large, and significant increase in the cross-sectional dispersion of spreads (controlling for fundamentals), indicating that investors paid more attention to differences in country characteristics after the crisis than they had done before. This is strong evidence for a risk-reduction effect of (expected) IMF interventions, which could reflect the presence of moral hazard prior to 1998. However, it is

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[6]See, in particular, Cline and Barnes (1997) and Eichengreen and Mody (2000).

[7]We also report the results of our tests when applied to the Mexican and Asian crises to compare our results to the earlier literature.

[8]In a different context, Kamin and Kleist (1999) carry out a similar test in a regression with only one risk factor (credit ratings).

also consistent with the view that the international financial safety net established between 1995 and 1998 made crises less likely or less deep, without necessarily causing moral hazard.[9] In that interpretation, spreads rose after 1998 because the perceived curtailing of this safety net made emerging market economies a riskier place, to the detriment of everyone. On the basis of our empirical evidence, one cannot distinguish between these two explanations. Therefore, our results should be interpreted as a confirming a necessary, but not sufficient condition for the presence of moral hazard.

The remainder of the paper is organized as follows. In the context of a simple model of international lending, Section II derives several alternative testable implications of the hypothesis that IMF interventions lower investor risk. Section III discusses the implementation of these tests in the context of an empirical model of spread determination, the validity of the Russian crisis as an "experiment" for our purposes, and our empirical methodology. Section IV presents our results, which are based on two distinct datasets: a dataset of launch bond spreads based on Capital Data's "Bondware" as well as J.P. Morgan's dataset of secondary market bond spreads contained in the "EMBI Global" Bond Index. Section V interprets the results and concludes.

II. A Simple Model

Suppose one had a clear-cut event affecting the perceived likelihood of future official crisis lending to emerging market economies. Then, it should be possible to use financial market reactions to such an event—such as changes in emerging market bond spreads—to test whether and how this event affects investor risk. Assuming that any such risk reduction is *not* primarily driven by a reduction in the probability or severity of emerging market crises themselves, this amounts to a test for investor moral hazard, in the sense that in the presence of official intervention investors are more likely to be bailed out when a crisis occurs. In this section, we develop three testable implications of this hypothesis in the context of a simple model of international lending. Methodological issues related to the implementation of these tests—in particular, the selection of a suitable event and the econometric modeling—are left to the next section.

A. Setup

Consider a world where multiple, risk-neutral lenders compete for loans in hard currency to debtor countries. For simplicity, we assume that debtor economies can only be in one of two states: either they suffer from a crisis or they do not. We assume that countries never repudiate their debt, but may default if they suffer a crisis. Then, country i's probability of default can be decomposed into the probability of a financial crisis in country i, θ_i, and the default probability *conditional* on a crisis, $(1-\lambda)$, where λ denotes the investors' "recovery rate", i.e., the probability of being repaid in a crisis. For the time being, this recovery rate is assumed to be identical across countries (this will be relaxed later). In contrast, we allow the probability of a crisis to vary as a function of a vector of observable country-specific fundamentals, $\mathbf{x}_i$, i.e., $\theta_i = \theta(\mathbf{x}_i)$.

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[9]This point is made by Lane and Phillips (2000).

Denoting the exogeneous gross risk-free interest rate as R^*, the ex ante gross lending rate is determined such that expected repayment equals the risk-free rate:

$$R_i = \frac{R^*}{1-(1-\lambda)\theta_i}.$$

The respective *spread* over the risk-free rate is then

$$s_i = R_i - R^* = R^* \cdot \frac{(1-\lambda)\theta_i}{1-(1-\lambda)\theta_i}. \tag{1}$$

We now introduce the possibility of international crisis lending as in Mexico in 1995, Korea in 1997 or Brazil in 1998 and 2002. Let b denote the perceived probability that a country will receive an international rescue package in the event of a crisis. For now, this probability is assumed to be the same for all countries. In general, the expectation of an international rescue package could affect emerging market spreads through three channels:

- It might affect observable fundamentals, e.g., through government policies: $\mathbf{x}_i = \mathbf{x}_i(b)$. Indirectly, this would also affect the crisis probability.
- It might *directly* affect the probability of a financial crisis, conditioning on fundamentals: $\theta = \theta(\mathbf{x}_i, b)$. For example, the presence of an international financial "safety net" might reduce the probability of runs on a country's debt or currency.
- It might affect the recovery rate in the event of a crisis: $\lambda = \lambda\,(b)$.

"Country moral hazard" usually refers to the first of these effects, i.e., the deterioration of the borrower country's policies in the face of a financial safety net. "Investor moral hazard" is typically identified with the last effect—an increase in the probability that investors will go scot-free in a crisis. This is the sense in which the term will be used in the discussion that follows, in spite of the fact that "investor moral hazard" really should refer to particular investor *actions,* such as an increase in risky lending or a reduction in monitoring, rather than an increase in the conditional repayment probability *per se*. However, in a standard set-up in which unobservable investor actions are explicitly modeled, an increase in the recovery rate would have precisely this effect, since it would insulate investors from the risk of a financial crisis, θ.

In the remainder of the paper we thus speak of investor moral hazard if international crisis lending increases the recovery rate *conditioning* on a financial crisis, i.e., the following property holds:

$$\frac{\partial \lambda\,(b)}{\partial b} > 0 \tag{2}$$

At first sight, this condition might appear to be inevitably satisfied. However, it is not true that international rescue packages invariably involve the bailout of private international investors. While the IMF traditionally did not lend to countries that were in default or arrears to their private creditors, it changed its practices in the mid-1980s, and in 1989 formally adopted a policy that explicitly allowed "lending into arrears". Thus, the extent to which investors make losses during crises that involve IMF intervention will depend on the particular case in question. Beginning with the 1997 Asian crises, the Fund has attempted to build measures into its programs that "involve" the private sector in crisis resolution. In practice, these have ranged from persuading banks to voluntarily extend credit lines to conditioning IMF support on debt or financial sector restructuring measures that involved substantial investor "haircuts".[10] Therefore, the question is whether in light of these policies, a higher probability of international financial rescues is perceived as increasing the probability of being bailed out in case of a financial crisis or not, and how strong this effect is.

In the tests that follow, we focus on investor moral hazard, abstracting from country moral hazard by taking $\mathbf{x}_i$ as given (in our empirical work, this means controlling for changes in $\mathbf{x}_i$). In addition, we will assume that θ does not depend on b, ruling out a direct effect of crisis lending on the probability of crises. As we will see below, this assumption is critical to interpret our tests as tests for moral hazard, as opposed to tests for an investor risk reduction effect that might be driven by the reduction of the likelihood (or severity) of financial crises, rather than an increase in the conditional recovery rate.

The central question is now how to test for investor moral hazard when λ is not directly observable.

B. Testable Implications of "Investor Moral Hazard"

Under the assumptions made in the previous subsection, spreads are determined as

$$s_i = R^* \cdot \frac{[1 - \lambda(b)] \cdot \theta(\mathbf{x}_i)}{1 - [1 - \lambda(b)] \cdot \theta(\mathbf{x}_i)}. \tag{3}$$

where $\mathbf{x}_i = (x_{i1}, ..x_{ij}, ..x_{ik})$. Based on this equation, we can now state three equivalent testable implications of investor moral hazard. For a given set of fundamentals, an increase (decrease) in the perceived likelihood of an international rescue

1. reduces (increases) the level of spreads for each country,
2. reduces (increases) the sensitivity of spreads with respect to changes in fundamentals, and
3. reduces (increases) the spread *difference* between any pair of countries (with initial spreads "close enough"), translating into a reduction (an increase) in the cross-country variance of spreads.

More formally, the first result can be written as

Proposition 1 *Holding constant the set of fundamentals* $\mathbf{X} = (\mathbf{x}_1', \mathbf{x}_2', ...\mathbf{x}_N')'$, *equation (3) implies that* $\frac{\partial \lambda}{\partial b} > 0$ *if and only if* $\frac{\partial s_i}{\partial b} < 0$ *for any country i.*

[10] See IMF(2000) and Cline (2002) for surveys of actual "private sector involvement" during the crises of the 1990s.

Proof: see Appendix.

An increase in the probability of rescue packages results in a lower perceived risk associated with international lending, thus reducing country spreads across the board, given fundamentals. Under the stated conditions, this directly provides a test for moral hazard: In the presence of moral hazard, events that increase the perceived probability of international rescue packages should result in lower spreads, when controlling for changes in fundamentals. We will refer to the test based on Proposition 1 as the "*level test*".

Assume that all fundamentals are defined such that θ_i is increasing in all the components of $\mathbf{x}_i$ (in other words, all fundamentals are expressed as "risk factors"). Then we can state our second result as follows:

Proposition 2 *Holding constant the set of fundamentals* $\mathbf{X} = (\mathbf{x}'_1, \mathbf{x}'_2, ...\mathbf{x}'_N)'$, *equation (3) implies that* $\frac{\partial\lambda}{\partial b} > 0$ *if and only if* $\frac{\partial^2 s_i}{\partial x_{ij}\partial b} < 0$ *for any country i and any country-specific fundamental* x_{ij}.

Proof: see Appendix.

From an investor's standpoint, a higher probability of getting off "scot-free" renders the idiosyncratic characteristics of each country less important, weakening the link between fundamentals and spreads (in the extreme, with $\lambda = 1$, all countries would pay the same risk-free interest rate, regardless of their fundamentals). This proposition provides a second test for investor moral hazard: In the presence of moral hazard, events that increase the perceived probability of international rescue packages should reduce the size of the *slope coefficients* linking country spreads and fundamentals. We will refer to this test as the "*slope test*".

Finally, define $\Delta s \equiv s_m - s_n, m \neq n$, where s_m and s_n are the interest rate spreads of two countries m and n. If s_m and s_n are "close enough" in the sense that Δs can be approximated reasonably well by a first-order Taylor expansion, we can prove the following proposition:

Proposition 3 *Holding constant the set of fundamentals* $\mathbf{X} = (\mathbf{x}'_1, \mathbf{x}'_2, ...\mathbf{x}'_N)'$, *equation (3) implies that* $\frac{\partial\lambda}{\partial b} > 0$ *if and only if* $\frac{\partial \Delta s}{\partial b} < 0$ *for any two countries* m *and* $n, m \neq n$ *for which we can approximate* $\Delta s \equiv s_m - s_n$ *by a first-order Taylor expansion.*

Proof: see Appendix.

This proposition shows that a higher probability of being bailed out reduces the spread *difference* between any pair of countries (with initial spreads "close enough"). This means that the higher bailout probability not only lowers the level of the spread as in proposition 1, but that the decrease is more pronounced for countries with higher spreads. Intuitively, as investors pay less attention to differences in fundamentals across countries, the differences between country spreads should also narrow. This further implies that, for any given set of fundamentals, the dispersion of spreads decreases when the probability of being bailed out increases.[11] Our test can then be formulated as follows: In the presence of moral hazard, events that increase the perceived probability of international rescue packages should reduce the cross-sectional variance of the spreads. We will refer to this test as the "*variance test*".

C. Robustness to Changes in Model Assumptions

If equation (3) holds, the propositions in this section all specify necessary and sufficient conditions for investor moral hazard, thus providing three alternative and equivalent testable implications. However, when the underlying assumptions are relaxed, the three conditions may cease to be sufficient and remain only necessary. At the same time, the equivalence among the three tests may cease to exist.

Consider first what happens if one allows for a beneficial role of international crisis lending in crisis prevention or mitigation. In our set-up this means allowing the probability of a financial crisis, θ_i, to depend on b, with $\frac{\partial\theta(\mathbf{x}_i,b)}{\partial b} < 0$. For example, a crisis-preventing effect may arise if international crisis lending eliminates self-fulfilling debt runs à la Sachs (1984), or provides the domestic authorities with the hard currency necessary to implement domestic financial safety nets and prevent bank runs triggered by shifts in exchange rate expectations (Jeanne and Wyplosz, 2001). One can show that in this case, the "level test" will never be able to distinguish the effects of investor moral hazard from those of a reduction in the crisis probability and that the "slope" and "variance tests" can make this distinction only under conditions that are not necessarily satisfied in practice. The inability to distinguish moral hazard from "true risk reduction" attributable to international crisis lending thus constitutes a fundamental identification problem which we share with the remaining literature in this area, as explained in the introduction.[12]

Another assumption made in the previous subsection is the invariance of the recovery rate across countries. This assumption is less critical, and can be relaxed by allowing λ to depend on $\mathbf{x}_i$ i.e., $\lambda = \lambda(\mathbf{x}_i, b)$.This formulation encompasses two cases that are likely play a role in practice. First,

[11]For this to be true, it is sufficient that the difference between all *neighbouring* spreads decreases.

[12]To the extent that international crisis lending has beneficial effects only via ruling out an inferior equilibrium in the context of multiple equilibria, this would help us deal with the identification problem, since risks of this kind are typically not priced (we are grateful to Giancarlo Corsetti for bringing this point to our attention). However, this does not entirely solve the problem for two reasons. First, recent models employing the global games methodology yield unique equilibria and thus well-defined crisis probabilities (Morris and Shin, 1999). To the extent that an IMF intervention makes runs less likely, this would be reflected in spreads. Second, the fact that in our model international crisis lending can have a beneficial role only through its effect on the probability of crises *ex ante* is merely a consequence of our assumption that all crises are of the same severity. If we allowed international lending to have a beneficial effect *ex post* (for example, by cushioning the capital account reversal and thus the output decline in a crisis), then the anticipation of official lending would have an effect on spreads even if it had no effect in terms of preventing the crisis.

the recovery rate could depend on country fundamentals directly, regardless of expectations of international crisis lending. For example, the efforts that a country makes to repay investors in a crisis (e.g., through fiscal adjustment) is likely to depend on observable fundamentals. Second, the likelihood of international official intervention in a crisis country will generally depend on country characteristics. For example, the international official community might be less prone to extend crisis loans to countries with chronically poor policies, and it may be more prone to extend crisis loans to large countries with systemic impact. In this interpretation, the parameter b would represent a general taste parameter of the IMF and its shareholders—its general propensity to engage in large-scale crisis lending—while the likelihood that a particular country will receive assistance will depend on the interplay of b and $\mathbf{x}_i$.

In the Appendix, we show that if $\lambda = \lambda(\mathbf{x}_i, b)$ all propositions go through, provided we impose two weaker conditions instead. The first states that an increase in b affects the expected recovery rate uniformly across countries, the second rules out the (pathological) case where a country with a "much smaller" crisis probability has a higher spread due to a "much smaller" recovery rate. Hence, the general ordering of spreads should depend on θ and should not be reversed by the ordering of the recovery rates.

III. Empirical Methodology

A. Regression-Based Tests for Moral Hazard

We start from a standard model of the determination of bond spreads

$$s_{it} = \mathbf{x}_{it}\beta + u_{it} \; , \tag{4}$$

where s_{it} denotes the bond spread of country i at time t, and $\mathbf{x}_{it}$ is a $1 \times k$-vector containing the country's fundamentals at time t that determine the spreads of sovereign bonds. These fundamentals can be country- and/or time-specific. The term u represents a random error. This equation will be the basis of all our regressions.

Consider now an event that *reduces* the perceived probability of future bailouts.[13] The general estimation procedure will be to estimate a pooled model over the whole period, i.e., before and after the event, without restricting the coefficients of the model to be the same before and after the event. For the ease of exposition, assume that there are only two points in time: "before" the event ($t = 0$) and "after" the event ($t = 1$).

Then, bond spreads before the event can be described by the model

$$s_{i0} = \mathbf{x}_{i0}\beta^0 + u_{i0}, \tag{5}$$

[13]Since we are looking at the unexpected *absence* of a further international rescue package for Russia in August of 1998, this is the relevant case for our empirical analysis.

while the model changes to

$$s_{i1} = \mathbf{x}_{i1}\beta^1 + u_{i1} \tag{6}$$

after the event due to a potential structural break. Denoting H_0 the null hypothesis that moral hazard is not present, and H_1 the presence of moral hazard, the three tests derived in our theoretical framework can be restated as follows in the context of the empirical model:[14]

1. Under H_0 (i.e., no moral hazard), the slopes of the regression equation should be unaffected by an event that reduces expected international crisis lending. Under H_1 (i.e., moral hazard), however, we would expect all slopes to increase after the event (in absolute value) because investors bear a larger part of the repayment risk and will price risk factors more than before. This is the test referred to as the *slope test* in section 2. It can be carried out as a simple t test on the significance of the change of each individual slope.[15] In the case of an event that decreases moral hazard, the test can be formulated as follows:

$$\begin{aligned} H_0 &: |\beta_k^1 - \beta_k^0| = 0, \quad k = 1, ...K \\ H_1 &: |\beta_k^1 - \beta_k^0| > 0, \quad k = 1, ...K \end{aligned}$$

   Note that this test refers only to the slopes of the regression and not to the intercept.[16]

2. Under H_0, the *level* of spreads should not be affected by an event that reduces expected international crisis lending. Under H_1, however, the level of spreads should increase for every country, holding fundamentals constant. More formally, the change in the level of spreads can be decomposed into three components:

$$\begin{aligned} & s_{i1} - s_{i0} \\ &= \mathbf{x}_{i1}(\beta^1 - \beta^0) + (\mathbf{x}_{i1} - \mathbf{x}_{i0})\beta^0 + (u_{i1} - u_{i0}) \\ &= \mathbf{x}_{i0}(\beta^1 - \beta^0) + (\mathbf{x}_{i1} - \mathbf{x}_{i0})\beta^1 + (u_{i1} - u_{i0}) \end{aligned} \tag{7}$$

   The first term is the change in the level of spreads induced by the change in β, the second term the change in the level of spreads caused by the change in the fundamentals, and the third term reflects the impact of a change in the error term.[17] Here, we are only interested in the first term, which captures the effect of a change in the pricing of risks on the level of spreads. Thus, in the case where the event entails a potential decrease in moral hazard, the *level test* takes the

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[14]As has been discussed above, we have to assume that the expectation of reduced future crisis lending has no risk-increasing effect, given fundamentals. Without this assumption, all tests have to be reinterpreted as tests of the *joint* hypothesis that either moral hazard or a true risk-increasing effect is present, or both.

[15]A similar test can be found in the paper by Kamin and Kleist (1999).

[16]Our model predicts that the "theoretical intercept", i.e. the spread at $\theta = 0$ is equal to zero irrespective of the occurrence of international bail-outs. The intercept of our regression, however, is not identical to this theoretical intercept. Therefore, the implications for the "empirical intercept" are not obvious.

[17]This is the well-known Oaxaca decomposition that has also been used by Eichengreen and Mody (2000).

following form:

$$\begin{aligned} H_0 &: \quad \mathbf{x}_{it}(\beta^1 - \beta^0) = 0 \\ H_1 &: \quad \mathbf{x}_{it}(\beta^1 - \beta^0) > 0 \end{aligned}$$

The test can be carried out as a linear Wald test in which we compare the fitted spreads that result from the models estimated before and after the event.[18] Note that the above decomposition and thus the choice of x_{it} is not unique: when controlling for fundamentals, one can either use the fundamentals before or after the event. In fact, this choice can affect the results of the test. Therefore, we present the results for both choices.

3. Under H_0, the cross-sectional variance of the spreads should remain unchanged after the event. Under H_1, however, the difference in spreads between each pair of countries should increase, which, in turn, implies an increase in the cross-sectional variance of spreads (controlling for changes in fundamentals). More formally, we can write the variance *across countries* before the event as

$$Var(s_0) = \beta^{0\prime}Var(\mathbf{X}_0)\beta^0 + \sigma_0^2 \tag{8}$$

and the variance after the event as

$$Var(s_1) = \beta^{1\prime}Var(\mathbf{X}_1)\beta^1 + \sigma_1^2, \tag{9}$$

where $\mathbf{X}_t$ is the $N \times k$-matrix of the fundamentals of all countries at date t and σ_0^2 and σ_1^2 are the variances of the error terms. The change in the variance of spreads can be decomposed into three components:

$$\begin{aligned} & Var(s_1) - Var(s_0) \\ &= \left[\beta^{1\prime}Var(\mathbf{X}_1)\beta^1 - \beta^{0\prime}Var(\mathbf{X}_1)\beta^0\right] + \\ & \left[\beta^{0\prime}Var(\mathbf{X}_1)\beta^0 - \beta^{0\prime}Var(\mathbf{X}_0)\beta^0\right] + \left[\sigma_1^2 - \sigma_0^2\right] \\ &= \left[\beta^{1\prime}Var(\mathbf{X}_0)\beta^1 - \beta^{0\prime}Var(\mathbf{X}_0)\beta^0\right] + \\ & \left[\beta^{1\prime}Var(\mathbf{X}_1)\beta^1 - \beta^{1\prime}Var(\mathbf{X}_0)\beta^1\right] + \left[\sigma_1^2 - \sigma_0^2\right] \end{aligned} \tag{10}$$

The first term is the change in the variance induced by the change in β, the second term the change in the variance caused by the change in the fundamentals, and the third term reflects the impact of a change in the variance of the error term.[19] Again, we are mainly interested in the first term, which captures the effect of a change in the pricing of risks on the variance of spreads. Thus, if the event entails a potential decrease in moral hazard, the variance test takes

[18] This test is very different from the one employed by Zhang (1999) who allows only the intercept to change after the event. Thus, Zhang *assumes* that the coefficients on fundamentals are unchanged before and after the event, which would not be true if moral hazard were in fact present (see Section 2).

[19] This type of decomposition is known in the labor literature on the evolution of the distribution of incomes over time.

the following form:[20]

$$\begin{aligned} H_0 &: \quad \beta^{1\prime} Var(\mathbf{X}_t)\beta^1 = \beta^{0\prime} Var(\mathbf{X}_t)\beta^0 \\ H_1 &: \quad \beta^{1\prime} Var(\mathbf{X}_t)\beta^1 > \beta^{0\prime} Var(\mathbf{X}_t)\beta^0 \end{aligned}$$

We refer to this test as the *variance test*. The variance test will be carried out as a nonlinear Wald test (see Appendix for statistical details). Note that the above decomposition is again not unique: the choice of $\mathbf{X}_t$ can affect the results of the test, and we report the results for both alternatives.

It is important to clarify the relations between our three tests. If all slopes increase in absolute value, the variance is also going to increase and so are the levels (unless there is a decrease in the intercept strong enough to reverse the effect of the slopes). Thus, there is no point in doing all three tests in this situation. The interesting case is one in which some, but not all slopes show significant increases, while some may even show decreases. In the slope test, this would imply a rejection of H_0, which predicted no change in slopes. However, this rejection would not be very convincing if the increase in some slopes were accompanied by decreases in others. Indeed, H_1 predicts that *all* slopes should increase. The question is whether the slope coefficients showing significant increases "outweigh" those showing decreases, so that we can accept the presence of moral hazard with some confidence instead of concluding, for example, that the regression model is misspecified or the experimental event is ambiguous, so that no lessons can be drawn.

How should one decide whether the positive slope movements outweigh the negative ones? One natural way of weighing the slopes is to look at the impact of the change of the slopes on fitted spreads, controlling for fundamentals. This is the logic behind the level test. Unfortunately, the results from this test also are very unlikely to be unambiguous. First, the results from this test may differ across countries and second, the choice of $\mathbf{X}$ may affect the test results. Therefore, we also employ the variance test, which allows us to summarize the overall effect of the changing slopes on *all* countries in a way suggested by our model.

Some caveats remain with respect to the interpretation of the variance test. First, there continues to be an ambiguity with respect to the choice of $\mathbf{X}$. Second, it is important to note that a rejection of the null hypothesis in the variance test does not require all fitted spreads to go up. For the variance test, the direction of the change in fitted spread is irrelevant as long as the spreads move farther apart from each other. Third, our theoretical model predicts that the increase in the variance is driven by an increase in the distance of neighbouring spreads, with the "order" of countries being unchanged. Yet, the order of countries does not enter the variance test. Therefore, the results from the variance test can only be interpreted in combination with the results from the level and slope tests.

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[20]Since the variance test employs the variance of *fitted* spreads, it should not be affected by a change in the volatility of spreads after the event, as long as we use heteroskedasticity-consistent standard errors.

B. The Russian Crisis as a Valid Experiment

A critical element of our testing strategy for moral hazard is the choice of an event that constitutes a valid experiment for the purpose of the test. Such an event has to satisfy three conditions:

1. It has to change investors' perception of the extent or the character of future international crisis lending.
2. It has to be unexpected.
3. It must not lead to a reassessment of risks other than through the expectations of future international rescues.

Arguably, the events following the Russian default in August 1998 satisfy all three conditions reasonably well. The Russian crisis unfolded when the Russian authorities announced a de facto devaluation of the ruble, a unilateral restructuring of ruble-denominated public debt, and a moratorium on foreign debt repayments on August 17, 1998. In our judgement, the poor state of the Russian economy was hardly surprising. In fact, Russia had been downgraded by all three major rating agencies in the first half of 1998, which suggests that investors were well aware of the increasing economic risks.

The real surprise was that the international community did not prevent the default of a country that was widely believed to be "too big and too nuclear to fail", as witnessed by the enormous build-up of Eurobonds outstanding—from $4.6 billion in March 1998 to $15.9 billion in July 1998—and the oversubscription of all new issues, in spite of worsening fundamentals.[21] As a result, the absence of international support during the Russian plight was widely interpreted as a sign of a generally higher reluctance of the international community to support crisis countries, particularly if these countries had not complied with former reform programs. In the words of David Folkerts-Landau, Global Head of Research at Deutsche Bank and former head of capital market studies at the IMF: "The rules of the game have changed... If a country has a significant volume of domestic debt outstanding, if that country is forced into the arms of the IMF... I believe that we should assume from here on that any such program will ask the foreign holders of domestic debt to take a major loss... Clearly, one had the right to be surprised in Russia and face a write-down there." Similarly, George Soros is quoted to have stated after the Russian crisis that "[Financial markets] ... resent any kind of government interference but they hold a belief deep down that if conditions get really rough the authorities will step in. This belief has now been shaken."[22] On this basis, the first two of the above conditions would seem to be satisfied.

The third condition is harder to satisfy. There is at least one interpretation of the events in

[21]Therefore, the existence of a moral hazard problem associated with international lending to Russia seems undisputable. Indeed, this is recognized almost universally and is not the subject of our study. The important question is whether the events in Russia led investors to revise their expectations about the riskiness of investments in emerging markets *other* than Russia.

[22]Quoted in Blustein (2001), pp. 276 and 277, respectively.

Russia that has nothing to do with expectations of international bailouts, namely that the crisis "reminded" investors of the risks existing in emerging economies, which led to a general repricing of risks (the "wake-up call" interpretation).[23] This argument, which is surely valid in the case of the Mexican and the Asian crises, seems less credible for the Russian crisis. First of all, the two preceding emerging market crises (Tequila, and particularly Asia) should have been sufficient to "wake up" investors. Second, it is not clear that the Russian default, which resulted from an old-fashioned fiscal sustainability problem, contained any information with respect to the risks in other emerging economies.[24] We therefore believe that the Russian crisis did not primarily change investors' evaluation of country risk, but rather their perception about the extent and nature of the international financial safety net.

C. Estimation Strategy

In applying our tests to the Russian crisis, a complication arises from the fact that the Russian crisis was followed by a prolonged period of turbulence in emerging markets. During this high-volatility episode, one cannot reasonably suppose that there was a stable relationship between macroeconomic fundamentals and bond spreads, as is assumed in the static models that are estimated in the literature on emerging market bond spreads.[25] Ignoring this problem—i.e., estimating the relationship between spreads and fundamentals before and after the default using a sample that includes the post-default turbulence—will bias our results in the direction of rejecting the null hypothesis, as both levels and the cross-sectional variance of spreads sharply rose in the immediate aftermath of the default, before returning to more normal levels.

There are two alternative ways to deal with this problem. One is to simply exclude the periods immediately following the crisis from our regressions. For example, one could exclude the second half of 1998 and perhaps the first quarter of 1999, until markets calmed down after the Brazilian currency crisis in early 1999. An alternative approach is to have no exclusion period, but estimate the model using a specification and/or estimation procedure which is be able to deal with the presence of financial turbulence in the data. For example, one could use a flexible dynamic specification which allows for several lags in the dependent variable, and/or a GARCH process in the residuals. One could even give up on trying to model the average dynamic of emerging market bond spreads altogether, by including an average index of spreads, such as the EMBI Global (EMBIG), on the right hand side of the regression. This amounts to modeling the cross-sectional deviations of individual country spreads from the EMBIG as opposed to

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[23]In the literature, one also finds the informal argument that the losses sustained in the Russian crisis might have led to a general reduction in the risk tolerance of investors (the "appetite for risk" interpretation). However, we are not aware of a model formalizing this kind of reasoning.

[24]The best alternative interpretation that we have heard so far is that the events in the aftermath of the Russian crisis, in particular the LTCM crisis, revealed the high degree of interdependencies of financial markets in the world.

[25]See Cantor and Packer (1996), Kamin and Kleist (1999), Cline and Barnes (1997), Eichengreen and Mody (2000).

country spreads themselves. If we do this, our "level test" would no longer apply in the form presented above, since the post-crisis model is estimated taking the average increase in spreads as a given. However, our "slope test" and "variance test" would remain valid, and have the same interpretation as before.

The downside of the first approach is that we could bias the results by getting the exclusion period wrong. For example, if our choice of exclusion period is guided by actual crisis events, but market volatility persisted significantly beyond these events, we would have a problem. More generally, we obviously do not want our results to depend on a particular choice of exclusion period. The downside of the second approach is that modeling the extreme swings in spreads witnessed after the Russian crisis requires a lot flexibility. If we use a model that is too restrictive, we might still bias our results in the direction of rejecting the null. In addition, there is the usual trade-off between flexibility (or unbiasedness), and efficiency. We could make our model very flexible by including many dynamic terms and controls, but given that we have a relatively small number of countries and time periods, this might come at the expense of not being able to estimate any parameter with reasonable precision.

Faced with these options and trade-offs, our strategy is as follows. We make the first approach—the one that excludes the period of financial turbulence, i.e., estimates the model using pre- and post- default "tranquil" periods—our primary vehicle. In addition, we use several variants of the second approach to test the robustness of our results. Moreover, we explore whether the results are sensitive to the precise definition of the exclusion period, and in particular whether a larger exclusion window weakens our main finding to any significant degree. This is not the case (see section IV.E).

Using a simple measure of financial turbulence, it is easy to see why the results turn out to be quite insensitive to the choice of the exclusion period. Figure 1 graphs the predicted conditional variance of changes in the EMBIG, using a simple GARCH(1,1) model estimated over the period January 1998 until August 2002, using daily data.[26] The main lesson from the Figure is that periods of high market volatility literally stand out; they are easy to identify and to relate to reported events. The figure also shows that by March of 1999, conditional volatility was essentially down to pre-August 1998 levels. So even though conditional volatility is indeed persistent, the persistence does not seem so large as to influence volatility much beyond the crisis events, and whether one ends the exclusion period in February, March, or June of 1999 has no impact on the results.

[26]The equation that is being estimated is a first order autoregression of first differences in the EMBIG, allowing the error term to follow a GARCH(1,1) process. As expected, the conditional variance is quite persistent: the coefficient on the ARCH term is about 0.3 and that on the GARCH term is about 0.7. The results are very close if one estimates the model without the autoregressive term, and if one includes an ARCH-in means to term in the main equation.

IV. Empirical Analysis

A. Data

In our analysis, we use two different data sources for bond spreads: launch spreads contained in Capital Data's "Bondware" dataset and secondary-market spreads included in J.P. Morgan's Emerging Markets Global Bond Index (EMBI Global). Since both datasets have their strengths and weaknesses, we use both of them in our empirical analysis in order to check the robustness of our results.

The use of the EMBI Global dataset is more straightforward since it is a balanced panel of *secondary* market spreads. While its predecessors (EMBI, EMBI+) have been used extensively in the academic literature on emerging market bond spreads (Cline and Barnes, 1997, Zhang, 1999, Lane and Phillips, 2000), the much broader—albeit shorter—EMBI Global does not appear to have been used so far. It is made up of US-$ denominated sovereign or "quasi-sovereign"[27] bonds that satisfy certain criteria, guaranteeing, e.g., a sufficient liquidity of the bonds. Spreads are available at daily frequency for 21 countries since January 1, 1998.[28] The instruments in the index are mainly Brady bonds and Eurobonds, but the index also contains a small number of traded loans as well as local market instruments. The spread of a bond is calculated as the difference between the bond's yield and the yield of a US government bond with a comparable issue date and maturity. A country's bond spread is then calculated as a weighted average of the spreads of all bonds, that satisfy the above-mentioned criteria, where the weighting is done according to market capitalization. In the case of Brady bonds, "stripped" spreads are provided.

Capital Data's "Bondware" dataset contains launch spreads of sovereign and public[29] foreign currency bonds of 54 emerging countries. The spread of a bond is calculated as the difference between the bond's yield and the yield of a government bond of the country issuing the respective currency with a similar issue date and maturity. In contrast to the EMBI Global, the Bondware dataset does not include Brady bonds. Therefore, the two datasets are almost disjoint. The use of the Bondware dataset is more complicated, since it contains *primary* spreads that are observed only at the time of issue. Thus, this dataset is a highly unbalanced panel, which raises additional econometric problems due to a potential selection bias (see Eichengreen and Mody, 2000). However, "Bondware" has an important advantage over the EMBI Global dataset, namely its much broader coverage of countries. This property is crucial since one of our tests (the variance test) relies on asymptotic results in the cross-sectional dimension. The selection problem can be tackled by estimating a standard Heckman correction model (see below).

On the right hand side of the regressions, we use a rich set of macroeconomic fundamentals that have been compiled from a number of different sources (see Appendix for a complete list of the

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[27]"Quasi-sovereign" means that the bond is either guaranteed by a sovereign or that the sovereign is the majority shareholder of the respective corporation.

[28]A number of additional countries joined the database in later periods.

[29]Public means that the public ownership of the respective corporation is higher than 50%.

variables and their sources). In choosing the set of right-hand-side variables, we tried to capture the most important aspects of a country's macroeconomic performance, using the fundamentals that have been suggested in the literature on bond pricing.[30] The economic variables can be grouped into the following categories: Domestic economic condition (real GDP growth, inflation, fiscal balance, domestic credit growth), external sector (current account, external debt), and international interest rates (US ten-year yield and spreads on high-yield U.S. corporate bonds as a liquidity proxy). In addition, we included political variables (political instability and violence), other country characteristics (regional dummies, economic size), and credit ratings.

In the literature on bond pricing, it has been suggested that it is sufficient to include credit ratings to capture the macroeconomic performance of a country (Cantor and Packer, 1996, Kamin and Kleist, 1999). This is contradicted by the fact that one usually finds a large number of significant macroeconomic variables even when ratings are included. Conversely, the inclusion of ratings has been shown to be crucial even when macroeconomic fundamentals are included (Cantor and Packer, 1996, Eichengreen and Mody, 2000). We therefore include both macroeconomic fundamentals and the rating information. We follow Eichengreen and Mody (2000) in including not the ratings themselves, but rather a residual from a regression of the ratings on all included macroeconomic fundamentals. This assumes that the correlation between the included fundamentals and the ratings is entirely due to the fact that the ratings have been calculated on the basis of these fundamentals. The residual impact of the ratings might be due to either other omitted macroeconomic fundamentals that are used in the calculation of ratings or to the ratings themselves.

In the regressions based on the EMBI Global dataset, we use the whole range of right-hand-side variables, while the regressions using the Bondware dataset use a much more parsimonious specification to avoid the exclusion of too many countries from the dataset due to missing data on the right hand side.

B. Spreads Before and After the Russian Crisis: A First Impression

Before we start our formal econometric analysis, it is useful to have a look at the raw bond spread data. Figure 2 shows the evolution of daily bond spreads for the emerging market countries contained in JP Morgan's EMBI Global index (EMBIG).[31] The basic pattern is well-known: in August 1998, virtually all spreads shot up, and their cross-sectional variance widened sharply. By April of 1999, however, most of them—with the exceptions of Russia and Ecuador—seem to have returned to their approximate pre-crisis levels. From Figure 2, it is thus not obvious that the Russian crisis was followed by a permanent increase in the cross-sectional mean and variance

[30]E.g., Cline and Barnes (1997) and Eichengreen and Mody (2000).

[31]The graph shows the spreads for all countries for which data existed at the inception of the index (i.e., since January of 1998), except for Nigeria, which is not used in our analysis due to gaps in the right-hand-side data used for the regressions. The countries are: Argentina, Bulgaria, Brazil, China, Colombia, Croatia, Ecuador, Korea, Malaysia, Mexico, Morocco, Panama, Peru, Philippines, Poland, Russia, South Africa, Thailand, Turkey, and Venezuela.

of spreads. However, a much clearer impression emerges once Russia and Ecuador (which had idiosyncratic difficulties in 1999 and 2000) are removed from the sample (Figure 3). Now, the cross-sectional variance of spreads appears to be clearly larger in the post-crisis period and so does the average level of spreads.

These impressions are confirmed by Table 1 (left column), which shows the cross-sectional mean and standard deviation of spreads, based on monthly data, for the pre-crisis, crisis, and post-crisis periods. After the crisis, the mean spread rises by about 100 basis points and the average standard deviation approximately doubles (excluding Russia and Ecuador).

The evolution of launch spreads contained in the "Bondware" database is not as easily graphed, since the data consist of single datapoints for each issue, rather than continuous country-specific lines. Moreover, the selection problem makes the raw data more difficult to interpret. For example, the average level of spreads after the Russian crisis is biased downward by the fact that Russia drops out as an issuer. Nevertheless, after excluding Russia from the sample, the raw data confirm the pattern suggested by the EMBIG spreads (right column of Table 1).[32] In particular, both the cross-sectional average and the cross-sectional standard deviation of spreads remain at substantially higher levels in the post-crisis period than prior to the Russian crisis.

The crucial question is now to what extent these changes are attributable to changes in fundamentals, and whether these changes are statistically significant when controlling for changes in fundamentals.

C. Tests Using Bondware Data

Econometric issues

As Eichengreen and Mody (2000) have pointed out, ordinary-least-squares estimates of the relationship between *launch* bond spreads and fundamentals suffer from a selection bias: a country's spread is observed only when the country actually issues a bond. It is very likely that the issue decision depends on factors that influence the level of the spread as well. For instance, we might think that countries with extremely high (latent) spreads are excluded from the market due to adverse selection issues.[33] Therefore, the observability of the spreads cannot be considered as "random", but it depends on the spreads themselves, which has to be taken into account in the econometric analysis.

We follow Eichengreen and Mody (2000) in solving this problem by estimating a standard sample selection model in the spirit of Heckman (1979). Our econometric model thus consists of two equations. The first equation is the spread equation

$$\widetilde{s}_{it} = \mathbf{x}_{it}\beta + u_{it}, \tag{11}$$

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[32] A graphical representation of the Bondware data is available from the authors upon request.

[33] This type of argument can be found in the model on credit rationing by Stiglitz and Weiss (1981).

where $\widetilde{s}$ denotes the latent spread, which is unobserved. Instead, we do observe the actual spread, s, according to the following observation rule:

$$\begin{aligned} s_{it} &= \widetilde{s}_{it} \quad if \quad \widetilde{z}_{it} > 0 \\ s_{it} &= \text{not observed} \quad if \quad \widetilde{z}_{it} \leq 0, \end{aligned} \tag{12}$$

where $\widetilde{z}$ is another latent variable, which is also unobserved. The relationship between this latent variable and the observed country characteristics is described by the selection equation

$$\widetilde{z}_{it} = \mathbf{w}_{it}\gamma + v_{it}. \tag{13}$$

However, instead of $\widetilde{z}$ we observe z and the corresponding observation rule can be written as

$$\begin{aligned} z_{it} &= 1 \quad if \quad \widetilde{z}_{it} > 0 \\ z_{it} &= 0 \quad if \quad \widetilde{z}_{it} \leq 0. \end{aligned} \tag{14}$$

The variable z is a dummy variable indicating whether there was a bond issue in a certain period or not. As usual, we assume that the two errors are jointly normal, with ρ denoting the correlation between u and v. In our case, we would expect ρ to be negative. The matrix $\mathbf{W}_t = (\mathbf{w}'_{1t}, \mathbf{w}'_{2t}, ...\mathbf{w}'_{Nt})'$ includes all variables contained in the matrix $\mathbf{X}_t$ and a number of instruments needed for identification. In order to qualify as instruments, these variables must affect the issue decision, but not the level of the spread (unless we want to rely on functional form identification). We use four such variables in our selection equation:

- *Debt issued* in the form of bonds in the year preceding the observation divided by the debt stock at the beginning of that period. This variable captures the effect that countries are less likely to issue new bonds if they have issued large amounts of debt in the near past.
- The *number of bond issues* in the year preceding the observation, as a proxy for the degree of a country's issuing activities. A country that issued a large number of bonds in the past year is more likely to issue a bond in the next month than a country that issued only one or two bonds that year.
- The *natural logarithm of per capita GDP* in 1993, as a proxy for the economic development of a country. A country with higher per capita GDP typically has a more developed financial sector, increasing the probability of bond issues.
- A dummy variable that is equal to one for the five countries affected most by the Asian crisis.[34] The idea is that the Asian countries might have been excluded from capital markets after the Asian crisis regardless of their fundamentals.

The set of macroeconomic fundamentals in the spread equation was chosen such that we capture the most relevant macroeconomic risk factors, while trying to retain a large number of countries. As mentioned above, the wide coverage of countries is the strength of the Bondware dataset, which is of particular importance for the variance test. Therefore, the right-hand side of the regressions using launch spreads is somewhat more restricted than in the EMBI regressions to

[34] Thailand, Indonesia, Korea, Malaysia, Philippines.

avoid an undue reduction in the number of countries included in the estimation. The estimation is done by full maximum likelihood, which is preferable to Heckman's two-step procedure due to its asymptotic efficiency.

Test results for the Russian crisis

Table 2 contains pre- and post-crisis regression results for the Russian crisis and the results from the slope test described above. We show results for three different specifications, which are inspired by the previous literature relating spreads to fundamentals. Model (1) is a specification similar to the one found in the paper by Eichengreen and Mody (2000).[35] Models (2) and (3) are variants of model (1), which drop the variable "External debt/GDP" because it turned out to have the "wrong" sign in model (1). Instead they include additional variables such as inflation, the current account, a measure of political stability ("Political instability and violence"), and a measure of the maturity structure of external debt ("Short-term debt/total debt"). Note that all macroeconomic variables enter the regressions in a way that takes into account reporting lags. This usually means using the first lag rather than the contemporaneous realization. In some cases, we used moving averages to reflect the fact that past trends rather than the latest realization might affect investors; these are denoted as "MA" in the tables. For the reasons outlined above, we excluded the time period between July 1998 and March 1999 from our regressions.

The upper panel of the table shows coefficients and p-values for the spread equation, based on regressions which were run on a pooled pre- and post-crisis sample, with all variables in the main equation being interacted with pre-and post-crisis dummies. For each model, the column "test for equality" indicates the p-values of the tests whether the coefficients from the pre- and post-crisis samples are significantly different from each other. Rejections at the 10 percent level are typed in boldface if the change is in the direction predicted by H_1. The lower panel of the table refers to the estimation results for the selection equation. Only the coefficients of the four variables used for identification are reported, while the other coefficients are suppressed because they are of little interest. The coefficient ρ denotes the estimated correlation of the disturbance terms of the two equations.

Looking first at the selection equation (lower panel), we find that the variables "Number of previous bond issues" and "GDP per capita (1993)" are highly significant and show the expected signs. The maximum-likelihood procedure converged after only 2 or 3 iterations, which supports our identification procedure. However, the coefficient ρ is not significantly different from zero.

[35]The main differences are as follows: We chose different instruments for the selection equation because we consider our way of identification to be more credible. Moreover, we only use public bonds, while Eichengreen and Mody also use private bonds. Finally, Eichengreen and Mody estimate their model in semi-logarithmic form, with the spread (but not all of the right-hand-side variables) expressed in logs. This makes little difference to the substance of our regression results, but does not allow us to perform the variance test, since our model does not make any predictions about the variance of log spreads. Thus our models are all estimated using spreads rather than log spreads.

This suggests that the selection problem is less severe in this sample than expected. Therefore, we also ran the regressions without a Heckman correction. The results for the spread equation are very similar and are thus not reported.[36]

The coefficients in the spread equations mostly show the expected signs and are generally highly significant for the period after the crisis, while the same is not true for the period before the crisis. This may be due to the relatively small number of observations in that period.[37] The results from the slope test lend some support to the moral-hazard hypothesis, but are not perfectly conclusive. Almost all coefficients change in the direction predicted by H_1, but only some of these changes are statistically significant. In particular, the null hypothesis of equal slopes can be rejected at a 10 percent significance level for the rating residual, inflation, and "Political instability and violence". For GDP growth, the null can be rejected in models (1) and (2), but not in model (3). The null hypothesis cannot be rejected for the current account and the Brady dummy, and the results for the debt variables are difficult to interpret due to their wrong signs.

Another interesting result concerns the coefficient of the US high-yield bond spread, which is positive, but insignificant in the period before the crisis, but becomes negative and highly significant after the crisis. It has often been claimed that the evolution of spreads after the Russian crisis can be explained by a general reluctance of investors to take risks, which would suggest a positive correlation of high-yield bond spreads and emerging market spreads. Our analysis shows, however, that the partial correlation is negative after the crisis, once one controls for other macroeconomic fundamentals. This contradicts the conventional wisdom.

Consider now the level test, which is particularly instructive in view of the somewhat ambiguous results from the slope test (see Table 3). This test tells us whether the overall effect of the changes in coefficients observed in Table 2 is to increase spreads, as one would expect if the driving force behind those changes were moral hazard. We performed the level test for each country, for each month, and for all three models. Table 3 shows the number of significant increases and decreases of fitted spreads for each country and model (out of a potential maximum of 27, which is the number of months in our regression sample).

Table 3 contains several noteworthy findings. First, the overall evidence strongly supports the notion that spreads increased significantly after the Russian crisis (controlling for fundamentals) as predicted under the moral hazard hypothesis H_1. There are many significant increases in fitted spreads, while there are no significant decreases. Second, this finding does not apply equally to all countries. Specifically, there exist eight countries for which we do not find a significant increase in any of the three models. Interestingly, five of these are Asian countries which—with the exception

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[36]In contrast, the coefficient ρ is negative and significant in the regressions on the Mexican and Asian crises (cf. Table A1).

[37]The external debt variables ("External debt / total debt" and "Short-term debt / total debt") often show wrong signs, which might be due to a simultaneity problem, as foreign debt tends to flow to countries with relatively good fundamentals and, hence, low spreads. This problem is familiar in this type of bond spread regressions.

of Malaysia—did not directly suffer a crisis during 1997-98. It might well be that these countries experienced an overshooting of their spreads after the Asian crisis erupted. The normalization of spreads after 1998 might mask any moral hazard effect. It should also be noted that the average credit rating of countries without a significant increase (between A, A2 and A-, A3) was well above the one of the remaining countries (BB+, Ba1). The third noteworthy finding concerns the level of the increase in fitted spreads. There is a strong and significant negative correlation (-0.62) between the increase in spreads and the countries' ratings. In other words, the increase in spreads was higher in countries with worse ratings, which is in line with the moral hazard hypothesis. Summing up, the results from Table 3 are consistent with the moral-hazard interpretation.

Finally, Table 4 presents the results of the variance test, which focuses on the implications of moral hazard on the cross-sectional dispersion of spreads rather than the level of spreads for each country. For each model and each time period, the table compares the variance of fitted spreads using the coefficients from the model estimated on pre-crisis data, with the one based on the model estimated on post-crisis data. The column "test for equality" shows the p-values from the variance test, i.e., it shows whether the two fitted variances are significantly different from each other or not. The results are striking: no matter which period is chosen to calculate the fitted variances, the null hypothesis of equal variances is rejected at high confidence levels. The post-crisis model always significantly overpredicts the pre-crisis variance, while the pre-crisis model always significantly underpredicts the post-crisis variance. This constitutes strong evidence for a stronger differentiation between countries after the Russian crisis, confirming the impression one first obtains on the basis of the raw data. In combination with the results from the level test, these results can be interpreted as strong evidence consistent with the moral-hazard hypothesis. Not only do we find that the cross-sectional variance increases after the event, but we also find that the increase in spreads is strongest for countries with weak fundamentals. Thus, there seems to be a much stronger differentiation between "good" and "bad" countries following the Russian crisis.

Test results for the Mexican and Asian crises

We now discuss the results from applying our test procedures to the Mexican and Asian crises. Appendix Tables 9 to 11 contain the results for the Eichengreen-Mody specification (model (1) in Table 2).[38] These results are presented mainly to facilitate a comparison with the existing literature, even though we do *not* think that these two episodes constitute valid experiments for a test of moral hazard. As in the previous subsection, the crisis period itself is excluded from the regressions.

After the Mexican crisis, there is only one country-specific variable that shows a significant change after the crisis, namely the Brady dummy (Table 9). Note, however, that the slope increases, while the moral-hazard interpretation in this case would predict a decrease in the slopes. Fitted spreads increase significantly for all but one country after the crisis (Table 10), which again contradicts

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[38]The results for the Mexican and Asian crises proved to be less robust than the ones for the Russian crises. The results for the alternative specifications are available from the authors upon request.

a moral-hazard interpretation. Finally, the cross-sectional variance of spreads increases, but the increase is insignificant (Table 11).[39] These results cannot easily be reconciled with a pure moral-hazard interpretation. In fact, according to the moral-hazard hypothesis, the above effects would have to go in the *opposite* direction of what is actually observed, at least if one believes that the large Mexican bailout *increased* expected future crisis lending and thus moral hazard.[40] Instead the results support the following interpretation: First, there seems to have been a general reluctance to take risks after the crisis, as suggested by the combination of higher emerging market bond spreads and the positive and significant coefficient of the US high-yield bond spread after the crisis. Second, there seems to have been a reassessment of risks after the crisis, leading to a stronger discrimination against countries who had rescheduled their debt in earlier times, which shows up in the increase in the Brady dummy and consequently in the increase in the fitted spreads of the Brady countries. It is well possible that these two effects more than compensated for an existing moral-hazard effect, so that the latter cannot be detected in the data.

After the Asian crisis, we would not expect to see any moral-hazard effects as the policies of the official community with respect to the Asian countries were roughly in line with the strategy employed in Mexico, so that this event did not contain new information with respect to future international crisis lending. Therefore, it is not surprising that again we do not detect any moral hazard in our three tests. In the slope test, there is only one country-specific variable that changes significantly (GDP growth), but this change in sign cannot easily be interpreted because the coefficient changes its sign and is significant in both cases. In the level test, 20 out of 34 countries experience significant increases in fitted spreads, and in the variance test, we cannot reject the null hypothesis of equal variances. Strikingly, almost one half of the countries with increases in fitted spreads are Asian countries. The highest increase is found in China, i.e., a country that was not directly involved in the crisis, but that was widely regarded as vulnerable to contagion from the Asian crisis countries at the time.[41] These results again support the interpretation that the crisis led to a reassessment of risks, which in this case affected mainly the Asian countries. The high positive coefficient on GDP (on a moving average basis) after the crisis might reflect the overshooting of spreads in these countries, which had excellent growth records prior to the crisis.

In conclusion, neither the response of spreads to the Mexican crisis nor their response to the Asian crises provide any evidence for a moral hazard effect. In the Mexican case any moral-hazard effect may have been more than compensated by other opposing effects, while in the Asian case we would not have expected to see a moral hazard effect in the first place. In both crises, we find increases in fitted spreads. These seem to have been caused by reassessments of risks due to the experiences made in the crises. In addition, investors seem to have been reluctant to take

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[39]Note that our results go in the same directions as the ones found by Zhang (1999).

[40]Some people have in fact argued that the large bailouts and the following controversial discussions about moral hazard might have *dampened* expectations about future international crisis lending (Willett, 1999).

[41]This result is robust to the choice of specification. In contrast, the implausibly high increase in fitted spreads in Singapore disappears if one uses the other two model specifications.

risks after the Mexican crisis.[42] There is no indication that investors generally differentiated more strongly between "good" and "bad" countries as we have seen after the Russian crisis.

D. Tests Using EMBI Global Data

We now turn to the regressions and tests based on the 18 EMBI Global (EMBIG) countries of Table 1 and Figure 3. Unlike the previous dataset, the EMBIG dataset constitutes a balanced panel; thus, there is no selection issue.[43] As in the previous section, regressions are based on monthly data, and the exclusion period runs from July 1998 until March 1999. Since the EMBIG spread data starts in January of 1998, only tests for the Russian crisis can be performed with this dataset.

Table 5 contains pre- and post-crisis regression results for three alternative models as well as the results from the "slope" tests. The first two models are roughly analogous to models (1) and (2) of Table 2, i.e., a model based on Eichengreen-Mody,[44] and a modification of that model that omits the variable "External debt/GDP" and instead includes fiscal balance and the current account (we also tried inflation together with or instead of the fiscal balance, but it did not have statistically significant effects). Model (3) is a new model, selected through a general-to-specific procedure, which attempts to make better use of our rich right-hand-side dataset than models (1) and (2).

The results from the slope test are again consistent with the moral hazard view, and stronger than for the other data set (see Table 5). With the exception of the coefficient on "External Debt/GDP", which changes sign, all coefficients on country fundamentals in the simple "Eichengreen-Mody" model behave as one would expect if Russian default reduced investor moral hazard. GDP growth, the ratings residual, and the arrears dummy have significantly larger effects (in absolute terms) after the crisis than before the crisis in all specifications. In model (2), two further variables, a positive fiscal balance and a current account surplus, have significantly larger effects after the crisis, as predicted under the moral-hazard hypothesis. In model (3), all coefficients but one change in the expected direction, although the magnitude of the change is not always statistically significant. The only coefficient that contradicts the moral hazard interpretation in this model is the one on the Asian dummy. As argued in the previous section, this could reflect the recovery of Asian debt prices between early 1998 and the 1999-2000, which may not be adequately controlled for by the remaining right hand side variables and may swamp any moral hazard effect. Finally, it is interesting to observe that in models (1) and (2) we again find a large drop in the coefficient on the U.S. high yield corporate bond spread after the crisis, although it does not reverse sign as in Table 2.

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[42]The coefficient on the US high-yield spread is also positive and very large after the Asian crisis, but it is insignificant.

[43]One might of course argue that the country selection into the EMBIG itself constitutes a selection problem. This possibility is ignored here.

[44]This model is closer to the original Eichengreen-Mody model in that it includes a variable capturing arrears (rather than the brady dummy as a proxy).

Although less clear-cut than in Table 3, the results from the level test are also generally supportive of the moral hazard interpretation (Table 6). Using any of the three models of Table 5, we now obtain instances of both significant increases and significant decreases in spreads, controlling for fundamentals; however, the increases far outnumber the decreases. Consistent with our findings in Table 3, the group of countries that does *not* provide clear-cut support for the hypothesis that the Russian default led to a permanent increase in investor risk tend includes a number of Asian countries, as well as Eastern European reformers such as Poland and Croatia. For most other countries, the results support the moral hazard interpretation.

Finally, consider Table 7, which presents the results of the variance test. This time, the results are even stronger than in Table 4. All models estimated using pre-crisis data strongly underpredict the post-crisis variance of fitted spreads, while the reverse is true for the models estimated on post-crisis data. The difference between the two sets of fitted variances is always highly significant. Thus, the variance test bears out the first impression obtained on the basis of the raw data, namely, that the Russia-Brazil crisis period was associated with a structural break whose overall effect was to significantly increase the cross-sectional variance of spreads, conditioning on fundamentals.

Summing up, the results from the EMBIG dataset generally support the moral-hazard hypothesis in that we find a stronger differentiation across countries that translates into a significant and very robust increase in the cross-sectional variance of spreads. There is somewhat less evidence for a *general* increase in the levels of spreads than based on the Bondware dataset. While spreads increase in most countries (controlling for fundamentals), they seem to move in the opposite direction in a few cases.

E. Robustness

We have already seen that our main findings are robust with respect to both a number of variations of the basic model used in the literature on emerging market bond spreads, and the use of two alternative datasets with very different characteristics. We now examine robustness with respect to several alternative estimation strategies, as discussed in section III.C. Since most of these involve estimating dynamic models of bond spreads, they require time series data and can thus not be performed using primary issue spreads. For this reason, the robustness checks in this section are performed using EMBIG secondary market data only.

Using model (3) of Table 5 as our point of departure, we checked the robustness of our results along the following dimensions:

- with respect to the choice of exclusion period: using either a different exclusion period which extends the period excluded in the regressions so far by three months, or using no exclusion period whatsoever;
- with respect to the estimation technique: assuming that the error follows a GARCH (1,1) process in lieu of the robust OLS estimator used so far, or using panel GLS estimators which explicitly assume serial correlation of the errors, correlation across country units, and cross-country heteroskedasticity;

- adding lagged dependent variables to the model;
- adding the composite EMBIG, i.e., the average level of spreads, as a control.

This resulted in estimating the baseline model in 36 variants (3 estimation techniques, times 3 exclusion strategies, times two alternatives regarding the inclusion of lagged dependent variables, times two alternatives regarding the inclusion of the EMBIG as a control).[45] Of these, the GLS variants are not worth showing since they turned out to be very close to the results using robust OLS estimation, except that the standard errors are slightly narrower and consequently the test results are slightly stronger. Similarly, extending the exclusion period by three months made virtually no difference except for a slight loss in the precision of the estimates. Finally, the specifications that used both lagged dependent variables and the EMBIG as a control turned out to produce very similar results as the ones that only use lagged dependent variables as a control. One of the two sets can thus be dropped. This leaves us with just 12 models, 6 of which are estimated for the original exclusion period and another 6 without any exclusion period.

For these 12 models, the results from our three tests are summarized in Table 8. The "slope test" is summarized in the central column by simply listing the variables from model (3) in Table 5, out of a maximum of eight or nine,[46] for which the test rejects the null hypothesis of no moral hazard. The "level test" is summarized by classifying the countries appearing in Table 6 in three groups: countries where bond spreads are unambiguously higher after the crisis (these are countries which in Table 6 would show only significant increases), countries showing only decreases, and countries with mixed results, i.e., where the test goes one way or the other depending on which period is used (in Table 6, these would be countries with positive entries in both columns). Finally, the column "variance test" gives the number of periods in which the equality of variances is rejected in the direction consistent with the moral hazard hypothesis. Note that the line for model (1) simply reproduces the results from model (3) in Tables 5, 6 and 7.

The main results are as follows. First, everything else equal, estimating the model without an exclusion period tends to strengthen the results (compare models (1) and (2) with their counterparts (7) and (8), respectively). This is attributable to the much higher level and cross-sectional variance of spreads during the crisis period, as is apparent in the raw data. It was for this reason that we excluded this period in the baseline regression. Second, estimating the model using a GARCH(1,1) process in the errors tends to slightly weaken the results, in the sense that we have less rejections in the slope and level tests, but leaves the main findings unchanged. Indeed, the results seem less sensitive to the estimation approach than to the choice of basic model in Table 5. Third, the main results from both the slope and the variance test are unchanged when we add the EMBIG composite as a control, i.e., when we give up on modeling the average dynamic of spreads and instead concentrate on deviations from that average. Fourth, the main

[45] Actually, we estimated a somewhat larger number of models since we tried several alternative feasible GLS estimators.

[46] Depending on whether one counts the variable "political instability and violence", which is always insignificant and could have been dropped from the model.

results survive (although weakened) even when we include both lagged dependent variables and the EMBI composite into the model, except in the case where we additionally assume that the errors follow a GARCH process. At that point, we really seem to be stretching what can be estimated based on our short time series, and the tests no longer reject, mainly because the coefficients on the fundamentals are estimated too imprecisely. But it takes the combination of two lagged dependent variables and GARCH errors to get to that point.

V. Conclusions

This paper developed and implemented a set of statistical tests to detect whether international crisis lending reduces the risk borne by emerging market debt holders. We applied these test to the events surrounding the Russian crisis of 1998, which was widely interpreted as signaling a greater reluctance of the international community to engage in large-scale lending.

We obtained three main findings. First, the events during the second half of 1998 generally made spreads more sensitive to country fundamentals. The absolute value of the coefficients in a regression of spreads on country fundamentals tends to increase, often significantly so. Second, for most countries, there was an increase in the levels of emerging markets bond spreads, controlling for changes in fundamentals. The exception are some countries with traditionally stronger fundamentals, which experienced constant and in a few cases decreasing spreads. Third, the events during the crisis period had a large positive effect on the cross-sectional variance of spreads. This increase is attributable to larger differences between the spreads of high-spread and low-spread countries. Thus, after the Russian crisis, investors seem to have paid much greater attention to differences in the countries' risk characteristics.

In the context of our simple model of international lending, these findings can be interpreted as evidence for the existence of moral hazard. However, this relies on the assumption that international crisis lending does not reduce true economic risk, by making crises less likely, or less deep when they do occur. Without this assumption, our tests tell us only that the presence of an international financial safety net—for better or for worse—had significant risk-reducing effects from an investor standpoint. Therefore, our findings should be interpreted as confirming a *necessary,* but not sufficient condition for the presence of investor moral hazard.

Another potential caveat concerns the role of the Russian crisis, which we have interpreted in terms of the official sector's willingness to avert large defaults. Alternatively, one might argue that Russia's default drew investors' attention to the possibility of default in other emerging markets, leading to higher spreads in countries with weak fundamentals and larger cross-sectional differentiation of spreads across countries (the "wake up call" interpretation). However, this argument is convincing only if the Russian default conveyed some new information about emerging market risk. One can easily think of such "lessons" with regard to the earlier two crises of the 1990s: the possibility of self-fulfilling runs at the international level, "crony capitalism", or vulnerabilities due to currency mismatches. In contrast, the Russian default and currency collapse were driven by a relatively traditional fiscal crisis which had been looming for some time. It does not seem to have conveyed new information about emerging market crises other than that the willingness of international official lenders to support an insolvent country—even one widely considered "too big to fail"—was evidently more limited than had been previously assumed.

It still remains to be explained why our findings do not show a uniform increase in emerging market bond spreads, but instead a heterogeneous reaction, depending on the strength of country fundamentals. One plausible interpretation is that investors did not revise their expectations about future crisis lending uniformly for all countries, but that these revisions were undertaken only for countries likely to run into solvency problems. After all, the events during the summer and fall of 1998, when a major lending package to Brazil followed on the heels of the Russian "non-bailout", did not suggest a *general* unwillingness of the international community to lend to countries in trouble. Instead, they showed a reluctance to rescue an insolvent country with a poor recent track record of reform and no sign of improvement. This can explain why spreads did not rise across the board. The observation that some spreads actually *fell* can be explained in several ways. The continued recovery in Asia and the consequent fall in Asian spreads may have been driven by a return in confidence that is not fully picked up by our fundamentals. The fact that investors "got out" of countries with weak fundamentals after the Russian crisis may have benefitted those with relatively strong fundamentals.[47] Moreover, the decision of the U.S. Congress to approve the U.S. contribution to the IMF quota increase in October of 1998 may have led to a perception that the Fund was now generally better equipped to deal with emerging market crises. Everything else equal, this would have reduced emerging market spreads everywhere. In combination with the signal imparted by the Russian crisis, the effect might have shown only for countries with strong fundamentals.

Finally, one has to be careful in drawing policy conclusions from our results. Even if we accept the existence of moral hazard, this does not mean that international rescues should not take place at all. The trade-off between providing insurance and maintaining "good" incentives is a universal feature of measures that reduce risk, from explicit insurance contracts to public safety measures. To make a judgement about the right extent of the international financial safety net, one must compare its incentives costs and insurance benefits, rather than just prove the existence of moral hazard. This said, advocates of large-scale crisis lending sometimes make their case by arguing that there is no evidence that the anticipation of large-scale crisis lending has substantially reduced perceived investor risk.[48] In view of the results of this paper, we believe that it will be much more difficult to take this position in the future.

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[47]This argument requires some degree of segmentation between emerging and advanced capital markets, so that funds withdrawn from one emerging market countries have an effect on emerging market liquidity elsewhere.

[48]See, for example, Cline (2000).

THE VARIANCE TEST

The variance test is used to test the equality of variances before and after an event, controlling for fundamentals. The null hypothesis can be written as

$$H_0: \quad \beta^{1\prime} Var(X_t)\beta^1 - \beta^{0\prime} Var(X_t)\beta^0 = 0, \tag{A-1}$$

which is a nonlinear function of the parameter vector $\binom{\beta^0}{\beta^1}$. This function will be named $f(\beta^0, \beta^1)$ in the following. In order to find the distribution of $f(\hat{\beta}^0, \hat{\beta}^1)$ we approximate it by the Delta method around the true parameter values:

$$f(\hat{\beta}^0, \hat{\beta}^1) \approx f(\beta^0, \beta^1) + \frac{\partial f}{\partial \widehat{\beta}^0} \cdot (\hat{\beta}^0 - \beta^0) + \frac{\partial f}{\partial \widehat{\beta}^1} \cdot (\hat{\beta}^1 - \beta^1) \tag{A-2}$$

Since $(\hat{\beta}^0, \hat{\beta}^1)$ are jointly normal under the null (at least asymptotically), the above expression is also normal since it is a linear combination of $(\hat{\beta}^0, \hat{\beta}^1)$. The variance of the expression can be easily calculated, leading to the following Wald test statistic:

$$W = f(\hat{\beta}^0, \hat{\beta}^1)' \, [GVG']^{-1} \, f(\hat{\beta}^0, \hat{\beta}^1) \overset{as}{\sim} \chi^2(1) \text{ under } \mathrm{H_0}, \tag{A-3}$$

where

$$G = \left(\frac{\partial f}{\partial \widehat{\beta}^0}, \frac{\partial f}{\partial \widehat{\beta}^1} \right), \tag{A-4}$$

$$V = \widehat{Var} \begin{pmatrix} \hat{\beta}^0 \\ \hat{\beta}^1 \end{pmatrix}, \tag{A-5}$$

and $(\hat{\beta}^0, \hat{\beta}^1)$ the estimators from the pooled model, allowing for different coefficients before and after the event. In order to account for heteroskedastic errors, one should use robust standard errors in the regressions.

PROOFS FOR THE PROPOSITIONS IN SUBSECTION II.B

Proposition 1 *Holding constant the set of fundamentals* $X = (x_1', x_2', ...x_N')'$, *equation (3) implies that* $\frac{\partial \lambda}{\partial b} > 0$ *if and only if* $\frac{\partial s_i}{\partial b} < 0$ *for any country i.*

Proof. Defining the default probability as $\mu(x, b) = [1 - \lambda(b)] \cdot \theta(x)$ and omitting subscripts, we can write the spread as

$$s = R^* \cdot \frac{[1 - \lambda(b)] \cdot \theta(\mathbf{x})}{1 - [1 - \lambda(b)] \cdot \theta(\mathbf{x})} = R^* \cdot \frac{\mu(\mathbf{x}, b)}{1 - \mu(\mathbf{x}, b)}.$$

Hence, the spread depends negatively on the default probability μ. Holding constant fundamentals, the derivative of s with respect to b is

$$\frac{\partial s}{\partial b} = -R^* \cdot \frac{\theta(\mathbf{x}) \cdot \frac{\partial \lambda}{\partial b}}{[1 - \mu(\mathbf{x}, b)]^2}.$$

This implies that $\frac{\partial s}{\partial b} < 0 \Leftrightarrow \frac{\partial \lambda}{\partial b} > 0$, q.e.d.

Proposition 2 *Holding constant the set of fundamentals* $X = (x_1', x_2', ...x_N')'$, *equation (3) implies that* $\frac{\partial \lambda}{\partial b} > 0$ *if and only if* $\frac{\partial^2 s_i}{\partial x_{ij} \partial b} < 0$ *for any country i and any country-specific fundamental* x_{ij}.

Proof. Starting again from the spread equation (3), we find (omitting the country subscripts)

$$\frac{\partial^2 s}{\partial b \partial x_j} = -R^* \cdot \frac{\partial \lambda(b)}{\partial b} \cdot \frac{[1 + \mu(\mathbf{x}, b)] \cdot \frac{d\theta(\mathbf{x})}{dx_j}}{[1 - \mu(\mathbf{x}, b)]^3}.$$

Since $\frac{d\theta(\mathbf{x})}{dx_j} > 0$, $\frac{\partial^2 s}{\partial b \partial x_j} < 0 \quad \Leftrightarrow \quad \frac{\partial \lambda}{\partial b} > 0$, q.e.d.

Proposition 3 *Holding constant the set of fundamentals* $X = (x_1', x_2', ...x_N')'$, *equation (3) implies that* $\frac{\partial \lambda}{\partial b} > 0$ *if and only if* $\frac{\partial \Delta s}{\partial b} < 0$ *for any two countries* m *and* $n, m \neq n$, *for which we can approximate* $\Delta s \equiv s_m - s_n$ *by a first-order Taylor expansion.*

Proof. Write s as a function of x and b

$$s_i = s(\mathbf{x}_i, b)$$

and define $\Delta s = s_m - s_n$ (the same notation will be used for first differences of other variables). Assume (without loss of generality) that $\Delta s > 0$.

Approximate the spread of country m by

$$s_m \cong s_n + \sum_{j=1}^{K} \frac{ds(\mathbf{x}_n, b)}{dx_j} \Delta x_j.$$

The derivative in the sum is (omitting the country subscripts)

$$\frac{ds(\mathbf{x}, b)}{dx_j} = R^* \cdot \frac{(1 - \lambda(b)) \cdot \frac{d\theta(\mathbf{x})}{dx_j}}{[1 - \mu(\mathbf{x}, b)]^2}.$$

Plugging in this expression, we find

$$s_m \cong s_n + \sum_{j=1}^{K} R^* \cdot \frac{(1 - \lambda(b)) \cdot \frac{d\theta(\mathbf{x})}{dx_j}}{[1 - \mu(\mathbf{x}, b)]^2} \Delta x_j ,$$

or, rewriting

$$\Delta s \cong \sum_{j=1}^{K} R^* \cdot \frac{(1 - \lambda(b)) \cdot \frac{d\theta(\mathbf{x})}{dx_j}}{[1 - \mu(\mathbf{x}, b)]^2} \Delta x_j = \frac{R^*}{[1 - \mu(\mathbf{x}, b)]^2} \cdot [1 - \lambda(b)] \cdot \Delta\theta. \tag{A-1}$$

From $\Delta s > 0$ it follows that

$$\Delta\theta > 0 \tag{A-2}$$

We are interested in the partial derivative of expression (A-1) with respect to b:

$$\begin{aligned}
\frac{\partial \Delta s}{\partial b} &\cong \sum_{j=1}^{K} \frac{\partial^2 s(\mathbf{x}_n, b)}{\partial x_j \partial b} \Delta x_j \\
&= -\frac{R^*}{[1 - \mu(\mathbf{x}, b)]^3} \cdot \frac{\partial \lambda(b)}{\partial b} \cdot \sum_{j=1}^{K} \left\{ [1 + \mu(\mathbf{x}, b)] \cdot \frac{d\theta(\mathbf{x})}{dx_j} \right\} \cdot \Delta x_j \\
&= -\frac{R^*}{[1 - \mu(\mathbf{x}, b)]^3} \cdot \frac{\partial \lambda(b)}{\partial b} \cdot [1 + \mu(\mathbf{x}, b)] \cdot \Delta\theta
\end{aligned}$$

Now the proof is easily completed. From $\Delta\theta > 0$ it follows directly that $\frac{\partial \Delta s}{\partial b} < 0 \Leftrightarrow \frac{\partial \lambda}{\partial b} > 0$, q.e.d.

So far, we have assumed that the recovery rate λ does not depend on the fundamentals x. We will show now that the proofs of all three propositions still go through if λ depends on x as long as two regularity conditions are satisfied. First, we assume that an increase in b affects the expected recovery rate—i.e., generates investor moral hazard—uniformly across countries:

$$\frac{\partial^2 \lambda}{\partial x_i \partial b} = 0 . \tag{A-3}$$

The second condition will be introduced below. The proof of proposition 1 is remains unchanged and is omitted. The cross-derivative needed for proposition 2 contains an additional term:

$$\frac{\partial^2 s}{\partial b \partial x_j} = -R^* \cdot \frac{\partial \lambda(\mathbf{x}, b)}{\partial b} \cdot \frac{[1 + \mu(\mathbf{x}, b)] \cdot \frac{d\theta(\mathbf{x})}{dx_j} - 2[\theta(\mathbf{x})]^2 \cdot \frac{d\lambda(\mathbf{x},b)}{dx_j}}{[1 - \mu(\mathbf{x}, b)]^3}.$$

Since $\frac{d\theta(\mathbf{x})}{dx_j} > 0$ and $\frac{d\lambda(\mathbf{x},b)}{dx_j} < 0$, we still find that $\frac{\partial^2 s}{\partial b \partial x_j} < 0 \quad \Leftrightarrow \quad \frac{\partial \lambda}{\partial b} > 0$, q.e.d..

The proof of proposition 3 is a little more involved. Again we approximate the spread of country m by

$$s_m \cong s_n + \sum_{j=1}^{K} \frac{ds(\mathbf{x}_n, b)}{dx_j} \Delta x_j.$$

The derivative in the sum is (omitting the country subscripts)

$$\frac{ds(\mathbf{x}, b)}{dx_j} = R^* \cdot \frac{(1 - \lambda(\mathbf{x}, b)) \cdot \frac{d\theta(\mathbf{x})}{dx_j} - \theta(\mathbf{x}) \cdot \frac{d\lambda(\mathbf{x},b)}{dx_j}}{[1 - \mu(\mathbf{x}, b)]^2}.$$

Therefore, the approximated spread can be written as

$$\begin{aligned} \Delta s &\cong \sum_{j=1}^{K} R^* \cdot \frac{(1 - \lambda(\mathbf{x}, b)) \cdot \frac{d\theta(\mathbf{x})}{dx_j} - \theta(\mathbf{x}) \cdot \frac{d\lambda(\mathbf{x},b)}{dx_j}}{[1 - \mu(\mathbf{x}, b)]^2} \Delta x_j && \text{(A-4)} \\ &= \frac{R^*}{[1 - \mu(\mathbf{x}, b)]^2} \cdot [(1 - \lambda(\mathbf{x}, b)) \cdot \Delta\theta - \theta(\mathbf{x}) \cdot \Delta\lambda]. && \text{(A-5)} \end{aligned}$$

From $\Delta s > 0$ it follows that

$$(1 - \lambda(\mathbf{x}, b)) \cdot \Delta\theta - \theta(x) \cdot \Delta\lambda > 0 \qquad \text{(A-6)}$$

The partial derivative of this expression with respect to b is

$$\begin{aligned} \frac{\partial \Delta s}{\partial b} &\cong -\frac{R^*}{[1 - \mu(\mathbf{x}, b)]^3} \cdot \frac{\partial \lambda(\mathbf{x}, b)}{\partial b} \cdot \sum_{j=1}^{K} \left\{ [1 + \mu(\mathbf{x}, b)] \cdot \frac{d\theta(\mathbf{x})}{dx_j} - 2[\theta(\mathbf{x})]^2 \cdot \frac{d\lambda(\mathbf{x}, b)}{dx_j} \right\} \cdot \Delta x_j \\ &= -\frac{R^*}{[1 - \mu(\mathbf{x}, b)]^3} \cdot \frac{\partial \lambda(\mathbf{x}, b)}{\partial b} \cdot \left\{ [1 + \mu(\mathbf{x}, b)] \cdot \Delta\theta - 2[\theta(\mathbf{x})]^2 \cdot \Delta\lambda \right\} \end{aligned}$$

The proof is easily completed if we assume the following regularity condition:

$$\Delta s > 0 \Longrightarrow \Delta\theta \geq 0 \qquad \text{(A-7)}$$

This condition guarantees that it is the cross-country differences in crisis probabilities rather than in conditional repayment probabilities that determines the ranking of country spreads. Hence, it rules out the case that the country with the higher spread has a lower probability of a crisis. We will show later that this assumption is stronger than what we need for our argument.

Under this assumption, the proof is straightforward. From condition (A-7) we know that $\Delta\theta \geq 0$. We have to distinguish two cases according to whether $\Delta\lambda$ is positive or negative.

If $\Delta\lambda < 0$[49], we see immediately that $[1 + \mu(\mathbf{x}, b)] \cdot \Delta\theta - 2[\theta(x)]^2 \cdot \Delta\lambda > 0$, from which we get that $\frac{\partial \Delta s}{\partial b} < 0 \Leftrightarrow \frac{\partial \lambda}{\partial b} > 0$.

If $\Delta\lambda > 0$, condition (A-6) ensures that $[1 + \mu(\mathbf{x}, b)] \cdot \Delta\theta - 2[\theta(x)]^2 \cdot \Delta\lambda > 0$ because

$$\frac{\Delta\theta}{\Delta\lambda} > \frac{\theta}{1-\lambda} > \frac{2\theta^2}{1+\mu}. \tag{A-8}$$

In the case where $\Delta\theta < 0$, we have to make sure that $\Delta\theta$ is not "too large" in absolute terms compared to $\Delta\lambda$. This can be guaranteed by the following condition:

$$\frac{\Delta\theta}{\Delta\lambda} < \frac{2\theta^2}{1+\mu} \tag{A-9}$$

This condition rules out the (pathological) case where the country with the higher spread has a much smaller crisis probability, but also a much smaller repayment probability than another country. This completes our proof.

[49]If $\Delta\theta = 0$, this is the only relevant case.

Figure 1: Estimated Conditional Volatility of Changes in the EMBIG Composite Spread, 1998–2002

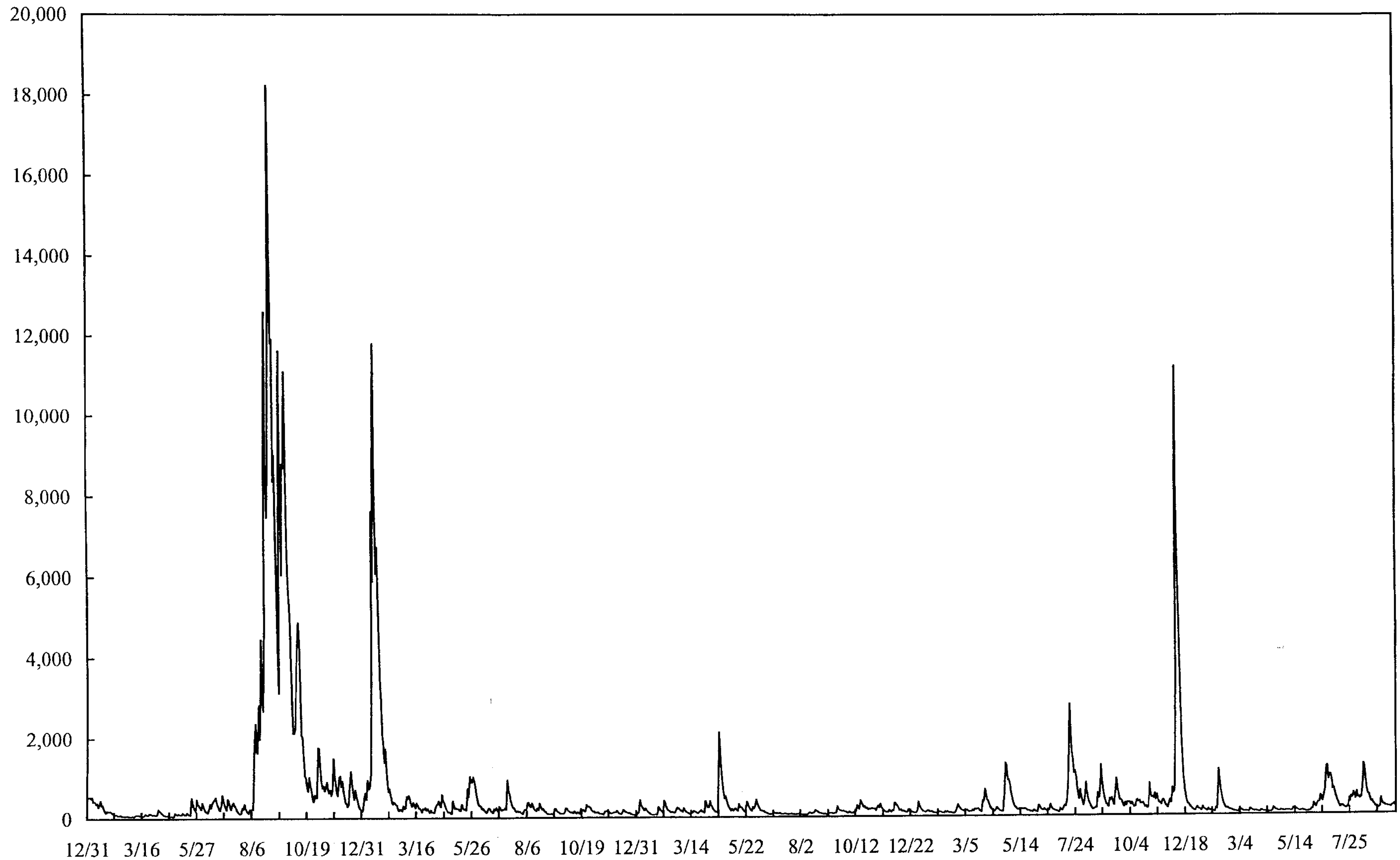

Figure 2: EMBI Global Daily Strip Spreads, 1998–2000, 20 Countries (includes Russia and Ecuador)
(in basis points)

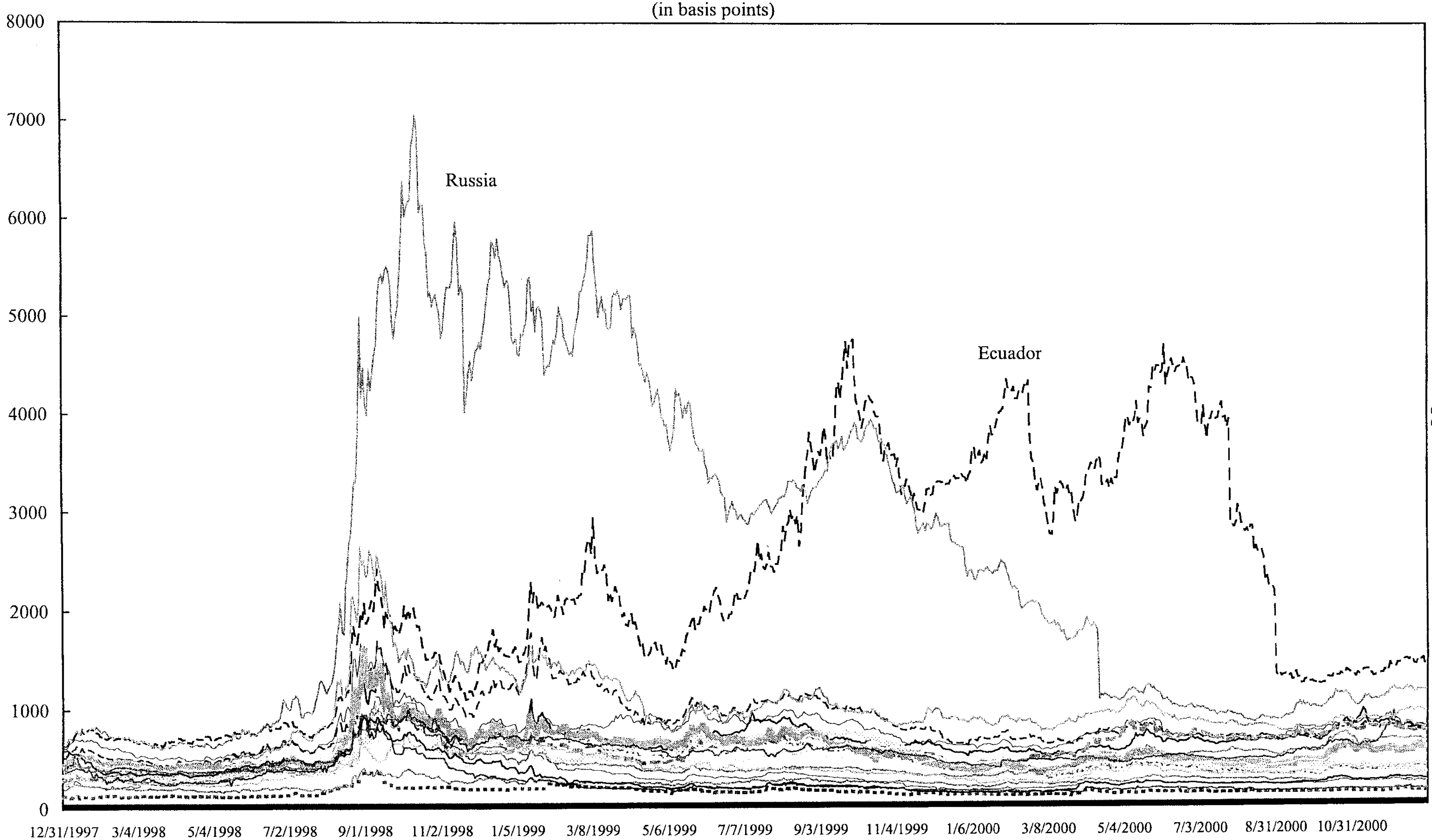

Figure 3: EMBI Global Daily Strip Spreads, 1998–2000, 18 countries (excludes Russia and Ecuador)
(in basis points)

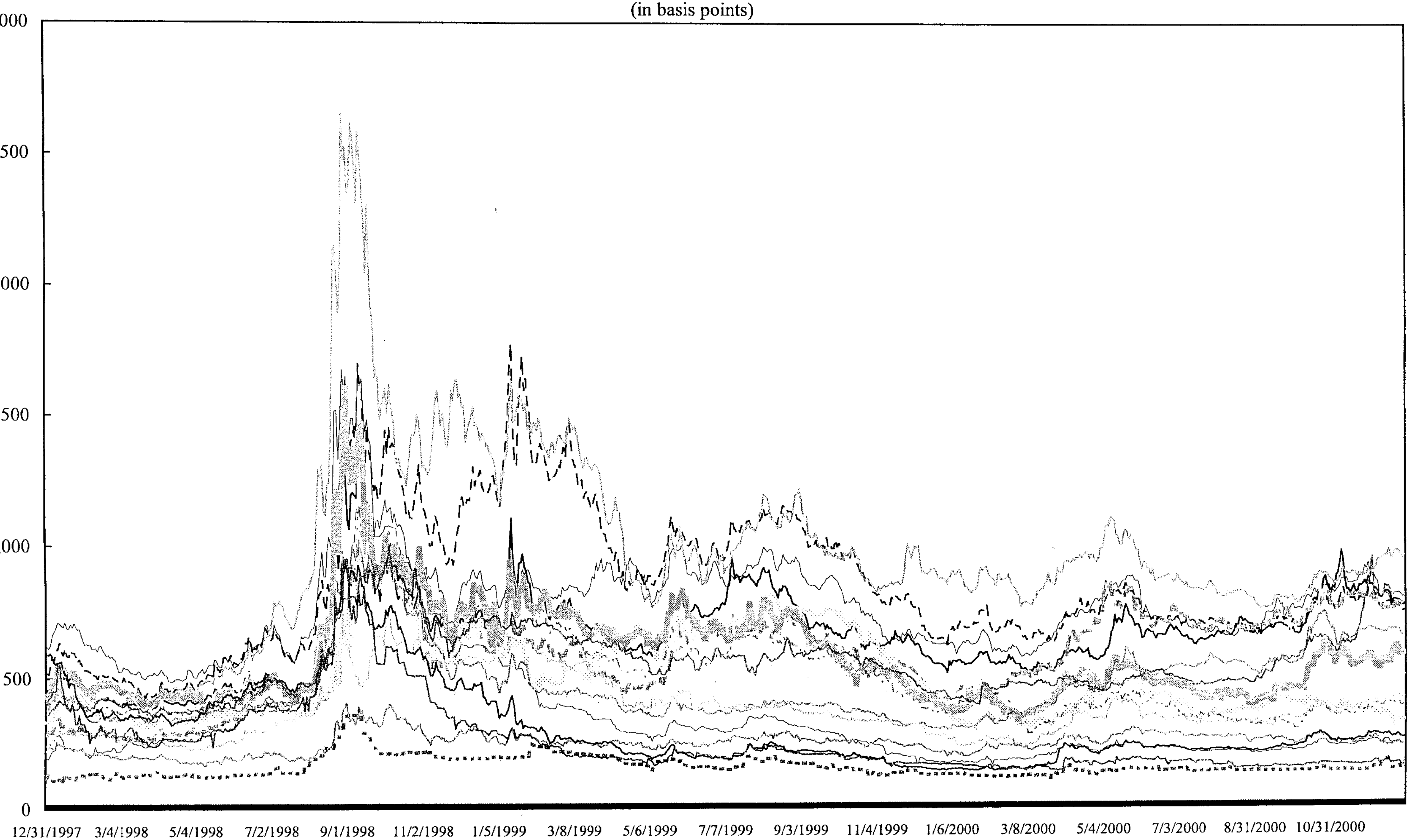

Table 1. Mean and Cross-Sectional Dispersion of Spreads Before and After the Russian Crisis: Summary Statistics

Period		18 EMBIG Countries (excluding Russia and Ecuador) 1/		39 Bondware Countries (excluding Russia and Ecuador) 2/		
		mean	std. dev. 3/	mean	std. dev. 4/	Number of countries
Precrisis	1998:Q1	354	108	330	178	11
	1998:Q2	393	122	272	154	16
Crisis	1998:Q3	776	333	302	292	7
	1998:Q4	668	295	437	214	18
	1999:Q1	561	237	419	222	19
Postcrisis	1999:Q2	532	258	370	194	31
	1999:Q3	567	283	416	179	26
	1999:Q4	438	217	387	182	22
	2000:Q1	406	207	379	188	28
	2000:Q2	479	230	325	209	17
	2000:Q3	447	211	387	182	22
	2000:Q4	532	261	341	212	12

1/ Argentina, Bulgaria, Brazil, China, Colombia, Croatia, Korea, Morocco, Mexico, Malaysia, Panama, Peru, the Philippines, Poland, Thailand, Turkey, Venezuela, and South Africa.

2/ Argentina, Brazil, Bulgaria, Chile, China, Colombia, Costa Rica, Croatia, Cyprus, the Czech Republic, El Salvador, Estonia, Hong Kong SAR, Hungary, India, Israel, Jamaica, Kazakhstan, Korea, Latvia, Lebanon, Lithuania, Malaysia, Malta, Mexico, Morocco, the Philippines, Poland, Romania, Singapore, the Slovak Republic, Slovenia, South Africa, Taiwan Province of China, Thailand, Tunisia, Turkey, Uruguay, and Venezuela. The Dominican Republic, Ecuador, Egypt, Guatemala, Indonesia, Jordan, Mauritius, Oman, Pakistan, Panama, Peru, and Saudi Arabia did not issue bonds during this period.

3/ Refers to average cross-sectional standard deviation during period.

4/ Refers to standard deviation of all available bonds during period.

Table 2. Launch Spread Data: Estimation of Alternative Models Before and After Russian Crisis, and Results for "Slope Test"

Variable	(1) "Eichengreen-Mody" 1/ before crisis 2/ Coef.	p	after crisis 3/ Coef.	p	Test for equality p 4/	(2) Alternative specification (A) 1/ before crisis 2/ Coef.	p	after crisis 3/ Coef.	p	Test for equality p 4/	(3) Alternative specification (B) 1/ before crisis 2/ Coef.	p	after crisis 3/ Coef.	p	Test for equality p 4/
Constant	-1249.03	0.88	1254.34	0.00	0.77	-2294.99	0.78	1122.59	0.00	0.67	-5091.50	0.58	1131.48	0.00	0.50
Real growth (MA)	-5.80	0.78	-47.10	0.00	**0.06**	-1.62	0.93	-41.76	0.00	**0.03**	-1.85	0.93	-33.89	0.00	0.15
Inflation						-0.89	0.34	2.90	0.00	**0.00**					
Current account (MA)						-0.93	0.56	-3.77	0.02	0.18	0.33	0.80	-1.287	0.36	0.38
External debt/GDP	-0.92	0.33	-0.63	0.04	0.77										
Brady dummy	46.27	0.45	103.86	0.00	0.36	15.66	0.78	62.70	0.01	0.42	25.52	0.64	77.58	0.00	0.36
Political instability + violence											-97.09	0.00	-159.27	0.00	**0.09**
Short-term debt / total debt											1.71	0.40	-0.46	0.60	0.33
Rating (residual)	-31.37	0.00	-53.26	0.00	**0.03**	-48.85	0.00	-68.62	0.00	**0.10**	-28.05	0.02	-53.40	0.00	**0.04**
US 10-year yield	218.37	0.86	-51.27	0.06	0.83	421.44	0.72	-48.63	0.06	0.69	735.27	0.57	-44.61	0.10	0.55
US high-yield bond spread	113.97	0.85	-82.14	0.00	0.74	71.93	0.89	-74.67	0.00	0.78	392.82	0.55	-69.77	0.01	0.48
F test (p) 5/					**0.00**					**0.00**					**0.00**
Observations in subsample	306		1071			306		1071			306		1071		
Observation with bond data	27		158			27		158			27		158		
Number of countries 6/	51		51			51		51			51		51		

Selection Equation 7/	(1) Coef.	p	(2) Coef.	p	(3) Coef.	p
Debt issued in preceding year	0.16	0.64	0.11	0.76	0.20	0.60
Number of previous bond issues	0.08	0.00	0.08	0.00	0.08	0.00
GDP per capita (1993)	0.45	0.00	0.34	0.01	0.43	0.00
Dummy for Asian crisis countries	0.24	0.14	0.30	0.09	0.26	0.12
rho	-0.04	0.86	0.06	0.80	0.00	1.00

1/ Estimated on pooled sample 1998:01 - 2000:12, excluding 1998:07 - 1999:03, and allowing for different coefficients for pre- and post crisis periods. All estimations use robust standard errors.

2/ 1998:01 - 1998:06.

3/ 1999:04 - 2000:12.

4/ p values based on two-sided tests; boldface indicates rejection of equality at the 10 percent level in the direction consistent with H1 (see text).

5/ p value refers to the joint hypothesis that all slopes are the same in the two periods.

6/ Argentina, Brazil, Bulgaria, Chile, China, Colombia, Costa Rica, Croatia, Cyprus, the Czech Republic, the Dominican Republic, Ecuador, Egypt, El Salvador, Estonia, Guatemala, Hong Kong SAR, Hungary, India, Indonesia, Israel, Jamaica, Jordan, Kazakhstan, Korea, Latvia, Lebanon, Lithuania, Malaysia, Malta, Mauritius, Mexico, Morocco, Oman, Pakistan, Panama, Peru, the Philippines, Poland, Romania, Saudi Arabia, Singapore, Slovak Republic, Slovenia, South Africa, Taiwan Province of China, Thailand, Tunisia, Turkey, Uruguay, Venezuela.

7/ Reports only coefficients for instruments and correlation coefficient of disturbance terms of the two equations (rho).

Table 3. Launch Spread Data: Summary Results for "Levels Test" (Russia Crisis)

	Eichengreen-Mody: Rejections Indicating ...		Alternative model A: Rejections Indicating ...		Alternative model B: Rejections Indicating ...	
	Significant increase of spreads 1/	Significant decline of spreads 2/	Significant increase of spreads 1/	Significant decline of spreads 2/	Significant increase of spreads 1/	Significant decline of spreads 2/
Argentina	6	0	6	0	6	0
Brazil	6	0	6	0	6	0
Bulgaria	6	0	6	0	6	0
Chile	0	0	0	0	0	0
China	0	0	0	0	0	0
Colombia	6	0	6	0	3	0
Costa Rica	6	0	6	0	6	0
Croatia	1	0	2	0	3	0
Cyprus	2	0	0	0	0	0
Czech Republic	2	0	6	0	1	0
Dominican Republic	6	0	5	0	6	0
Ecuador	6	0	6	0	6	0
Egypt	6	0	6	0	1	0
El Salvador	6	0	6	0	6	0
Estonia	5	0	6	0	1	0
Guatemala	6	0	6	0	1	0
Hong Kong SAR	0	0	0	0	0	0
Hungary	6	0	6	0	6	0
India	2	0	4	0	4	0
Indonesia	6	0	6	0	6	0
Israel	0	0	0	0	0	0
Jamaica	6	0	6	0	6	0
Jordan	6	0	6	0	6	0
Kazakhstan	6	0	6	0	6	0
Korea	2	0	1	0	2	0
Latvia	6	0	6	0	6	0
Lebanon	6	0	6	0	5	0
Lithuania	6	0	6	0	6	0
Malaysia	0	0	0	0	0	0
Malta	2	0	0	0	0	0
Mauritius	6	0	4	0	2	0
Mexico	6	0	6	0	6	0
Morocco	6	0	6	0	6	0
Oman	6	0	4	0	5	0
Pakistan	6	0	6	0	6	0
Panama	0	0	1	0	2	0
Peru	0	0	0	0	0	0
Philippines	6	0	6	0	6	0
Poland	1	0	2	0	3	0
Romania	6	0	6	0	6	0
Saudi Arabia	6	0	6	0	1	0
Singapore	0	0	0	0	0	0
Slovak Republic	6	0	5	0	5	0
Slovenia	0	0	1	0	0	0
South Africa	6	0	6	0	4	0
Taiwan Province of China	0	0	0	0	0	0
Thailand	6	0	6	0	5	0
Tunisia	6	0	6	0	6	0
Turkey	6	0	6	0	6	0
Uruguay	6	0	6	0	1	0
Venezuela	6	0	6	0	6	0
Sum rejections	215	0	215	0	181	0
Number countries showing rejection	41	0	41	0	40	0
Total number of countries	51		51		51	

1/ No. of periods in which fitted spread based on post-crisis model is significantly *higher* than fitted spread based on pre-crisis model (potential maximum per country: 27, significance level 5%).

2/ No. of periods in which fitted spread based on post-crisis model is significantly *lower* than fitted spread based on pre-crisis model (potential maximum per country: 27, significance level 5%).

Table 4. Launch Spread Data: Cross-Sectional Variances of Fitted Spreads Before and After Russian Crisis, and Results for "Variance Test"

	(1) Eichengreen-Mody			(2) Alternative Specification (A)			(3) Alternative Specification (B)		
	Fitted variance using coefficients estimated ...		Test for equality	Fitted variance using coefficients estimated ...		Test for equality	Fitted variance using coefficients estimated ...		Test for equality
Month	before crisis 1/	after crisis 2/	(*p*) 3/	before crisis 1/	after crisis 2/	(*p*) 3/	before crisis 1/	after crisis 2/	(*p*) 3/
1998:01	11,266	33,661	***0.033***	26,355	93,187	***0.025***	8,513	32,419	***0.000***
1998:02	11,130	33,660	***0.032***	17,889	73,368	***0.001***	8,366	32,279	***0.000***
1998:03	11,174	33,739	***0.031***	14,152	55,165	***0.000***	8,367	32,351	***0.000***
1998:04	11,115	33,371	***0.034***	13,903	53,231	***0.000***	8,314	32,019	***0.000***
1998:05	11,412	34,118	***0.031***	14,447	54,715	***0.000***	8,585	32,848	***0.000***
1998:06	11,692	34,694	***0.030***	15,011	55,907	***0.000***	8,757	33,511	***0.000***
1999:04	11,708	32,264	***0.033***	14,277	43,183	***0.000***	9,188	35,152	***0.000***
1999:05	11,708	32,264	***0.033***	14,207	43,226	***0.000***	9,188	35,152	***0.000***
1999:06	11,750	32,473	***0.032***	14,571	43,761	***0.001***	9,281	35,376	***0.000***
1999:07	11,748	32,222	***0.034***	14,640	43,375	***0.001***	8,665	34,865	***0.000***
1999:08	11,742	32,185	***0.034***	15,122	43,653	***0.001***	8,658	34,826	***0.000***
1999:09	11,798	32,559	***0.032***	15,718	44,611	***0.001***	8,751	35,204	***0.000***
1999:10	12,220	34,110	***0.025***	15,954	46,653	***0.001***	9,121	36,770	***0.000***
1999:11	12,296	34,672	***0.023***	15,976	47,362	***0.001***	9,228	37,355	***0.000***
1999:12	11,723	33,128	***0.027***	14,514	44,225	***0.000***	8,771	35,750	***0.000***
2000:01	10,726	35,025	***0.005***	14,960	45,338	***0.000***	8,642	37,685	***0.000***
2000:02	10,767	35,172	***0.005***	15,137	44,914	***0.001***	8,680	37,818	***0.000***
2000:03	10,725	35,338	***0.005***	15,336	44,776	***0.001***	8,707	37,989	***0.000***
2000:04	10,102	36,656	***0.001***	14,959	45,158	***0.001***	8,418	37,818	***0.000***
2000:05	10,126	36,669	***0.001***	15,181	45,048	***0.001***	8,485	37,848	***0.000***
2000:06	10,159	36,629	***0.001***	15,437	44,891	***0.001***	8,498	37,781	***0.000***
2000:07	10,160	36,672	***0.000***	15,659	44,917	***0.001***	8,910	38,001	***0.000***
2000:08	10,386	37,624	***0.000***	15,182	45,731	***0.001***	9,105	38,968	***0.000***
2000:09	9,619	34,335	***0.001***	14,820	41,362	***0.001***	8,157	35,289	***0.000***
2000:10	9,583	34,216	***0.001***	14,711	41,002	***0.001***	8,293	35,303	***0.000***
2000:11	9,766	34,869	***0.001***	15,146	42,243	***0.001***	8,536	35,991	***0.000***
2000:12	9,819	35,063	***0.001***	14,994	42,876	***0.001***	8,614	36,201	***0.000***

1/ Regression coefficients estimated on 1998:01 to 1998:06 data.
2/ Regression coefficients estimated on 1999:04 to 2000:12 data.
3/ *p* values based on two-sided tests.

Table 5. EMBIG Data: Estimation of Alternative Models Before and After Russia Crisis, and Results for "Slope Test"

Variable	(1) "Eichengreen-Mody" 1/					(2) Alternative Specification (A) 1/					(2) Alternative Specification (B) 1/				
	before crisis 2/		after crisis 3/		Test for equality	before crisis 2/		after crisis 3/		Test for equality	before crisis 2/		after crisis 3/		Test for equality
	Coef.	p	Coef.	p	p 4/	Coef.	p	Coef.	p	p 4/	Coef.	p	Coef.	p	p 4/
Constant	766.12	0.43	805.08	4.35	0.97	848.94	0.84	775.12	0.00	0.94	3195.00	0.00	1474.04	0.00	0.00
Real growth	-6.44	0.00	-25.40	-15.62	**0.00**	-7.88	-7.86	-22.34	0.00	**0.00**	-3.83	0.00	-10.02	0.00	**0.00**
Fiscal balance						-12.29	-4.11	-24.59	0.00	**0.01**	-21.26	0.00	-29.93	0.00	0.12
Current account (MA)						3.29	1.09	-6.56	0.00	**0.01**	0.75	0.79	7.17	0.01	0.10
Real credit growth (MA)											-2.77	0.00	-5.83	0.00	**0.00**
External Debt/GDP	1.27	0.00	-0.62	-1.48	0.00										
Arrears dummy	113.54	0.00	253.86	9.38	**0.00**	141.64	9.65	227.81	0.00	**0.00**					
Asia dummy											72.39	0.01	-79.54	0.00	0.00
Latin dummy											151.06	0.00	227.29	0.00	**0.00**
Political instability and violence											11.42	0.40	14.44	0.27	0.87
Size (log $GDP in 1993)											-35.75	0.00	-40.01	0.00	0.58
Rating (residual)	-34.32	0.00	-70.59	-13.65	**0.00**	-37.75	-10.24	-76.88	0.00	**0.00**	-39.33	0.00	-51.38	0.00	**0.01**
US ten year yield	-167.67	0.23	-57.55	-2.24	0.44	-171.71	-1.17	-65.62	0.01	0.48	-447.13	0.00	-97.00	0.00	0.00
US high-yield bond spread	151.56	0.03	21.38	2.96	0.07	146.52	2.01	16.65	0.02	0.08					
F-test (*p*) 5/					**0.00**					**0.00**					**0.00**
Observations in subsample	108		378			108		378			108		378		
No. of countries 6/	18		18			18		18			18		18		
k	7		7			8		8			14		14		
R Squared															

1/ Estimated on pooled sample 1998:01 - 2000:12, excluding 1998:07 - 1999:03, and allowing for different coefficients for pre- and post crisis periods. All estimations use robust standard errors.

2/ 1998:01 - 1998:06.

3/ 1999:04 - 2000:12.

4/ *p* values based on two-sided tests; boldface indicates rejection of equality at the 10 percent level in the direction consistent with H1 (see text).

5/ *p* value refers to the joint hypothesis that all slopes are the same in the two periods.

6/ Full sample of 18 countries: Argentina, Bulgaria, Brazil, China, Colombia, Croatia, Korea, Morocco, Mexico, Malaysia, Panama, Peru, the Philippines, Poland, Thailand, Turkey, Venezuela, and South Africa.

Table 6. EMBIG Data: Summary Results for "Levels Test"

	Eichengreen-Mody Rejections Indicating ...		Alternative model A: Rejections Indicating ...		Alternative model B: Rejections Indicating ...	
	Significant increase of spreads 1/	Significant decline of spreads 2/	Significant increase of spreads 1/	Significant decline of spreads 2/	Significant increase of spreads 1/	Significant decline of spreads 2/
Argentina	5	0	6	0	27	0
Brazil	6	0	6	0	26	0
Bulgaria	6	0	6	0	25	0
China	0	4	0	12	11	9
Colombia	3	0	4	0	26	0
Croatia	0	2	0	1	19	0
Korea	6	2	6	4	12	3
Malaysia	0	4	0	2	14	8
Mexico	5	1	5	1	26	0
Morocco	4	0	5	0	26	0
Panama	0	5	0	1	25	0
Peru	6	0	6	0	26	0
Philippines	4	1	6	1	17	5
Poland	3	2	2	1	24	1
South Africa	6	1	4	1	26	0
Thailand	6	3	6	1	17	2
Turkey	3	0	4	0	26	0
Venezuela	4	0	1	0	27	0
Sum rejections	67	25	67	25	400	28
Number of countries showing rejection	14	10	14	10	18	6

1/ No. of periods in which fitted spread based on postcrisis model is significantly *higher* than fitted spread based on pre-crisis model (potential maximum: 27, significance level 5%).
2/ No. of periods in which fitted spread based on postcrisis model is significantly *lower* than fitted spread based on pre-crisis model (potential maximum: 27, significance level 5%).

Table 7. EMBIG Data: Cross-Sectional Variances of Fitted Spreads Before and After Russian Crisis, and Results for "Variance Test"

		(1) Eichengreen-Mody			(2) Alternative Specification (A)			(2) Alternative Specification (B)		
		Fitted variance using coefficients estimated ...		Test for equality	Fitted variance using coefficients estimated ...		Test for equality	Fitted variance using coefficients estimated ...		Test for equality
Month	Actual Variance 1/	before crisis 1/	after crisis 2/	(p) 3/	before crisis 1/	after crisis 2/	(p) 3/	before crisis 1/	after crisis 2/	(p) 3/
1998:01	14474	9710	43731	***0.000***	10139	42992	***0.000***	11109	52238	***0.000***
1998:02	11714	9676	41690	***0.000***	9892	41513	***0.000***	11176	52856	***0.000***
1998:03	8909	9199	38662	***0.000***	9156	38260	***0.000***	10643	51417	***0.000***
1998:04	10099	8549	28118	***0.000***	7948	30750	***0.000***	10064	45233	***0.000***
1998:05	14559	8525	27828	***0.000***	7913	30764	***0.000***	10032	43850	***0.000***
1998:06	20586	8503	27889	***0.000***	7888	31006	***0.000***	10003	42743	***0.000***
1999:04	53108	8216	42717	***0.000***	9351	39337	***0.000***	10465	45869	***0.000***
1999:05	80188	8236	44277	***0.000***	9447	40460	***0.000***	10495	47062	***0.000***
1999:06	67609	8405	46932	***0.000***	9714	42610	***0.000***	10778	48586	***0.000***
1999:07	78276	8006	42754	***0.000***	9161	38880	***0.000***	10304	47063	***0.000***
1999:08	95792	7847	43728	***0.000***	9062	39696	***0.000***	10441	48638	***0.000***
1999:09	66819	8218	46344	***0.000***	9527	42477	***0.000***	11200	50501	***0.000***
1999:10	50033	8361	49467	***0.000***	9833	45244	***0.000***	11406	51572	***0.000***
1999:11	52809	8592	51260	***0.000***	10158	47080	***0.000***	11736	53015	***0.000***
1999:12	38487	8985	53784	***0.000***	10702	49515	***0.000***	12400	55338	***0.000***
2000:01	48464	9272	49832	***0.000***	10622	51334	***0.000***	12208	50902	***0.000***
2000:02	38017	9373	48124	***0.000***	10587	50158	***0.000***	12490	50528	***0.000***
2000:03	42238	9441	46617	***0.000***	10516	48860	***0.000***	12677	49575	***0.000***
2000:04	52401	8119	33336	***0.000***	8146	40705	***0.000***	13237	47214	***0.000***
2000:05	57975	7984	31876	***0.000***	7932	39272	***0.000***	13393	46736	***0.000***
2000:06	47930	7996	31616	***0.000***	7991	39202	***0.000***	13498	46753	***0.000***
2000:07	43618	8000	31526	***0.000***	8003	38599	***0.000***	13497	46470	***0.000***
2000:08	41079	8007	31385	***0.000***	8015	38377	***0.000***	13500	46192	***0.000***
2000:09	49346	8016	31356	***0.000***	8031	38232	***0.000***	13518	46476	***0.000***
2000:10	63240	7984	31981	***0.000***	8084	39041	***0.000***	13365	46428	***0.000***
2000:11	78292	8263	33196	***0.000***	8413	41037	***0.000***	13819	47014	***0.000***
2000:12	64069	8272	33360	***0.000***	8435	41237	***0.000***	13831	46846	***0.000***

1/ Regression coefficients estimated on 1998:01 to 1998:06 data.
2/ Regression coefficients estimated on 1999:04 to 1999:12 data.
3/ p values based on two-sided tests.

Table 8. Robustness Checks

Model	Exclusion Period?	Controling for EMBIG Composite?	Lagged Dependent Variable?	Estimator	"Slope" Test	"Levels" Test			"Variance" Test
					Country-specific variables showing significant absolute increase in coefficient	Countries showing significant ...			Periods showing significant rejection
						increase only	mixed	decrease only	
(1)	original	no	no	OLS robust	real growth, real credit, Latin America dummy, ratings residual	12	6	0	all
(2)	original	no	no	GARCH(1,1)	real growth, real credit, Latin America dummy	9	9	0	all
(3)	original	yes	no	OLS robust	fiscal balance, current account, real credit, Latin America dummy, ratings residual	n.a.	n.a.	n.a.	all
(4)	original	yes	no	GARCH(1,1)	real growth, real credit, Latin America dummy	n.a.	n.a.	n.a.	all
(5)	original	yes	yes	OLS robust	(none)	n.a.	n.a.	n.a.	all
(6)	original	yes	yes	GARCH(1,1)	(none)	n.a.	n.a.	n.a.	0
(7)	none	no	no	OLS robust	real growth, real credit, Latin America dummy, ratings residual	13	5	0	all
(8)	none	no	no	GARCH(1,1)	real growth, Latin America dummy, ratings residual	12	6	0	all
(9)	none	yes	no	OLS robust	fiscal balance, real credit, Latin America dummy, ratings residual	n.a.	n.a.	n.a.	all
(10)	none	yes	no	GARCH(1,1)	Latin America dummy	n.a.	n.a.	n.a.	all
(11)	none	yes	yes	OLS robust	Latin America dummy, ratings residual	n.a.	n.a.	n.a.	all
(12)	none	yes	yes	GARCH(1,1)	(none)	n.a.	n.a.	n.a.	0

Table 9. Launch Spread Data: Estimation Results Before and After Mexican and Asian Crises, and "Slope Test" 1/

	(1) Mexican Crisis					(2) Asian Crisis				
	before crisis 2/		after crisis 3/		Test for equality	before crisis 4/		after crisis 5/		Test for equality
Variable	Coef.	*p*	Coef.	*p*	*p* 6/	Coef.	*p*	Coef.	*p*	*p* 6/
Constant	219.2	*0.27*	305.1	*0.01*	*0.71*	429.6	*0.00*	-11800.6	*0.13*	*0.11*
Real growth (MA)	-15.15	*0.00*	-11.50	*0.00*	*0.45*	-13.22	*0.00*	49.03	*0.00*	*0.00*
External debt/GDP	-0.01	*0.86*	-0.10	*0.09*	*0.41*	-0.12	*0.20*	-0.86	*0.28*	*0.35*
Brady dummy	70.37	*0.03*	152.57	*0.00*	*0.02*	115.64	*0.00*	64.31	*0.23*	*0.35*
Rating (residual)	-25.47	*0.00*	-24.48	*0.00*	*0.84*	-26.46	*0.00*	-34.67	*0.00*	*0.25*
US 10-year yield	2.89	*0.82*	-50.11	*0.00*	*0.01*	-56.34	*0.00*	1756.61	*0.12*	*0.11*
US high-yield bond spread	-0.67	*0.98*	57.12	*0.00*	*0.15*	53.40	*0.01*	692.95	*0.17*	*0.21*
F test (*p*)					*0.00*					*0.00*
Observations in subsample	253		575			612		204		
Observations with bond data	46		175			140		24		
Number of countries 7/	23		23			34		34		

Selection equation 8/	Coef.	*p*	Coef.	*p*
Debt issued in preceding year	-2.07	*0.03*	-1.28	*0.16*
Number of previous bond issues	0.12	*0.00*	0.11	*0.00*
GDP per capita (1993)	-0.06	*0.62*	0.04	*0.79*
rho	-0.25	*0.10*	-0.60	*0.00*

1/ Using model (1) of Table 2 in both cases. Regression (1) is based on the sample 1994:01 - 1997:06, regression (2) on the sample 1996:01 - 1998:06. All estimations use robust standard errors.
2/ 1994:01 - 1994:11
3/ 1995:06 - 1997:06
4/ 1996:01 - 1997:06
5/ 1998:01 - 1998:06
6/ *p* values based on two-sided tests.

7/ Sample in model (1): Argentina, Brazil, Chile, China, Colombia, Cyprus, Hong Kong SAR, Hungary, India, Indonesia, Israel, Korea, Malaysia, Malta, Mexico, the Philippines, Singapore, Taiwan Province of China, Thailand, Trinidad and Tobago, Turkey, Uruguay, and Venezuela. Sample in model (2): Argentina, Brazil, Chile, China, Colombia, Cyprus, the Czech Republic, Hong Kong, Hungary, India, Indonesia, Israel, Jordan, Korea, Malaysia, Malta, Mauritius, Mexico, Pakistan, Peru, Philippines, Poland, Romania, Saudi Arabia, Singapore, the Slovak Republic, South Africa, Taiwan, Thailand, Trinidad and Tobago, Tunisia, Turkey, Uruguay, and Venezuela.

8/ Reports only coefficients for instruments and correlation coefficient of disturbance terms of the two equations (rho).

Table 10. Launch Spread Data: Summary Results for "Levels Test," Mexican and Asian Crises

	Mexican Crisis			Asian Crisis		
	Rejections Indicating ...		Average	Rejections Indicating ...		Average
	significant increase of spreads 1/	significant decline of spreads 2/	Change in bp (only significant) 3/	significant increase of spreads 1/	significant decline of spreads 2/	Change in bp (only significant) 3/
Argentina	10	0	152	0	0	...
Brazil	10	0	137	0	0	...
Chile	4	0	105	6	0	290
China	5	0	108	11	0	1206
Colombia	2	0	109	2	0	139
Cyprus	3	0	102	0	0	...
Czech Republic	...	...	...	0	0	...
Hong Kong SAR	1	0	101	0	0	...
Hungary	0	0	...	0	0	...
India	2	0	109	6	0	253
Indonesia	3	0	106	6	0	310
Israel	3	0	105	4	0	162
Jordan	...	...	...	0	0	...
Korea	4	0	108	6	0	288
Malaysia	5	0	105	6	0	364
Malta	3	0	103	2	0	140
Mauritius	...	...	...	4	0	155
Mexico	9	0	150	0	0	...
Pakistan	...	...	...	2	0	139
Peru	...	...	...	6	0	266
Philippines	10	0	138	0	0	...
Poland	...	...	...	4	0	183
Romania	...	...	...	0	0	...
Saudi Arabia	...	...	...	0	0	...
Singapore	1	0	112	1	0	962
Slovak Republic	...	...	...	5	0	216
South Africa	...	...	...	0	0	...
Taiwan Province of China	3	0	115	5	0	200
Thailand	5	0	104	5	0	176
Trinidad and Tobago	1	0	111	2	0	108
Tunisia	...	...	...	4	0	131
Turkey	3	0	104	3	0	158
Uruguay	10	0	147	0	0	...
Venezuela.	10	0	150	0	0	...
Sum rejections	107	0		90	0	
No. countries showing rejection	22	0		20	0	
Total number of countries	23			34		

1/ Periods in which fitted spread based on post-crisis model is significantly *higher* than fitted spread based on pre-crisis model.
2/ Periods in which fitted spread based on post-crisis model is significantly *lower* than fitted spread based on pre-crisis model.
3/ Estimated average increase or decrease of spreads in basis points for all significant increases/decreases.

Table 11. Launch Spread Data: Cross-Sectional Variances of Fitted Spreads Before and After Mexican and Asian Crises, and Results for "Variance Test"

	(1) Mexican Crisis			(2) Asian Crisis		
	Fitted variance using coefficients estimated ...		Test for equality	Fitted variance using coefficients estimated ...		Test for equality
Quarter	before crisis 1/	after crisis 2/	(p) 3/	before crisis 1/	after crisis 2/	(p) 3/
1994:01	6947	10654	*0.089*			
1994:02	6917	10613	*0.089*			
1994:03	6575	10339	*0.072*			
1994:04	6939	10655	*0.090*			
1994:05	7042	10795	*0.093*			
1994:06	7258	10958	*0.106*			
1994:07	7342	11108	*0.104*			
1994:08	7342	11108	*0.104*			
1994:09	7342	11108	*0.104*			
1994:10	7342	11108	*0.104*			
1994:11	7186	10904	*0.101*			
1995:06	7759	11540	*0.116*			
1995:07	7659	11397	*0.116*			
1995:08	7659	11397	*0.116*			
1995:09	7619	11385	*0.111*			
1995:10	7429	11287	*0.095*			
1995:11	7468	11343	*0.095*			
1995:12	7521	11412	*0.095*			
1996:01	7774	11591	*0.115*	9165	27584	*0.215*
1996:02	7658	11495	*0.110*	9038	27210	*0.219*
1996:03	7658	11495	*0.110*	9038	27210	*0.219*
1996:04	7658	11495	*0.110*	8965	27497	*0.212*
1996:05	7739	11596	*0.111*	9047	27607	*0.212*
1996:06	7739	11596	*0.111*	9047	27607	*0.212*
1996:07	7739	11596	*0.111*	9047	27607	*0.212*
1996:08	7739	11596	*0.111*	9047	27607	*0.212*
1996:09	7739	11596	*0.111*	8961	27285	*0.214*
1996:10	7668	11566	*0.104*	8937	27462	*0.211*
1996:11	7668	11566	*0.104*	9028	27644	*0.210*
1996:12	7759	11660	*0.107*	9097	27948	*0.206*
1997:01	7659	11712	*0.091*	9090	22348	*0.237*
1997:02	7587	11577	*0.093*	9029	22348	*0.235*
1997:03	7749	11701	*0.102*	9122	22506	*0.233*
1997:04	7426	11250	*0.101*	8851	22492	*0.225*
1997:05	7412	11218	*0.103*	8880	22321	*0.231*
1997:06	7194	10829	*0.109*	8705	22703	*0.216*
1998:01				8723	21138	*0.243*
1998:02				8682	20492	*0.259*
1998:03				8601	20544	*0.251*
1998:04				8494	20678	*0.244*
1998:05				8759	21234	*0.237*
1998:06				8938	21748	*0.230*

1/ Regression coefficients estimated on the basis of precrisis data (see Table A1).

2/ Regression coefficients estimated on the basis of postcrisis data (see Table A1).

3/ p values based on two-sided tests.

Table 12. List of Variables

Variable Name	Variable Description	Unit	Frequency	Source
Arrears dummy	= 1 if Arrears/total debt > 5% in any of the past 3 years	Dummy	A	GDF
Asia dummy	= 1 if country is in Asia	Dummy		Own calculations
Brady dummy	= 1 if Brady debt > 0 at some point	Dummy		BIS
Current account	Current account / GDP, lagged, MA refers to 4-year moving average	Percent	A	IFS
Debt issued in preceding year	Total amount of bonds issued in the past 4 quarters/ total debt at the beginning of the first quarter	Million USD	Q	BIS (locational), Bondware
Dummy for Asian crisis countries	= 1 if Asian crisis country (Thailand, Indonesia, Korea, Malaysia, Philippines)	Dummy		Own calculations
External debt/GDP	External debt / GDP, lagged	Percent	A	BIS, IFS
Fiscal balance	Fiscal balance / GDP, lagged	Percent	A	IFS
GDP per capita (1993)	Logarithm of PPP adjusted GDP per capita in 1993	GDP in USD		WEO
High yield	Yield of Merrill Lynch J0A0 index (US high-yield corporations with below investment grade rating), end of month (EMBIG) or monthly average (Bondware)	percent per annum	M	Bloomberg
Inflation	Consumer price inflation, lagged	Percent	M	IFS
Latin dummy	= 1 if Western Hemisphere	Dummy		Own calculations
LIBOR	LIBOR, monthly average (EMBIG) or end of month (Bondware)	Percent	M	IFS, Bloomberg
Number of previous bond issues	Number of bond issues in the past year		Q	Bondware
Political instability and violence	"Instability and violence" in 1997	Index, -2.5 (very unstable), 2.5 (very stable)		World Bank Governance Database
Rating	= average of available ratings or only available rating	Index (1 = Caa3/CCC-, 19 = Aaa/AAA)		Standard and Poor's, Moody's
Rating (residual)	Residual from regression of ratings on fundamentals (cf. ratings)			Own calculations
Real credit growth	Real domestic credit growth, lagged, MA refers to 4-year moving average	Percent	M	IFS
Real growth	Real GDP growth, lagged, MA refers to 4-year moving average	Percent	A	IFS
Short-term Debt/Total Debt	Short-term debt / total debt, lagged	Percent	SA	BIS (consolidated)
Size	Log of nominal GDP in US$ in 1993	GDP in US$	A	IFS
U.S. high-yield bond spread	= High yield – LIBOR	percent per annum	M	Bloomberg
U.S. 10-year yield	Yield of 10-year US government bonds, end of month (EMBIG) or monthly average (Bondware)	percent per annum	M	IFS, Bloomberg

Notes: BIS = Bank for International Settlements
GDF = Global Development Finance (World Bank)
IFS = International Financial Statistics (IMF)
INS = Information Notice System (IMF)
WEO = World Economic Outlook Database (IMF)

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