NBER WORKING PAPER SERIES

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Working Paper 20391
http://www.nber.org/papers/w20391

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
August 2014

We thank Cristina Arellano, Francesco Bianchi, Craig Burnside, Larry Christiano, Jonathan Eaton, Refet Gurkaynak, Sebnem Kalemli-Ozcan, Gabor Kezdi, Narayana Kocherlakota, Sergio Rebelo, Adam Szeidl, Balazs Szentes, Mark Wright and participants at the AEA Annual Meeting, ESWC, SED Meeting, ESSIM, NBER Summer Institute and at seminars at Duke University, National Bank of Hungary (MNB), Northwestern University, UNC Chapel Hill for helpful comments and suggestions. Part of the work was done while Ilut was a visiting scholar at and Benczur was affiliated with the National Bank of Hungary (MNB). The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 20391
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JEL No. C73,D86,F34,G12,G14,G15

ABSTRACT

This paper presents direct evidence for relational contracts in sovereign bank lending. Unlike the existing empirical literature, its instrumental variables method allows for distinguishing a direct influence of past repayment problems on current spreads (a "punishment" effect in prices) from an indirect effect through higher expected future default probabilities ("loss of reputation"). Such a punishment provides positive surplus to lenders after a default and decreases the borrower's present discounted value of the net benefits of future borrowing, which create dynamic incentives. Using data on bank loans to developing countries between 1973-1981 and constructing continuous variables for credit history, we find evidence that most of the influence of past repayment problems is through the direct, punishment channel.

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1 Introduction

A central issue in limited contract enforceability is that if the identity of a defaulting party is forgotten, then it can reenter the market with a fresh start. The acts of countries and major banks, however, are remembered, thus they cannot simply walk away after breaking their commitments. This implies that one can view sovereign bank lending as a long-term relationship (a relational contract) between borrowers and lenders. In such contracts, parties honor their obligations in order to influence the terms of future interactions. This means that a default or any other form of misbehavior leads to a positive surplus to lenders and a drop in the present discounted value of the net benefits of future borrowing for the borrower. Usual punishments are an increase in borrowing costs or a capital market exclusion.

There is indeed evidence that a default leads to an increase in spreads (Ozler (1993), Eichengreen and Mody (1999), Reinhart et al. (2003), Cruces and Trebesch (2013)). This literature, however, has been unable to identify the precise mechanism of this effect. In particular, it cannot tell apart a retaliation argument from a signaling alternative: in the latter, a default reveals some adverse information about the expectation of the debtor's future output (for example, the type of the debtor), which hurts its future outcomes (Eaton (1996), Sandleris (2008)).

Under pure signaling,[1] countries try to avoid default in order not to reveal their type. But once there is an update of their type, they get loans that are "actuarially fair", conditional on available public information about their future nonrepayment. Past defaults by the sovereign should only matter to the extent that they influence investors' expectations of future default (and recovery rates). Notice that this mechanism only requires that the country still needs financial markets after a default, but it does not have to involve a long-run relationship.

Under punishment, extra costs are added to future prices, giving positive surpluses to lenders even conditional on updated information. This creates dynamic incentives even in the absence of hidden types. To maintain such surpluses, the existence of repeated borrower-lender interactions is essential.[2] This is what we label as "relational contracts".

We present empirical evidence that a sovereign default is followed by such positive lender surpluses. Such evidence has immediate consequences for understanding and modeling sovereign risk, as it points to the presence of dynamic incentives as a repayment mechanism.[3]

[1] Samuelson (2006) labels such a scenario as "reputation". The sovereign risk literature, on the other hand, often refers to the retaliatory contracts as embodying a "reputation punishment." To avoid confusion, we will refer to our two channels as "punishment" and "signaling".

[2] This is clear in the case of a simple retaliation mechanism. We argue later that even a default-driven increase in risk-aversion requires some form of lender-borrower lock-in.

[3] For example, Alfaro and Kanczuk (2005) find that pure signaling is quantitatively insufficient to make sovereign debt sustainable. Our mechanism would increase repayment incentives.

It is also relevant for the broad context of repeated games, relational contracts and reputations. In both cases, there is very little direct evidence on dynamic incentives themselves – the sovereign risk literature, for example, usually calibrates its models to match various aggregate outcomes, or the timing and frequency of default.

The purpose of our paper is to contribute to the empirical sovereign risk literature in two major respects. First, and most importantly, our methodology is able to distinguish between any direct effect of a bad repayment history on the spread, and the indirect one going through increased default probability. Second, we deploy different econometric techniques and variables in order to control for country fixed effects, an important problem that has not been thoroughly dealt with in most of the previous research.

The data on sovereign bank loan spreads is from the World Bank's publication "Borrowing in International Capital Markets" for the period 1973-1981, on 36 developing countries. This period was the heyday of syndicated bank lending to sovereigns. 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.

Our estimation strategy is a structural-form asset pricing regression. The starting point is that the spread is determined by expected default risk and credit history. We also add the BAA rated US corporate bond spread to capture the risk aversion of lenders. As default risk is an unobserved variable, we replace the expectation term with its realization and merge the prediction error into the regression residual. This creates an identification problem, as the realization is correlated with the prediction error. Assuming that expectations are rational, we can use any variable (debtor characteristics) available at the time of pricing as a valid instrument. Identification is thus based on the following intuition: with the exception of credit history (punishment as a dynamic incentive) and the BAA spread (risk aversion), all fundamentals affect the spread only through their impact on the probability of default. Notice that the reduced-form regression of such a framework simply regresses the spread on various debtor characteristics.

In the reduced-form estimation, we find that recent default history has a significant positive influence on the spread, but the inclusion of country fixed effects is necessary. Country effects substantially weaken the effect of distant default history. The conclusion of our benchmark structural-form specification is that future default risk, an overall risk-aversion measure (the BAA-rated US corporate bond spread) and recent default history can robustly and meaningfully describe the spread.

In the structural form, the coefficient estimate for default risk is around 0.41. This means that if we increase our default risk measure by one standard deviation, the implied increase in the spread is 26% of its standard deviation. For credit history, the structural-form estimate is very similar to the reduced form. Its magnitude is around 7.3. A one standard deviation increase in the recent default variable accounts for 15% of the standard deviation of the measured spread. In a simple calculation, we show that the implied punishment is large enough to ensure timely payment of a country with a subjective discount factor of at least 0.89 (assuming a 0.95 world discount factor). Overall, the structural-form estimation offers strong statistical and economic evidence that credit history has a dominantly direct punishment effect on loan spreads. Though we discuss some alternative explanations, we argue that the most plausible interpretation of this result points to the presence of relational contracts in sovereign bank lending.

The structure of the paper is the following. The second section comprises a literature review on the role of credit history. The next part develops our empirical strategy. The description of data and the main econometric problems is presented in the fourth section. The fifth part describes our results, the sixth section reports a simulation exercise illustrating the performance of our empirical approach; while the last section concludes.

2 Related theoretical and empirical literature

The sovereign risk literature has identified two main channels through which a default can influence future borrowing terms. Under signaling, a default reveals some information about the debtor, which hurts its future outcomes (borrowing terms or third party decisions like private investment), so the debtor avoids default in order to send a favorable signal about its fundamentals (as in Sandleris (2008)) or type (as in Eaton (1996)). Though a default does decrease the present discounted value of the net benefits of future borrowing, it does not lead to a positive lender surplus. This implies that signaling also works in a perfectly competitive setting: as long as the country still needs financial markets after a default, it will try to pretend to be a better debtor type than its true type.

Borrower-lender relationships provide additional repayment incentives in the form of punishments, like exclusion from future borrowing. In most cases (like Eaton and Gersovitz (1981), Kletzer and Wright (2000), Wright (2002), Yue (2009)), this punishment is an out-of-equilibrium threat.[4] In some models this is already sufficient to ensure full repayment (Eaton and Gersovitz (1981), Kletzer and Wright (2000), Wright (2002)). Consequently, there is no default or punishment in equilibrium. In Yue (2009), the punishments are the threat points of the renegotiation process, so there is default in equilibrium, but no punishment.

[4] This need not imply the lack of credibility: both in Kletzer and Wright (2000) and Wright (2002), the threats are subgame perfect and renegotiation proof.

There are some models with default and/or punishment along the equilibrium path. In most cases, however, default is a consequence of incomplete markets (debt contracts are non-contingent). Examples include Yue (2009), Sandleris (2008), and Arellano (2008). The recent literature on unsecured household debt (for example, Chatterjee et al. (2007) or Livshits et al. (2007)) also allows for default, with an exogenously set bankruptcy procedure (unrelated to reputation) as the punishment.[5] While these models analyze full default, there is recent theoretical interest in characterizing sovereign debt models with partial default and accumulation of arrears (Arellano et al. (2013), Walsh (2014)), a feature that is important in our empirical implementation. The theory of repeated games with imperfect monitoring implies that even contingent contracts could feature occasional episodes of punishment and potentially "default"as well (Green and Porter (1984)).

Kletzer and Wright (2000), and Kovrijnykh and Szentes (2007) view sovereign borrowing as a relational (or dynamic) contract between borrowers and lenders. In this case, there is an implicit or explicit agreement on lending and repayment terms. Any deviation (default) would initiate some punishment, decreasing the gains from future borrowing and giving a surplus to lenders. This latter property is the key distinguishing feature of punishments (or relational contracts in a broader context) from signalling. We now go into some details to clarify how these positive surpluses arise.

In Kovrijnykh and Szentes (2007), there is no explicit breaking of the contract. Instead, they have a "debt overhang" situation, when the borrower cannot fully repay in a given period, which gives a de facto monopoly power to the incumbent lender. After "writing off" part or all of the initial debt, the incumbent can neglect other lenders and can extract some monopoly rents. If one interprets a debt overhang as a full write-off of the initial (sunk) debt, then the monopoly contract in fact implies an interest rate giving extra profits to the incumbent – precisely after a default episode.

The one-period loan contract interpretation of Kletzer and Wright (2000) also features nonzero lender surplus after a default. Each period starts with some nonnegative repayment R, followed by a zero expected profit loan contract of size L. Suppose that there is default in a certain period, so R is not paid and L is not granted. Next period, the lender expects the borrower to give her the highest (state contingent) R in order to continue the relationship. One can interpret this punishment as a new loan of $L - R$, but for an expected present discounted value of repayment equal to $L > L - R$.

In a competitive market, a competing lender might try to offer a cheaper loan. In dynamic relations, however, positive surpluses can be maintained by repeated lender interactions. In

[5]Within the framework of this literature, our results could be interpreted as bankruptcy leading to an exclusion from regular credit markets, but still having access to a specialized, more expensive form of credit.

Kletzer and Wright (2000), the punishment is compatible with external competition, due to a "cheat the cheater" response of the other lenders. This leads to an implicit seniority of preexisting loans.[6] Kovrijnykh and Szentes (2007) assume such a seniority explicitly, which gives the incumbent lender monopoly power after a debt overhang. The common feature is that preexisting debt limits the impact of outside competition, and allows for punishments which give positive surplus to the lender even in the case of potential entrants.[7]

Switching to empirics, there is a diverse literature aimed at detecting behavior in line with dynamic incentives.[8] Focusing on sovereign borrowing, there is some direct evidence on the repayment incentives of a sovereign debtor (see the survey of Panizza et al. (2009)). The main issue for our discussion is how a country's credit history affects its borrowing costs.

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 a history of debt reschedulings has a weak positive effect on the probability of an issuance while it significantly increases the spread that successful issuers pay. Ozler (1993) uses data on 64 countries for the period 1968-1981, which was one of rapid international lending expansion. Using a pooled OLS regression with time-specific dummies, she finds that the spread is influenced by repayment history between the 1930s to the 1960s, but not before 1930. Reinhart et al. (2003) employs 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 credit ratings.

Our analysis extends this research line in two major ways. The first one concerns the treatment of country fixed effects, which is incompatible with the inclusion of time invariant variables like dummies for repayment problems. We resolve this issue by constructing a continuous measure of recent default.

Utilizing a continuous past repayment indicator, the analysis of Cruces and Trebesch (2013) also handles fixed effects. The authors first assemble a dataset of investor losses from sovereign debt, and then add the size of the haircut as an explanatory variable to a fixed effects, reduced-form regression of the spread. They find that higher haircuts lead to higher subsequent bond spreads and longer periods of capital market exclusion.

Our second, more important contribution is a structural and causal empirical approach.

[6]Drelichman and Voth (2011) find historical evidence in favor of lender's coalitions employing punishment strategies that were sustaining lending based on the "cheat-the-cheater" mechanism described in Kletzer and Wright (2000).

[7]An alternative mechanism is described by Wright (2002): for syndicated loans, each bank wants to maintain a good reputation in this cooperation, which makes them tacitly collude in a punishment.

[8]Greif (1993) and Milgrom et al. (1990) find evidence for repeated games with imperfect monitoring in medieval trade; while Porter (1983) has similar findings in the US railroad industry.

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), Reinhart et al. (2003) and Cruces and Trebesch (2013) do – one cannot separate the two effects.

3 The empirical strategy: identification in a structural-form asset pricing regression

The starting point is that the spread reflects the risk of future non-repayment. A simple linear specification can be derived from risk-neutrality, profit maximization, and partial default on the principal but not on the interest. A similar specification can be obtained as an approximation in a more general setting. In particular, assuming a CARA utility function with a coefficient of absolute risk aversion of a, a haircut of λ and that a random fraction z is defaulted, a first order approximation[9] of the spread is

$$s_t = \frac{1}{a}\ln\left(1 + E\left(e^{az_{t+1}\lambda} - 1\right)\right) \approx \frac{1}{a}E\left(e^{az_{t+1}\lambda} - 1\right) \approx \underbrace{\frac{e^{a\lambda} - 1}{a}}_{\bar{\lambda}} E_t[z_{t+1}].$$

Allowing a non-unit effect of the world interest rate, adding potential extra elements and a pricing error, we get our empirical specification:

$$s_{it} = \alpha + \beta R_t + \bar{\lambda} E(d_{it}|Z_{it,}R_t) + \Theta X_{it} + \varepsilon_{1it}. \tag{1}$$

Here s_{it} is the spread paid by country i on loans obtained at time t, R_t is the benchmark interest rate, d_{it} is the risk of default on the loans obtained by country i at time t, X_{it} is a vector of various extra factors, and $Z_{it} \supseteqq X_{it}$ contains information available at the time of pricing. The error term ε_{1it} is orthogonal to any time t information (Z_{it} and R_t). The two extra effects in X_{it} are credit history and the BAA rated US corporate bond spread, the latter capturing the risk aversion of the banking sector.

Estimating (1) involves latent expectations of the risk(s) based on information at the time of pricing. Three main solutions have been adopted to overcome this issue. One widely used approach has been to assume specific functional form relations between the

[9]The main step is to replace the nonlinear function $e^{az\lambda} - 1$ (as a function of z) by the arch connecting its starting point and ending point ($e^{a\lambda} - 1$). See online Appendix B for further details. Note that the more complex asset pricing model of Longstaff et al. (2011) also turns out to be well approximated by the product of the haircut and the risk-neutral arrival rate of the credit event. Nevertheless, we extensively discuss the impact of risk aversion on our results and interpretation in Section 5.4. We also perform a simulation exercise where we estimate a nonlinear pricing equation with a linear regression (see Section 6)

spread, the risk probability and the economic fundamentals to get, by substituting one into the other, an estimable reduced-form equation. Examples include Edwards (1986), Ozler (1993), Eichengreen and Mody (1999), Easton and Rockerbie (1999). Another solution has been to use proxies for the probabilities, like credit ratings (Kamin and von Kleist (1999)). A third approach has been to use multiple issuances of the same borrower, assuming a common default 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.

We adopt a rational expectations approach and employ the errors-in-variables method (EVM) as a solution. The realization of the expectation term in (1) can be written as

$$d_{it} = E(d_{it}|Z_{it,}R_t) + \varepsilon_{2it}, \tag{2}$$

where $E(\varepsilon_{2it}|Z_{it,}R_t) = 0$ due to rational expectations. Substitute this into (1):

$$s_{it} = \alpha + \beta R_t + \bar{\lambda} d_{it} + \Theta X_{it} + \underbrace{\varepsilon_{1it} - \bar{\lambda}\varepsilon_{2it}}_{\varepsilon'_{1it}}. \tag{3}$$

Now, d_{it} is not orthogonal to the compound error term, since it is not orthogonal to the prediction error ε_{2it} (see equation (2)) and ε_{1it} (possible simultaneity problem). But according to the EVM approach, one can use the information set $\{Z_{it},\ R_t\}$ as valid instruments, since this set is correlated with the default event (from the prediction equation (2)) and uncorrelated with the error term (from the rational expectations assumption and the pricing equation (1)).[10]

This method provides consistent though not fully efficient estimates even when the information set is incomplete or the functional form of the prediction equation is unknown and potentially highly nonlinear (this was already shown by Wickens (1982)). In particular, estimates of the structural form (3) are robust to potentially omitted fundamentals from the set Z_{it}, while the reduced form (4) is subject to omitted variable bias. In other words, we do not need to worry about not using all potential instruments in a structural form or not knowing the functional form of the prediction equation, as long as we have enough good instruments. This is a major advantage, given the potential sensitivity of empirical results to functional form assumptions, selectivity bias and omitted variables.

It is important to stress that a variable included in X_{it} affects the spread through two channels: through an impact on predicted future default $E(d_{it}|Z_{it,}R_t)$, and a direct effect

[10] The pricing error might be correlated with realized future default, but it is still orthogonal to Z_{it} and R_t. It means that those variables remain valid instruments even in this case.

through Θ. The total effect is captured by the reduced-form equation

$$s_{it} = \alpha' + \beta' R_t + \Gamma' Y_{it} + \Theta' X_{it} + \varepsilon_{it}, \tag{4}$$

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

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

the structural form (1) imposes the following restrictions on the reduced form (4):

$$\alpha' = \alpha + \bar{\lambda}\alpha'';\ \beta' = \beta + \bar{\lambda}\beta'';\ \Gamma' = \bar{\lambda}\Gamma'';\ \Theta' = \Theta + \bar{\lambda}\Theta''. \tag{5}$$

This immediately shows the decomposition of the total effect Θ' into the direct effect Θ and the indirect effect $\bar{\lambda}\Theta''$. 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 $\bar{\lambda}$ is identified through the restriction $\Gamma' = \bar{\lambda}\Gamma''$, while Θ is obtained as $\Theta = \Theta' - \bar{\lambda}\Theta''$. This is exactly what an instrumental variables estimation of equation (1) does in one step. With the exception of credit history (dynamic contracts) and the BAA spread (risk aversion), all fundamentals affect the spread only through their impact on the probability of default. They can be thus utilized to separate the direct impact of credit history from its indirect impact through d_{it}.

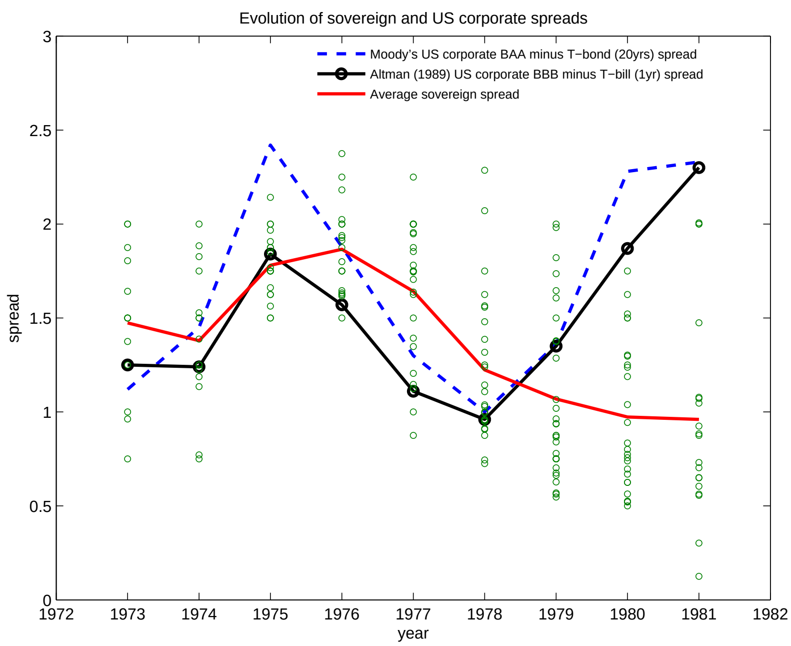
4 Data, variables and estimation issues

The choice of the time period was mostly driven by Ozler (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. We use the period 1973-1981, which witnessed particularly intense syndicated bank lending to sovereign borrowers. 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 the spread (over the 1-year LIBOR) 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".

As we have no access to contract-level characteristics of loans or their future repayment patterns, we average over all contracts of the same country at a given time period. Since the economic fundamentals are mostly available at the annual frequency, 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. As an alternative, we also discuss results which use an average spread weighted by loan quantities only (like Edwards (1986) and Ozler (1993)).[11] This transformation means that we are left with 201 yearly observations. Data availability (arrears, country fundamentals) and the need of first differencing further reduce the working sample, to 161 observations from 37 countries. Finally, we excluded the most extreme values of our constructed default variables.[12] The final sample for which we report the results is 154 observations and 36 countries.

Figure 1 illustrates the evolution of the sovereign spreads in the sample, together with the average BAA- and BBB-rated US corporate bond spreads (see the figure notes for details). The figure shows that sovereigns pay similar spreads to BAA- or BBB-rated US companies.

Figure 1: The evolution of sovereign spreads for 1973-1981 (compared to average US corporate bond spread; in percentage points)



The average BAA-rated US corporate bond spread is the difference between the Moody's BAA corporate bond yield and the fixed 20-year maturity Treasury yield series from the Federal Reserves' website. The average spread on BBB-rated US corporate bonds, over the one-year Treasury yield, is computed by Altman (1989), whose methodology takes into account expected mortality and loss rates. The spreads are measured in percentage points. Note that the two series correspond to very similar risk levels.

[11]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. The World Bank's Global Development Finance database also reports average interest rates weighted by quantities. For a more detailed discussion, see footnote 6 of the working paper version (Benczur and Ilut (2011)).

[12]Specifically, we eliminated 3 observations for the recent default that were more than 10 times the sample standard deviation and the bottom and top 1% for the future default variable.

The variation in spreads is very large in 1981, suggesting that commercial banks were distinguishing between the borrowers, even before the "unexpected" debt crisis of 1982. Finally, one can notice that the sovereign spreads and the corporate spreads strongly comove until 1978, and then move in the opposite direction between 1979-1981.

4.1 Repayment history indicators

There is no clear indication from theory regarding the choice of the repayment history variable. Moreover, in the context of repeated games with imperfect monitoring, punishment is invoked by some imperfect indicator of cheating, and not an outright default episode. The repayment history variable should still be related to the overall loss creditors incurred due to repayment problems. Our choice is thus mostly guided by data availability: binary indicators of repayment problems are available both historically and recently; arrears data is reported by the World Bank from 1971; while debt forgiveness and rescheduling numbers 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.[13]

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 based on Ozler (1993), which includes data for 1956-1968, and Lindert and Morton (1989), which refers to the period 1940-1970.[14] The first two rows of Table 1 present 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 35% of the countries in the sample had some repayment problems during 1940-1970.

While this indicator is very similar to that used in Ozler (1993), the indicators reflecting recent history are our own. 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 arrears to private creditors (both interest and principal) on long-term debt outstanding, available since 1971 from the Global Development Finance CD-ROM. As Cline (1984) notes, debt reschedulings are usually preceded by the accumulation of arrears, thus their presence and size can be a good indicator of potential creditor losses.

[13] There are reasons to believe that recent and distant history have 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 happening afterwards did. In the models of Yue (2009), Kovrijnykh and Szentes (2007), and Benjamin and Wright (2009), it is also recent default (arrears) that matters; in fact, once a country eliminates its arrears, it gets a clear credit history.

[14] We found another indicator of Ozler (1993), referring to privately held bonds, to be insignificant.

Table 1: The "recent" and "future" default variables[a]

Variable	Total obs. /countries	Obs. with 0	Mean	Std. Dev.	10%	Median	90%
Distant default[b]	201/37	70	0.348	0.477	0	0	1
Restricted sample	154/36	56	0.363	0.482	0	0	1
Recent default[c]	198/37	152	0.0037	0.015	0	0	0.0047
Restricted sample	154/36	115	0.003	0.011	0	0	0.0031
Future default[d]	201/37	43	0.143	0.357	-0.0001	0.0015	0.465
Restricted sample	154/36	30	0.146	0.337	-0.0005	0.002	0.659

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

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

c The indicator uses the time t-1 stock of arrears to private creditors and divides it by the time t-1 loan amount. Information refers to the whole sample.

d The indicator adds arrears to private creditors for 8 years in the future and divides them by the loan amount. Information refers to the whole sample. For a discussion of negative values, see online Appendix A.

Our measure of recent default is based on the sovereign borrower's stock of accumulated arrears. The benchmark time t variable controlling recent default is the stock of arrears at time $t-1$ divided by the amount of loans disbursed at time $t-1$. There are several reasons to favor this measure. One, it uses the most recent information available to the investor at the moment of pricing on the amount of arrears accumulated. Second, by normalizing the stock of arrears with the amount disbursed, we are consistent in constructing the recent and future repayment problem indicators as proportional to the loan amounts. Third, as we will discuss in a later section, this measure allows for a meaningful economic interpretation of the estimated coefficient on the recent default indicator.

4.2 Future default variables

The future default variable should closely reflect the realization of proportional losses on a loan. As demonstrated by Sturzenegger and Zettelmeyer (2007), Benjamin and Wright (2009), and Cruces and Trebesch (2013), 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: from any given period onwards, we cumulate arrears to private creditors for a period similar to the average maturity, normalized by the total amount of loans disbursed in the same period (the detailed procedure is explained in the Appendix).

Rows 3-6 of Table 1 provide some brief descriptive statistics of our benchmark choice for future and recent default. For recent default around 75% 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. Table 2 in online Appendix A provides detailed statistics of default indicators at the country level.

4.3 Economic fundamentals

The first set of fundamentals consists of variables that we expect to influence the spread only indirectly. The sources for these variables are Global Development Finance, International Financial Statistics and World Development Indicators. Besides data availability, we are following most of the literature in considering a wide set of country-specific economic fundamentals that could influence the pricing of sovereign loans.[15] We retain for the benchmark specifications the variables that are the most influential in their reduced-form effect on the spread (reserves per imports, growth of per capita GDP), that are important in the first stage regression (GDP per capita, investment growth), plus debt per GDP.

We construct two additional variables that are related to the international financial environment of a country. The first, "Experience" is designed to capture the importance of relationship banking.[16] It is constructed by cumulating the number of years in which the borrower received loans (according to our data source). The second, "Proportion of countries with arrears in the region", is aimed at capturing a regional contagion effect from one country going into arrears (see Edwards (2000) for an overview of the literature on contagion). It is obtained by dividing the number of countries with arrears from the same major geographic region by the total number of countries in that region.[17]

The second group of fundamentals contains variables that can potentially have a direct effect on the spread. It is important to control for such factors, otherwise their direct effect might be picked up by recent default. Besides recent default and the benchmark rate, there are two major factors that can easily have a direct impact on sovereign debt prices. One is the overall risk aversion of the market, as shown for sovereign CDS spreads by Longstaff

[15] We consider the following variables as candidates: debt per GDP, reserves per imports, debt service per exports, current account per GDP, exports per GDP, savings per GDP, growth of per capita GDP, growth of gross investment, GDP per capita, inflation, credit to private sector per GDP.

[16] Ozler (1992) finds that such a measure is significant in explaining variation in sovereign loan spreads for developing countries over the 1968-1981 period.

[17] Motivated by the findings of Ozler (1992), we also add her dummy variable that reflects whether the country has gained sovereignty before or after 1930.

et al. (2011), while the other is market liquidity, as shown for US corporate bonds in Elton et al. (2001). In case of new sovereign *loan* disbursements from the 70s, liquidity should not be an issue: these are primary issuances of loans, without active secondary market trading. For the benchmark interest rate, we use the LIBOR USD 1-year rate. To control for risk aversion, we use the US BAA and BBB corporate bond spreads (see Figure 1 for definitions).

4.4 Estimation issues

Both the reduced- and the structural-form specification are subject to two major econometric problems: the need to control for country fixed effects, and the validity of the strict exogeneity assumption.[18] Online Appendix A contains an in-depth discussion of how we handle these issues, here we just briefly outline our strategy.

In the reduced form, we use a pooled OLS, a random effects, a fixed effects and a first differencing estimator. In the structural form, we use the first difference estimator while appropriately instrumenting the future default variable: it eliminates the individual effects, and the right choice of instruments resolves the endogeneity problem caused by the prediction error in a way that requires only the sequential exogeneity assumption. 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 lags of first differences) leads to more precise estimates, but at the cost of making the direct comparison with the reduced-form results of Table 2 more difficult.[19]

The whole structural-form 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. For this reason, we report various measures that summarize the first-stage regression, and adopt different instrumenting techniques (two stage least squares, limited information maximum likelihood (LIML) and in particular the Fuller k-class estimators) to assess the strength of our instruments. While there is a debate in the literature on which estimator is best suited for weak instruments, theorists increasingly endorse the Fuller estimators as the best choices (Hahn et al. (2004) and Andrews and Stock (2005)). We also report an overidentification test that is appropriate in a setting with heteroskedasticity and autocorrelation (the Hansen's J statistic), and is consistent even with intra-cluster correlation.

[18] Strict exogeneity means that the idiosyncratic error terms, conditional on the individual effect, are uncorrelated with past, present and future values of the regressors. Sequential exogeneity, on the other hand, only requires no correlation with the contemporaneous and past values of the regressors.

[19] Our simulation exercise (presented in online Appendix B) further illustrates the importance of strict/sequential exogeneity and the proper choice of instruments under first differencing.

5 Results

5.1 The reduced form

The results of the reduced-form estimation are presented in Table 2 and refer to four specifications. The left hand side variable is the quantity and maturity weighted annual spread. 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. The significance of the distant default replicates the main finding of Ozler (1993). When running a fixed effects estimation (Column 2), the main changes are that GDP growth becomes statistically significant and that, while similar, the coefficient on the proportion of countries in the region with arrears becomes less significant. The main difference in running random effects (Column 3) is that we can now estimate again the role of the distant default variable, with a coefficient now almost half in value and insignificant. As expected, first-differencing (Column 4) leads to less precise estimates. The effect of recent default loses its significance, though the point estimate remains very similar.

In conclusion, we find that recent default and many additional economic fundamentals significantly influence the spread, even after controlling for country specific effects. There is also some indication that the significance of distant default might be a consequence of omitted country effects.[20] This latter finding questions by itself the existing empirical literature (such as Ozler (1993) and Reinhart et al. (2003)), on the role of distant default history in pricing sovereign loans.

5.2 The structural form

The results of our benchmark specification (3) are presented in Table 3. We choose as our main structural-form estimation the one in first differences, using the the Fuller k-class estimator (with $k = 1$, Column 1).[21] Overall, there are four important findings we discuss here: the influence of the future default indicator, the coefficients of the benchmark yield and the US corporate spread, and most importantly, the effect of the recent default indicator.

Starting with the first, future default's point estimate is around 0.41, significant at the 10% level. Although the mean of this indicator is just 0.15, this is not very indicative of

[20]The comparison of the distribution of country effects across groups with different default history might convey some additional information. Indeed, defaulters (categorized through the distant default dummy) are not being charged significantly higher spreads: the difference between the means of the two groups is not significantly different from zero.

[21]The other columns will be utilized in the discussion of the strength of our instruments. Online Appendix A briefly discusses the level specification.

Table 2: Reduced-form estimation: the determinants of the spread[a]

	Pooled OLS (1)	Fixed Effects (2)	Random Effects (3)	First Difference (4)
Benchmark yield	−0.107	−0.112	−0.111	−0.099
	(−11.82)***	(−10.46)***	(−12.64)***	(−11.09)***
US BAA-TB spread	0.231	0.232	0.231	0.247
	(7.66)***	(7.89)***	(8.40)***	(9.00)***
Recent default	5.623	8.41	7.033	6.237
	(2.00)*	(2.88)***	(2.23)**	(1.37)
Distant default	0.142	-	0.083	-
	(2.21)*	-	(1.17)	-
Reserves to imports	−1.00	−0.540	−0.721	−0.388
	(−4.54)***	(−2.36)**	(−3.54)***	(−2.85)***
Debt to GDP	0.137	0.270	0.189	0.305
	(0.76)	(1.32)	(1.12)	(1.27)
GDP growth	−0.451	−0.799	−0.691	−0.369
	(−1.37)	(−2.76)***	(−2.66)***	(−1.27)
Investment growth	-0.05	0.036	0.006	-0.018
	(-0.64)	(0.46)	(0.09)	(-0.29)
GDP per capita	-0.05	0.01	-0.243	0.146
	(-0.08)	(0.01)	(-0.47)	(0.18)
Countries with arrears	0.490	0.540	0.577	−0.216
in the region	(2.10)**	(1.24)	(2.62)***	(−0.56)
Experience	−0.026	−0.031	−0.026	−0.024
	(−1.19)	(−1.12)	(−1.11)**	(−0.95)
New sovereign	0.014	-	0.030	-
	(0.18)	-	(0.38)	-
Constant	2.650	2.821	2.763	–
	(9.29)***	(10.81)***	(12.59)***	–
No. of obs	196	196	196	158
R^2	0.65	0.62	0.64	0.57

a The t statistics are in parentheses; the standard errors are corrected for clustering at the country level. *, **, *** denote 0.1, 0.05 and 0.01 significance levels, respectively.

Table 3: Structural-form estimation: the determinants of the spread[a]

	Fuller	LIML	2SLS	OLS
	(1)	(2)	(3)	(4)
Future default	0.409	0.417	0.379	0.125
	(2.00)*	(1.98)*	(2.05)**	(2.14)**
Benchmark yield	-0.111	-0.111	-0.111	-0.106
	(-12.14)***	(-12.12)***	(-12.21)***	(-12.62)***
US BAA spread	0.225	0.225	0.226	0.233
	(7.77)***	(7.76)***	(7.82)***	(7.79)***
Recent default	7.33	7.33	7.301	7.05
	(1.79)*	(1.79)*	(1.78)*	(1.7)*
First stage relevance:[b]				
Partial R^2 for future default	0.258	0.258	0.258	-
Kleibergen-Paap rk Wald F statistic[c]	3.991	3.991	3.991	-
Kleibergen-Paap rk Wald stat p-value[d]	0.000	0.000	0.000	-
Structural form:				
Overidentification test p-value[e]	0.754	0.756	0.745	-
Anderson-Rubin Wald test p-value[f]	0.061	0.061	0.061	-
Number of observations	154	154	154	154

a The dependent and explanatory variables are first differenced. The first three columns report, in order, regressions where future default is instrumented using: the Fuller k-class estimator with scaling parameter equal to 1 (Fuller), the limited information maximum likelihood (LIML) estimator, and the two stage least square estimator (2SLS). The future default variable is instrumented by the first lag of the following variables: debt/GDP, reserves to imports, GDP growth, investment growth, GDP per capita, proportion of countries with arrears in the region, experience, new sovereign dummy and a distant default dummy. The fourth column reports the OLS regression. The t statistics are in parentheses; the standard errors are corrected for clustering at the country level. *, **,*** denote 0.1, 0.05 and 0.01 significance levels, respectively.

b The reduced-form regression of the instrumented indicator on the full set of instruments.

c The F statistic of the joint significance of excluded instruments.

d The Kleibergen-Paap rk Wald test of the null hypothesis that the equation is underidentified.

e The Hansen J-statistic test of the null hypothesis that the instruments are uncorrelated with the error term and that the excluded instruments are correctly excluded from the structural equation.

f The weak-instrument robust Anderson-Rubin test of the null hypothesis that the coefficient on the endogenous regressor in the structural equation is zero.

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 95th percentile, then this would raise the spread by approximately 33 basis points, accounting for 52% of the difference between the median and the 95th percentile of the observed spread. Alternatively, a one standard deviation increase accounts for 26% of the standard deviation of the spread.

Consequently, we interpret the coefficient on future default as sizable. 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". Our evidence, however, is not decisive regarding the question of whether those loans were priced correctly and whether lending institutions were taking excessive risks.[22]

A second general feature is the significance of the benchmark rate, which was also present in the reduced-form results. This result is equivalent to the finding that the loan rate responds less than one-in-one to the world interest rate. This conclusion is found also in Eichengreen and Mody (1999) and Uribe and Yue (2006).

A third robust result is that the US BAA spread has a significantly positive coefficient, indicating that an increase in the overall risk aversion of the (US) financial sector pushes up sovereign loan spreads as well. This finding echoes the results of Longstaff et al. (2011).

Economic effect of recent default

The central results concern the channels of influence of the recent default indicator. Its estimate is around 7.3, significant at the 10% level. This is quite similar to the point estimate of Cruces and Trebesch (2013).[23] If we increase the credit history indicator from its median to the 95th percentile, the spread goes up by 15.5 basis points, and for certain countries in our sample, there is a 32-68 basis point direct punishment component in the spread. These are large effects since the spread increases by 64 basis points from its median to the 95th percentile.[24] Alternatively, a one standard deviation increase in the recent default variable accounts for 15% of the standard deviation of the measured spread.

[22] As phrased in the Federal Reserve's event history item: "In 1977, during a speech at the Columbia University Graduate School of Business, then-Fed Chairman Arthur Burns criticized commercial banks for assuming excessive risk in their Third World lending." (http://www.federalreservehistory.org/Events/DetailView/46)

[23] Though they find much larger punishments in their sample, it is not due to a larger coefficient, but instead, to more severe default episodes.

[24] This type of calculation refers to only a one period ahead increase in the spread, though this punishment lasts potentially for more periods. More specifically, given that the recent default indicator reflects a stock of accumulated arrears, the effect lasts until this stock is eliminated. As Tedeschi (1994) suggests, even a punishment that is relatively small in each period can deter deviations, as long as it can last for a potentially very long time. Though punishment in Tedeschi (1994) takes the form of different output levels, it is nevertheless a punishment in terms of future cooperation – which, in that context, concerns output decisions.

Using a simple calculation we can show that the implied punishment for arrears is large enough to ensure timely payment of a country which is sizably less patient than the world discount factor. We analyze a country trading off whether to pay its debt now or pay it later. Recall that our benchmark recent default measure can be described as: $a_t = \frac{A_{t-1}}{L_{t-1}}$, where A is the stock of arrears and L denotes loan disbursements.

Let θ be the structural-form pricing coefficient on a_t. Now, suppose that at time t some payment P_t is due and that $A_{t-1} = 0$. If the country decides to postpone paying P_t until next period, then P_t becomes arrears for $t+1$: $a_{t+1} = \frac{P_t}{L_t}$, and the spread next period goes up by $\Delta s_{t+1} = \theta \frac{P_t}{L_t}$.

Consider first the case in which the country decides to fully repay the loan next period, so that the discounted payments at $t+1$ are:

$$\beta \left(\Delta s_{t+1} L_{t+1} + \frac{P_t}{\bar{\beta}} \right) = \beta \left(\theta \frac{P_t}{L_t} L_{t+1} + \frac{P_t}{\bar{\beta}} \right),$$

where β is the country's subjective discount factor, and $\left(\bar{\beta}\right)^{-1}$ is the world benchmark interest rate. The country is then indifferent between paying now or next period if

$$P_t \Lambda_t = \beta \Lambda_{t+1} \left(\theta \frac{P_t}{L_t} L_{t+1} + \frac{P_t}{\bar{\beta}} \right), \tag{6}$$

where Λ_t denotes the marginal value of an extra dollar and we assumed perfect foresight. To focus squarely on the punishment effects in prices of entering into arrears, we assume that in steady state $\Lambda_t = \Lambda_{t+1}$, so that not repaying today does not lead to a change in the mapping from exogenous states to marginal utilities. Further, denoting by g the growth rate L_{t+1}/L_t, the indifference condition (6) becomes

$$\theta = \frac{1}{g} \left(\frac{1}{\beta} - \frac{1}{\bar{\beta}} \right). \tag{7}$$

Based on our results $\theta = 0.07$, so for a parametrization of $\bar{\beta} = 0.95$, $g = 1$, the lower bound on the country's subjective discount factor that ensures repayment is $\beta = 0.891$. Formula (7) shows that our estimated punishment effect can be interpreted as the difference between the shadow interest rate and the world benchmark rate, adjusted for debt growth.

We can easily extend the example to the case where the country pays only a fraction α of its arrears (but it does not go into additional arrears later on). It can then be easily showed that the country is indifferent between paying now or never repaying if

$$\theta = \frac{1}{g} \left(\frac{1}{\beta} - 1 \right). \tag{8}$$

When $g = 1$, formula (8) shows that the punishment effect is simply equal to the net shadow interest rate of the country. The lower bound on the country's subjective discount factor that ensures repayment is $\beta = 0.935$. In other words, at higher discount factors than this lower bound, according to our estimates, future spreads increase by enough to deter choosing the option of never repaying. Based on these illustrative numbers, we conclude that our estimated price punishment effect (7.3% times the proportion of arrears to disbursements) can be considered as economically meaningful and large.

Further diagnostics

Now we briefly discuss some diagnostic statistics for the structural-form pricing regression reported in Table 3. The null of homoskedasticity is not rejected by the Pagan-Hall general test statistic (a p-value of 0.59), which suggests that, given its inferior small sample properties, an asymptotically efficient GMM is not needed (as pointed out for example in Hayashi (2000)). The Arellano-Bond test also fails to reject at 5% the null of no serial correlation, tested up to five lags, of the residuals.

Though the 0.258 value of the first-stage partial R^2 suggests that the instruments are relevant, the appropriate Kleibergen-Paap rk Wald F-statistic[25] has a value of 3.99. This is below the "rule of thumb F value of 10", but it is above the 10% relative bias critical value for the Fuller(1) estimator (3.74). To further assess the strength of our instruments, we look at three additional estimation methods: limited information maximum likelihood (LIML), 2SLS and OLS. Note, however, that this first stage is essentially a crisis prediction and early warning step, which is in general a challenging task.

LIML (Column 2 of Table 3) yields almost identical point estimates. The Kleibergen-Paap rk Wald F-statistic is above the 10% maximum size critical value (3.81). Under 2SLS (Column 3), the point estimates are very similar to the Fuller(1) and LIML results, but slightly lower. This is consistent with the notion that 2SLS is more biased, but note that the bias pushed down the coefficient only by a little. The tabulated Stock and Yogo (2005) critical values are much higher for 2SLS, implying that the F-statistic of 3.99 is above the 25% maximum size and 30% relative bias critical values.[26]

Under OLS (Column 4), the coefficient on the endogenous future default variable is around 0.1, much lower than the 2SLS one of 0.38. This is in line with the results that weak instruments lead to a bias of the 2SLS coefficient towards the OLS estimate. At

[25]See Baum et al. (2007) for details and discussion of the diagnostic statistics utilized in this part.

[26]It is important to note here that these critical values are based on the assumption of i.i.d. disturbances. Baum et al. (2007) write that "we are not aware of any studies on testing for weak instruments in the presence of non-i.i.d. errors. In our view, however, the use of the rk Wald statistic, as the robust analog of the Cragg-Donald statistic, is a sensible choice and clearly superior to the use of the latter in the presence of heteroskedasticity, autocorrelation or clustering." (page 24)

the same time, we find that in this OLS regression the coefficients of the other exogenous regressors, including recent default, change very little. It shows that even for a biased and inconsistent OLS estimator for future default, the coefficients of the other exogenous regressors are unaffected.

We further report the Anderson-Rubin test of the null that the coefficient of the endogenous regressor in the structural equation is zero. This test has been shown to be robust to any form of weak instruments and to the presence of non i.i.d. disturbances. We find that the test rejects the null at 10%.

The Kleibergen-Paap rk Wald test of whether the excluded instruments are relevant rejects the null of underidentification with a p-value around 0. The overidentification test of all instruments (in the form of a Hansen J-statistic) fails to reject the null with a p-value of around 0.75. Overall, though there is some indication of weak instruments, our final reading is that these diagnostics suggest that the structural-form regression is well specified and the instruments are valid and sufficiently relevant.

5.3 Robustness

Here we discuss various alternatives in the construction of some key variables in the structural form regression: the annual average spread, the future default variable and the US corporate spread. Some of these alternative regressions are presented in Table 1 of online Appendix A. First we reran our estimations (the ones reported in Tables 2 and 3) with a quantity-weighted average spread (instead of weighing by quantity and maturity). Reassuringly, the point estimates, their significance level, and the diagnostic statistics have remained almost identical. This result suggests that maturity weighting is not likely to affect our main identified channels. The difference between the spread adjusted only for quantity and our benchmark spread variable is positive, suggesting that at the loan level, maturity is unconditionally negatively correlated with spreads.[27] However, the value for the difference is very small, amounting to only 0.8 basis points. Moreover, when we regress this difference on a variety of controls, including the future and recent default measures, we do not find any significant variation. We conclude that changes in the maturity composition of the loan portfolio is unlikely to drive our results.[28]

Regarding our future default indicator, we consider an alternative when we exclude a three year grace period from the summation in equation (9). This also leads to very similar estimates and conclusions. Additionally, we also used the second lags of our benchmark

[27] This relation is different from the typical finding for bonds: for example Arellano and Ramanarayanan (2012) and Broner et al. (2013) find that longer maturity bonds have larger spreads.

[28] Online Appendix A also discusses the impact of the borrower's risk profile on loan maturity. The broad finding is that recent default does not influence maturity.

set of fundamentals as instruments. The point estimate on future default remains similar, although the standard error increases. This is also true in case of varying the instrument set: standard errors change but point estimates are stable.

Finally, instead of the Moody's US-corporate BAA spread, we have reran our estimations with the US BBB-rated corporate bond spreads of Altman (1989). As evident from Figure 1, these two measures are very similar, and they lead to very similar results.[29]

5.4 Channel decomposition

An important exercise for our conclusion is to compare the point estimates for the recent default measure in the reduced- and structural-form estimation (recall equation (5)). We find that the estimated coefficient in the structural form is very close to the reduced form. We interpret this as evidence that the effect of recent default on the spread is almost entirely a direct one, pointing to a positive lender surplus (price punishment).

We certainly acknowledge that our finding of a positive lender surplus can be consistent with alternative explanations as well. Given the literature on asset pricing, and the fact that we analyze the pricing of new syndicated sovereign bank loans which rules out liquidity as an alternative, the main candidates are a risk premium and a systematic expectational error (overreaction). Here we discuss these two alternative explanations.

Risk premium

We argue along several lines why we prefer our maintained interpretation. First, Longstaff et al. (2011) argue that for sovereign credit default swap spreads, most of the variation in returns is driven by compensation for bearing global risk. We control for global risk aversion by including in our structural form the US corporate BAA and BBB spread. This factor is positive and very significant in determining country-specific spreads, confirming the results of Longstaff et al. (2011). Nevertheless, our identified extra effect of past credit history remains significant even after controlling for this risk factor.

Second, as long as the model-implied (local) risk premium is proportional to expected default, then the estimated coefficient on the future default combines the effect going through expected default and through the risk premium.[30] Even if the true pricing equation is nonlinear in expected default, one can still adopt a linear approximation (as we did in

[29] Ideally, one would like to use below-investment grade spreads to reflect shifts in risk-aversion. Though Altman (1989) does report such series, they only start in 1979. Our measure of "proportion of countries with arrears in the region" can also serve as an entirely regional proxy for risk-aversion. The variable, however, is not significant as an extra right hand side variable, nor influential for overidentification not to reject.

[30] For example, in the sovereign bond pricing models of Borri and Verdelhan (2010) and Arellano and Ramanarayanan (2012), the risk premium is proportional to expected default.

section 3). The question is whether it can happen that credit history (or other extra terms) are picking up the approximation error term. In our simulation exercise (see section 6), we address this concern. We find that this is not the case.

Finally, a default may lead to a change in risk-aversion, like in Lizarazo (2013). In that case, a default may lead to a higher spread, implying positive surpluses for a hypothetical lender with unchanged preferences. An endogenous change in risk-aversion, however, leads to a more complicated true specification than a simple additive punishment term. We have experimented with such a setup in our simulation exercise. We found that unless we include interaction terms between future default and past default (or a proxy for the risk-aversion shock), the overidentification test always rejects. A non-rejection can thus be viewed as an evidence against an endogenous change in risk-aversion. When applying this argument to our actual data, we have a much smaller sample size, and the overidentification test may have limited power. Such tests tend to overreject in small samples (see Hayashi (2000)). Given that we do not get a rejection in our actual empirical exercise, it is rather indicative of a simple additive punishment specification.[31] Based on this, we maintain that our results are more likely to suggest a direct punishment mechanism than a mechanism through changing risk-aversion. Still, that also fits the broader picture of a default leading to a change in future terms of interaction (prices), which is a dynamic incentive.

More importantly, such an increase in risk-aversion also hinges on some form of repeated interaction between lenders and borrowers. Suppose a default leads to an increase in risk-aversion of the country's creditors. If these are well diversified investors, then a single country's default should not impact their wealth or risk-aversion in any major way. So they need to be specialized investors (like the ones mentioned in Broner et al. (2013), page 90), and the country must be unable to switch to new creditors after a default.

Rational expectations

Our errors-in-variables methodology builds on rational expectations. This raises the possibility that what we interpret as a direct effect of default history might be simply an expectational error. The behavioral finance literature indeed presents ample evidence of biased expectations of investors, and proposes a market sentiment component of the stochastic discount factor (see, for example, Shefrin (2008), page 3). Our focus, however, is on new, large and individual loan deals offered by large banks, which limits the role of individual mistakes and market sentiment.

[31] We also tried such an interaction regression with our actual data. Future default remains similar in size and significance, recent default increases but becomes marginally insignificant, while the interaction term is negative but insignificant. Together with the non-rejection of overidentification of the specification without the interaction term, this further supports our interpretation of an additive punishment component.

A Knightian uncertainty premium can also lead to "expectational errors" when investors (banks) exhibit ambiguity aversion. In such a framework, rational but ambiguity-averse lenders over-react to bad news about a country's fundamentals (see for example Epstein and Schneider (2008)). For that explanation to hold it must be that lenders systematically over-react to the information of past repayment problems by over-predicting future default. It is hard to find direct evidence against the possibility of a systematic overprediction of default probabilities, since we do not have data on subjective expectations. The argument that our identified extra effect reflects such pessimistic beliefs would require that default in our sample happened on average less frequently than expected by lenders. However, given that our data concentrates mostly on the period *before* the sovereign debt crisis of the 1980s, often viewed as a period with unexpected default events, it is unlikely to be the case.[32]

To summarize, our main structural-form result is that past repayment problems have a direct influence on the spread on top of the indirect influence through expected default. We interpret that evidence as a positive lender surplus following a default episode, indicating the presence of relational contracts (dynamic incentives) in sovereign bank lending. We nevertheless acknowledge alternative interpretations of the structural-form result. We believe that out of these alternatives, the relational contracts story is the most natural candidate and well developed theory for studying syndicated sovereign bank lending.

6 A simulation exercise

In this section, we briefly report the outcome of a simulation exercise, using artificial data on fundamentals, default behavior and sovereign spreads. We do not derive a full blown theory, nor perform a true calibration exercise. Instead, we develop a purely illustrative 'statistical model', at the minimum necessary complexity still allowing us to demonstrate the performance of our error-in-variables approach.

We want to evaluate the following main features.[33] (1) The ability to estimate the coefficient of future default. (2) Telling apart punishment from signaling. (3) The ability to separate an increase in risk aversion after a recent default from a direct punishment effect. (4) The performance of our linear approximation when the true pricing equation is nonlinear.

[32]Two further issues should be noted here. First, a similar argument as with risk aversion can be invoked: if the pessimistic expectations are proportional to the ones implied by the rational expectations assumptions, and the linear approximation of the nonlinear model is acceptable, then our extra effect is orthogonal to the difference in beliefs and cannot be explained by systematic expectational errors. Second, at a more general level, a systematic expectational error can still act as a repayment incentive similar to a price punishment. However, repeated interactions are not necessarily important in this case.

[33]Online Appendix B also illustrates the implications of endogeneity and strict/sequential exogeneity. To save space, we do not discuss these results here.

Note that nonlinearity can play a role in two distinct places. The first is in the pricing equation, where the spread is in general some nonlinear function of expected future repayments. This is addressed directly by point (4) from before. The second is the prediction equation, where the conditional expectation of future repayments is a nonlinear function of fundamentals. This we address by increasing the strength of signaling, leading to a more nonlinear prediction equation.

Our framework tries to match our empirical setup as much as possible. The spread is priced as described in Section 3. Future 'repayment difficulties' are driven by some random fundamentals and potentially by recent default (representing the signaling hypothesis). The proportion of debt that might be in default is uncertain, and it increases with the repayment difficulty – this allows for a continuous realized repayment indicator. There is also a potential ad hoc extra (punishment) component added to the spread. See online Appendix B for full details.

6.1 Various data generating processes

There is a single country, and its fundamental x_t follows an AR(1) process. The proportion (z) of debt that might be in default is uncertain, and it *decreases* with the fundamental. Realized default at $t+1$ is $d_{t+1} = \lambda z_{t+1}$, where λ is the haircut, and z_{t+1} is a piecewise linear function of the fundamental, with two cutoff points $\bar{x}_t$ and $\bar{x}_t - \frac{1}{\alpha_2}$. In the benchmark case, the default cutoff point is constant. In case of signaling, the default cutoff point is a function of past default: $\overline{x}_t = \overline{x} + \gamma z_{t-1}$ (normal signaling). In some cases, we also explore a stronger and more nonlinear signaling specification, of the form $\overline{x}_t = \overline{x} + \gamma_2 z_{t-1}^2$ (strong signaling). Finally, we consider an endogenous change in risk aversion, induced by past default: $a_t = a + \kappa z_{t-1} + \xi_t$. For instrumenting realized default, any (nonlinear) function of the fundamental x_{t-1} is a candidate. Based on the reduced form and the first stage, we use x_{t-1} and x_{t-1}^2.

The parameters of the simulation were chosen such that (1) we get a reasonable reduced-form (RF) fit, (2) we get sufficiently significant and precise point estimates in all structural-form (SF) regressions, and (3) future default is an important determinant of the spread, relative to the pricing error. Some of the variables (risk-aversion, the haircut and the default cutoff $\bar{x}$) also reflect certain data patterns and the findings of Cruces and Trebesch (2013).

6.2 Results for different scenarios

First we cover the cases where the true pricing equation is linear. We discuss results for constant risk-aversion, estimation in levels, considering all combinations of punishment and signaling. Then we look at cases when risk aversion is time varying, here we also include one table from online Appendix B. Finally, we switch to cases when the true pricing equation is nonlinear, but we are still estimating its linear approximation. To simplify the terminology, we refer to z_{t-1} as *past default*, while to z_{t+1} as *future default*.

6.2.1 Linear pricing, constant risk-aversion, levels

We are interested in the following aspects of Questions 1 and 2 from earlier: (1A) When using $E\left[z_{t+1}\right]$, how close is the estimated value to the true value? (1B) In general, do we get good and precise estimates of $\frac{e^{a\lambda}-1}{a}$, and a good RF/first stage fit? (2A) Is z_{t-1} significant when it should be? Is its value close to the truth? (2B) If z_{t-1} should be included, can we detect its omission using the overidentification test? (2C) Does the channel decomposition work?

Under no signaling or punishment, the reduced form offers a good fit, with past default indeed being negligible and insignificant. The point estimate of the future default is quite precise and close to the truth. Note that even the true specification does not lead to a completely precise estimate of the true parameter. The partial R^2 is reasonably good. The irrelevant extra term z_{t-1} is insignificant. Whenever applicable, the overidentification test does not reject.

The results under signaling (both normal and strong) but no punishment show exactly the same patterns. The fact that results are very similar under normal and strong signaling also illustrates that the nonlinearity in the prediction equation does not pose a difficulty to our framework: large arrears (recent default) lead to large increases in (re-)default probabilities, yet the pricing equation can be perfectly estimated in the same linear form. There are two further points worth discussing. On the one hand, the RF can be subject to (some) omitted variable bias: when omitting the now relevant variable z_{t-1}, the point estimate of the constant increases by 4% under normal, and by 10-15% under strong signaling. On the other hand, omitting z_{t-1} from the instruments does not lead to any visible change in the SF estimate. This latter result is the "Wickens finding", showing that we do not need to worry about not using all potential instruments.

When we add punishment but no signaling, the reduced form offers a similarly good fit, with past default being highly significant. Omitting it from the regression changes the point estimates substantially. The incorrect specification is also revealed by the overidentification test, provided that past default is included in the instrument set. Once we add past default as an extra right hand side variable, we again get all point estimates close to the true values and overidentification is not rejected.

Finally, we consider both signaling (normal and strong) and punishment. The results are

Table 4: Endogenous changes in risk-aversion; signaling, no punishment

LHS variable: s_t	(1)	(2)	(3)	(4)	(5)
past default			0.02		0.03
			(6.83)		(2.52)
risk-aversion shock				0.02	-0.008
				(6.37)	(-0.76)
future default	0.371	0.376	0.353	0.354	0.353
	(51.32)	(51.75)	(46.10)	(45.99)	(45.95)
constant	-0.007	-0.008	-0.006	-0.02	-0.002
	(-2.54)	(-3.09)	(-2.68)	(-6.02)	(-0.28)
R^2/ partial R^2	0.13	0.13	0.11	0.11	0.11
overid	0.02	0.00	0.04	0.00	0.02

T statistics are in parentheses. The instruments are the following. Column 1: x_{t-1}, x_{t-1}^2. Columns 2 and 5: x_{t-1}, x_{t-1}^2, z_{t-1}, $\xi_t + \kappa z_{t-1}$. Column 3: x_{t-1}, x_{t-1}^2, $\xi_t + \kappa z_{t-1}$. Column 4: x_{t-1}, x_{t-1}^2, z_{t-1}.

quite similar to those without signaling. In this case, we can also verify that the channel decomposition works.

6.2.2 Linear pricing, time-varying risk-aversion, levels

We are mostly interested in checking whether the finding of a punishment effect can also indicate variations in risk-aversion (Question 3). For this reason, here we only discuss results for the case of signaling but no punishment.

Under an endogenous shift in the risk-aversion parameter (Table 4), past default and the risk-aversion shock are both significant in the RF (not reported), as they should be. Since they are highly correlated, their significance is low. In the structural form, using only future default as a right hand side variable leads to slightly inconsistent estimates and a clear rejection of overidentification (Columns 1 and 2). When adding one of past default and the risk-aversion shock as a right hand side variable (Columns 3-5), the point estimate of future default moves very close to the truth, the extra term is significant, but overidentification is still rejected. When adding both extra terms, the risk-aversion shock becomes insignificant, and recent default is estimated much less precisely. The fact that overidentification is still rejected suggests that this case is different from a linear punishment scenario.

A shift in risk-aversion means that the slope coefficient of expected future default should go up after a default. This calls for the inclusion of the interaction of past default and/or the risk-aversion shift with future default. When considering one of the extra effects, and adding its direct effect and the interaction with future default, only the interaction is significant.

When considering both effects, none of the variables are significant. Overidentification, however, is not rejected in any of the cases.

Our methodology is thus not necessarily able to distinguish the impact of an endogenous shift in risk-aversion from a direct punishment effect. The former, however, leads to a more complicated specification, which is signalled by the rejection of the overidentification test.

6.2.3 Nonlinear pricing

Here we are running regressions with constant risk-aversion, no endogeneity, in levels and with all combinations of signaling (normal and strong) and punishment; but with the general nonlinear pricing equation $s_t = \frac{1}{a_t} ln \left(E_t \left(e^{a_t z_{t+1} \lambda} \right) \right)$. We are interested in the following: (4A) When calculating the estimated risk-aversion parameter from the linear regression, how close is that to the true parameter? (4B) Can it happen that past default or other extra terms are picking up an approximation error term? For 4B, it is again most instructive to look at the case with signaling but no punishment, as that gives the highest chance of getting a "false positive" for past default. Since the point estimates of all other terms are very similar in all four cases, we restrict our attention to signaling but no punishment.

Results (not reported) are very similar to those under the linearized data generating process (section 6.2.1). When including past default, x_{t-1} or x_{t-1}^2 as extra terms on the right hand side, none of them would be significant or lead to any change in the other point estimates. Future default is always significant. The implied estimate of the risk-aversion parameter is close to the true value. In short, our linear EVM method is quite successful in estimating a nonlinear true pricing equation as well.

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 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. This strategy is a structural-form rational-expectations asset pricing regression in which the spread may be influenced by multiple risks and factors. Using the errors-in-variables method, we replace the expected risk term with its realization and instrument the latter with information available at the time of pricing. We also add default history as an extra right hand side variable, to check whether it influences the spread not only through expected default risk, but has an extra effect on it.

The reduced-form estimation provides evidence that, after controlling for fixed effects, borrower and regional characteristics, recent repayment history is significant, while the inclusion of country effects weakens the impact of distant default history. Though this result is somewhat different from that obtained by Ozler (1993), it nevertheless implies that although country effects matter, credit history plays a role in determining sovereign spreads. The structural-form regression provides strong evidence of an extra effect of credit history (a punishment in *prices*), above the one going through predicted default losses. The finding that credit history matters beyond predicting future default points to the presence of relational contracts in sovereign bank lending, where repayment incentives are incorporated into future borrowing terms.

In terms of default costs, we believe that in reality there is a complex mix of trade and political sanctions, spillovers to other transactions and relationships, signaling and relational contract considerations. Our main result is that there is evidence of this last effect: an extra surcharge in loan prices, which points to the presence of relational contracts in sovereign bank lending.

Though sovereign bank loans no longer constitute a major source of international financing, our findings are still relevant for the theory of sovereign risk. In particular, our results call for developing contract theory models that generate default on contingent loans and then a price punishment. Alternatively, they can serve as a basis for adding a different type of "ad hoc punishment" to macro models with sovereign risk: a price punishment after a default. It can also be interpreted as being excluded from regular credit markets, but still having access to a specialized, more expensive form of credit. It would be important to evaluate the quantitative consequences of such a modeling assumption. And finally, the presence of large and specialized institutional bond investors may lead to the re-emergence of the importance of relationships in sovereign borrowing. With a single investor holding 5-10% (or an even larger share) of a country's foreign bonds, it can have a non-negligible impact on the country's borrowing conditions.

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Appendix: Construction of our default indicators

Omitting the country index for notational convenience, let us denote the yearly GDF data on arrears to private creditors[34] for year s by A_s, with s ranging from 1970 to 1989. Recent default at time t is then

$$R_t = \frac{A_{t-1}}{L_{t-1}},$$

where L_{t-1} is the amount of new loan disbursements (GDF code DT.DIS.PCBK.CD).

We then define the (net) flow of arrears as $N_t = A_t - A_{t-1}$. To construct our future default measure, we need one extra step: as those arrears may refer to more than one existing bank loan, we need to allocate them among contracts. In a given year t, we assume that arrears affect all the loans that have not matured in a proportional way. Formally, let $Y_{s,s-j}$ denote the time s arrears on a time $s-j$ contract, with size L_{s-j}. First we split N_s among contracts from years $s-1, s-2, \ldots, s-8$ in proportion to their size:

$$Y_{s,s-j} = N_s L_{s-j}/(L_{s-1} + ... + L_{s-8}),$$

$j = 1, 2, ..., 8$ (the average loan maturity in our sample). Then we cumulate the arrear fragments $Y_{t+1,t}, Y_{t+2,t}, ..., Y_{t+8,t}$ into which a time t contract goes over its lifespan:

$$D_t = Y_{t+1,t} + Y_{t+2,t} + ... + Y_{t+8,t}, \tag{9}$$

and the future default measure at time t is then normalized as

$$F_t = \frac{D_t}{L_t}.$$

The main assumptions are that (1) all the time t arrears affect all the loans that have not matured yet, and (2) the size of the contract specific arrears is proportional to the size of the contract. We motivate these 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. Online Appendix A discusses the prevalence of such clauses in some depth.

[34]The sum of principal arrears to private creditors, code DT.AXA.PRVT.CD, and interest arrears to private creditors, code DT.IXA.PRVT.CD.