Determinants of Spreads on Sovereign Bank Loans: The Role of Credit History*

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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 model that links sovereign loan spreads to realized repayment behavior. Unlike the existing empirical literature, its instrumental variables method allows for distinguishing a direct influence of past repayment problems (a "pure reputation" effect) 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 the reputation channel. Overall, 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 debt and corporate debt. These determine Eaton and Gersovitz (1981) to draw a distinction between the two types of debt in terms of the importance of any net worth criterion. They emphasize that this criterion is essentially irrelevant for the sovereign case and thus, the binding constraint on 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 debt. Several repayment reasons are typically proposed. First, countries that renege on their debts may have their overseas assets seized by foreign creditors. Second, delinquent countries may be subject to direct sanctions, such as trade sanctions. Third, countries that default signal their bad economic fundamentals which in turn affects the ability of the domestic investors to borrow from abroad. Fourth, countries with poor repayment reputation may see their subsequent access to world credit markets impaired, and this impairment reduces their welfare.

While all these penalties are of interest, this paper is concerned with the last explanation. The effect of reputation 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. The argument, based on the ability of the banks to credibly punish badly behaved borrowers, is that the debtor's desire to maintain a "good" reputation is able to sustain, by itself, a positive amount of lending, which takes the form of a credit ceiling.[1]

Since exclusion has essentially never been permanent, the focus of the theoretical methodology has shifted towards understanding more the price punishment. For example, recent models consider different repayment incentives simultaneously, but still investigate whether past default history has any effects on the future credit terms.[2] The conclusion of these approaches is that a negative repayment history can raise the spread on sovereign debt through two main channels. The first is an increased default probability, which is either affected endogenously by past default or just signaled by it. The second one is the punishment surcharge imposed by lenders, which is not related to default risk, and could be referred to as a "pure reputation" effect. The existence of this type of impact is the basic assumption for the theories that analyze reputation as a sole repayment incentive.

Whether credit history is an important repayment incentive remains mostly an empirical question. In this respect, the largest part of previous research has analyzed the

[1] See Eaton and Fernandez (1995) and Obstfeld and Rogoff (1996, chapter 6) for a review on repayment incentives.

pricing of sovereign loans, more specifically the spread above world interest rates. The most frequent empirical research approach in analyzing the determinants of spread 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.[3] 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 main purpose of this paper is to investigate empirically the role of credit history in influencing spreads on sovereign debt. The paper contributes to the previous empirical work in several 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. Secondly, 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 World Bank's "Borrowing in International Capital Markets" for the period 1973-1981, on 46 developing countries. As Ozler (1993) points out, one should focus on a period of market expansion, like 1973-1981, in order to distinguish the impact of a sovereign borrower's credit history from that of a widespread panic. Moreover, analyzing this period allows comparisons with some of the most cited empirical papers on the subject.[4] While most of the variables utilized in the paper are those suggested by the literature, we create some that have not been used before. Although continuous default measures should perform better, the previous literature used only dummy variables. We address this data issue by constructing continuous measures of past and future default, which are based on arrears data from the World Bank's Global Development Finance.

The main estimation strategy used in the paper is a structural asset pricing approach.[5] The starting point is that the spread is determined by expected default risk. Then, like in the errors in the variables method, we replace the expectation term with the realization of the default event and instrument the latter with information available at the time of pricing, which is given by the debtor's characteristics, including credit history. Thus, overidentification receives a central role: it tests whether information affects the spread only through predicted default or there is an extra channel of influence. This setup identifies the risks priced by the market and offers a structural interpretation for the influence of the borrower's characteristics.

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[2] See for example Eaton (1990) and Chang and Sundaresan (2001)

[3] As in the seminal paper by Edwards (1986) and more recently in Eichengreen and Mody (1999).

[4] Like Edwards (1986) and Ozler (1993) who examined the same period.

[5] As in Benczur (2001).

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 inflation, can robustly and meaningfully describe the spread. Also, our instruments are relevant and valid. The structural form estimation provides strong statistical and economic evidence of a direct, "pure reputation" effect of recent default history.

The structure of the paper is the following: the first section comprises a theoretical and empirical review of the literature on the reputation's role and its possible channels of influence for the spread. The second part explains the empirical strategy of the structural asset-pricing setup. The description of data, the definition and justification of the variables used is presented in the third section. The fourth part discusses the main econometric problems related to estimation and describes the reduced- and structural-form results, while the last one concludes.

2. LITERATURE REVIEW ON THE ROLE OF CREDIT HISTORY

2.1 Review of theoretical models

Much of the existing sovereign debt literature is devoted to identifying incentives for countries to repay and for investors to lend; or in other words, to explain why sovereign debt markets exist at all. One stream of research, pioneered by Eaton and Gersovitz (1981), points to the need for the borrower to access the credit markets repeatedly as a motivation for a sovereign borrower to repay their debts. In these models, the sovereign borrower will be excluded from the world credit market once they repudiate their debt obligations. Hence to maintain access to future loans, they have to develop a "good reputation" by repaying the existing debt. There is a maximum safe level of borrowing at which the costs of default just exceed the benefits, which could mean that the country has access only to a partial insurance.

Similar to Eaton and Gersovitz (1981), Grossman and Van Huyck (1988) also model risk sharing through sovereign debt using the argument of reputation. In their model, the lender can distinguish the "excusable" default, which is associated with implicitly understood contingency, from the "non-excusable" default. Atkeson (1991) extended the work of Grossman and Van Huyck (1988) by modeling the asymmetric information between the borrower and the lender, under which the latter cannot monitor the former's activity. Along the equilibrium path, a bad state is always associated with a punishment phase in order to preserve the borrower's incentive not to cheat in the future.

Papers based on reputation also explicitly and implicitly assume that the sovereign country is unable to enter into another financial agreement (i.e. save abroad and earn the market rate of return) after it defaults on the initial debt service. Relaxing such an assumption might destroy the self-enforcing equilibrium based on reputation, as Bulow and Rogoff (1989) forcefully point out. They also argue that instead of the reputation for repayment, it is the threat of economic and political sanction held by the lender that enforces a positive lending equilibrium.[6] However, there are some important qualifications to this argument that can save the reputation approach (see for example Cole and Kehoe (1996)).

All this literature analyzes the cost of reputation as the exclusion from the international financial market. But exclusion has virtually never been permanent. While there were such cases in history, it was mostly times of overall contraction (like the one from the 1930s to 1970s), in which the market did not differentiate between previous defaulters and non-defaulters. Thus, it seems more appealing to model reputation in a more specific framework that allows for re-borrowing and expresses its punishment more in terms of a higher spread than in a denial of a loan. Moreover, one can also modify the infinite horizon assumption, in which reputation has value because lenders adhere to a "trigger strategy" (never lending again if the borrower ever defaults) and analyze a finite horizon. We present here two such approaches.

The micro-finance literature usually analyzes two types of borrowers, whose identities are their private information, in two periods. For example, Eaton (1990) employs this approach for a sovereign debtor, by considering one type as "good" in that it can be punished directly for nonpayment, and the other as "bad", which suffers no penalty if it defaults. One important result is that only when lenders can recommit to discriminate between borrowers, will the solvent bad borrowers repay in the first period. The overall conclusion is that there is a variety of equilibria for each of the different situations, depending on the parameters of the models, with some of them implying that those who default pay more in interest or are denied loans.

There is a more recent approach, as in Chang and Sundaresan (2001), which uses a dynamic model of sovereign borrowing to examine optimal consumption and default strategies. The dynamic perspective allows the study of the effect of credit history, given the

[6] Another approach to understand default costs is that governments repay their sovereign debt to affect agents' expectations about the fundamentals of the economy (Sandleris, 2004). A default signals bad fundamentals and thus decreases the domestic firms' net worth and their ability to borrow, which in turn generates a contraction in foreign lending to them and a decline in investment. The author proves that such an effect is by itself enough to provide incentives to repay and that a default should be reflected in higher default risk.

need to reenter the credit markets in the future.[7] They start from the Bulow and Rogoff's (1989) insight that the essential repayment incentive is the political and economic sanctions held by the lender, but also explore the effect of reputation.

They add arrears as a proxy for "bad" credit history, and argue that higher arrears imply a more costly re-borrowing opportunity for the sovereign borrower since he must, at least partially, clear the higher accrued arrears, before re-signing other loan contracts. They find that higher arrears imply an unfriendly re-borrowing opportunity and hence the borrower will wait longer and build up a higher wealth level before re-entering the credit market. Also, the more valuable the future re-borrowing opportunity, the more conservative the consumption policy is (e.g. cutting the consumption rate as the wealth decreases). Hence, one would expect that the borrower with good credit history (lower arrears) will have a lower probability of default (or high probability of re-borrowing) and a longer conditional expected time to default (or shorter conditional expected time to re-borrow). They go on to illustrate this point by using a Monte Carlo simulation approach.

In these theoretical models, one can distinguish two channels through which credit history affects the sovereign debtor. One is the increased probability of default and the other is what could be called a "pure reputation" effect. According to the first one, past repayment problems are either simply a signal of a future default problem or will endogenously imply a higher default probability. The second channel means an increase in spread or a denial of a loan without any implication coming from the future default probability, but only because the lenders punish the defaulters so to enforce better initial payment. This happens either when "reputation" is the only workable tool for giving incentives for the debtors (like in the traditional "trigger strategy" type of reputational equilibrium) or when it is used to complement the direct sanctions and improve the payment behavior of the pool of borrowers (like in the micro-finance literature).

Accounting for the two possible channels of influence is essential for understanding "reputation" as a repayment incentive. If past default increases the spread through a higher probability of future default, and hence, lowers the probability of repayment, then the *expected* repayment is not altered. In this case, "reputation" cannot represent a repayment incentive. Only if past default would increase the spread on top of raising the probability of future default, would the expected repayment get higher and maintaining a "good reputation" would be a reasonable incentive to repay.

The main purpose of the paper is to address these important issues from an empirical perspective. While the theoretical literature on the reputation effects is quite rich there is surprisingly little empirical research that focuses on the role of credit history. Before moving

[7] Yue (2005) and Kovrijnykh and Szentes (2005) are also recent examples of models where default is followed by a temporary worsening of borrowing terms.

on to presenting the approach used in this paper, we will describe some related empirical results.

1.2. Review of empirical literature

There is some direct evidence on the repayment incentives of a sovereign debtor. Conklin (1998) finds extensive evidence consistent with sanction -based theory (such as in Bulow and Rogoff (1989)) in his analysis of the lending by a Genoese - led cartel to Philip II of Spain (1556 - 1598). He shows that the King tried to renege on his debt and the Genoese applied an additional penalty to enforce their claims, and the King ultimately repaid. According to the author’s estimate, the King’s observed debt ceiling, the cost of enduring the penalty and its ability to repay are all consistent with the predictions of Bulow and Rogoff (1989). On the other hand, English (1996) reached the opposite conclusion based on his analysis of American States default during the years 1840’s. He shows that even though eight States defaulted between 1841 and 1843, most of them chose not to do so despite the fact that creditors could not enforce repayment by imposing military or trade sanctions. He speculates that states repaid their debts in order to maintain their reputation in debt markets, though he does not find any direct historical evidence that state governments had any reputational concerns.

The more relevant issue for our discussion is how a country’s credit history affects the borrowing cost of a sovereign. The usual approach has been to assume only one determinant of the spread (default risk), and postulate a relationship between the spread and the probability of default, and another between the probability of default and economic fundamentals. Merging the two equations, one gets a reduced-form type of relationship between the spread and the fundamentals. This approach has been the base of a very wide empirical literature that analyzes the pricing of bonds and bank loans, both for developing and developed countries.[8] Among these, a few studies include a variable that reflects credit history. For example, 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 are forced to pay.

A very important contribution to the issue of reputation is Ozler (1993), which has been cited by most theoretical papers as the main evidence of an effect of repayment history on credit terms.[9] 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

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

repayment history (which she identifies as the 1930s through 1960s), but not by distant history (before 1930). Also, the countries that acquired sovereignty only after the 1930s are charged higher interests rates.

Reinhart *et* al (2003) is a recent 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 (or of what the authors call "debt intolerance") 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 and Reinhart *et al* 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. Such a dummy will thus not be able to differentiate sufficiently among countries. Moreover, "punishment" considerations are likely to make lender governments and financial markets focus on a "more recent" history, of events that happened for example in the last 10 years, instead of the last 30 years. 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 traditional framework cannot succeed when there is more than one determinant of the spread (besides default risk). More importantly, it is unable to identify the causal reasons why a fundamental affects the spread. Furthermore, it relies on two restrictive assumptions about the functional form of the relation between the spread and the risk probability and that between the risk probability and economic fundamentals.

The inability to distinguish between different channels of influence is particularly important for the credit history case. As underlined in the review of the theoretical literature, there are two ways in which history could affect the spread. Looking only at the reduced-form results – as Ozler and Reinhart *et al* do –, one cannot separate the two effects. The above concerns have motivated the empirical setup of this paper, which will be described in the next section.

2. The empirical strategy: a structural asset pricing regression

The starting point in the asset-pricing decision is that the spread reflects some perceived risk differences. The main problem in estimating this is that the process is forward-looking, involving unobserved expectations of the risk (possibly multiple risks) based on information at the time of pricing. Mainly three solutions have been tried to overcome this

[9] See for example Eaton (1990) or Obstfeld and Rogoff (1996, p.379)

issue. As reviewed in the previous section, one widely used approach has been to assume specific functional form relations between spread, risk probability and the economic fundamentals to get, by substituting one into the other, an estimable reduced form equation. 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 probability. Cumby and Pastine (2001) employ this idea and find that different issues involve different default probabilities. But both these two methods, like the first one, suffer from a common problem: they cannot identify more than one source of risk and test for a systematic extra effect of a country's characteristic.

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 will imply 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,[10] one can start from the representation of the structural form of the pricing equation:

$$s = \alpha + \beta R + \lambda_1 E[r_1 \mid Z] + \ldots + \lambda_n E[r_n \mid Z] + \varepsilon_1 . \tag{1}$$

where s is the spread, Z contains information available at the time of pricing, R is the benchmark interest, and $r_1, \ldots, r_n$ are future risk variables, like default risk. The paper will include in the main specification only the default risk as a rationally expected variable. Let $p_{it} = E[d_{it} \mid Z_{it}]$ denote the conditional probability that, as of information available at the time of pricing (Z_{it}), country *i* does not fully repay its outstanding bank loans in the future (neglecting the possibility of a selective default). Then our basic specification for the spread is as follows:

$$s_{it} = r_{it} - R_t = \alpha + \beta R_t + \lambda p_{it} + \varepsilon_{it} . \tag{2}$$

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

$$r - R = p(1-x) . \tag{3}$$

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

[10] See Benczur (2001) for further details.

$$d_{it} = E[d_{it} \mid R_{it}, Z_{it}] + \varepsilon_{2it}, \tag{4}$$

where Z_{it} is the information set of country and world fundamentals, available at the time of pricing. Thus, they usually correspond to data from year t-1 and earlier. Also, given rational expectations, $E[\varepsilon_{2it} / R_{it}, Z_{it}] = 0$. Then equation (2) becomes:

$$s_{it} = \alpha + \beta R_t + \lambda E[d_{it} \mid R_{it}, Z_{it}] + \varepsilon_{1it} = \alpha + \beta R_t + \lambda d_{it} - \lambda \varepsilon_{2it} + \varepsilon_{1it} \tag{5}$$

Now, d_{it} is not orthogonal to the compound error term, since it is not orthogonal to ε_{2it} (prediction error) and ε_{1it} (possible simultaneity problem). But according to the EVM approach, one can use the information set $\{R_{it}, Z_{it}\}$ as valid instruments, since it is correlated with the default event (from the prediction equation) and uncorrelated with the error term (from the rational expectations assumptions). Actually, as Wickens (1982) argues, one important advantage of this method is that it will provide 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 one (the number of risk factors in general), 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. But more importantly, the instruments might be proxying for a missing term in the structural equation, which might be due to the direct effect of some factor on the spread, above the one coming from the influence on the predicted probability. This would mean replacing (5) with the following modification:

$$s_{it} = \alpha + \beta R_t + \lambda d_{it} + \theta X_{it} - \lambda \varepsilon_{2it} + \varepsilon_{1it}, \tag{6}$$

where *X* is an added RHS variable. Then, there are two important points to check: first, whether, compared to equation (5), *X* has to be included as extra RHS variable to restore the acceptance of overidentification. Second, whether the estimated θ is significant. The two points ought to be connected, in the sense that if *X* is the cause of the rejection of overidentification and wrongly excluded from the model, its coefficient should be significant. But in applied work, it is possible that only one of the indications is present.[11]

[11] Because the overidentification test is not powerful in small samples or that the estimation is inefficient.

This setup allows analyzing whether there will be any direct channel through which credit history affects sovereign spreads. While this is the basic idea behind out structural estimation strategy, we need to resolve some additional obstacles in the empirical application. Some of them refer to data availability and variable usage, while other to specific econometric issues.

3. DATA AND VARIABLES DESCRIPTION

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 their limitations.

The choice for this period was determined 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. The initial dataset contains information on 757 commercial bank loan contracts to 46 developing countries and were obtained from various issues of the World Bank's "Borrowing in International Capital Markets".[12]

Since the economic fundamentals are mostly available at the annually frequency, we constructed yearly measures for the spread. Just like Easton and Rockerbie (1999), we used a weighted average of the original spreads, using as weights the loan quantities and maturities:

$$\overline{s}_{it} = \frac{\sum_{i=1}^{k} s_{it} q_{it} m_{it}}{\sum_{i=1}^{k} q_{it} m_{it}}$$

where s_{it}, q_{it}, and m_{it} denote the spread, loan quantity, and maturity of the *i*th loan contract for one country during one year. Edwards (1986) and Ozler (1993) used only the loan quantity. Our approach, however, will account directly for the simultaneity of the price and maturity of the loan in the lending decision. This transformation means that we will be left with 252 yearly observations and an unbalanced panel.

Unfortunately, data availability and influential outliers reduce the working sample. Thus, data for arrears, used for the recent default measure, is available only for 38 countries, resulting in 203 observations. Other country fundamentals are also not available for some countries, and the use of first lags, which turns out to be essential throughout the paper, means that one observation from each country is lost. Furthermore, the presence of a very small number of extreme outliers, mainly in the constructed variables of default, seemed to make the results sensitive and hard to interpret so we excluded them from the main regressions. The final sample for which we report the results is 127 observations and 33 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.

Past and future default variables

We differentiate between a "distant" and a "recent" default history. There are reasons to believe that recent history matters more, or at least as much, as the more distant one. Indeed, Ozler (1993) finds that repayment difficulties happening before the 1930s did not significantly matter for spread in the 1970s, while those afterwards did.

For "*distant*" history we use two indicators. One includes 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. The second dummy variable is more exhaustive and adds to the first one by including any repayment difficulties on bonds during 1958-1967 (source: Ozler (1993)). The two variables are meaningful because they refer to a period in which all countries were receiving loans, so one does not have to assume that "no history" means "good history". The results are more robust and conclusive when we use the first indicator, of repayment problems on bank loan debt.

Table 1 presents some of its basic summary statistics, the number of observations and countries that had past 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 these indicators are very similar to those used in Ozler (1993), the more important contribution is the indicators reflecting "*recent*" history. Besides our *a priori* view that recent defaults should have larger importance, this indicator allows including a continuous variable instead of a dummy to reflect past repayment behavior: we construct it

[12] Data on fees and commissions are not reliably available. It is noted, however, that these costs are low relative

from data on debt arrears (both interest and principal arrears) on long-term debt outstanding, available since 1971 from the *Global Development Finance* CD-ROM.

The use of arrears to define a payment problem is reasonable but subject to one important qualification. One might argue that data on repayment reschedulings should also be included. There are many reasons why we still stick to arrears. The first is the limited availability of data on rescheduling arrangements.[13] Second, as Cline (1984) notes, debt reschedulings are usually preceded by the accumulation of arrears on debt payments. This implies that reschedulings are only important to the extent that they take place independently of arrears (Peter, (2001)). Third, most of the theoretical models on the pricing of sovereign debt use the arrears as indicator for the repayment history (e.g., Chang and Sundaresan (2001)).

The indicators for recent default history are computed using the following steps:

1. For each country we use the time period 1971-1981 and for each of these years, we add the arrears (on interest and principal to both official and private creditors), calculate the absolute change from one year to the other and normalize this change by dividing with the total debt stock.[14] Let this measure be denoted by $arrears_{jt}$, where j=1,…,33 for countries[15] 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,…,33 and each t=1,..,11 with t_0=0 (for year 1970). Thus it is an indicator for the discounted repayment problems of the last 10 years.

What this formula actually does is, for each year, to add the variable $arrears_{jt}$ starting from the first lag and going back until 1970. The addition of these past values is weighted by decreasing 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.[16]

A further step could be taken to separate the effects of recent history. One might argue that a closer repayment problem would influence the spread only indirectly, through increased default probability, since it is very likely to reflect problems that could generate

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to the spreads (see Edwards (1986)).

[13] See Peter (2001) for a survey on the literature on this, also showing that most of these studies have used arrears as at least one of the indicators for default.

[14] we use the following data series: *DT.DOD.DECT.CD* for Total Debt stocks; *DT.IXA.DPPG.CD* for interest arrears and *DT.AXA.DPPG.CD* for principal arrears

[15] Data is not available on the CD-ROM version for eight countries: Cyprus, Greece, Portugal, Spain, Iran, Oman, South Africa, United Arab Emirates. For the first four countries we used the paper version, but data is still not available there for the last four.

[16] We tried various additional ways of cumulating arrears: not using these coefficients at all, or instead using a different functional form, like a quadratic decay. In general, the results did not change.

future defaults. On the other hand, a more distant default is expected to affect rather directly the spread, through a punishment surcharge asked by the lenders. We have experimented with a separation into the last three years and the preceding seven. The first choice did not lead to stable and meaningful estimates, while the second gave essentially the same results as the full ten year measure.

We construct our future default measure by using again the GDF data on debt arrears. This variable should reflect the realization of losses for a loan offered, so one could add the arrears for a period similar to the average maturity. Since private lending was dominant in the analyzed period, we include only the private arrears in the default indicator, and not combine them with the official arrears, like in the credit history case.[17] Also, different from the recent default history case, we do not normalize the accumulation of arrears with the total debt stock. While both the absolute version and the relative one have reasonable motivations, we find that the normalized measure does not give meaningful results. Hence we add the absolute size of private arrears for the next 8 years following each year in the sample period (1973-1981),[18] undiscounted. Simply adding the flows for all years would mean adding the same observation as many times as there are observed contracts before that,. Instead we split equally the flow for one year by this number of previous participation on the market, assuming equal probability of a previous contract to go into arrears. The choice of 8 years was determined by the average maturity on the loans, which was 7.8 years.[19]

Table 2 provides some brief descriptive statistics of our benchmark choice for future and recent default. For recent default, around 50% of the observations are equal to 0, for both the full sample and the reduced one, of 127 observations, with around 30% of the countries having a complete unblemished record, and respectively around 40% and 20% for the future indicator. This latter result is explainable by the 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 the other factors, including here the economic fundamentals. The sources for this category of variables were *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 large number of variables: *debt to GDP*, *reserves to imports, debt service to exports, current account per GDP, exports to GDP, growth of per capita GDP, growth of gross investment, GDP per capita, inflation, credit to private sector per GDP, new sovereignty.* This latter dummy variable reflects whether the country has gained sovereignty

[17] Although robustness checks were made adding official arrears.

[18] Data for South Africa and United Arab Emirates is missing.

before or after 1930. The source is Ozler (1993), who shows that more recent sovereigns have been charged higher rates.

In addition to these, we constructed four more variables that could depict other important factors:

Currency crisis: a dummy for an at least 50% devaluation in the given year, abandoning a pegged exchange rate for a float, or moving from managed to free float.

*Terms of trade change*s: the number of terms of trade changes (either improvements or worsening) in the last 5 years.

Repeated borrowings: This variable is designed to capture the importance of relationship banking. The first time a borrower appears during the 1973-1981 timeframe, the variable takes the value 1; it is then incremented for each year the borrower reappears.

Proportion of countries with arrears in the region: This variable 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 will use the LIBOR USD 1-year rate. This is employed both in the computation of the spread and in the regressions as controlling for the time coordinate.

4. Estimation issues and results

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

4.1 The reduced form

The reduced form in a panel framework is:

$$s_{it} = \alpha + \beta R_t + \Gamma Z_{it} + c_i + \varepsilon_{it}, \tag{7}$$

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 individual country effects could render the usual pooled OLS estimates either inconsistent and biased (if the heterogeneity is in the form of fixed effects-FE), or just inefficient (for random effects-RE). Unfortunately, the FE estimation (or first-differencing – FD) makes it impossible to include time-invariant variables, like the distant

[19] Robustness checks were made using the 7 years maturity.

default dummy. This is important in our case because the estimated coefficient on the distant default dummy in a pooled OLS regression might be picking up the missing country fixed-effect. 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 double the estimate on the default dummy (see Table 3). Moreover, the standard deviation of the country fixed effects is 0.3, and hence it could be argued that the estimated default dummy explains 50% of the typical variation of the country fixed-effects.

The particular results of the reduced-form estimation are presented in Table 3 and refer to five specifications. As Column (1) shows, most of the explanatory variables are significant and have the expected sign in the pooled OLS specification. The recent default indicator is an important exception. However, the correctly specified first difference regression[20] in Columns (4) and (5) contradicts some of these findings, as the estimated coefficients for recent default, debt to GDP and the proportion of countries in the region with arrears change significantly. This suggests that country fixed effects are important and they do affect some variables. For debt to GDP and the proportion of countries with arrears, the effect is mostly in the precision of the estimates. It indicates that for these variables either the time variation is less important than the cross-sectional one, or there is not enough variation in their first-difference for a precise estimation.

Interestingly, the fixed effects estimation in Column (2) does not pick up these differences entirely, which points to a failure of strict exogeneity. This is a key assumption behind the FE, RE and FD methods, basically requiring that the idiosyncratic error terms, conditional on the individual effect, be uncorrelated with past, present or future values of the regressors. If this fails, then all these methods are inconsistent. The same reason could explain the doubtful conclusion of a Hausman test for comparing the FE with the RE estimation (presented in Column (3)) which seemingly would not reject the use of RE.

Formally, the strict exogeneity assumption means: $E[\varepsilon_{it}^{'} / Z_{is}, c_i] = 0$, 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 some indicators. For example, a current pricing error that increases the spread might in turn make the government raise future international reserves in order to be better able to avoid any repayment problems. Wooldridge (2002) suggests a test for this: use the FE estimator but also include future values of some variables that are likely to break

[20] The recent default variable, which will be subject to the failure of the strict exogeneity assumption, is instrumented with its first or second lag.

the assumption, and check for their significance. If the future values are significant, then there is evidence that the assumption is likely to fail, at least for those variables.

The strongest case for a failure of the strict exogeneity assumption is the recent default variable, also being one of our key variables of interest. The endogeneity between the spread and the accumulation of arrears is straightforward. The first lead of the recent default variable in the FE specification is indeed significant, supplying further evidence on the failure of strict exogeneity.

When there are concerns about strict exogeneity, the general approach is to use a transformation to remove c_i, and then search for instrumental variables, assuming only sequential exogeneity (Wooldridge, 2002). According to the latter 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:

$$s_{it} - s_{i(t-1)} = \beta(R_t - R_{t-1}) + \Gamma(Z_{it} - Z_{i(t-1)}) + (\varepsilon'_{it} - \varepsilon'_{i(t-1)}) \quad (8)$$

One can notice that if strict exogeneity fails, then there is a problem here as well, since $E[\varepsilon'_{i(t-1)} / Z_{it}] \neq 0$. 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.[21]

Column (4) shows the influence of instrumenting: when this correction is not performed, the recent default variable remains insignificant, just like in Columns (1) to (3). Instead, when instrumented with its first lag (as in Column (4)), the variable is highly significant.

Another reason for the failure of strict exogeneity might be the difference between the "true" recent default and our proxy. If this is the case, then under the assumptions of independence between the proxying error and the true measure (the classical error in variables assumption) and no serial correlation in these errors, the second lag of the "imperfect" measure can be used as a valid instrument for the first difference (see Wooldridge, 2002). Using the second lag (as in Column (5)) instead of the first one gives similar results, but decreases precision due to the smaller sample. Thus we conclude that the proxying problem is not detrimental.

In conclusion, there are five different estimations that we perform for the reduced-form analysis: the basic pooled OLS, fixed-effects, random effects, and first-difference with using the first or the second lag as instruments. Except for the first and the last two, all the others are inconsistent if strict exogeneity fails. Our findings suggest that fixed effects do play a role

[21] In fact, it would be sufficient to instrument those that are suspected to break the strict exogeneity assumption. But this choice is not obvious and readily testable.

(invalidating the pooled OLS specification), and that most economic fundamentals, including recent default, are influencing the spread. Regarding strict exogeneity, it is influential for the crucial recent default indicator, but the proxying problem is modest.. Based on this, Column (4) of Table 3 is the appropriate estimation method, incorporating fixed effects by first differencing and employing an IV correction (first lag) for the failure of strict exogeneity. As a result, default history (the recent default indicator) still matters, even after controlling for fixed effects. In order to separate this reduced form effects through the two main channels of influence, we now turn to the structural form.

4.2 The structural form

The starting point in estimating the structural-form is equation (5):

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

where d_{it} is instrumented with time t information *(Z).* As our reduced form results indicate the presence of fixed effects, we focus only on this case in our structural analysis.

4.2.1 Estimation issues

Given the presence of fixed effects, equation (5) should be written as

$$s_{it} = \alpha + \beta R_t + \lambda d_{it} + c_i - \lambda \varepsilon_{2it} + \varepsilon_{1it}. \tag{9}$$

This might be estimated with the usual panel-data methods. However, the strict exogeneity assumption is much more problematic and crucial in the structural form 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. Basically, this formal result comes from the effect of the prediction error on the future values of the variables. Given this problem, both FE and RE estimators are inconsistent. The solution is again to eliminate the individual effect, for example by first-differencing and then deal with the lack of strict exogeneity by instrumenting the problem variable(s). This requires only the sequential exogeneity assumption.

This would mean estimating:

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

There are many candidates for instrumenting the future default variable, as any information that is available at the time of pricing is valid. However, the first difference specification causes further complications. The rational expectation assumption guarantees that the prediction error ε_{2it} is orthogonal to any information available at time *t*. Then, as Keane and Runkle (1992) show, only sequentially exogeneity holds: $E[Z_{is}\varepsilon_{2it}] = 0$ and $E[Z_{is}\varepsilon_{1it}] = 0$, for

$s \leq t$. This observation restricts the set of available instruments: Z_{it} is no longer a proper instrument, since $E[Z_{it}\varepsilon_{2i(t-1)}] \neq 0$. 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*. One alternative is to use the lagged difference $Z_{i(t-1)} - Z_{i(t-2)}$. The disadvantage is that differencing worsens the signal-to-noise ratio, thus the estimations become less precise. Another choice is to use both $Z_{i(t-1)}$ and $Z_{i(t-2)}$ separately, which should make the estimates more efficient asymptotically, but might exhibit worse small sample properties.

If some variable, *X*, is believed to be part of the model as extra RHS variable, then equation (6) is first-differenced and becomes:

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

In this case, the RHS variable $X_{it} - X_{i(t-1)}$ should also be instrumented.

In conclusion, in the structural-form we use the first difference estimator with appropriate instrumenting: it eliminates the individual effects, and the right choice of instruments resolve both the issue of the prediction error and the lack of strict exogeneity. The appropriate instruments involve the first and second lags of the regular instruments (time *t* information). Using their levels (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. Though we believe that the first difference estimator is more suitable, we nevertheless performed various versions of IV on the pooled data in levels. Since these results are much less robust and meaningful, we do not report them in any details, but discuss them briefly instead.

Before presenting and discussing the results, we briefly comment on the choice of instruments. 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. The first point is important because if the instruments have little explanatory power, then they will cause a bias in the estimated IV coefficients (Staiger and Stock, 1997). In order to check for this, we report two measures that summarize the first-stage regression. One is the partial R^2 of the first stage regression, while the other is the F test of the joint significance of the excluded instruments in the first stage regression, as reported by the ivreg2 command of Stata (see Baum et al (2003) for more details).

The validity of the instruments can be checked by testing their orthogonality to the error terms. A rejection of the overidentification would mean, in general, that the instruments are either not truly exogenous or that they are wrongly excluded from the regression (model misspecification). However, in the present case, the logic behind the choice of the instruments has been such that they are uncorrelated with the error terms, so a rejection of the overidentification test reveals the misspecification of the model. Hence, the instruments that cause the rejection of overidentification should be included in the regression as extra RHS variables. There is a variety of choices for this test 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, and it is consistent even with intra-cluster correlation.[22]

4.2.2. Results

Estimation in levels is plagued with several problems which render it quite unreliable. Besides the presence of the fixed effects and the failure of strict exogeneity, the level specification lacks explanatory power of the instruments, compared to a first difference one.[23] The advantage of the level specification is that the pooled level IV and the RE estimation can separate the effect of distant default by channels. These specifications suggest that past distant default affects the spread through future default risk, and not through a true reputation channel. However, we would not make a strong case for this conclusion; instead we look for a more definitive answer about the effect of the time-varying recent default indicator.

As motivated in Section 4.2.1 we choose as our main structural form estimation the one in first differences. The results are presented in Table 4 and they provide many interesting insights. Overall, there are three important issues and findings we would discuss: the influence of the future default indicator, the coefficient of the benchmark yield, and the channel decomposition. Starting with the first, the future default's point estimate is strikingly robust across all methods (being around 1),[24] and in almost all of them significant at 10% level. Although the mean of this indicator is just 0.07, this is not very indicative of its influence, because the variance is large and for many countries the indicator's value is around 0.3 and even 1. If we consider an increase in the indicator from its median to the 90th percentile, then this would raise the spread by approximately 0.23. Consequently, the coefficient can be considered as sizable, as the sample mean of the spread is 1.35. This is an important finding, because it suggests that, at least to some extent, the expected default risk was priced in the lending decision and that the debt crisis of 1982 was, to this extent, "anticipated".

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[22] 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.

[23] The first stage regression statistics are (in pooled levels IV) : partial R^2 of 0.11 and p-value of the F statistic of 0.09. The p-value of the overidentification test is 0.05. The conclusion that the instruments have low explanatory power and that the overidentification rejection cannot be overturned by inclusion of some other extra RHS variables is also present in the FE or RE specification of the structural form estimation. The fact that the instruments are „weak" can also cause the overidentification test to overreject the null (see Staiger and Stock, 1997).

[24] The future default indicator is the 8 years measure and is expressed in billions of dollars. While running robustness checks for various sample sizes and instruments used we find that its point estimate ranges from 0.5 to

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 (2005). The world interest rate appears to shift the demand curve by commercial banks for emerging market debt – a rise in the benchmark rate increases the probability of loans while lowering the spread, suggesting that when interest rates rise banks are willing to lend more and at lower spreads (Eichengreen and Mody (1999)).

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 summarizing statistics indicate that the instruments are relevant, with a partial R^2 of 0.27 and a p-value of the F test of the joint significance of the excluded instruments of 0.0067. However, the overidentification test does not offer strong evidence of a correct specification, with the p-value being 0.127. We interpret this that the influence of (some) fundamentals (used as instruments) on the spread does not go 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 0.31, the variable is very significant and has the right sign. Note that the variable is treated as endogenous for the same reason of failure of strict exogeneity as explained in the reduced-form estimation.

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 inflation is such an additional variable: it has a positive, significant and robust coefficient and slightly improves the overidentification result (as in Column (3)). It should be noted however that the variable essential to increase the p-value of the overidentification test is the recent

1.5 and that a reasonable mean of the estimates obtained is around 1. The estimates of 0.7-0.8 in Table 3 are part of the results we obtained throughout the process.

default indicator.[25] Thus, after running several robustness checks we interpret Column (3) as our benchmark specification for the structural form.

In terms of robustness, 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 terms of valid observations or instruments. We present in Table 4 three additional regressions that might be of interest. Column (4) uses as instrument for the recent default variable its second lag, thus being comparable to the estimation in Column (5) of Table 3. Column (5) of Table 4 uses second differences as instruments, which turns out to reduce the predictive power of the instruments (as indicated by the first stage statistics), but still results in similar estimated coefficients. Column (6) is an exactly identified estimation with an expected worse first stage R^2, which explains the less significant and smaller estimated coefficient on future default. The null of homoskedasticity in the regression of Column (3) is not rejected by the Pagan-Hall general test statistic (p- value of 0.36), which suggests that, given its inferior small sample properties, an asymptotically efficient GMM is not needed.[26]

Compared to the structural form estimation in levels, the same first-lag instruments have a much stronger explanatory power in the first difference specification. It seems that future default risk is more difficult to predict in levels than in first-difference.

Summing up, we find that future default risk, recent default history and inflation as an extra term offers 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 inflation can be attributed to their effect on the probability of default. Comparing the structural and reduced form estimates for these extra variables, their similarity suggest that their effect on the spread goes almost entirely through a separate channel than the default likelihood.

In terms of size, an increase in the recent default measure from its median to its 90th percentile would lead to an increase in the spread by 0.35 (around 25% of the average spread), while for inflation the same computation leads to an effect of 0.09. These numbers suggest an order of magnitude from which we conclude that the "pure reputation" effect of the recent default history is a sizable determinant of the spread, as are the future default risk and benchmark interest rate, while the extra effect of inflation is small.

The result that recent default has an extra effect on the spread could be interpreted in a different way from reputation: from an ex-post perspective, the extra spread might have reflected an overreaction to borrower misbehavior. However, such an overreaction is more likely to happen to small, uninformed investors, and not to large banks that offer syndicated loans, as was the case for the period analyzed in our paper. Moreover,

[25] If one includes first inflation as an extra RHS variable, besides future default, the p-value stays the same (at 0.11), while when the recent default is included, the value of 0.42 is obtained

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

even if this was the case, it has the same effect from the country's perspective: once it defaults it faces a higher cost of borrowing in the future, in terms of *expected* repayment, no matter whether this comes from a punishment strategy from the lenders, or it is just the result of a systematic pricing error.

We offer two explanations for the structural effect of inflation. The first involves a systematic pricing error: either banks were systematically wrong in predicting default, or the countries themselves chose, ex post, to invalidate the expectations and defaulted less than anticipated. The second explanation views inflation as a proxy for some other risk factor. Given the dominance of fixed exchange rate regimes in the 1970s, an increase in inflation could have signaled a currency crisis risk. In this case, investors are rational but constrained: a currency crisis might imply a contagion from the local currency market to foreign currency markets, or investors may suffer a systematic decrease in their "risk-bearing ability" or willingness during currency crisis.[27]

5. CONCLUDING REMARKS

This paper analyzed the effect of credit history on sovereign spreads. While investigating whether there is such an impact at all, the main focus was to identify in what way it comes into play. More specifically, there are theoretical reasons to believe that credit history can affect the spread through two channels: one of increased expected default risk, and another via a "punishment" surcharge.

We extended the existing empirical literature along two dimensions. One is that we used a continuous measure of past (recent) default. The traditional dummy variable measures for default history are unlikely to differentiate sufficiently between countries and are difficult to be estimated together with individual country effects. Our approach also incorporated such fixed effects.

Our other, more important, contribution is the empirical strategy that allows for the distinction of the two 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. 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 also an extra effect on it.

[27] See Benczur (2001), who finds that the currency crisis indicator has an extra impact on spread. Although the same indicator is used in this paper, its effect is insignificant. However, since the period studied is different and the indicator might not reflect perfectly a crisis event, one should still consider the effect of this type of event.

The reduced-form estimation provides evidence that, after controlling for other borrower's 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 there is a role of credit history in determining sovereign spread. The comparison of various specifications also indicates that fixed effects do matter, and its proper treatment is to use first differences and then use lags as instruments (strict versus sequential exogeneity).

As for the structural-form regression, when performed in its reliable version of first-differencing, it provides strong evidence, both statistically and economically, of an extra effect of credit history (a "pure reputation" effect), above that going through the predicted default probability. The major structural specification includes the benchmark LIBOR interest rate, the expected default risk, the recent default indicator and the inflation indicator. All these variables are significant and robust to different specifications, and imply interesting conclusions. The negative coefficient for the benchmark yield means that the spread increases less than one-in-one with the yield, a result documented by previous studies as well. The positive and sizable coefficient for future default entails that default risk has been indeed priced in, and the debt crisis of 1982 was, in this respect, "anticipated". The third variable, recent default implies that almost all of its reduced-form effect on the spread goes through as an extra effect, above the one of future default risk. Inflation also appears to affect the spread, to a large degree, through a direct effect and not through the default probability. We offered two possible explanations: a systematic pricing error related to inflation or an extra risk proxied by inflation (most likely a currency crisis risk).

In terms of the default costs, we do believe that in reality there is a complex mix of trading and political sanctions, spillovers to other transactions and relationships, signaling and reputation considerations. Our main result is that there is evidence of the latter effect.

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APPENDIX

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

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

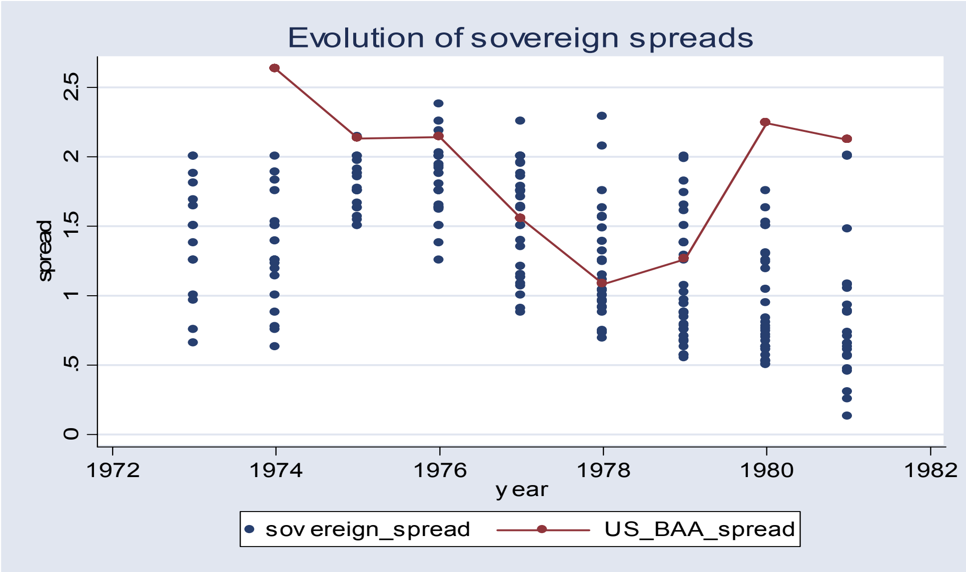

Table 1: The "distant" default variable (a)

Variable: distant default dummy	Total observations /countries	Obs. with 1	Defaulters	Mean	Std. Dev.
whole sample	252/46	70	12 (b)	0.277	0.448
restricted sample	127/33	40	11(c)	0.315	0.466

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

(b): Argentina, Brazil, Chile, India, Indonesia, Jamaica, Liberia, Peru, Philippines, Turkey, Uruguay, Venezuela

(c): as in (b) less: Argentina

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

Variable	Total obs. /countries	Obs. with 0	Partial non-defaulters (b)	Non-defaulters (c)	Mean	Std. Dev.	Min.	Max.
recent default (d)	203/38	100	27	11 (e)	0.141	0.426	-0.120	5.034
restricted sample	127/33	65	20	11 (f)	0.070	0.180	-0.120	0.989
future default (g)	240/44	111	31	10 (h)	0.126	0.485	-0.867	4.794
restricted sample	127/33	50	19	8 (i)	0.074	0.236	-0.262	1.985

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

(b): Countries with some observations in the sample considered equal to 0.

(c): Countries with all observations in the sample considered equal to 0.

(d): The indicator for the last 10 years repayment history that uses linearly decreasing weights. It adds private to official arrears. Information refers to the whole sample

(e): Algeria, Argentina, India, Malawi, Malaysia, Pakistan, Papua New Guinea, Peru, Sri Lanka, Thailand, Trinidad-Tobago

(f): As in (e), less Argentina, Malawi, but including also Colombia, Zambia

(g): The indicator adds private arrears for 8 years in the future. Information refers to the whole sample.

(h): Chile, Gabon, Malaysia, Mauritius, Papua New Guinea, Portugal, Spain, Sri Lanka, Thailand, Uruguay

(i) : As in (h) less Portugal, Spain

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

	Pooled OLS	*Fixed Effects*	*Random Effects*	*First Difference[(b)]*	*First Difference[(c)]*
	(1)	(2)	(3)	(4)	(5)
Benchmark yield	-0.0988	-0.0978	-0.099	-0.0906	-0.0887
	(-9.59) **	(-7.12)**	(-9.56)**	(-9.20)**	(-5.54)**
Distant default (1940-1970)	0.1253		.0458		
	(1.53)*		(0.44)		
Recent default (last 10 years)	-0.047	0.0031	-0.1773	0.6374	1.2639
	(-0.17)	(0.00)	(-0.75)	(2.67)**	(1.11)
Debt/GDP	0.3875	0.1352	0.2362	-0.0606	-.0763
	(2.18)**	(0.52)	(1.17)	(-0.22)	(-0.24)
Reserves to imports	-0.6687	-0.9215	-0.8027	-0.4957	-0.666
	(-4.24)**	-(3.57)**	(-4.62)**	(-2.32)**	(-3.14)**
GDP growth	-0.4278	-0.4652	-0.547	-0.4691	-0.2978
	(-1.69)*	(-1.24)	(-1.55)*	(-1.79)**	(-1.14)
Inflation rate	0.0017	0.0027	0.0024	0.003	0.0026
	(0.73)	(1.55)*	(1.51)*	(2.64)**	(2.39)**
Repeated borrowings	-0.0577	-0.0303	-0.0476		
	(-2.06)**	(-1.31)*	(-2.51)**		
Countries with arrears (% of region total)	0.7327	0.4771	0.6376	0.1096	0.2607
	(3.13)**	(0.77)	(2.58)**	(0.22)	(0.43)
Constant	2.733	2.934	2.977		
	(9.29)**	(6.52)**	(7.18)**		
No. of Obs.	127	127	127	127	99
p- value[(d)]	0.0000	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 recent default variable (first differenced) is instrumented with its first lag. Partial R^2 of the first stage is 0.3176.

(c) The recent default variable (first differenced) is instrumented with its second lag and the first lags of the instruments used in the structural form estimation. Partial R^2 of the first stage: 0.2181.

(d) : 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)*	*(4)*	*(5)*	*(6)*
Future default	0.7448	0.7619	0.754	0.7295	0.8511	0.483
	(1.88)**	(1.86)**	(1.85)**	(1.71)**	(1.76)**	(1.18)
Benchmark yield	-0.1001	-0.094	-0.098	-0.0982	-0.1027	-0.096
	(-9.75)**	(-8.88)**	(-9.07)**	(-7.39)**	(-5.35)**	(-9.76)**
Recent default		1.702	1.355	1.384	0.78	0.801
		(2.92)**	(2.49)**	(1.24)	(0.6)	(2.17)**
Inflation			0.0028	0.003	0.0032	0.003
			(2.89)**	(2.51)**	(2.49)**	(3.13)**
First-stage relevance [(c)] :						
Partial R^2 [(d)] for future default	0.2787	0.2792	0.2793	0.3132	0.1556	0.177
F statistics [(e)]	3.35	3.35	3.35	3.93	2.71	7.01
p- value	0.0067	0.0067	0.0067	0.0028	0.0239	0.003
Partial R^2 [(d)] for recent default		0.3805	0.3594	0.2063	0.1331	0.277
F statistics [(f)]		7.24	7.24	5.47	1.25	7.24
p- value		0.0112	0.0112	0.0262	0.309	0.0112
Structural form:						
F statistics	50.34	31.93	24.13	12.54	16.00	33.98
Overidentification test [(g)]:						Exactly
p-value	0.1277	0.312	0.4219	0.2923	0.1773	identified
Number of observations	127	127	127	99	97	127

(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, debt to GDP, GDP growth, arrears in the region, experience on the market, inflation rate, and two indicators for default history: distant (any repayment problems for years 1940-1970) and recent (arrears for last 10 years).

(2) As (1) but including the recent default (first differenced) as extra endogenous RHS variable.

(3) As (2) but including inflation (first differenced) as extra exogenous RHS variable.

(4) As (3), with the instruments in first lags except recent default, which is in second lag.

(5) As (3) with all the instruments in second differences.

(6) As (3), but the regression is exactly identified. The instruments are the first lags of debt to GDP and recent default.

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

(d): The “squared partial correlation" between the excluded instruments and the endogenous regressor(s) correcting for possible intercorrelations between the instruments when multiple endogenous variables are present (the Shea Partial R^2).

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

(f): From the F test of the significance of lagged recent default in predicting differenced recent default. In (4) the joint significance of the first and second lag is tested, and in (5) the significance of the second difference is tested.

(g): The Hansen J-statistic