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Sovereign default risk, the IMF and creditor moral hazard

Ilan Noy *

Department of Economics, University of Hawaii, Manoa, 2424 Maile Way, Honolulu, HI 96822, United States

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Abstract

The IMF potentially creates moral hazard when it provides bailouts to countries in a financial crisis. We ask whether a creditor moral hazard is observable in the data. We test the hypothesis that recent unprecedented bailouts – starting with the 1994 Mexican crisis – changed international investors' perception of default risk on international borrowing. Our events-study approach identifies important and unexpected IMF-related events and examines the dynamics of the unexplained component of the risk premia on sovereign bonds surrounding the identified events. In contrast with many policy discussions, we conclude that no change in the moral hazard effect is observed for the last decade.

JEL classification: F33; F34; G14

Keywords: Event study; International Monetary Fund; Moral hazard

1. Introduction

The last decade has been a decade of spectacular currency, balance-of-payments and banking crises and equally extravagant and controversial bailouts by the International Monetary Fund (IMF). The problem of moral hazard in international crisis lending has consequently become very prominent in policy and academic discussions. A concern with moral hazard was one of the principal issues discussed in the Meltzer Commission's report to the U.S. Congress on the International Financial Institutions (Meltzer, 2002).[1]

* Tel.: +1 808 956 7749; fax: +1 808 956 4347.
E-mail address: noy@hawaii.edu.

[1] The term 'bailout' is used here, as elsewhere in the literature, even though these support packages are subsidized loans that are almost always repaid on time. The magnitude of the subsidy is debatable since IMF loans face a different default

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Evidently, criticism of the IMF is widespread; and moral hazard is only one of several reasons why the IMF faces such opposition from professionals and the public and coming from different political persuasions. Nevertheless, moral hazard remains the most prominent reason for criticism coming from several prominent researchers and policymakers (e.g., Calomiris, 2000; Bordo and Schwartz, 2000; and Niskanen, 1999). Yet, in spite of numerous policy discussions on the topic, the available empirical work on this topic remains inconclusive.

The current literature differentiates between moral hazard on the creditors' and on the debtors' sides. From the debtors' perspective, the expected insurance/bailout enables domestic borrowers to increase their risk exposure beyond the optimal level, as, in case of a negative shock that will leave them unable or unwilling to repay in full, they will be at least partially bailed out.[2] Here, we focus on the other side of moral hazard in international bailouts—namely the creditors' moral hazard effect. If creditors believe that there is a significant positive probability that they will be bailed out, the risk premium they demand changes.

An (implied) insurance of bond issues or inter-bank lending can lead to the following: (1) an increase in the amount lent; (2) a decrease in the price of loans so that it no longer reflects insurance-free risk; (3) a change in the composition of investment away from uninsured investment (e.g., equity) to insured flows (e.g., sovereign bonds); and (4) a change in the composition of international portfolios away from less risky but less profitable investment opportunities to more risky but more profitable ones if outcomes are positive.

The moral hazard effect is clearly potentially important. It might imply that IMF-led bailouts lead to sub-optimal equilibria in which there is both a dead-weight-loss and a redistribution of resources away from domestic or foreign taxpayers (who pay for the loan and its subsidy) to the international creditors that are bailed out. Detractors of the IMF and the large bailouts it had orchestrated occasionally blame the very occurrence and severity of the recent crises on these moral hazard effects.[3]

From a positive perspective, a theoretically tight argument for the presence of moral hazard as a result of international lending does not imply that the moral hazard effect is necessarily of major import. It thus becomes apparent that the importance of the moral hazard issue should be treated as an *empirical* question.[4]

We test the available data for empirical evidence of a moral hazard effect as a result of large international post-crisis bailouts. The literature that deals with this empirical question finds conflicting results. While Lane and Phillips (2000) and Kamin (2002) do not find much evidence of a significant moral hazard effect, Dell'Ariccia et al. (2000) interpret similar data differently

risk than private or other public lending. For analysis of repayment experiences to the IMF and the subsidy involved, see Aylward and Thorne (1998) and Zettelmeyer and Joshi (2005).

[2] Dreher and Vaubel (2004) is one of only a few empirical papers to examine this issue.

[3] For discussions of these issues see, for example: Eichengreen, 2000; Jeanne and Zettelmeyer, 2005; Lerrick and Meltzer, 2003; Mussa, 1999; and Rogoff, 1999).

[4] The normative significance of moral hazard concerns is outside the scope of this paper. Normative factors that might outweigh these concerns in the decision whether to intervene can be the costs of refusing a bail-outs to the debtor country, costs to the creditors in refusing to bail them out or possible spill-over effects from default. Also, exogenous constraints that might make moral hazard concerns irrelevant might be regulatory and institutional constraints on IMF intervention or international or domestic political-economy issues related to the demand or supply of bailouts. Furthermore, there is a clear trade-off between moral hazard concerns and other considerations so it might be the case that the socially optimal level of moral hazard is positive (i.e., in a second-best world, it might be optimal for a multilateral financial institution that maximizes world welfare to choose to create a moral hazard effect by supplying insurance). Some of the normative aspects are examined in Atkeson (1991), Cordella and Levy-Yeyati, 2004, Corsetti et al. (2003), Döbeli and Vanini (2004), and Powell and Arozamena (2003).

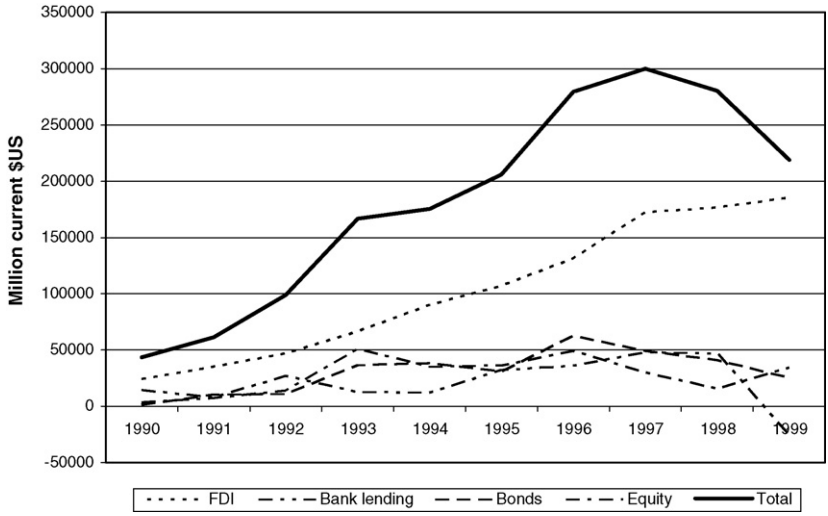

Fig. 1. Capital flows for developing countries—by types.

and conclude that there is a noticeable moral hazard effect in international debt markets. Dreher (2004) provides a recent survey of this empirical literature.

We use event-study methodology, adapted from the corporate finance literature, and a different set of aggregated data than was used before. Furthermore, we examine other possible explanations that might account for our empirical findings. Thirdly, we exploit newer data – most importantly surrounding the December 2001 Argentine sovereign default – to provide further evidence supporting our findings.

Critics of IMF bailouts cite the unprecedented scale of the Mexican bailout of 1995 as the watershed in moral-hazard-inducing bailouts. Accordingly, we should observe changes in international capital markets following that event and the introduction of even larger bailout programs in East Asia in 1997–1998. In Fig. 1, we present data on the volume of international capital flows in the 1990s. We observe a very pronounced increase in capital flows up to and including 1997. But, when divided into its various components, almost all of this increase is attributed to a boom in foreign direct investment. Bond issues, flows that are most likely to 'enjoy' implicit insurance through those bailouts, do not show any marked trend over that time period. Bank lending, which also seems to be a (partially) 'insured' type of flow does show a trend that is consistent with a moral hazard hypothesis. In annual data, between 1995 and 1997 bank lending increased significantly. Then, it leveled off in 1998 and dropped precipitously in 1999.

By decomposing flows by regions, we observe that much of the volatility in bank lending is driven by local trends and does not represent a global phenomenon. Bank lending to East Asia started dropping in 1997 and inflows turned to large outflows in 1998. In contrast, bank lending peaked dramatically in 1998 for Latin America but bank flows dried up completely in 1999. This contrast seems to contradict the standard moral hazard hypothesis that imply similar changes in behavior of flows worldwide in reaction to perceived changes in the IMF's lending policy. Bond flows appear to be even more volatile and a moral hazard framework cannot explain trends there either. The absence of any evidence on the moral hazard effect on quantities leaves us with the possibility that moral hazard manifested itself in its effect on the *price* of insured flows, namely spreads. These spreads are the focus of our empirical estimations.

Using a different methodology and different aggregated data, we contribute in a few distinct ways: First, by using an events study methodology and a different dataset, we exploit higher-powered tests to examine the moral hazard hypothesis. Second, by looking in some detail at changes in market perceptions around the time of events we suggest a different interpretation of results. Finally, by using data surrounding the IMF's decision to support and subsequently to abandon Argentina, we are able to shed additional light on the extent of moral hazard during the last decade.

2. The literature

A rapidly growing theoretical literature on currency crises deals with the moral hazard issue. Dooley (2000), in one of the earliest contributions, describes an insurance model and sees the implied insurance (partially financed by these rescue packages) as the driving force behind the outpouring of capital flows into emerging markets and the reversal of these flows that is occasioned by shifting expectations as the defining moment of the crises themselves.[5] More recent papers that incorporate moral hazard explicitly into an international model of lending are Aizenman and Turnovsky (2002), Corsetti et al. (2003), Dekle and Kletzer (2001), Döbeli and Vanini (2004), Jeanne and Zettelmeyer (2005), and Powell and Arozamena (2003).

A number of empirical papers have looked at the determinants of emerging markets' spreads, but do not look specifically at the moral hazard question. Nevertheless, their methodology and findings provides us with a benchmark from which to develop a more complete identification of the determinants-of-spreads model parameters that we require for our empirical exercise. Eichengreen and Mody (2002), in the model closest to ours, use both macroeconomic variables and the orthogonal component of publicly available credit ratings as determining spreads.[6]

The implied assumption is, of course, that while spreads are determined by the perceived level of risk associated with each bond these are in turn determined by whatever is observable to market participants at the time of issue or of trade (for secondary market spreads). In our work, we investigate the resulting estimation errors, which we cannot attribute to these observable fundamentals. We associate these errors with a moral hazard effect.

Eichengreen and Mody (2002) try to explain the continuous drop in spreads following the Mexican crisis all the way up to the summer of 1997. They suggest that more liberal financial market conditions in lending countries and a possible arbitrary shift in pricing behavior (an irrational exuberance) on the part of international lenders; both led to decreasing spreads. Their result seems to tie in with the Mauro et al. (2002) argument that investors, at least up to August 1998, seem to be paying less attention to country specific information.

Zhang (1999) and Lane and Phillips (2000) are the first papers that attempt to look directly at the magnitude of the moral hazard that results from IMF financing (bailouts). As they interpret it, their preliminary work suggests that even if there is an IMF moral hazard effect, its magnitude is not very significant. They base their findings on an examination of secondary market spreads for dollar denominated bonds following a series of events they identify as affecting the perception of international investors on available financing for bail-outs.

[5] The insurance has to be accompanied by lax regulation that enables domestic borrowers (banks) to siphon off (maybe to off-shore centers) some of these borrowed monies.

[6] Other papers that examine spreads are: Min, 1998; Larraín et al., 1997; Cline and Barnes, 1997; Kamin and von Kleist, 1999; and Mauro et al. (2002).

In almost all the events they identify they do not discern any significant change in the time series of spreads (defined as a change of more than one standard deviation). Lane and Phillips (2000) argue that as IMF financing is small (as ratio to GDP or to external debt stock), and since financing from other sources is limited as well, it is not clear to investors whether they will not be too far back in the queue. The perceived bailout probabilities (a moral hazard effect) will thus only be indirectly incorporated into the perceived default risk if a possible bailout affects the probability of default *ex ante*. Notably, Lane and Phillips (2000) do find a significant movement of spreads following the Russian default (and the glaring absence of an IMF bail-out in that case).

Dell'Ariccia et al. (2000) focus on the Russian default of August 1998. Their empirical model is based on a standard bond-spread determination model. Having developed more powerful tests, they interpret their evidence on the Russian default as showing that a significant moral hazard effect existed prior to the default (i.e., there was a perceptible shift in market sentiments as regarding the likelihood of future defaults).

As Lane and Phillips (2000) only look at the statistical properties of the spreads' series, Dell'Ariccia et al.'s (2000) modeling technique is more convincing. Yet, Dell'Ariccia et al. (2000) note that in order for their testing strategy to be valid the event they identify (the Russian default) has to satisfy three conditions: (1) it has to change investors' perceptions on the likelihood or the extent of future bailouts; (2) it has to be unexpected; and (3) it must not lead to a reassessment of risks other than through the expectations of future international rescues. Whether the third condition holds for the Russian case appears debatable. Dell'Ariccia et al. (2000) plausibly claim that the Asian crisis of the previous year should have been sufficient to 'wake up' investors to the dangers of lending to emerging markets so that the Russian default did not contain any new information besides the absence of a bailout. Additionally, they note that the unraveling of the Russian stabilization program and the consequent default, a classic fiscal crisis in a fairly small economy that is not tightly connected to the rest of the world, would not have contained any information relevant to other emerging markets.

Nevertheless, Dell'Ariccia et al.'s usage of infrequent data (quarterly) and large window (three quarters) also assumes that the Russian crisis was the only event that affected bond spreads systematically between August 1998 and March 1999. Yet, in the weeks following the Russian default two important and equally unexpected events occurred: the long-term-capital-management (LTCM) crisis which was partially resolved once the New York Fed orchestrated a private sector bailout, and a dramatic revision downward in expectations for the world economy for 1999.

Dungey et al. (2002) examined both the Russian Crisis and LTCM and concluded that the LTCM crisis was a much larger liquidity shock that affected countries' spreads more than the Russian default. Thus, the Dell'Ariccia et al. (2000) findings, which they interpret as a weakening of the pre-Russian-default moral hazard, can equally be interpreted as caused by the massive decrease in liquidity as a result of the LTCM crisis.[7]

Kamin (2002) concludes that both price (spread) and quantity data do not seem to indicate a significant moral hazard effect.[8] More recently, Lee and Shin (2004), dispute these results and

[7] Dell'Ariccia et al. (2002) do note that ". . .results should be interpreted as confirming a necessary, but not sufficient condition for the presence of moral hazard." (p. 7).

[8] Kamin (2002) does not distinguish between different types of capital flows. One would expect to see an increase for those types of flows that are more likely to be indeed bailed out. As already demonstrated above, even after differentiating between different flows one cannot find strong support for a moral hazard hypothesis.

find that countries with stronger political connections to the IMF were perceived as more likely to be bailed out.[9],[10]

3. Event-study methodology

Financial event-study methodology is outlined in Campbell et al. (1997). By regressing a panel data set that contains standard spread determination variables we obtain estimated coefficients for our variables. We then use those estimated coefficients to construct estimated 'abnormal' spreads around event months and examine their statistical properties.

We use the Lane and Phillips (2000) identification of events and group them into four major moral hazard inducing events: (1) 1/1995: The Mexican Program; (2) 8–12/97: Thai, Indonesian and Korean Program, IMF Board's approval of new Structural Reserve Facility and a suggestion of quota increase; (3) 10–12/98: U.S. Congress ratifies quota increase and the IMF approves a large program (600% of quota) for Brazil; and (4) 3/2000: Large Argentine Stand-By approved. We exclude a 4 month window after each event and therefore estimate our 'normal' model on the following periods: 1–11/1994, 6/1995–6/1997, 5–6/1998, 5/1999–1/2000, 6–12/2000.

We take the probability of a balance-of-payments crisis to be a function of the country and time-specific macroeconomic characteristics (X_{it}) and the external environment (X^{W}). The probability of being repaid if a crisis occurred is assumed to be a function of the amount of resources available for repayment (reserves, and existing credit lines—X^{RES} and the likelihood of IMF bailouts (p^{IMF}). Initially, and following previous practice, we assume a linear structure for the spread determination model:

$$s_{\mathrm{it}} = \beta_i + X_{\mathrm{it}}\beta_1 + X_t^{\mathrm{W}}\beta_2 + X_{\mathrm{it}}^{\mathrm{RES}}\beta_3 + u^{\mathrm{IMF}} \tag{1}$$

Where $u_{\mathrm{it}}^{\mathrm{IMF}}$ is an error term that should negatively depend on p^{IMF}. We estimate this model for all the estimation window observations ($T_{-1} \times I \times N$ observations).[11] We use the estimated coefficients from this sample to find the parameters of a 'normal spreads' model, i.e., Eq. (1).[12]

We then construct

$$\hat{u}_{\mathrm{it}}^{\mathrm{IMF}} = s_{\mathrm{it}} - X'\hat{\beta} \tag{2}$$

for all the event window data ($T_0 \times I \times \mathrm{N}$ observations). We examine the properties of the error term $\hat{u}_{\mathrm{it}}^{\mathrm{IMF}}$ around the event months.[13] By assuming that our normal spreads determination model is fully specified we derive conclusions on the probability of payment variable by looking at the statistical qualities of the estimated error term around the events days.

[9] Lee and Shin (2004) hypothesize that the moral hazard effect should be stronger for countries that are identified more closely with the IMF. Their 'proximity' to the IMF measure is based on UN voting patterns (as they relate to US voting patterns) and bi-lateral trade flow volumes with the IMF's major shareholders.

[10] Additional papers that examine the IMF's effect on equity markets are Haldane and Scheibe (2004) and Evrensel and Kutan (2006).

[11] I is the number of countries and N the number of events. T_{-1} is the estimations window, T_0 is the event window, and T_1 is the post event window.

[12] This procedure insures that our estimates are not biased because of the changes following these events.

[13] This methodology prevents at least one of the problems noted by Dell'Ariccia et al. (2002); namely that a change in IMF policy that might change the magnitude of the moral hazard effect can be expected to change the relationship between other macro variables and the spread level (our LHS variable).

A Durbin–Watson statistic for all iterations of the model strongly indicates that the error terms are autocorrelated.[14] We therefore estimate the model using the Prais–Winsten algorithm. The Prais–Winsten procedure is a two-step FGLS procedure that utilizes the estimated correlation coefficient obtained from the Durbin–Watson statistic from the first-stage OLS regression as the initial autocorrelation value and reiterates the second step FGLS using the whole sample till convergence (typically 2–3 iterations).[15]

4. Data

For the spreads data, we use indices of emerging market spreads for secondary-market sovereign bonds (US$ denominated) over US treasury bills. These indices, the EMBI, are available from *JP Morgan*.[16] The EMBI is uniform across time and between countries. The countries included in the EMBI data set are: Argentina, Brazil, Bulgaria, Colombia, Ecuador, Korea, Mexico, Morocco, Panama, Peru, Philippines, Poland, Russia, Turkey, and Venezuela. As daily data is volatile, we prefer to use an average of daily rates. We use monthly data—which will smooth the data sufficiently but still maintain enough information.[17]

A preliminary examination of the data series reveals some interesting observations. A graph of all the monthly country-series reveals that spreads peaked first during the beginning of 1995 (the Mexican crisis), and were then followed by a continuous and gradual decline of spreads for almost all countries up to October 1997 (Fig. 2). The Asian crisis was followed by only a small increase in spreads (relative to the Tequila crisis) and by the second quarter of 1998 spreads have gone back to their pre-crisis levels. The rise in spreads was much larger and across-the-board during mid 1998 and the Russian default. Increases were especially high for Ecuador, Russia and Venezuela but all other countries (excluding Poland) experienced large increases comparable to the ones in 1995. This period was again followed by a period of decline but even at the end of 2000 spreads were still, in general, higher and more dispersed than their lowest levels of mid 1997.

Some of the macroeconomic data we use in our spread-determination model are not available monthly. Still, the model assumes the affect of macroeconomic data on spreads through its affect on expected default probabilities (through the information set available to investors). Thus, using quarterly data in a monthly panel for some of the variables does not pose a conceptual problem. We require the variables observed by international investors when they determine the likelihood of default and if these are available only in quarterly frequency they should be sufficient—provided attention is paid to when variables become public knowledge.

[14] Durbin–Watson statistic is less than 0.5 and the estimated ρ is 0.8.

[15] For technical details see Greene (2000, pp. 546–550) and Greene (2002, E7 pp. 4–7). The standard errors might be weakly estimated if there is a problem of heteroskedasticity. The events analyzed might involve a brief but very large change in the variance. Since we identify only a small number of very large events, a standard heteroskedasticity correction is irrelevant. This might reduce the estimations' power.

[16] An alternative source is the data on issues of individual bonds (both commercial and sovereign) from the *Bondware* database. Selectivity bias is a problem with the *Bondware* data as the decision whether to issue debt is endogenous and dependent on the same variables that supposedly affect spreads (Dell'Ariccia et al., 2002).

[17] There is a tradeoff here. Using daily data will significantly increase the power of tests that use event-study methodology. Our problem is both that none of the data we use for our spread determination model is readily available on a daily or weekly frequency (or if it is it does not change much) and that the event timing is not entirely clear. We are not sure when did markets become aware of the impending event—be it a bailout or an absence of one. See Morse (1984) for discussion of using daily vs. weekly or monthly data, and Lee and Varela (1997) for event day uncertainty.

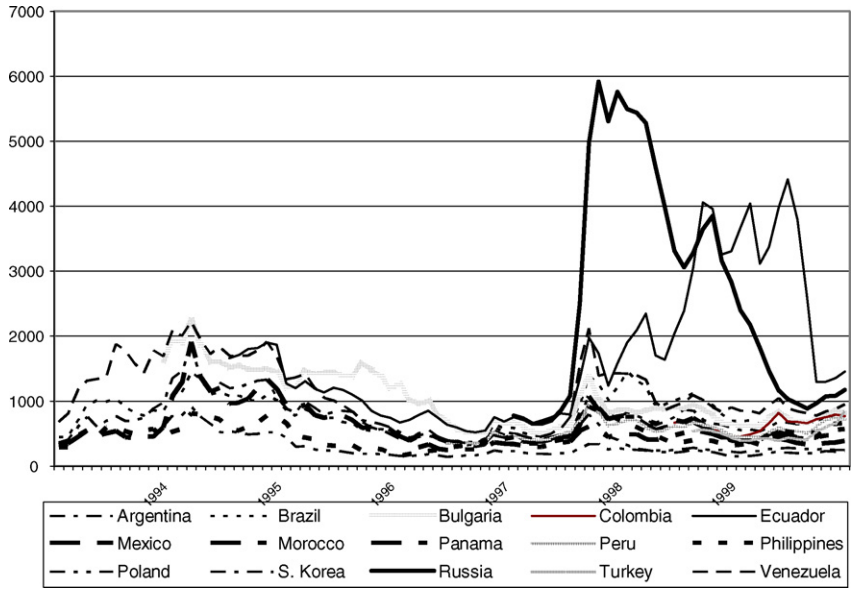

Fig. 2. Monthly country spreads.

Our baseline model includes variables that measure the liquidity of the country and its ability to repay in the short run (debt to GDP, net foreign assets to GDP, reserves to GDP and current account flows to exports), and other macroeconomic variables commonly used to assess creditworthiness (inflation rate, high inflation dummy, fiscal deficit to GDP and export growth). To proxy for institutional and political variables that are used by investors in assessing risk we use Standard & Poor's sovereign credit ratings. As these ratings are correlated with the other macroeconomic variables, we use only the orthogonal component of the credit rating variable. We also use two variables to proxy for the world economic conditions as these affect emerging markets (US Federal Funds rate and developing countries exports' price index).

5. Results

We begin by assuming that market participants possess perfect foresight (i.e., $E_{t-1}X_t = X_t$). We regress our panel for the pre-events periods (estimation window) without lagging any of the explanatory variables. Results are given in Table 1 column (1). Most coefficients have the expected signs and several are significant. But, importantly for our model, the adjusted R^2 from this regression is high (0.64). The relatively high explanatory power will therefore enable us to better identify the abnormal returns in the event-window data.

Using these results we obtain abnormal returns as specified in Eq. (2) for the periods of interest: 2–5/95 - following Mexico, 1–4/98 - following East Asia, 1–4/99 - following Brazil, and 2–5/00 - following Argentina. In the perfect foresight case no evidence of moral hazard is present. In almost all cases the averages of the abnormal returns across countries, and by periods, are positive. A presence of a significant moral hazard effect should have resulted in negative abnormal returns (as the estimated spreads should have been higher than actual spreads). Results are reported in Table 2. These conclusions do not change if we exclude the crisis countries (Mexico, Korea, Philippines, Russia, Brazil and Argentina) during their respective crises (Table 3). Notably,

Table 1
Estimation-window sample

	Perfect foresight	Lagged variables
CAEXP	269.24*** (7.47)	114.83*** (2.77)
DBTGDP	−3453.00*** (3.72)	−450.55 (0.33)
FXRGDP	386.06* (1.76)	475.92* (1.85)
NETFAS	0.14 (0.29)	−0.20 (0.39)
CPI	−0.08 (0.69)	−0.03 (0.24)
CPID	425.26*** (5.84)	546.64*** (6.32)
DLEX	0.08 (0.19)	0.26 (0.56)
GOVGDP	1674.57*** (3.30)	199.97 (0.36)
RESRTNG	−298.99*** (9.74)	−182.50*** (5.95)
EXPP	−26.45*** (4.58)	−17.95*** (2.77)
USRATE	−53.92 (1.05)	−46.40 (0.78)

t-statistics are in parentheses for all coefficient estimates. (***), (*) indicate significant levels at 1 and 10 %, respectively.

Table 2
Abnormal spreads—averages for post event data

	Mean	STD	Max	Min	*N*
Following moral hazard inducing events	107.2	1505.1	2708.2	−1144.8	108
1 month following event	132.4	1203.9	2708.2	−583.3	27
2 months following event	160.0	1209.7	2698.9	−752.8	27
3 months following event	107.0	1243.0	2558.3	−1144.8	27
4 months following event	29.7	1275.2	1883.0	−1107.0	27

Perfect foresight normal model - OLS.

Table 3
Abnormal spreads—averages for post event data

	Mean	STD	Max	Min	*N*
Following moral hazard inducing events	78.8	702.2	2907.5	−1129.7	100
1 month following event	99.7	726.8	2907.5	−793.6	25
2 months following event	113.4	740.7	2865.6	−795.4	25
3 months following event	76.6	731.4	2720.5	−1129.7	25
4 months following event	25.6	646.2	2053.4	−1091.2	25

Perfect foresight model - excluding same-country crisis observations.

outliers do not drive these findings. Almost all, and in some cases virtually all, abnormal returns are positive.

Separately, we compute abnormal returns without relying on the perfect foresight assumption. We regress our baseline model with lagged variables (assuming $E_{t-1}X_t = X_{t-1}$).[18] Results are reported in Table 1 column (2). The predictive power of our equation stays similar. Once more, the averages of the abnormal returns across countries, and by periods, are positive and a presence of a moral hazard effect is not detected (Table 4).

[18] We assessed the frequency with which variables are published and lagged most variables 3 months (1 quarter) while, for example, lagging the inflation variables only one month. Results appear insensitive to the exact construction of the lags.

Table 4
Abnormal spreads—averages for post event data

	Mean	STD	Max	Min	*N*
Following moral hazard inducing events	85.2	858.6	3694.0	−1465.5	100
1 month following event	90.7	940.3	3694.0	−1465.5	25
2 months following event	118.0	904.9	3665.1	−974.2	25
3 months following event	107.0	862.2	3474.4	−885.4	25
4 months following event	24.9	767.7	2816.8	−1327.0	25

Model with lagged variables - excluding same-country observations.

Table 5
Abnormal spreads—averages for post Russian default data

	Mean	STD	Max	Min	*N*
Perfect foresight model					
Month following Russian default	368.5	314.8	1095.7	−53.9	10
2–4 months following Russian default	−57.6	301.0	613.8	−585.2	27
Model with lagged variables					
Month following Russian default	337.5	467.3	1108.7	−695.1	10
2–4 months following Russian default	−10.8	467.9	880.0	−857.6	27

By assuming normal distribution of the error term under the null hypothesis of no moral hazard we can use *t*-statistics to identify moral hazard effects. In none of the results reported can the null of no moral hazard be rejected. For a non-parametric test, we use the sign test, but arrive at the same conclusions; no rejection of the 'no moral hazard' null.[19] Based on these findings, we can conclude that the large bailouts of the 1990s either did not create an additional moral hazard effect or alternatively the additional moral hazard effect they created is hidden behind repeatedly worsening worldwide outlook for which we are unable to control for. We find this second possibility hard to believe for much of the time period we examined.

In an attempt to better identify the effects of specific events, we implement the same methodology but with daily data. The only variables that are available on a daily basis are the interest rates and the S&P credit rating variables. Yet, these are not sufficient to estimate a powerful enough 'normal' model and enable a more detailed look on the evolution of 'abnormal returns' around moral-hazard-inducing events. Even though the adjusted R^2 is 0.40, the inclusion of the S&P ratings and interest rates does not contribute much to the explanatory power of the model beyond the country-specific fixed effects.[20]

The evidence on the Russian default is different. As we have already observed, spreads clearly increased substantially following the Russian default. Our procedure shows that these increases were significantly larger than what actual changes in the macroeconomic variables we controlled for predicted. Averages of abnormal returns following the Russian default are large, postive, and highly significantly different from zero (Table 5). This can be interpreted as evidence that the Russian default occasioned investors to reduce their expectations of future bailouts, increase perceived default risk, and reduced the moral hazard effect.

[19] MacKinlay (1997) describe the non-parametric sign test in more detail.
[20] An *F*-test cannot reject the null that these variables are jointly insignificant.

Many observers of the Russian crisis seem to agree that a moral hazard effect was indeed a major driving force in directing large amounts of capital into the Russian debt market prior to the crisis. Undoubtedly, Russia was taken to be the model case of the 'too large to fail' doctrine (or the 'too nuclear to fail' doctrine).[21] What is more interesting is that when the international multilateral organizations failed to intervene in August 1998 and Russia defaulted on its domestic bonds, other countries' spreads increased substantially as well. It seems that international investors did take the absence of intervention to mean that the IMF might no longer be willing to intervene in other countries' crises, as well. This is Dell'Ariccia et al.'s (2000) interpretation.

Yet, the increase in spreads can also be accounted for by a major revision of expectations on the part of investors. While Dell'Ariccia et al. (2000) discount this possibility, the IMF reported, in its *World Economic Outlook* of October 1998:

> International economic and financial conditions have deteriorated considerably in recent months as recessions have deepened in many Asian emerging market economies and Japan, and as Russia's financial crisis has raised the specter of default. Negative spillovers have been felt in **world stock markets**, emerging market interest spreads, acute pressures on several currencies, and further drops in already weak commodity prices. . . **World growth of only 2 percent is now projected for 1998, a full percentage point less than expected in the May 1998 *World Economic Outlook* and well below trend growth. Chances of any significant improvement in 1999 have also diminished, and the risks of a deeper, wider, and more prolonged downturn have escalated**.

Williamson (2001) uses even stronger language: "The weeks following the collapse of the Russian program marked the apogee of the most dangerous economic crisis that the world has seen in recent decades." (p. 60). In our empirical work detailed in the two previous subsections, we controlled for credit ratings in attempt to control for the macro-economic outlook but these ratings are notoriously sluggish and are typically revised downward only *ex post* (see Reinhart, 2002). Our inability to control for emerging markets' deteriorating economic outlook may be the reason behind our findings for the Russian case.[22] Because we cannot differentiate between these two hypotheses surrounding the Russian case, we require another default event which did not significantly change perceptions with regard to future economic developments worldwide.

The dramatic events in Argentina are, from this paper's perspective, immensely interesting. Any conclusions obtained from the Russian non-bailout case are fragile, as we cannot differentiate between the effect of the non-bailout event for Russia, the LTCM crisis, and the general worsening outlook for the world economy. The events surrounding the Argentine default represent a Russia-like absence of international bailout but in the absence of any other major financial crisis or much worsening world economic outlook (both of which were concurrent with the Russian case). Thus, the events surrounding the Argentine crisis offer us a better test case for the validity of the moral

[21] For a detailed chronology of the fiscal crisis that led to the Russian default and its aftermath, see Kharas et al. (2001). They assert: "Portfolio investors might have been anticipating a large bailout that would at least postpone a crisis and keep their one-way bet for a few more lucrative months" (p. 42). Kharas et al. (2001) quote market commentary from the same time to support their claim.

[22] This does not preclude the possibility that the reason why there was a worsening outlook for the world economy was exactly the presumption that large multilateral bailouts will no longer be forthcoming. This possibility, on the one hand, reinstates the moral hazard hypothesis, but, on the other hand, it suggests a powerful incentive for the multilateral lending institutions to continue maintaining bailout guarantees in spite of the moral hazard effect. One can doubt this interpretation as the LTCM bailout followed the Russian default (September 1998), as did a decision by the U.S. congress to increase IMF quotas (October 1998). Both of these should have allayed fears that bailouts will no longer be forthcoming.

Table 6
Abnormal spreads—averages for post Argentine default data

	Mean	STD	Max	Min	*N*
Perfect foresight model					
Month following Argentine default	−42.5	253.4	1022.3	−532.3	8
2–4 months following Argentine default	156.4	322.9	996.7	−555.2	22
Model with lagged variables					
Month following Argentine default	−73.3	215.8	1114.7	−1386.9	8
2–4 months following Argentine default	131.3	315.0	783.8	−638.6	22

hazard argument. While Argentina does not appear to fit the 'too nuclear to fail' argument, the international bilateral financial institutions were heavily invested in the success of the Argentine economy. It was seen as very important to the region, and specifically as a major test-case – or a poster child – for the new liberalization program, the 'Washington Consensus' policies forcefully advocated by the IMF throughout the 1990s (Pastor and Wise, 2001; Joyce and Noy, 2005). Thus, many expected the IMF to bail out Argentina.

On June 15th 2001, Domingo Cavallo, The Argentine finance minister, announced a complicated system of subsidies and tariffs that amounted to a 'dual exchange rate regime.' Since this was in clear contravention of previous IMF advice it was widely expected that the IMF would withdraw its program.[23] Several weeks of financial turmoil ensued but an agreement with the IMF was reached on August 21st, provided additional financing of $5bn. This program appeared to be a strong indication of the IMF's willingness and commitment to aid Argentina in its fiscal adjustment; and by extension, of its continued commitment to providing multilateral bailouts in spite of strong objections from the U.S. Treasury.[24]

The Argentine authorities started publicly discussing an orderly debt restructuring in November; a bank run ensued and the authorities imposed strict capital controls and restrictions on deposit withdrawals. On December 5th the IMF refused to provide a $1.3bn loan installment and after another failed attempt to reach an agreement (on December 17th) the political situation quickly deteriorated, De la Ruá's government was swept from office and Argentina defaulted on its debt.

The IMF's commitment through most of 2001 to support the Argentine government (and especially the August signing of a new agreement) was an important signal to the markets. Thus, one would expect that if a moral hazard exists in international debt markets that spreads would have increased following the abandonment of Argentina and its political and economic collapse in December of 2001. This has not happened.

In Table 6 we replicate the previous tables for the post Argentine-default case. Not surprisingly, we find no evidence of an 'abnormal' (statistically significant) increase in spreads as a result of the default (i.e., an increase that is not explained by fundamentals). While the averages are positive, the 'abnormal' spreads' average is close to zero and statistically indistinguishable from it. We repeat this exercise for both our empirical spread determination models but the results are consistently the same.

[23] "A common view was then that the de facto dual exchange rate might be the final straw to break relations with the Fund" (Powell, 2003).

[24] One possibly important difference was that the IMF now seemed to have been supportive of the idea of a voluntary restructuring of Argentina's debt. Thus, while the bailout might have been intended to facilitate that debt restructuring, it still entailed some losses for international investors; the IMF did not attempt to provide a full bailout to international investors.

6. Conclusion and suggestions for further research

We test the available data for empirical evidence of a moral hazard effect and find weak evidence consistent with the moral hazard hypothesis as a result of IMF policies between 1994 and 2002 only for the Russian case. We find no evidence of an increase in moral hazard as a result of the large Mexican and Asian bailouts of 1994–1998. We argue that the events surrounding the Russian 1998 default are clouded by a general pessimism regarding the outlook for the world economy following the default. Consequently, any evidence that shows an increase in spreads following the absence of an IMF bailout in that case can easily be attributed to this shift in market sentiments and not to a decreasing moral hazard effect. An examination of the Argentine default of 2001 yield no evidence of an IMF-induced moral hazard effect, as well.

Naturally, a failure to reject the null of no-change-in moral hazard is not the same as arguing that there is no moral hazard created by IMF lending practices. Three possibilities arise: (1) our statistical tests are not powerful enough; (2) there is no quantitatively observable moral hazard effect; or (3) the perceptions of the likelihood of bailouts did not change following the Mexican, and other large bailouts or their absence in other instances, and had been relatively constant over the last decade. This last possibility does not imply that the moral hazard effect is insignificant but rather that it is relatively constant and had been around for longer than our sample period (1994–2002).

We lastly consider some possible extensions to the empirical work applied here. Extending the analysis to the pre-1994 era and investigating whether one can observe any change in international flows prior to that date might provide us with information related to the third possibility. The paucity of pre-1994 data on spreads and the absence of any obvious IMF policy shifts before 1994 prevent us from undertaking this exercise.

A shortcoming of our event-study approach is that the event choices are ad-hoc. In our case, these are based on ‘popular’ perceptions regarding which IMF bailouts actually surprised international markets and therefore affected measured spreads. An alternative strategy might be based on identifying these surprise-events by employing results from the literature that looks at the likelihoods of participation in IMF programs (e.g., Conway, 1994; Hutchison, 2003; Hutchison and Noy, 2003; and Knight and Santaella, 1997). We can identify surprises as cases in which the likelihood to participate is below a certain threshold but a program was actually observed. While this algorithm does not contain an ad-hoc component it assumes a satisfactory predictive capacity for the empirical IMF participation model. Yet, the explanatory power of these models is generally weak and does not capture the relative, and possibly unquantifiable, importance of specific programs in changing market perceptions.

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Appendix A. Data sources

Variable	Code	Source
Liquidity and solvency variables:		
Debt to GDP ratio	DBTGDP	IFS 85A.ZF
Net foreign assets to GDP	NETFAS	IFS 11.ZF, 16C.ZF

Appendix A (*Continued*)

Variable	Code	Source
Reserves to GDP ratio	FXRGDP	IFS 1D.DZF
Current account flows relative to exports	CAEXP	IFS 78ALD.ZF
Macroeconomic variables:		
Inflation rate	CPI	IFS 64.XZF
High inflation dummy (CPI>30)	CPID	
Fiscal deficit to GDP ratio	GOVGDP	IFS 80.ZF
Export growth	DLEX	IFS 70.DZF
S & P's credit ratings	RESRTNG	An index (1-21) derived from the S & P's classification (D to AAA).
External variables:		
US Federal Funds rate	USRATE	IFS 60.BZF
Developing countries exports' price index	EXPP	IFS 20174.DZF

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