The Role of Credit History in Sovereign Bank Lending[*]

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Abstract

This paper is an empirical investigation into the role of credit history in determining the spread on sovereign bank loans. It employs an error-in-variables approach used in rational-expectations-macro-econometrics to set up a structural link between sovereign loan spreads and realized repayment behavior. Unlike the existing empirical literature, its instrumental variables method allows for distinguishing a direct influence of past repayment problems on spreads (a "punishment" effect in *prices*) from one that goes through increased default probabilities. Using developing country data from the period 1973-1981 and constructing continuous variables for credit history, we find that past default is a significant determinant of the spread, even after including country fixed effects. Moreover, its reduced-form effect is very similar to its structural form effect, indicating that most of the influence of past repayment problems is through this extra punishment channel. Overall, reserves to imports, past and predicted future default are substantial determinants of sovereign bank loan spreads.

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1. INTRODUCTION

The absence of a well established bankruptcy code, the limited access to borrower's collateral and a lengthy, costly restructuring process drive a wedge between sovereign and corporate debt. These determine Eaton and Gersovitz (1981) to conclude that the binding constraint on sovereign debt repayments is generally a country's *willingness to pay* rather than its *ability to pay*. The question then arises as to what incentives a sovereign borrower has to repay its debt. From various repayment reasons (like seizure of overseas assets, direct sanctions, reduced benefits from commodity trade, default signaling bad economic fundamentals and thus worsening borrowing terms), this paper is concerned with reputation. That particular mechanism means that countries with poor repayment history (reputation) may see their subsequent access to world credit markets impaired, and this impairment reduces their welfare.

The reputational cost of default could be addressed in terms of two punishments, a quantity (denial of a loan or credit ceiling) versus a price one (higher cost of borrowing). The literature on the first issue treats the penalty of default in terms of exclusion from future access.[1] Exclusion, however, has essentially never been permanent; moreover, a partial exclusion imposed by a group of banks might lead to more expensive but not necessarily less loan disbursements.[2] In fact, a complete cutoff from borrowing is equivalent to prohibitively expensive loan offerings. Our focus is thus to understand the price punishment.

In general, a negative repayment history can raise the spread on sovereign debt through two main mechanisms. The traditional channel is an increased default probability, which is either affected endogenously by past default or just signaled by it.[3] Lenders can, however, impose a punishment surcharge, which is not related to default risk, and could be referred to as a "pure punishment" effect. There are other mechanisms that can lead to a surcharge in loan prices over the "actuarially fair" spread: for example, banks may try to recover some of their losses by offering more expensive loans. A default may also induce a change in the set of banks willing to lend to the country, and the new creditors may have a different (higher) cost of funds.[4]

It remains mostly an empirical question to determine whether maintaining a good credit history is an important repayment incentive; and if so, through what channels of influence. In this respect, the largest part of previous research has analyzed the pricing of

[1]Examples include Eaton and Gersovitz (1981), Eaton (1996), Yue (2006), Kovrijnykh and Szentes (2007).

[2]Kovrijnykh and Szentes (2007) develop a related argument: once a country defaults, it gets locked in with its incumbent lender, who can exercise monopoly power, resulting in a higher price.

[3] For example: Eaton (1996), Sandleris (2006).

[4] One potential mechanism is described by Wright (2002): for syndicated loans, in which each bank has a share of the profits, then each bank's incentive to maintain a good reputation in this cooperation makes them tacitly collude in punishing a country in default.

sovereign loans. The most frequent empirical research approach has assumed only one source of risk (default) and postulated two relationships: one between the spread and the probability of default, and the other between the probability and economic fundamentals. The basic method was to substitute the second equation into the first and thus to obtain a reduced-form type of relationship between the spread and fundamentals.[5] This literature has found past default as a determinant of debt prices, but has been unable to properly address the question of how such an effect arises.

The purpose of this paper is to contribute to this literature in two major aspects. First, and most importantly, it focuses on the distinction of any direct effect of a bad repayment history on spread, above the indirect one going through increased default probability. Second, different econometric techniques and variables are used in order to control for country heterogeneity (fixed effects), a problem that is important in the framework of this analysis, and has not been thoroughly dealt with in most of the previous research.

The data on spreads is from the World Bank's publication "Borrowing in International Capital Markets" for the period 1973-1981, on 46 developing countries. The choice of the time period was mostly driven by Ozler's (1993) observation that a period of market expansion is needed to distinguish the impact of an individual borrower's repayment history from the impact of a widespread panic. Since we are interested on the pricing of sovereign bank loans, this period is ideal because it witnessed particularly intense syndicated bank lending to sovereign borrowers. While most of the variables utilized in the paper are those suggested by the literature, we create continuous measures of past and future default, which are based on arrears data from the World Bank's Global Development Finance. These variables are compatible with country fixed effects.

The main estimation strategy used in the paper is a structural asset pricing approach.[6] The starting point is that the spread is determined by expected default risk. As it is an unobserved variable, we replace the expectation term with its realization and a prediction error. This creates an identification problem, as the realization is correlated with the prediction error. Assuming that expectations are rational, we can use any information available at the time of pricing as valid instruments (debtor characteristics, including credit history).

A reinterpretation of this procedure is that we want to attribute the reduced form explanatory power of certain fundamentals to their predictive power for future default. Overidentification thus receives a central role: it tests whether information affects the spread only through predicted default or there is an extra channel of influence. In particular, we first show that if we consider default risk as the only determinant of the spread, the p value of the overidentification test is small. Then we add credit history as an extra right hand side variable, and show that the p value increases substantially, and the coefficient of credit history is

[5] See for example Edwards (1986), Ozler (1993), Eichengreen and Mody (1999), Easton and Rockerbie (1999).

[6] As in Benczur (2001).

positive and significant. Finally, we search for additional variables which are pivotal for overidentification.

In the reduced form estimation, we find that both recent and distant default history have a significant positive influence on the spread, but the inclusion of country fixed effects is necessary. The conclusion of our benchmark structural specification is that future default risk, recent default history and an extra term, given by reserves to imports, can robustly and meaningfully describe the spread. Also, our instruments are relevant and valid.

In the structural form, the coefficient estimate for default risk is around 0.35. It means that if we increase its measure from its median to the 90th percentile, the spread goes up by 17 basis points. Compared to the average spread of 133 basis points, it can be considered as sizable. Similarly, raising the reserves to imports ratio from its median to the 90th percentile, the spread goes up by 24 basis points. For credit history to affect the spread in a quantitatively meaningful way, we need to look at countries with an extremely poor record: for certain countries in our sample, there is a 32-56 basis point direct punishment component in the spread (keeping default risk unchanged). As the structural and reduced form coefficients of credit history are very similar to each other, it is statistical and economic evidence that credit history has a dominantly direct, "pure punishment" effect on loan spreads.

The structure of the paper is the following: the second section comprises a review of the empirical literature on the role of credit history. The next part explains the empirical strategy of the structural asset-pricing setup. The description of data and variables is presented in the fourth section. The fifth part discusses the main econometric problems related to estimation and describes the reduced- and structural-form results, while the last one concludes.

2. RELATED LITERATURE

There is some direct evidence on the repayment incentives of a sovereign debtor. Conklin (1998) finds extensive evidence consistent with sanction-based theory in his analysis of the lending behavior by a Genoese-led cartel to Philip II of Spain (1556 - 1598). On the other hand, English (1996) reached the opposite conclusion based on his analysis of American states' default during the 1840's.

The more relevant issue for our discussion is how a country's credit history affects the borrowing cost of a sovereign. Eichengreen and Mody (1999) use data on 4500 loans over the 1991-1997 period and employ a pooled OLS regression, corrected for sample selectivity. They notice that the history of rescheduling has a weak positive effect on the probability of an issue while it significantly increases the spread that successful issuers pay.

An important contribution to the issue of reputation is Ozler (1993), which has been cited by many theoretical papers as the main evidence of an effect of repayment history on credit terms.[7] She uses data on 64 countries for the period 1968-1981, which was one of rapid international lending expansion. The econometric technique is a pooled OLS regression with time-specific dummies. Her main results are that the spread is influenced by more recent repayment history (which she identifies as the 1930s through 1960s), but not by distant history (before 1930).

Reinhart *et al* (2003) is a documentation of the effect of repayment behavior on sovereign debt. Employing a cross-sectional regression with multiyear averages of measures for default risk, history of repayment, inflation rates and external debt as controls, they find that a history of defaults weakens a country's ability to borrow large amounts on reasonable terms, because a bad credit history is reflected in lower creditworthiness (proxied by the country's credit rating).

Our analysis departs from that of Ozler (1993) and Reinhart *et al* (2003) in two major ways. The first one concerns the treatment of country fixed effects. By including time invariant variables like dummies for repayment problems, Ozler can no longer have country fixed effects. We resolve this issue by constructing a continuous measure of recent default, which allows us to use both country fixed effects and default history indicators.

The second, more important contribution is a structural and causal empirical approach. The ability to distinguish between different channels of influence is particularly important for the credit history case. As we argued before, there are two ways in which history could affect the spread. Looking only at the reduced-form results – as Ozler (1993) and Reinhart *et al* (2003) do –, one cannot separate the two effects.

3. THE EMPIRICAL STRATEGY: A STRUCTURAL ASSET PRICING REGRESSION

The starting point in the asset-pricing decision is that the spread reflects perceived risks and potential extra effects like a punishment surcharge for past defaults. Estimating such a specification involves unobserved expectations of the risk(s) based on information at the time of pricing. Mainly three solutions have been adopted to overcome this issue. One widely used approach has been to assume specific functional form relations between the spread, the risk probability and the economic fundamentals to get, by substituting one into the other, an estimable reduced form equation.[8] Another solution has been to use proxies for the probabilities, like credit ratings (see for example Kamin and von Kleist, 1999). A third approach has been to use multiple issues of the same borrower, assuming the same default

[7] See for example Eaton (1996) or Obstfeld and Rogoff (1996, p.379)

[8] Examples include Edwards (1986), Ozler (1993), Eichengreen and Mody (1999), Easton and Rockerbie (1999).

probability (Cumby and Pastine, 2001). All these methods suffer from a common problem: they cannot identify more than one source of risk and test for a systematic extra effect of certain country characteristics.

Benczur (2001) suggests a rational expectations approach and proposes the errors in variables method (EVM) as a solution for these problems. According to EVM, one can replace in the pricing equation the expectations of the risks with their realizations. This implies a correlation between the actual realizations and the errors. With the assumption of rational expectations then the actual realizations can be instrumented with any set of information available at the time of pricing.

More formally, one can start from the representation of the structural form of the pricing equation:

$$s_{it} = \alpha + \beta R_t + \lambda E[d_{it} \mid Z_{it}, R_t] + \lambda_1 E[r_{1it} \mid Z_{it}, R_t] + \ldots + \lambda_n E[r_{nit} \mid Z_{it}, R_t] + \theta X_{it} + \varepsilon_{1it}, \quad (1)$$

where s_{it} is the spread paid by country *i* on loans obtained at time *t*, R_t is the benchmark interest rate, d_{it} measures the loss from default, r_{1it}, …r_{nit} are additional risk variables, X_{it} is a vector of various extra factors, and Z_{it} contains information available at the time of pricing. Notice that Z contains all of the X variables as well. The error term ε_{1it} is orthogonal to any time t information (Z and R).

The spread is measured as the difference between the interest charged for the loans of a particular country and the benchmark world interest rate. The paper will include in the main specification only default risk as a rationally expected variable, and credit history and the reserves to imports ratio as potential extra terms. In particular, $E[d_{it}|Z_{it},R_t]$ denotes the conditional expectation of the creditor loss from the event that country *i* does not fully repay its outstanding bank loans in the future, as of information available at time the time of pricing (Z_{it} and R_t). Assuming first that no extra effects are present, our basic specification for the spread is as follows:

$$s_{it} = \alpha + \beta R_t + \lambda E[d_{it} \mid \mathrm{Z}_{it}, \mathrm{R}_t] + \varepsilon_{1it}. \quad (2)$$

The linear term can be derived from risk-neutrality and profit maximization, and assuming partial default (with probability p) on the principal (denoted by x) but not on the interest (r): $(1-p)(1+r) + p(x+r) = 1+R$, which implies $s = r - R = p(1-x)$.

According to the EVM method, one replaces the expectation term in (2) with its realization:

$$d_{it} = E[d_{it} \mid R_t, Z_{it}] + \varepsilon_{2it}. \quad (3)$$

Given rational expectations, $E[\varepsilon_{2it} \mid R_t, Z_{it}] = 0$, thus equation (2) becomes:

$$s_{it} = \alpha + \beta R_t + \lambda E[d_{it} \mid R_t, Z_{it}] + \varepsilon_{1it} = \alpha + \beta R_t + \lambda d_{it} - \lambda \varepsilon_{2it} + \varepsilon_{1it}. \quad (4)$$

Now, d_{it} is not orthogonal to the compound error term, since it is not orthogonal to the prediction error ε_{2it} (see equation (3)) and ε_{1it} (possible simultaneity problem[9]). But according to the EVM approach, one can use the information set $\{R_t, Z_{it}\}$ as valid instruments, since this set is correlated with the default event (from the prediction equation (3)) and uncorrelated with the error term (from the rational expectations assumption (3) and the pricing equation (1)).

Actually, as Wickens (1982) argues, this method provides consistent, but not fully efficient, estimates even when the information set is incomplete or the functional form of the prediction equation is unknown. This is a major advantage given the problems in applied research of assumptions about the functional form, selectivity bias and omitted variables. The key element is whether the fundamentals are correlated with the default variable. If they are, then they can be used as valid instruments in the pricing equation, without having to specify the default prediction equation.

By using a set of instruments larger than the number of risk factors (one in our benchmark equation (2)), an overidentification situation arises. A rejection of this overidentification test could imply different conclusions. It might be that the rational expectations and risk neutrality assumptions are rejected. Or maybe the risk choice was the right one, but its indicator was imperfect. Our approach is that the instruments are proxying for a missing term in the structural equation, which is due to the direct effect of some factor on the spread, above the one coming from the influence on the predicted probability. In particular, if credit history has an extra "punishment" effect on top of influencing future repayment probabilities, it should show up as an extra determinant. Other factors like the level of reserves may also play a role. This means that we need to replace (4) with the following modification:

$$s_{it} = \alpha + \beta R_t + \lambda d_{it} + \theta X_{it} - \lambda \varepsilon_{2it} + \varepsilon_{1it} \,, \tag{5}$$

where X_{it} is a vector containing credit history and additional extra factors like the reserves to imports ratio. Then, there are two important points to check: first, whether, compared to equation (4), X_{it} has to be included as extra RHS variable to restore the no rejection of overidentification. In practice, it means looking at the increase in the p-value of the overidentification test brought about by the inclusion of a given extra term. Second, whether the estimated θ is significant. The two points ought to be connected, but it is possible that only one of the indications is present in small samples.

[9]This means the following. A pricing error (ε_{1it}) can lead to a higher future default loss d_{it}, the conditional expectation of which is a right hand side variable in the pricing equation. As the pricing error is assumed to be orthogonal to Z and R, it means that those variables remain valid instruments for this source of nonorthogonality as well. In fact, as long as the pricing error is orthogonal to any time t information, it can raise future default probabilities only through a correlation with the prediction error ε_{2it}.

It is important to stress that a variable included in X affects the spread through two channels: through an impact on predicted future default $E[d_{it}|Z_{it},R_t]$, and a direct effect through θ. The total effect is captured by the reduced form equation

$$s_{it} = \alpha' + \beta' R_t + \Gamma' Y_{it} + \Theta' X_{it} + \varepsilon_{it}, \tag{6}$$

where Y=Z\X. Denoting the *linear* conditional expectation of d_{it} by

$$d_{it} = \alpha'' + \beta'' R_t + \Gamma'' Y_{it} + \Theta'' X_{it} + \varepsilon_{2it}, \tag{3B}$$

the structural form (5) imposes the following restrictions on the reduced form (6):

$$\alpha' = \alpha + \lambda\alpha''; \quad \beta' = \beta + \lambda\beta''; \quad \Gamma' = \lambda\Gamma''; \quad \Theta' = \Theta + \lambda\Theta''.$$

This immediately shows the decomposition of the total effect Θ' into the direct effect Θ and the indirect effect $\lambda\Theta''$.

Moreover, it also illuminates the way we identify this decomposition: Γ'' and Θ'' are obtained from the prediction equation for d_{it}, the risk parameter λ is identified through the restriction $\Gamma' = \lambda\Gamma''$, while Θ is obtained as $\Theta = \Theta' - \lambda\Theta''$. This is exactly what an instrumental variables (IV) estimation of equation (5) does in one step. Finally, the overidentification test checks whether the *matrix* equation $\Gamma' = \lambda\Gamma''$ holds.

4. DATA AND VARIABLES

The description of data and, more importantly, the definition of variables are of large significance, because this paper employs some indicators that are different from the literature. This section explains their motivation, construction and limitations.

The choice of the time period was mostly driven by Ozler's (1993) observation that a period of market expansion is needed to distinguish the impact of an individual borrower's repayment history from the impact of a widespread panic. Thus, we use the period 1973-1981, which witnessed particularly intense syndicated bank lending to sovereign borrowers. In fact, bank loans were the dominant source of sovereign capital flows in the 70s, which was no longer true after the Debt Crisis. The initial dataset contains information on 757 commercial bank loan contracts denominated in dollars, to 46 developing countries and were obtained from various issues of the World Bank's "Borrowing in International Capital Markets".[10]

As we have no access to contract-level characteristics of loans or their future repayment patterns, we need to average over all contracts of the same country at a given time period. Since the economic fundamentals are mostly available at the annually frequency,

[10] Data on fees and commissions are not reliably available. It is noted, however, that these costs are low relative to the spreads (see Edwards, 1986).

we construct yearly measures for the spread. Just like Easton and Rockerbie (1999), we use a weighted average of the original spreads, using as weights the loan quantities and maturities:

$$s_{it} = \frac{\sum_{j=1}^{k} s_{ijt} q_{ijt} m_{ijt}}{\sum_{j=1}^{k} q_{ijt} m_{ijt}},$$

where s_{ijt}, q_{ijt}, and m_{ijt} denote the spread, loan quantity, and maturity of the *j*th loan contract for country *i* during year *t*. As an alternative, we also discuss results which use an average spread weighted by loan quantities only (like Edwards, 1986 and Ozler, 1993). Using maturity in the weighting allows for taking into account that the spread of a longer maturity debt influences average credit terms to a larger amount than the spread of a shorter maturity loan.[11] This transformation means that we are left with 252 yearly observations.

Data availability further reduces the working sample. Arrears data, used for our default measures, is available only for 40 countries, resulting in 211 observations. Other country fundamentals are also not available for some countries, and the use of first lags, which is essential for first differencing and other panel estimators, means that one observation from each country is lost. This reduces the sample to 157 observations from 37 countries. Finally, the smallest and largest values of our constructed variables are quite unrealistic, thus we excluded the top and bottom 1% for the recent default and 2% for the future default variable. The final sample for which we report the results is 149 observations and 36 countries.

Graph 1 illustrates the evolution of the sovereign spreads in the sample, together with the BAA-rated US corporations bonds' spread (taken from the Federal Reserves' website). This comparison shows that sovereigns pay similar spreads than BAA-rated US companies. Another aspect is that the variation in spreads during the sample seems to be constant, with two exceptions: for 1975, where the variation is very small, and in 1981, where it is large. This latter point is interesting because it suggests that commercial banks were distinguishing between the borrowers, even before the "unexpected" debt crisis of 1982.

[11] The World Bank's Global Development Finance database also reports average interest rates weighted by quantities. From a theoretical point of view, however, it is not obvious what the correct weighting should be. Suppose that a loan contract specifies an interest payment stream of a constant spread over the benchmark yield r_t+s for a period T, at the end of which the full principal q is repaid (notice that actual contracts might specify a different schedule for principal repayments). It is straightforward to see that the present discount value (PDV) of such a loan contract is $qs\left(1-\exp\left(-\int_0^T r_\tau d\tau\right)\right) = qs(1-1/R_T)$. Here r_t is the instantaneous required interest rate, while R_T is the required yield from time zero to T. Having two loan contracts (q_1,s_1,T1) and (q_2,s_2,T2), the equivalent contract (q_1+q_2,$\underline{s}$,$\underline{T}$) then must satisfy $q_1s_1(1-1/R_{T1})+q_2s_2(1-1/R_{T2})=(q_1+q_2)\underline{s}(1-1/R_{\underline{T}})$. This defines $\underline{s}$ for a given $\underline{T}$. In order for this to be a weighted average of s_1 and s_2, $\underline{T}$ must be such that $1/R_{\underline{T}}$ is the quantity weighted average of $1/R_{T1}$ and $1/R_{T2}$. Then $\underline{s}$ is a weighted average of s_1 and s_2, with relative weights $q_1(1-1/R_{T1})$ and $q_2(1-1/R_{T2})$. For T1≠T2, this in general requires a detailed knowledge of the entire benchmark yield curve. If the yield curve is flat ($r_t \equiv r$), r_{T1} and r_{T2} are not too large (in the sense that $1/R_{T1}$ and $1/R_{T2}$ are well approximated by 1-rT1 and 1-rT2), then the spread on the equivalent contract is precisely the quantity and maturity weighted average of s_1 and s_2.

Repayment history indicators

There is no clear indication from theory regarding the choice of the repayment history variable. In general, it should represent the overall loss creditors incurred due to repayment problems. Options include a simple zero-one indicator for outright defaults, reschedulings, arrears; or more continuous measures, which try to control for the size of the loss, by looking at the amount of debt forgiven, rescheduled or arrears.[12] Recognizing that any indicator is merely a proxy, our choice is guided by data availability and explanatory power.

In particular, binary indicators of repayment problems are available both historically and recently; arrears data is reported by the World Bank from 1971; while the size of debt forgiveness and reschedulings are reported by the World Bank only from 1989. For this reason, we use binary indicators for capturing "distant" repayment history and arrears data for "recent" history.

There are reasons to believe that recent and distant history has a different effect. Indeed, Ozler (1993) finds that repayment difficulties happening before the 1930s did not significantly matter for spreads in the 1970s, while those afterwards did. In the models of Kovrijnykh and Szentes (2007) and Yue (2006), it is also recent default (arrears) that matters; in fact, once a country eliminates its arrears, it gets a clear credit history.

For "*distant*" history we use an indicator of the presence of default or rescheduling of bank loan debt to official creditors in the period 1940-1970. This dummy variable was constructed by recording a '1' whenever there was a repayment problem indicated in two sources: in Ozler (1993), which includes data for 1956-1968, and Lindert and Morton (1989), which refers to the period 1940-1970.

Table 1 presents its summary statistics, the number of observations and countries that had repayment problems. It is important to note that the indicator has significant variation to be able to identify the effects. The mean for this variable is high, and shows that around 30% of the countries in the sample had some repayment problems during 1940-1970.

While this indicator is very similar to those used in Ozler (1993), the indicators reflecting "*recent*" history are our own contribution. Their construction allows including a continuous variable instead of a dummy to reflect past repayment behavior, enabling to control for country fixed effects and still include a default history measure. We construct this indicator from data on private arrears (both interest and principal) on long-term debt outstanding, available since 1971 from the *Global Development Finance* CD-ROM.

To reach this indicator, we add the change in the stock of arrears starting from the first lag and going back until 1971. The addition of these past values is weighted by decreasing

[12] Unless there are sufficient late payment fees, arrears are likely to create losses for the creditors. Besides, as Cline (1984) notes, debt reschedulings are usually preceded by the accumulation of arrears, thus their presence and size can be a more precise measure of creditor losses.

coefficients (1, 0.9,..., 0.1). This takes into account a "memory" that is fading in time, such that more distant events have a relatively smaller importance for the present than the more recent ones. We also discuss results from various additional ways of cumulating arrears: simply adding them up (weights 1,1,…1) or using a quadratic scheme (weights $1, 0.9^2, \ldots 0.1^2$).

Formally, the indicators are computed using the following steps:

1. For each country we use the time period 1971-1981 and for each of these years, we compute the flows of new private arrears as the first difference of the stock of arrears (for 1971, it is the stock itself). Let this measure be denoted by $arrears_{jt}$, where j=1,…,36 for countries and t=1,…,11 (corresponding to the period 1971-1981).
2. For the same balanced panel coverage we compute the following indicator:

$$recentdef10_{jt} = \sum_{k=1}^{t} arrears_{j(t-k)} * (1 - \frac{k-1}{10}),$$

for each country j =1,…,36 and each t=1,..,11. Thus it is an indicator for the discounted repayment problems of the last 10 years.

Finally, we further separate the effects of very recent history. One would expect that the indirect effect of a very close repayment problem (last 1-3 years) is more important than that of a more distant repayment problem, since it is very likely to reflect problems that generate additional repayment problems in the future. This should lead to a measure that is significant in the reduced form but has no direct effect in the structural form. Also, there might be a "grace" period applied when the banks decide whether to charge higher spreads for defaulting countries, in which case very recent history might not matter. Thus, we separate the ten years measure into the last three years and the preceding seven. The first choice, however, is not significant even in the reduced form, leading us to believe that there was indeed a grace period attached to previous defaults. The variable that adds the flows of arrears from time t-4 to t-10, on the other hand, is significant. Thus it is the one we report in our results section.

We would stress again that there is no clear indication from theory regarding the choice of the repayment history variable. In particular, one could also employ a similar measure based on arrears normalized by the amount outstanding. Such an indicator, however, does not have plausible reduced form estimates: as discussed in section 6.3, its parameter is usually negative and/or insignificant. Being guided by data availability and explanatory power, we take this as an indication that the absolute measure is more successful in capturing the effect of repayment history.

Future default variables

This variable should reflect the realization of the proportion of losses for a loan offered. As demonstrated by Sturzenegger and Zettelmeyer (2007), precise measures of realized repayments are very hard to compute for sovereign debt. Easton and Rockerbie (1999) argue that arrears are more indicative for repayment problems than default or rescheduling indicators. Based on these, we construct our future default measure by using again GDF data on arrears: we add private arrears for a period similar to the average maturity. As opposed to our measure for recent default, there is no ambiguity that this sum needs to be normalized by the total loan amount corresponding to that observation, in order to reflect relative losses.

Let us denote the yearly GDF data on arrears by X_{is}, with s ranging from 1970 to 1989. As those arrears may refer to contracts from multiple years, we need to allocate them among contracts. Our approach involves the following steps. First, we split X_{is} among contracts from years $s-1, s-2, \ldots s-8$ in proportion to new disbursements in the corresponding year:

$$\mathrm{Y}_{\mathrm{is,s\text{-}j}} = \mathrm{X}_{\mathrm{i,s}} \mathrm{ND}_{\mathrm{i,s\text{-}i}} / \left(\mathrm{ND}_{\mathrm{i,s\text{-}1}} + \ldots + ND_{i,s-8}\right), \tag{7}$$

$j=1\ldots8$ (the average loan maturity in our sample). Here $Y_{is,s-j}$ is the time s arrears on a time $s-j$ contract and $ND_{i,s-j}$ is the size of the time $s-j$ loan for country i. Then we cumulate the arrear fragments $Y_{it+1,t}, Y_{it+2,t} \ldots Y_{,it+8,t}$ into which a time t contract goes over its lifespan:

$$\mathrm{D}_{\mathrm{it}} = \mathrm{Y}_{\mathrm{it+1,t}} + \mathrm{Y}_{\mathrm{it+2,t}} + \ldots + \mathrm{Y}_{\mathrm{i,t+8,t}}. \tag{8}$$

Thus, in order to recover the amount of arrears affecting an active loan contract in year t, we assume that all the time t arrears affect all the loans that have not matured yet, and the size of the contract specific arrear is proportional to the size of the contract. We motivate this by two arguments: one is that there is no information available on which contracts these arrears correspond to; and second, the assumption that these flows can be attributed to several preceding loans is consistent with the cross-default clauses that these contracts included. According to such clauses once a country enters into default or any repayment problem that constitutes a break on the contract with one lender, this will be treated as default also by the other creditors. Alternatively, instead of using loan disbursements as weights, we discuss results with equal weights. We also consider the effect of the grace period, thus excluding from this summation contracts that are more recent than the average grace period of 3 years.

Table 2 provides some brief descriptive statistics of our benchmark choice for future and recent default. For recent default around 65% of the observations are equal to 0 for both the full and the reduced sample; this number is around 20% for the future indicator. The difference is due to more frequent arrears after 1981, but it shows that there were still countries that were not accumulating arrears in this period.

Economic fundamentals

An important part of the estimation is to properly control for other factors, including here the economic fundamentals. The sources for this category of variables are *Global Development*

Finance, International Financial Statistics and *World Development Indicators.* In choosing these factors, given also the data availability, we are following most of the literature in considering the following variables as candidates: *debt to GDP*, *reserves to imports, debt service to exports, current account per GDP, exports to GDP, savings to GDP, growth of per capita GDP, growth of gross investment, GDP per capita, inflation, credit to private sector per GDP.*

We construct two additional variables that are related to the international financial environment of a country. *Repeated borrowings* are designed to capture the importance of relationship banking. It is constructed by cumulating the number of months in which the borrower received loans. *Proportion of countries with arrears in the region* captures a regional contagion effect from one country going into arrears. It is constructed by dividing the number of countries with arrears from the same region by the total number of countries in that region. Finally, for the benchmark interest rate we use the LIBOR USD 1-year rate.

5. Estimation issues

Both the reduced and the structural form specification are subject to two major econometric problems: the need to control for country-level heterogeneity (fixed effects), and the validity of the strict exogeneity assumption. We explore these issues for the reduced form first, and then discuss the relevant specifications for the structural form.

The reduced form in a panel framework is:

$$s_{it} = \alpha + \beta R_t + \Gamma Z_{it} + c_i + \varepsilon_{it}, \tag{6B}$$

where ε_{it} is the idiosyncratic error term, Z_{it} are the economic fundamentals for country *i* known at time *t and* c_i is the unobservable individual effect.

The first major concern is that the usual pooled OLS estimates are incompatible with individual country effects. The usual procedure to correct for fixed effects is a fixed effects (FE) or a random effects (RE) estimator. A key assumption behind both methods is strict exogeneity, which requires that the idiosyncratic error terms, conditional on the individual effect, are uncorrelated with past, present or future values of the regressors. If this fails, then all the classic panel data methods and specification tests are inconsistent. Formally, the strict exogeneity assumption means: $E[\varepsilon_{it} \mid Z_{it}, c_i] = 0$, for all *t* and *s.* There are reasons to suspect that the assumption might fail, as any pricing error (ε) could affect the future values of certain indicators, like reserves, debt to GDP, participation on the market, proportion of countries in arrears.

Wooldridge (2002) suggests a test for this: use the FE estimator but also include future values of some variables that are likely to break the assumption. Their significance is evidence that the assumption is likely to fail, at least for those variables. When performing such a test, we do actually find that when we include leads of variables such as: proportion of countries with arrears, experience on the market, saving to GDP, debt to GDP, they are significant. It is worthwhile mentioning that the strict exogeneity assumption is likely to be satisfied or to have only a small inconsistency effect for our recent default variable since it captures arrears up to time t-4, thus it is insulated from the present time t through time t+3 pricing error.

The strict exogeneity assumption is even more problematic and crucial in the structural form (equation 4 with the country effects included) than in the reduced form. As Keane and Runkle (1992) strongly point out, in this type of models, there are never any strict exogenous variables or instruments. This formal result comes from the effect of the prediction error on the future values of the variables.

When there are concerns about strict exogeneity, the general approach is to use a transformation to remove the country effects c_i, and then search for instrumental variables, assuming only sequential exogeneity (Wooldridge, 2002). According to this assumption, the idiosyncratic errors, conditional on c_i, should be uncorrelated with the contemporaneous and past values of the regressors (instruments), but not with future values.

In this respect, a first-difference (FD) estimator is attractive:

$$s_{it} - s_{i(t-1)} = \beta(R_t - R_{t-1}) + \Gamma(Z_{it} - Z_{i(t-1)}) + (\varepsilon_{it} - \varepsilon_{i(t-1)}). \tag{9}$$

One can notice that if strict exogeneity fails, then there is a problem here as well, since $E[\varepsilon_{i(t-1)} \mid Z_{it}] \neq 0$. The sequential exogeneity assumption, however, implies that all the lags of Z (or their linear combination) can be used as potential instruments for $(Z_{it} - Z_{i(t-1)})$ and then the estimation is consistent.

For the structural form, this would mean estimating:

$$s_{it} - s_{i(t-1)} = \beta[R_t - R_{t-1}] + \lambda[d_{it} - d_{i(t-1)}] - \lambda[\varepsilon_{2it} - \varepsilon_{2i(t-1)}] + [\varepsilon_{1it} - \varepsilon_{1i(t-1)}] \tag{10}$$

However, the first difference specification causes further complications. The rational expectation assumption guarantees that the prediction error ε_{2it} is orthogonal to time *t* information, but this is not true about ε_{2it-1}. The remedy is to use $Z_{i(t-1)}$ or $Z_{i(t-2)}$ as instruments, as those variables are not correlated with any error at time *t* or *t-1*.

If some variable, *X*, is believed to be part of the model as extra RHS variable, then equation (5) is to be first-differenced, and it becomes:

$$s_{it} - s_{i(t-1)} = \beta[R_t - R_{t-1}] + \lambda[d_{it} - d_{i(t-1)}] + \theta[X_{it} - X_{i(t-1)}] - \lambda[\varepsilon_{2it} - \varepsilon_{2i(t-1)}] + [\varepsilon_{1it} - \varepsilon_{1i(t-1)}]. \tag{11}$$

Including extra RHS variables one could also worry about the possible failure of strict exogeneity for these variables. However, since in the benchmark specification these extra variables will be reserves to imports and recent default, for which we do not have reduced form evidence about the lack of strict exogeneity, we do not instrument them. Moreover, not instrumenting the extra RHS variables allows for a clearer and more direct comparison with the reduced form regression. Thus, whenever we refer to instrumenting in the structural-form we mean instrumenting only the future-default variable.

In conclusion, in the structural form we use the first difference estimator with appropriately instrumenting the future default variable: it eliminates the individual effects, and the right choice of instruments resolves the issue of the prediction error. The appropriate instruments include the first and/or second lags of the regular instruments (time *t* information). Using as instruments the levels of the variables (as opposed to the second difference) leads to more precise estimates, but at the cost of making the direct comparison with the reduced-form results of Table 3 more difficult.

The whole structural estimation framework is based on the validity of the instruments: they should be correlated with the instrumented variable and also uncorrelated with the error terms. In order to check for this, we report several measures that summarize the first-stage regression: the partial R^2 of the first stage regression, the F test of the joint significance of the excluded instruments in the first stage regression; and the Anderson canonical correlations likelihood-ratio test of whether the equation is identified, i.e., that the excluded instruments are relevant. As for the overidentification test, there is a variety of choices and we employ a test that will be appropriate in a setting with heteroskedasticity and autocorrelation. This is the Hansen's J statistic, which can be used either with IV or GMM (generalized method of moments), and it is consistent even with intra-cluster correlation.[13]

6. Results

6.1 Reduced form

The results of the reduced-form estimation are presented in Table 3 and refer to four specifications. The left hand side variable is a a quantity and maturity weighted annual spread. The recent default variable used is the t-4…t-10 private arrears flows, weighted by linearly decreasing weights. As Column (1) shows, most of the explanatory variables are significant and have the expected sign in the pooled OLS specification. In particular, the coefficient of both distant and recent default is positive and significant (columns 1, 3 and 1-4, respectively). When running panel data specific regressions like fixed effects (Column 2) and random effects (Column 3), the results do not change substantially except for two variables, savings to GDP and experience on the market. The former becomes more significant under

[13] This feature is important because Baum et al.(2003) cite evidence that show that the presence of intra-cluster correlation can readily cause a standard overidentification statistic to over-reject the null.

random effects, while the latter is not significant under OLS and RE but it is at 0.1 significance level under FE.

Column (4) is a specification that uses only the sequential exogeneity assumption: the variables are first-differenced and the proportion of countries with arrears is instrumented with its first lag.[14] The estimates' precision is smaller, due to the fact that there is less time-variation and that by first differencing there is one observation not used for each country. Still, the effect of recent default is positive and (marginally) significant, even after controlling for country effects.

It is less obvious whether distant default remains important after controlling for fixed effects, as the fixed effects (FE) estimation or first-differencing (FD) makes it impossible to include time-invariant variables. In this case, the comparison of the distribution of country effects across groups with different default history might convey some information. Indeed, the results from the reduced-form estimations suggest that defaulters (categorized through the distant default dummy) are being charged significantly higher spreads. The difference between the means of the two groups is 0.3, which is around one and half times the estimate on the default dummy (see Table 3). Moreover, the standard deviation of the country fixed effects is 0.3; hence it could be argued that the estimated default dummy explains around 70% of the typical variation of the country fixed-effects. This suggests that distant default is not simply picking up country fixed effects.

In conclusion, our findings suggest that fixed effects do play a role, that strict exogeneity is an unreasonable assumption for most of the variables (invalidating the classical panel data methods) but after controlling for these issues there are economic fundamentals, including recent default, that significantly influence the spread. There is also some indication that the significance of distant default is not just a consequence of omitted country effects.

6.2 The structural form

The results of our benchmark specifications are presented in Table 4. As motivated in Section 5, we choose as our main structural form estimation the one in first differences, since it provides the framework that allows us to make correct inference on the overidentification test and the RHS variables. We also briefly comment on some results from the level specification, as they can shed light on the channel decomposition of *past distant default*.

Overall, there are three important findings we would discuss: the influence of the future default indicator, the coefficient of the benchmark yield, and the effect of the recent default indicator. Starting with the first, the future default's point estimate is strikingly robust across all methods (being around 0.35), and in almost all of them significant at the 10% level.

[14] The difference with the not-instrumented FD version is mostly with respect to the significance of this variable.

Although the mean of this indicator is just 0.2, this is not very indicative of its influence, because the variance is large and for many countries the indicator's value is around 0.5 and even 1. If we consider an increase in the indicator from its median to the 90^{th} percentile, then this would raise the spread by approximately 0.17. Consequently, the coefficient can be considered as sizable, as the sample mean of the spread is 1.33. This is an important finding, because it suggests that expected default risk was priced in the lending decision and that the debt crisis of 1982 was, to this extent, "anticipated". Nevertheless, it remains true that for many countries this prediction was correct mainly in sign, and less in the size, as the quantitative effect of the future default risk is small.

A second general conclusion is the large significance of the benchmark rate, which was also present in the reduced-form results. Actually, the most robust and significant result of both the reduced and structural form is the negative coefficient on the benchmark yield. Given that the spread is defined as the difference between the loan rate and the LIBOR rate, this result is equivalent to the finding that the loan rate responds less than one-in-one to the world interest rate (the reaction coefficient is around 0.9). This conclusion is found also in Eichengreen and Mody (1999), Benczur (2003) and Uribe and Yue (2006).

The central results concern the channels of influence of the economic fundamentals, particularly that of the recent default indicator. The starting point is to use the benchmark yield and the future default indicator as the only explanatory variables, which is done in Column (1). The first-stage summary statistics indicate that the instruments are relevant, with a partial R^2 of 0.089 and a value for the F test of the joint significance of the excluded instruments of 16.83.[15] Also the Anderson canonical correlations likelihood-ratio test of whether the excluded instruments are relevant (the null being that the equation is underidentified) rejects the null with a p-value smaller than 0.04.

However, the overidentification test does not offer strong evidence of a correct specification, with the p-value being 0.198. Our strategy is to interpret that as the influence of some fundamentals (used as instruments) on the spread not going only through predicted default probability. Then, the next step is to look for variable(s) that could improve upon overidentification and/or be significant as extra RHS variables. Although these two criteria should give the same conclusion, the small sample implications might affect their power in detecting the right answer. The natural candidate is the recent default variable. Its inclusion gives the results displayed in Column (2): the p-value of the overidentification test increases to 0.327, and the variable is very significant and has the right sign.

While this might look like the final specification (given the specification tests) we still explore whether additional variables should be included. After a thorough search, we find that the reserves to imports ratio is such an additional variable: it has a positive, significant and

[15]This value is higher than the one (equal to 10) considered in Staiger and Stock (1997) as a rule of thumb critical value for considering the instruments weak.

robust coefficient and it also improves the overidentification result (the p-value becoming 0.463, as in Column (3)). [16] It appears then that both these extra RHS variables improve on the overidentification test. Thus, after running several robustness checks we interpret Column (3) as our benchmark specification for the structural form.

In terms of size, the recent default measure does not appear to have a large economic significance: given that there were not so many countries in our sample that had a large amount of private arrears, it is not surprising. Interestingly however, our analysis shows that this measure is statistically significant and in some cases economically too, as there are countries for which this extra "punishment" effect accounts for 32-56 basis points. For the reserves to imports ratio variable, the quantitative effect is more pervasive. An increase in the measure from its median to its 90th percentile would increase the spread by 24 basis points (around 20% of the average spread).

These numbers suggest an order of magnitude from which we conclude that the "punishment" effect of the recent default history is a sizable determinant of the spread, as are the future default risk, benchmark interest rate and reserves to imports ratio. Based on our specifications it appears that as countries worsen or improve on their risk factors there is an alteration in their credit terms but it is seems that this takes place significantly only for more considerable changes in risks.

Finally, we briefly touch upon the level specification. The only reason why it is relevant is that it can separate the effect of *distant* default by channels. Unfortunately, estimation in levels is plagued with several problems which render it unreliable. The presence of fixed effects calls for a FE or RE specification, where the strict exogeneity assumption fails, and it suffers from a very significant rejection of the overidentification test. Also, the instrumented future default risk appears very insignificant and the point estimate is much lower than the one resulting from the correctly specified first differenced estimation. We attribute these to the failure of the strict exogeneity assumption.

If instead we ignore fixed effects and run the pooled level IV the results are broadly close to our benchmark first-difference specification: the instruments have a similar first-stage explanatory power, there are extra structural effects coming from recent default and reserves to imports ratio, with the latter two variables also improving on the overidentification test up to 0.2. When the past default dummy is included as an extra RHS variable, it is not significant and decreases the p-value on the overidentification test. Thus, this specification would suggest that *past distant default* affects the spread through future default risk, and not

[16] Note that the first stage F-statistic for the excluded instruments decreases, but that is mostly because the added exogenous variable takes away (by construction it includes the lag of recent default, which is one of the excluded instruments) some of their significance. Also, note that the point estimate of the instrumented default risk is very similar when the extra exogenous variable is included.

through a punishment channel. Given the problems outlined earlier, we would not trust enough this specification to make a strong conclusion.

6.3 Robustness

While checking the robustness of our results, we found that many initial specifications were sensitive to a very small number of extreme outliers. The results presented here are, on the contrary, robust to most changes in the set of observations or instruments. The null of homoskedasticity in the regression of Column (3) is not rejected by the Pagan-Hall general test statistic (p- value of 0.49), which suggests that, given its inferior small sample properties, an asymptotically efficient GMM is not needed.[17] Also, there is no evidence for serial correlation of the errors in this first difference specification.

We now discuss several alternatives in the construction of three key variables: the annual average spread, the recent and the future default variable. We first check whether these alternative default indicators are good proxies (in the sense of having significantly positive reduced form effect),[18] and then for those which pass this "reduced form test" we check whether our benchmark results remain valid. In all cases, we followed the same outlier filtering applied to the particular default indicator at hand (excluding the top and bottom 1% for the recent default and 2% for the future default indicator).[19]

Starting with the first, we reran our estimations (the ones reported in tables 3 and 4) with a quantity-weighted average spread (instead of weighting by quantity and maturity). Reassuringly, the point estimates, their significance level, and the evolution of the overidentification test's rejection level have remained almost identical.

For the recent default measure, we varied (1) the discounting scheme (besides linear, we worked with a quadratic and a no discounting version), (2) the time window (t-4...t-10, t-1...t-10, t-1..t-3), and (3) whether it is an absolute or relative measure (in dollars or in proportion to some measure of the stock of debt). Besides negligible changes, quadratic discounting had some influence on the first stage fit and the overidentification test of the structural form. In particular, the partial F has increased and remained above 19 for all three cases (the Anderson test showed no major change in its p value), while the inclusion of recent default does not increase the p value of the overidentification test. As the point estimate of recent default remains highly significant and positive, this specification also confirms our benchmark estimates. Turning to the version without discounting, its reduced form parameter becomes insignificant, with the exception of the FE method; implying that it is not a successful proxy for capturing the total effect of recent default.

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[17]Even so, results from a GMM regression were found to be very close.

[18]In all cases, the reduced form effect of distant default remained similar in size and significance.

[19]Detailed results are available from the authors upon request.

Next we vary the time window for which we add up past arrears, combined with linear or no discounting. Again, these measures do not pass the reduced form test: with the exception of pooled OLS, the point estimates are insignificant and/or negative. Finally, we consider versions when we add up past arrears normalized by the amount of new loans of the corresponding year. Unfortunately, these proxies also fail the reduced form test, yielding highly insignificant and often negative point estimates.

Regarding our future default indicator, we consider two alternatives: in one case we split future arrears evenly among past contracts (instead of splitting in proportion of loan size, see equation 7), and in the other we exclude a three year grace period from the summation in equation 8. The first variant has a much lower first stage fit, and the point estimate of future default is also much less significant. This suggests that this indicator is less able to represent default risk in our structural equation. A third difference is that the inclusion of recent default alone does not improve on the overidentification p value, though its size and significance is similar to the benchmark. Finally, allowing for a three year grace period leads to very similar estimates and conclusions, with a more modest first stage fit.

6.4 The channel decomposition

An interesting exercise is to compare the point estimates from the reduced and structural form estimation for the extra risks. For the reserves variable, the estimated coefficient in the structural form is about 90% of that from the reduced form. We interpret this as evidence that this factor has some effect on the spread going through the expected default risk, but most of the influence is through a direct channel. For the recent default, the same comparison leads to the conclusion that its effect is almost entirely a direct one.

As we have already argued before, there can be a multitude of mechanisms that lead to a direct effect of credit history on loan spreads: a change in the set of participating banks, a monopoly rent, strategic punishment, loss recovery. Though it would be important to develop models that further refine these aspects, they all have the common empirical implication of a price surcharge above increased future risk.

Regarding the structural effect of reserves, we offer two interpretations. The first involves a systematic pricing error: either banks systematically overreacted to movements in reserves, or the countries themselves chose, ex post, to invalidate the expectations and defaulted less than anticipated. The second explanation views reserves to imports ratio as a proxy for a currency or liquidity crisis risk. Such an event might imply a contagion from the local currency market to foreign currency markets, investors may suffer a systematic decrease in their “risk-bearing ability” or willingness during currency crisis, or financial market liquidity may decline, making bank also reluctant to lend at unchanged terms.

Summing up, we find that future default risk, recent default history and the reserves to imports ratio offer a robust and meaningful description of the spread. Also, our instruments are relevant and valid. Thus, the reduced-form influence of all variables but the benchmark interest rate, recent default and reserves can be attributed to their effect on the probability of default. Comparing the structural and reduced form estimates for these extra variables suggests that their effect on the spread goes almost entirely through a separate channel than the default likelihood.

7. CONCLUDING REMARKS

We extended the existing empirical literature on the role of credit history in sovereign bank lending along two dimensions. One is that we used a continuous measure of past (recent) default, enabling us to control for country fixed effects. Our other, more important contribution is the empirical strategy that allows for the distinction of multiple channels of influence. The framework, suggested in Benczur (2001), is a structural asset pricing rational-expectations estimation, in which the spread may be influenced by multiple risks and factors. Using the errors in the variables method, we replace the expectation term with its realization and instrument the latter with information available at the time of pricing. Using the overidentification test, we then investigate whether there is any instrument that should be added as an extra RHS variable. We interpret any such variable as influencing the spread not only through expected default risk, but having an extra effect on it.

The reduced-form estimation provides evidence that, after controlling for fixed effects, borrower and regional characteristics, both recent and distant repayment history are significant. This makes the result similar to that obtained by Ozler (1993) and concludes that although country effects do matter, there is a role of credit history in determining sovereign spreads. The structural-form regression provides strong evidence of an extra effect of credit history (a "punishment" effect) in *prices*, above that going through predicted default loss. The major structural specification includes the benchmark LIBOR interest rate, the expected default risk, the recent default indicator and the reserves to imports indicator. All these variables are significant and robust to different specifications.

In terms of the default costs, we do believe that in reality there is a complex mix of trading and political sanctions, financial exclusion, spillovers to other transactions and relationships, signaling and reputation considerations. Our main result is that there is evidence of a variant of this last effect, an extra surcharge in loan prices, which calls for future theoretical refinements to our understanding of sovereign borrowing.

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APPENDIX

Graph 1: The evolution of sovereign spreads for 1973-1981

(compared to BAA-rated US corporations bonds' spread)

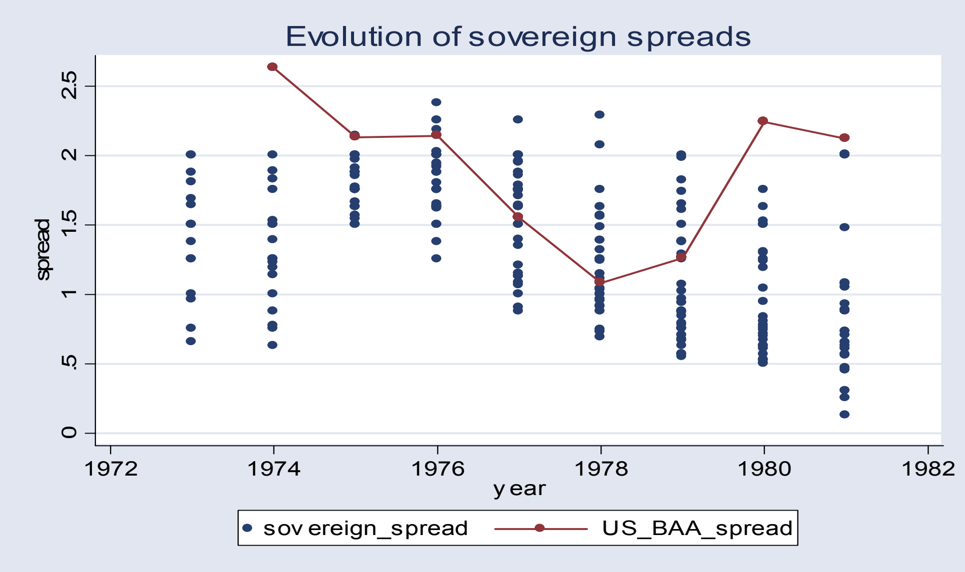

Table 1: The "distant" default variable [(a)]

Variable: distant default dummy	Total observations /countries	Obs. with 1	Mean	Std. Dev.
whole sample	252/46	70	0.335	0.473
restricted sample	149/36	52	0.349	0.478

(a): Constructed as a dummy variable for repayment problems on loans for 1940-1970. The dummy takes the value 1 for a repayment problem.

Table 2: The "recent" and "future" default variables [(a)]

Variable	Total obs. /countries	Obs. with 0	Mean	Std. Dev.	10%.	Median	90%.
recent default [(b)]	211/40	155	3.212	22.996	-0.286	0	0.2
restricted sample	149/36	102	1.187	5.829	-0.0571	0	0.229
future default [(c)]	207/39	43	0.376	2.115	0.003	0.0007	0.461
restricted sample	149/36	28	0.207	0.741	-0.0096	0.0009	0.481

(a): Constructed as continuous variables based on arrears data. A zero means no repayment problem.

(b): The indicator adds private arrears for the time t-4 to t-10 and uses linearly decreasing weights. Information refers to the whole sample.

(c): The indicator adds private arrears for 8 years in the future and divides them by the loan amount. Information refers to the whole sample.

Table 3: Reduced-form estimation: the determinants of the spread [(a)]

	Pooled OLS	*Fixed Effects*	*Random Effects*	*First Difference*[(b)]
	(1)	(2)	(3)	(4)
Benchmark yield	-0.097	-0.096	-0.094	-0.14
	(-10.78) **	(-9.42)**	(-11.36)**	(-2.92)**
Distant default (1940-1970)	0.22	-	0.19	-
	(2.54)**		(1,86)**	
Recent default (last 10 years)	0.0097	0.017	0.014	0.0099
	(1.67)*	(1.80)**	(1.68)**	(1.48)*
Reserves to imports	-0.84	-0.66	-0.73	-0.79
	(-4.96)**	(-3.41)**	(-5.04)**	(-2.93)**
Exports to GDP	0.80	1.11	0.81	1.37
	(2.47)**	(2.08)**	(2.61)**	(2.02)**
Savings to GDP	-0.70	-0.69	-0.83	-0.74
	(-1.26)	(-1.34)*	(-2.14)**	(-1.07)
Repeated borrowings	-0.0017	-0.0048	-0.0037	-0.57
	(-0.61)	(-1.66)**	(-1.34)*	(-1.72)**
Countries with arrears (% of region total)	0.50	0.88	0.55	4.98
	(2.06)**	(1.81)**	(2.41)**	(1.42)*
Constant	2.26	2.06	2.23	-
	(11.66)**	(9.10)**	(14.57)**	
No. of Obs.	178	178	178	149
p- value[(c)]	0.0000	0.0000	0.0000	0.0000

(a): The t statistics are in parentheses; the standard errors are corrected for clustering at country level. *, ** denote 0.2 and 0.1 significance levels, respectively.

(b): The proportion of countries of arrears (first differenced) is instrumented with its first lag.

(c): p- value of joint significance of the regressors

Table 4: Structural-form estimation: the determinants of the spread [(a)]

	Estimation Method [(b)]		
	(1)	*(2)*	***(3)***
Future default	0.361	0.378	0.352
	(2.26)**	(2.29)**	(2.28)**
Benchmark yield	-0.100	-0.099	-0.095
	(-10.52)**	(-9.98)**	(-9.92)**
Recent default		0.0291	0.0156
		(5.26)**	(3.10)**
Reserves to imports			-0.703
			(-2.95)**
First-stage relevance [(c)] :			
R^2 for future default	0.089	0.099	0.104
F statistics [(d)]	16.83	9.18	6.76
Anderson Canon. Corr. LR p-val[(e)]	0.03	0.01	0.01
Structural form:			
Overidentification test [(f)]:			
p-value	0.198	0.327	0.463
Number of observations	149	149	149

(a): The t statistics are in parentheses; the standard errors are corrected for clustering at country level. . *, ** denote 0.2 and 0.1 significance levels, respectively.

(b): The dependent and explanatory variables are first differenced, while the instruments are in general first lags of their levels.

(1) The future default variable is instrumented by the first lag of the following variables: benchmark yield, reserves to imports, savings/GDP, arrears in the region, experience on the market, exports/GDP, and recent default.

(2) As (1) but including the recent default (first differenced, not instrumented) as extra exogenous RHS variable.

(3) As (2) but including reserves/imports (first differenced, not instrumented) as extra exogenous RHS variable.

(c): The reduced form regression of the instrumented indicator(s) on the full set of instruments.

(d): From the F test of the joint significance of the excluded instruments in the first-stage regression.

(e): The Anderson Canonical Correlations Likelihood Ratio test of the null hypothesis that the equation is underidentified.

(f): The Hansen J-statistic

Table 5: The reduced form test (not for publication)

		Specification									
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
		benchmark	weighting	discounting		time window				relative	
pooled ols	point estimate	0.0097	0.0097	0.011	**0.0059**	0.0063	0.0052	0.0065	0.0063	**-12.348**	**-4.34**
	(p value)	0.103	0.108	0.039	**0.304**	0.04	0.093	0.133	0.112	**0.207**	**0.844**
FE	point estimate	0.0173	0.016	0.014	0.0155	**0.027**	**0.0168**	**-0.004**	**-0.00025**	**-6.976**	**47.186**
	(p value)	0.074	0.087	0.137	0.076	**0.761**	**0.145**	**0.658**	**0.979**	**0.723**	**0.317**
RE	point estimate	0.0139	0.013	0.014	0.0107	**0.006**	**0.0079**	**0.0033**	**0.0045**	**-8.11**	**6.64**
	(p value)	0.092	0.103	0.102	0.142	**0.227**	**0.176**	**0.618**	**0.482**	**0.626**	**0.815**
FD	point estimate	0.0099	0.0093	0.0079	**0.0138**	**-0.0136**	**-0.0047**	**-0.011**	**-0.0067**	**-63.8**	**15.26**
	(p value)	0.148	0.164	0.191	**0.375**	**0.161**	**0.743**	**0.173**	**0.637**	**0.153**	**0.819**
reduced form test		*pass*	*pass*	*pass*	*fail*	*fail*	*fail*	*fail*	*fail*	*fail*	*fail*
sample size	level	178	178	177	176	177	175	173	171	174	177
	FD	149	149	149	147	147	145	146	143	146	149

All columns report the reduced form coefficient and the p value of recent default. Column 1 is our benchmark, where we use (1) a quantity and maturity weighted annual spread, (2) linear discounting, (3) a time t-4…t-10 time window and (4) absolute arrears in constructing our recent default indicator. Column 2 uses only quantities as weights in constructing the annual average spread. Column 3 uses a quadratic, while column 4 uses no discounting. Column 5 uses a t-1…t-10 time window with linear discounting, while column 6 uses no discounting. Column 7 uses a time t-1…t-3 time window with linear discounting, while column 8 uses no discounting. Column 9 and 10 add up past arrears normalized by the amount of new loans of the corresponding year, for a t-1…t-10 and a t-4…t-10 time window (and linear discounting).
Numbers in bold indicate the reason why we classified the version as failing the reduced form test.

Table 6: Robustness of the structural form results (not for publication)

	Specification				
	(1)	(2)	(3)	(4)	(5)
	2 RHS variables				
first stage partial F	16.83	16.83	19.18	8.5	12.14
Anderson p value	0.03	0.031	0.039	0.181	0.04
overidentification p value	0.198	0.211	0.189	0.13	0.203
	3 RHS variables				
recent default point estimate	0.029	0.028	0.029	0.032	0.03
(p value)	0	0	0	0	0
first stage partial F	9.18	9.18	25.45	2.98	5.37
Anderson p value	0.016	0.016	0.016	0.228	0.02
overidentification p value	0.32	0.309	0.192	0.182	0.298
	4 RHS variables				
LIBOR point estimate	-0.094	-0.094	-0.094	-0.097	-0.095
(p value)	0	0	0	0	0
future def point estimate	0.352	0.357	0.341	0.243	0.365
(p value)	0.029	0.029	0.027	0.116	0.03
reserves point estimate	-0.702	-0.703	-0.693	-0.707	-0.695
(p value)	0.006	0.007	0.005	0.002	0.006
recent default point estimate	0.015	0.015	0.016	0.019	0.017
(p value)	0.004	0.006	0.002	0.024	0.003
first stage partial F	6.76	6.76	19.43	2.88	4.09
Anderson p value	0.012	0.012	0.009	0.208	0.015
overidentification p value	0.46	0.46	0.38	0.349	0.478

Column 1 is our benchmark specification. Column 2 uses only quantities as weights fir the spreads. Column 3 used quadratic discounting for recent default. Column 4 uses a future default measure where future arrears are split evenly among past contracts. Column 5 includes a 3 year grace period in future default. The sample size is 149 in column 1-4, and 150 in column 5.