SOVEREIGN BOND RESTRUCTURING: COMMITMENT VS. FLEXIBILITY*

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Abstract

Sovereigns in distress often engage in debt restructuring, typically negotiating with multiple classes of bondholders at once. We use natural experiments to investigate whether sovereign bondholders benefit from committing not to restructure. We find that committing not to restructure one class of bonds is valuable for not only that class, but, in contrast to received theory, for others too. We develop a model to rationalize these cross-bond spillovers. It points to a system of cross-bond equations that, we show, can be exploited to quantify natural experiments and to estimate unobservable elasticities in terms of a few sufficient statistics.

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1 Introduction

Sovereign default has been ubiquitous lately. Argentina, Ecuador, Lebanon, and Zambia defaulted in 2020. Other emerging-markets countries are pleading for debt relief.[1] Some defaults are hard defaults in which bondholders get little or nothing. But most defaults are soft/partial defaults in which they take a haircut but still get something in a debt restructuring.

Policy institutions like the IMF are now "working hard...to avoid a disorderly debt restructuring."[2] The G20 are too, agreeing to the Common Framework for Debt Treatments, which facilitates sovereign debt restructuring by, inter alia, helping dispersed bondholders coordinate.[3] But support for such policies is not universal. Detractors argue that they encourage opportunistic restructuring, decreasing the value of bonds and even choking off credit ex ante.[4] Bonds, they say, are made hard to restructure by design, as a way to commit not to do it.[5] They conclude that the commitment not to restructure must be valuable.

But the case for policy intervention turns on whether the value of this commitment accrues to bonds collectively or, as Bolton and Jeanne (2007, 2009) argue, to some individually at the expense of others. They say that committing not to restructure a bond, e.g., via governing law and covenants, could be like installing a burglar alarm: It could provide protection for an individual only by diverting costs onto others—other bonds are selectively restructured/other homes targeted by burglars—making it self-defeating collectively.

The policy debate needs empirical answers to these questions: (i) Is the commitment not to restructure one class of bonds valuable for that class? (ii) Is it valuable for that class due only to negative spillovers on other classes, like a burglar alarm? But the answers remain elusive. The evidence on the first is mixed, perhaps due to inherent identification challenges—bonds made hard to restructure (e.g., via foreign law or cross-default clauses) could differ in any number of ways from those made easy (e.g., currency, maturity, and other covenants).[6] There is no evidence on the second question as yet (to our knowledge).

In this paper, we bring evidence from natural experiments to bear on these questions. Our results suggest that the commitment not to restructure one class of bonds is valuable not only (i) for that

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[1]See, e.g., "Argentina clinches near-unanimous backing for debt restructuring" (*Financial Times*, August 31, 2020), "Ecuador debt restructuring faces setback after some creditors balk" (*Financial Times*, July 16, 2020), "Lebanon's sovereign default leaves creditors facing big losses" (*Financial Times*, March 10, 2020), "The 'blood, sweat and tears' behind Zambia's default" (*Financial Times*, November 19, 2020), and "Emerging economies plead for more ambitious debt relief programmes" (*Financial Times*, October 11, 2020).

[2]See imf.org/en/News/Articles/-current-sovereign-debt-challenges-and-priorities-in-the-period-ahead.

[3]See imf.org/en/About/FAQ/sovereign-debt; see, e.g., Krueger (2002) and White (2002) for earlier proposals.

[4]See, e.g., Dooley (2000) and Shleifer (2003).

[5]See, e.g., Dooley and Verma (2003).

[6]Chamon, Schumacher, and Trebesch (2018) and Clare and Schmidlin (2014) exploit heterogeneity in bonds' governing law; Becker, Richards, and Thaicharoen (2003), Bradley and Gulati (2014), Carletti et al. (2020), Eichengreen and Mody (2004), and Picarelli, Erce, and Jiang (2019) exploit heterogeneity in their covenants.

class but (ii) for other classes as well: Commitment has positive cross-bond spillovers; it seems more like a successful neighborhood crime watch than a self-defeating burglar alarm. This result contrasts with the predictions of existing models, which stress the burglar-alarm view that the commitment not to restructure one class of bonds leads others to take a haircut when a restructuring takes place.

Lacking a model to explain our findings, we develop one. It suggests that committing not to restructure one class of bonds can benefit all of them by making it unattractive to start a restructuring at all. The model, while highly stylized, explains a number of cross-sectional patterns in the data. It also points to heretofore overlooked cross-bond relationships that, we show, can be used to quantify the impact of experiments and to estimate otherwise unobservable elasticities in terms of a few sufficient statistics. Applying the methodology delivers sensible numbers, which could be useful for quantitative (i.e. less highly stylized) models of sovereign debt and for counterfactual policy analysis.

Our baseline experiment is a landmark ruling by the English High Court on a controversial device that has proved useful for sovereign debt restructuring lately.[7] The device, called "consent payments," effectively bribes bondholders to vote in favor of a restructuring, thereby trapping them in a prisoner's dilemma: Accepting a restructuring is a dominant strategy for individual (small) bondholders even if it harms them collectively (see Section 2.1). The defendant was a Brazilian soybeans firm that repeatedly restructured its English-law bonds, using consent payments to cut its coupon every time one came due for two years. Per the prisoners' dilemma, bondholders consented every time. Except the last. Two held out and sued, arguing that the consent payments constituted an illegal bribe. The ruling favored the defendant, setting the precedent to allow restructuring via consent payments in the future, thus limiting the ability to commit not to restructure. Being the Court's first ruling on consent payments, it had "worldwide ramifications" (White & Case (2013)).

The experiment provides a laboratory to address our questions on sovereign debt restructuring, as we can compare the same bonds on the trading days before and after it and thereby avoid relying on comparisons of different bonds. As the defendant was a firm, not a country, the shock was ostensibly orthogonal to the sovereign bond market. But it mattered for hundreds of sovereign bonds, as many countries issue under English law. Others do not. Their bonds provide us with a control group for difference-in-differences (DiD) analyses.

To address our first question, on the direct effect of the ruling on English-law bonds, we turn to a DiD regression. We compare the difference in yields around the ruling for our control group to a treatment group of bonds issued under English law. We find that treated bond yields increase by around 9–14 bps relative to control bonds, suggesting that making it easier to restructure one class of

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[7]E.g., Suriname deployed it repeatedly in 2020 and 2021 to defer interest payments on two bond series; see reporting by *Fitch* (fitchratings.com/research/sovereigns/fitch-upgrades-suriname-lt-fc-idr-to-cc-16-07-2020) and *LatinFinance* (latinfinance.com/daily-briefs/2020/11/17/suriname-seeks-breathing-room-for-debt-payments). Primary sources are available at bourse.lu/security/US86886PAB85/299502.

debt—i.e. decreasing the commitment not to—hurts that class.

To address our second question, on spillovers to other classes of debt, we take an analogous approach. Again, we compare the difference in yields around the ruling for our control group to a treatment group. This time the treatment group is non-English-law bonds issued by countries with some English-law bonds outstanding. These bonds are treated indirectly by the ruling as the issuing sovereign faces lower hurdles to restructure its English-law bonds. We find these bonds' yields respond similarly to directly treated bonds'. They increase by 10–13 bps relative to control bonds, suggesting that making it easier to restructure one class of debt hurts not only that class, but other classes as well—commitment is not a burglar alarm.

We interpret our results as a causal effect of the ruling on treated bonds. To do so, we rely on the assumptions that the ruling did not affect control bonds (SUTVA) and that yields on treatment and control bonds would have moved identically absent the ruling (parallel trends). We address concerns about SUTVA in two ways: (i) We omit the control entirely, running our regressions as plain event studies and (ii) we use a matched-sample approach, comparing English- to NY-law bonds for the direct effect and local- to local- for the indirect. Both ways, we find analogous results, suggesting our findings are indeed "treatment effects" (see Section 3.4). To address concerns about parallel trends, we study a narrow event window of two trading days around the court ruling (see Section 3.2). Still, as our treatment and control groups are not randomly assigned, we cannot rule out that another event in the same window (unknown to us) could affect the groups differently. But a data-driven placebo-type test suggests such an event is unlikely to generate our results spuriously (see Section 3.4).

Our interpretation also relies on the assumption of market efficiency, namely that short-term price changes reflect new information, not merely short-term inefficiencies due to, e.g., overreaction or fire sales. The assumption is relatively weak for the bond markets we study, which are frequented by professional investors and are particularly informative empirically (e.g., Philippon (2009)). Nonetheless, we show that our results do not rely on it: We repeat our analysis with a longer event window (one week). Our results are robust.

Our baseline results are based on a single ruling applying to a single restructuring device (consent payments). Yet we still interpret them as applying to sovereign bond restructuring broadly (external validity). To argue that our results are not context specific, we repeat our baseline analysis using three similar (though arguably more imperfect) experiments: two subsequent High Court rulings and the Argentine restructuring saga (Section 6). We find analogous results.

Next, we develop a simple model of a sovereign restructuring, which serves to investigate the mechanism behind our results, to interpret their magnitudes, and to understand policy counterfactuals. We model restructuring, a form of partial default, as a haircut imposed on creditors. The model is based on two key ingredients. The first is the willingness-to-pay problem, which is common in the literature: The

only thing that deters default is costs of default such as exclusion from financial markets (Eaton and Gersovitz (1981)) or trade sanctions (Bulow and Rogoff (1989)). As these costs are independent of the scale of default, the sovereign optimally defaults on all bonds if it defaults at all (except in extensions in Appendix C). The second is heterogeneity in the ease of restructuring, something that appears in only a few papers:[8] Creditors differ in the haircut suffered in default, with higher haircuts, i.e. lower repayments, corresponding to easier-to-restructure bonds. Specifically, there are (i) hard-to-restructure "rigid bonds" associated with a low haircut h_r and (ii) easier-to-restructure "flexible bonds" associated with a higher (expected) haircut $h_f > h_r$. For example, flexible bonds could correspond to local-law bonds, which a sovereign can restructure unilaterally by changing the law as Greece did in 2012.[9] (Such law-dependent haircuts arise from including litigation risk in a model of bond restructuring à la Gertner and Scharfstein (1991) (see Section 2.1).)

The model has close counterparts in our baseline experiment: Rigid bonds correspond to English-law, flexible to local, and, as shown in Section 2.1, the High Court ruling to an increase in the haircut on rigid bonds h_r (consent payments make bonds easier to restructure).

Our main results are comparative statics with respect to h_r that mirror the empirical effects of the High Court ruling. An increase in h_r has two effects on yields. (i) There is an effect that works via recovery values: It decreases the payoff to rigid bonds in default, increasing their yield. (ii) There is an effect that works via default probabilities: It encourages strategic default, increasing the yield on *both* types of bonds.

The model captures our main empirical findings: Per our first question, rigid yields increase (by both (i) and (ii)). Per our second, flexible yields in countries with outstanding rigid debt increase too (by (ii)). It also explains why the indirect effect is just as large as the direct effect: Bonds with the most to lose in default have the most to gain from deterring it (by $h_f > h_r$ and (ii)).

The model also generates new predictions to test. It suggests that, relative to the direct effect, the spillover effect should be larger when the difference in haircuts $h_f - h_r$ is larger—you care relatively more about deterring a restructuring when you lose more in a restructuring. Also, both effects should be stronger when countries are more indebted or their debt is more rigid. Empirical tests do provide significant support for all but one of our predictions (which is neither supported nor falsified), and thus add support for our model overall.

The model can also help us to interpret our experiments, which do not immediately lend themselves to quantification—the High Court's ruling on consent payments made restructuring easier, but by how much did it increase the haircut h_r? We use the model to back out this number. We derive a formula for the implied change in h_r in terms of our estimated yield changes and a few other sufficient statistics,

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[8]See Bolton and Jeanne (2007, 2009) and Carletti et al. (2020).
[9]See Zettelmeyer, Trebesch, and Gulati (2014).

notably estimates of risk-neutral default probabilities (from CDS) and expected haircuts. We estimate that the ruling increased expected haircuts on English-law debt by about 35 bps, a meaningful effect considering consent payments were already common practice before the ruling.

We show how to use the same sufficient statistics to translate our baseline estimates into an elasticity that could be useful for policy counterfactuals: We estimate that a 1% increase in haircuts increases the (risk-neutral) default probability by 7.63%. A back-of-the-envelope application of this number, in combination with Hébert and Schreger's (2017) estimates of the costs Argentine default, suggests that a one percentage point decrease in haircuts on Argentine debt could save 1.3 billion USD in deadweight costs.

Finally, a proviso: Although our results suggest the commitment *not* to restructure debt is good for sovereign creditors, and could even decrease deadweight costs of default, they say little about whether it is good for sovereign borrowers. It could be good, e.g., facilitating access to credit or bad, e.g., limiting the self-insurance role of default (Zame (1993)). One way to get at that question in future research could be to look at what sovereigns do following a shock to creditor commitment, something our experiment is unfortunately not well suited to do: Its strength in identifying price responses—a narrow event window—is a weakness in identifying behavioral responses, which do not happen fast enough for us to pick up.

2   Institutional Background and Data

Here we describe the ingredients behind our empirical analysis, which revolves around bond restructuring offers and consent payments, a contractual device used to facilitate them. We start with a primer on the problems of restructuring and how consent payments mitigate/exacerbate them (Section 2.1). We then summarize the main event in the paper: the first ever English High Court ruling on consent payments, which affirmed their legality (Section 2.2). Finally, we describe the data we use (Section 2.3).

2.1   Bond Restructuring and Consent Payments

Sovereign default is not uncommon. As Reinhart and Rogoff (2009) catalog, "[m]ost countries in all regions have gone through a prolonged phase as serial defaulters" (p. 49). Most defaults are negotiated partial defaults in which creditors take a haircut but still get something (Arellano et al. (2019); Cruces and Trebesch (2013)). For bonds, such partial defaults typically start with a restructuring offer from the sovereign.

Given bondholders are usually dispersed, bond restructurings are plagued by collective action problems. On the one hand, there is what is known as the "hold-out problem": An individual bondholder

could reject an offer that could make bondholders collectively better off. On the other hand, there is what is sometimes called the "hold-in problem": An individual bondholder could accept an offer that makes them collectively worse off.[10] (See Buchheit and Gulati (2000), Gertner and Scharfstein (1991), Kahan and Tuckman (1993), and Roe (1987).)

To see how the hold-out problem works, consider a sovereign that has so much debt that it is tempted to default outright. Collectively, bondholders might be better off taking a haircut to reduce the sovereign's debt burden and thereby increase the likelihood it repays them in full. But an individual bondholder still might not accept the haircut. By holding out, it can free ride on others' accepting, benefiting from the overall debt reduction without taking a haircut itself. As all bondholders have an incentive to act this way, the whole restructuring can fall apart even though it would have made everyone better off.

An initial defense against the hold-out problem is collective action clauses (CACs), a contractual term, now commonplace in sovereign bonds, that addresses free rider problems by allowing all bonds in a series to be modified by a specified majority.[11] But, while helpful (Fang, Schumacher, and Trebesch (2021)), CACs are not a panacea. Free rider problems persist when CACs cannot be aggregated across series (Gelpern, Heller, and Setser (2016)), and they "might be an invitation to litigation" otherwise.[12]

Another contractual device, so-called "consent payments," provides another line of defense against hold-outs by paying bondholders an effective bribe ("payment") when they agree ("consent") to changes in terms (of all bonds in a series). However, the legality of such payments is uncertain; hence hold-outs that reject the payments could end up getting paid in full following litigation.[13] In equations, if a bond holder with face value D accepts, it gets the (proportional) bribe b but decreases its face value by a haircut h. If it holds out, it forgoes the bribe but can litigate and be paid in full with probability π. Thus it accepts if

$$bD + (1 - h)D \geq \pi D + (1 - \pi)(1 - h)D \tag{1}$$

or $b/\pi \geq h$. In words, haircuts h are larger when consent payments are more generous (b is larger) or hold-outs are less likely to win in court (π smaller), two relationships we take off the shelf in the model below (see Section 4).

This is where the hold-in problem comes in: Consent payments can force bondholders into a prisoner's dilemma in which they tender even if restructuring is against their collective interest. The

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[10]These are not the only ways that collective action problems can lead to inefficiencies in sovereign-debt restructuring, something Pitchford and Wright (2012) demonstrate in a model of bargaining delays.

[11]Most countries enshrine some bondholder rights in legal statutes for corporate bonds (e.g., the Trust Indenture Act in the US). Not so for sovereign bonds, in which they are specified, by contract, in the indenture; see example prospectuses: under NY, English, and Italian laws, respectively: <u>Suriname</u>, <u>Poland</u>, and <u>Italy</u>.

[12]See creditslips.org/creditslips/2020/06/the-argentine-re-designation-drama-notes-from-two-frustrated-readers.html

[13]Panizza, Sturzenegger, and Zettelmeyer (2009) enumerate cases of such litigation; hold-outs received full payment or close to it in some cases but little or nothing in others. See Schumacher, Trebesch, and Enderlein (2021) for an overview of sovereign-debt litigation in recent decades.

sovereign is thus tempted to use consent payments simply to expropriate value from creditors.

What kinds of haircuts are feasible depends on how likely a legal jurisdiction is to protect bondholders—on how big π is. Our baseline experiment is a High Court case brought against a debtor that used consent payments to cut its interest payments year after year; the Court ruled in favor of the debtor, thereby lowering π. We also study another case brought against a debtor that use a related device to cut each €1000 in principal to a cent (see Section 6.1.1); this time the Court ruled against the debtor, thereby increasing π.

Overall, consent payments are a double-edged sword. By making it easier to restructure, they mitigate the hold-out problem but can exacerbate the hold-in problem (especially when combined with other anti-hold-out provisions like CACs).

2.2 The Ruling: Azevedo v Imcopa

At the center of our analysis is a May 2012 ruling by the English High Court that opposed a challenge to the legality of consent payments and, thus, made it easier to restructure bonds in exchange offers.[14] It applied to all English-law bonds, including foreign sovereign bonds, but was otherwise unrelated to the sovereign bond market.

The case, brought by two individual bondholders, Sergio Barreiros Azevedo and Vera Cintia Alvarez, against Imcopa Group, a Brazilian company in the soybeans business, represents a "landmark decision," according to the *Financial Times*.[15] The newspaper also stresses that consent payments, while not uncommon, had never been considered before by English courts, making the case "hugely important." However, it seems not to have been studied in the finance and economics literature. Hence, we offer a précis now.

Things started in 2008, when soybean prices plummeted from more than $16 a bushel in July to less than $8 in December.[16] Imcopa embarked on a plan to reduce its interest payments via a restructuring of its bonds, which were issued under English law. Over the next two years, it restructured its bonds every time an interest payment came due. Each time, it offered bondholders consent payments if they agreed to put off the interest payment. As predicted by the prisoner's dilemma described above (Section 2.1), nearly all bondholders accepted.

But the last time Azevedo and Alvarez held out. Equation (1) suggests they found the bribe b insufficient compensation for the chance π of getting paid in full following litigation. Indeed, they sued Imcopa in England, claiming, inter alia, that the consent payments constituted an illegal bribe.

The High Court ruled in favor of the defendant on May 30, 2012, on the grounds that the consent payments were offered to all bondholders, and therefore were not an illegal bribe. The decision was not

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[14]See Azevedo v Imcopa Importacao, Exportacao e Industria de Oleos Ltda [2012] EWHC 1849 (Comm) (30 May 2012)).
[15]See "The consent of the (bondholder) governed" (April 22, 2013).
[16]See, e.g., macrotrends.net.

a foregone conclusion (see, e.g., Jones Day (2012)). Azevedo and Alvarez appealed. But the original ruling was upheld on April 22, 2013.

2.3   Data Sources and Sample Construction

Bond data come from Dealogic DCM and Bloomberg's records of sovereign bond issues from 1980 onwards. We collect data on bonds outstanding between May 2012 and October 2014, a sample period that includes our baseline event (the High Court ruling described in Section 2.2) as well as our three tests of external validity (Section 6). The sample starts after the Greek restructuring in early 2012, in which almost all bonds under local law were written down but many under foreign (mainly English) law emerged unscathed; we thus focus on a period in which the market is aware that haircuts depend on the governing law.

We focus on bonds issued by countries with significant English- and NY-law borrowing and by relevant control countries, e.g., countries in Europe and Latin America. We include only bonds with some data available on governing law, our key variable of interest. Dealogic provides it for most issues; Bloomberg for fewer.

We must make a few adjustments when we merge the governing law data from the two sources, as sometimes multiple laws are listed. Occasionally, (i) Bloomberg lists two laws. In this case, if Dealogic provides a single governing law for the bond, we use that law; if not, we use the one that coincides with the currency denomination of the bond. Other times, (ii) Bloomberg and Dealogic list different laws. In this case, if one coincides with the currency denomination, we use that; if not, we use the one provided by Dealogic. (See also Kropp, Gulati, and Weidemaier (2018) for a discussion of data sources and quality for governing law.)

We exclude bonds with missing ISIN and those issued by publicly-owned companies, state/local governments, or Argentina, which, as of 2012, was still in litigation following its 2001 default and subsequent restructuring. The sample of bonds with available governing law and yield information that are outstanding during the sample period contains 2,239 bonds issued by 76 countries, denominated in 36 currencies, and 1,016,748 bond-day observations.

We collect daily yields and swap rates from Bloomberg, which we use to construct credit spreads and thereby difference out risk-free rate changes.

For our baseline analysis, our event window is two days: from May 29 to May 31, 2012. Since bonds are traded OTC in different time zones, this is the narrowest window that ensures that bonds traded in European, Middle Eastern, and Asian markets reflect the new information. (We also consider a wider event window as a robustness check, to show our results are not an artifact of short-term market overreaction; see 3.4.) On May 31, the sample contains 1,378 bonds, issued by 67 countries in 31 currencies. We winsorize yield and credit spread changes at 1% and 99%.

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Table 1 shows the summary statistics, aggregated by country, for bonds outstanding on the event date. The median number of bond issues a country has outstanding is 14; the median face value is $0.96bn; the median maturity is 8.04 years; the median yield is 4.74%. On average the countries in our sample have 36% of their outstanding face value under English law and 39% under local law. Table 12 shows statistics by country.

3 Empirical Framework and Results

Here we set up our empirical specifications (Section 3.1), we argue they have a causal interpretation (Section 3.2), we report our baseline empirical results (Section 3.3), and we argue for their robustness (Section 3.4).

3.1 Estimation Strategy

To evaluate the bond market response to a change in the ease of bond restructuring, we use a high-frequency difference-in-differences (DiD) approach. Our baseline model, which we label (R1), nests two DiD specifications, each addressing one of our motivating questions. We consider two measures of bond risk as dependent variables: yields and credit spreads.

In each of the two DiD specifications nested in regression (R1), the first difference is between inside and outside the event window, and the second difference is between our control group of bonds issued by countries with *no* outstanding English-law bonds and a treatment group. The specifications differ in this treatment group, which is either (i) English-law bonds ("directly treated" bonds) or (ii) non-English-law bonds issued by countries with *some* outstanding English-law bonds ("indirectly treated" bonds). Nesting these specifications has the advantage of estimating the coefficients on control variables on the full sample. (In Table 13, we show that absent controls the nested specification is equivalent to two separate DiD specifications.) Formally, we estimate:

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\begin{aligned}
x_{i,t} = {} & \beta_1 \mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}} + \beta_2 \mathbb{1}_t^{\text{Event}} \times \mathbb{1}_{i,c}^{\text{Indirect}} \\
& + \beta_3 \mathbb{1}_i^{\text{Direct}} + \beta_4 \mathbb{1}_{i,c}^{\text{Indirect}} + \alpha^\top \text{FE}_{i,t} + \varepsilon_{i,t}
\end{aligned}
\tag{R1}
$$

In our baseline specifications, the dependent variable x_{it} is the change in one of two high-frequency ($t-1$ to $t+1$) measures of bond risk: the yield or the credit spread. The yield has the advantage of being readily observable. But it has the disadvantage of potentially capturing not only risk but also currency- and maturity-specific risk-free rates. Hence we include currency- and maturity-time fixed effects in our yield regressions, assigning bonds to five-year maturity bins. The credit spread has the advantage of isolating risk by differencing out the risk-free rate. But it has the disadvantage that risk-free rates are not readily observable; indeed, they are hard to measure in the presence of sovereign risk and

convenience yields (see, e.g., Du and Schreger (2016), Kremens (2021), or Binsbergen, Diamond, and Grotteria (2021)). We construct the credit spread by subtracting the maturity- and currency-matched swap rate from the yield, as detailed in Appendix A.2. (These baseline specifications compare changes, the analog of "abnormal returns," in and out of the event window, as is standard in the event studies; however, as a robustness check, we also compare levels immediately before and after the event (see Table 10), as is more common in DiDs.)

The dummy variables are as follows: $\mathbb{1}_t^{\text{Event}} = 1$ if date t is the first trading day after the High Court ruling; $\mathbb{1}_i^{\text{Direct}} = 1$ if bond i is in the direct treatment group, i.e. if it is governed by English law; $\mathbb{1}_{i,c}^{\text{Indirect}} = 1$ if bond i is in our indirect treatment group, i.e. if it is not governed by English law but is issued by a country c which has English debt outstanding on the day of the High Court ruling.

The terms $\text{FE}_{i,t}$ denote vectors of fixed effects, including region-, currency-, and/or maturity-time, depending on the specification (detailed in Table 2).[17]

The key coefficients of interest are β_1 and β_2. They measure the changes in x_{it} around the event date for, respectively, directly treated and indirectly treated bonds relative to our control group. They represent averages across heterogeneous bonds conditional on the market conditions—countries' debt levels, investors' risk aversion, etc.—on the event date; i.e. they are local average treatment effects (LATE). We explore cross-bond heterogeneity in Section 5 and other market conditions in Section 6.

3.2 Identifying Assumptions

Our empirical strategy is a difference-in-differences (DiD) over a narrow event window. To interpret the estimates as being a causal result of the ruling, the expected dependent variable change over the window, conditional on our controls (but not on the ruling), must be equal for treated and control bonds.

There are two potential concerns about our identification: Treatment and control bonds could have (i) different trends unconditionally and (ii) different trends conditional on the date of the ruling due to a simultaneous event that affected treatment and control bonds differently.

These concerns are salient because the bonds are not allocated to treatment and control groups randomly; rather, countries choose under which law to issue. Those that choose English law could be different from those that choose local law. E.g., high-risk issuers could find it beneficial to issue bonds under foreign law (Chamon, Schumacher, and Trebesch (2018)). The choice of what foreign law to issue under is not random either. E.g., European countries issue most of their foreign-law debt under English law, whereas Latin American ones issue most of theirs under New York law. A shock to Europe is therefore likely to hit the treatment group (which contains more European issuers) harder than the

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[17]The regions are Europe, South America, Central America, Middle East/North Africa, Africa, Central Asia, East Asia. A list of sample countries and the regions we assign them to is in Appendix A.

control group (which contains more Latin American ones).

We cannot completely rule out concerns (i) or (ii), as the parallel trends assumption is inherently untestable. But we can mitigate each. To mitigate (i), we include controls for each type of bond over the entire sample to de-trend their yields (β_3, β_4, and γ_4). Net of these controls the expected trend in each bond should be zero, hence parallel. We mitigate (ii) in two ways. First, we include region-time and currency-time fixed effects in our regressions. These absorb variation on any given date common to a given region or currency. Second, we choose a narrow event window of just two days. To be a threat to our identification, an event must both affect treatment and control bonds within the same region differently and also happen at nearly exactly the same time as the ruling. We are not aware of any such relevant events released around the ruling. Additionally, we control for movements in risk-free rates parametrically (by adding currency- and maturity-time fixed effects in our yield regressions) and non-parametrically (by subtracting swap rates in our credit spread regressions).

We provide some visual reassurance that the parallel trends assumption is likely to hold in Figure 1 and Figure 2. Figure 1 plots the coefficient estimates for regression (R1) with placebo event dates (and associated two-day event windows). For each specification, only in the true event window are both the direct and indirect effects significantly greater than zero at the 5% level. Figure 2 plots the cumulative abnormal credit spread/yield changes around the event window by summing the estimated treatment effects and regression residuals. It shows a marked increase in yields on both directly and indirectly treated bonds in the event window, but not before or after it. The figures could arguably suggest a pre-trend in the control group; however, our event study estimates (which abandon the control group altogether) suggest it is not driving our results (see Section 3.4).

3.3 Do Sovereign Creditors Benefit from the Ability to Restructure?

Table 2 reports the results from estimating equation (R1), our DiD regression of English- and non-English-law yield reactions of countries with outstanding English-law bonds, each relative to non-English-law bonds of countries without English-law bonds.

3.3.1 The Effect on Directly Treated Bonds

After the ruling, the yields on English-law bonds increase by around 14 bps relative to the control group of bonds issued by countries with no English-law debt outstanding; the credit spreads increase by around 11 bps relative to the control ($\beta_1 = 0.1378$ for yields and 0.1101 for credit spreads).

The results suggest that the ruling, which made bonds easier to restructure, harmed bondholders. In the context of Section 2.1, that means bondholders care more about protection against the hold-in problem than the risk of the hold-out problem.

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3.3.2 The Spillover Effect on the Indirectly Treated Bonds

After the ruling, the yields and credit spreads on non-English-law bonds issued by countries with some outstanding English-law bonds increase by almost 14 bps relative to the control group of bonds issued by countries without them ($\beta_2 = 0.1347$ for yields and 0.1324 for credit spreads).

This result suggests a negative spillover from (making it easier to restructure) English-law bonds on other bonds, in line with easier-to-restructure English-law bonds failing to discipline a sovereign, harming holders not only of those bonds but of others as well. That channel also explains why the indirect effect is about the same size as the direct, a result verified in a within-country event study in Table 11, shown to be consistent with the disciplining role of rigid/foreign-law debt in Section 4.5.1 (see also Section 5.1).

3.4 Empirical Concerns and Robustness

We now turn to a number of robustness exercises.

3.4.1 Two Placebo Tests

There is always the concern that something else happened in the event window that affected treated and control groups differently, especially given the groups are not randomly assigned (see Section 3.2). Hence we conduct two data-driven exercises to argue that that is unlikely.

First, we do a "time-series placebo test." We assign the event dummy to each of the 650 days in our sample period outside our event window. We re-estimate regression (R1) and construct an empirical distribution of the estimated t-statistics of interest for these placebo events. We then calculate "placebo p-values" for the results, each counting the sample days on which we observe differences in yield changes between treatment and control groups that produce larger effects (as measured by their t-statistics) than those obtained for the event date.

The results are in Table 3. The result for the direct treatment effect (β_1) holds for yields (credit spreads) on 4.6% (5.1%) of days with a t-statistic as high as that on the true event date. The result for indirect treatment (β_2) holds on 6.3% (6.7%) of them. These results suggest the likelihood something happened that had an effect comparable to the ruling on *either* directly or indirectly treated bonds relative to the control is only about five percent. But the likelihood that something happened that had an effect comparable on *both* is lower; that happens on only 2.6% (2.3%) of sample days.

Most of the time, the "something" that happens is probably "noise." Occasionally, however, it could be a systematic event akin to our natural experiments. That interpretation lines up with the "data driven p-values" for each coefficient individually being slightly larger than those implied by Table 2 and with that for them jointly being small.

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Second, we do a "cross-sectional placebo test." We test whether (i) bonds governed by New York law (direct placebo) or (ii) bonds issued by countries which have such bonds outstanding (indirect placebo), react to the ruling (controlling for the true treatment effect by including all regressors from (R1)).

We find "nothing," i.e. small estimates with large standard errors, as reported in Table 3. This test says that not all foreign-law issuers are treated on the event date, only English-law ones are, allaying concerns that something else (unknown to us) happened on that date that affected foreign-law issuers differently from everyone else.

3.4.2 Longer Event Window

Using a narrow event window helps us argue our identifying assumptions hold (Section 3.2). But it has at least two shortcomings. First, our findings could reflect short-term market inefficiencies, like overreaction or fire sales. Second, if bonds do not trade over the window, the prices we observe might not be trading prices but only Bloomberg quotes (as most of the bonds trade OTC, we cannot observe volumes). Hence we consider a longer event window of a whole week, mitigating (i) by allowing for inefficiencies to work themselves out and (ii) by making it unlikely that a bond does not trade at all over the event window. We report the results in Table 4.

The effects are robust. Relative to the control group of bonds issued by countries with no English-law debt outstanding, the yields on English-law bonds increase by around 13 bps and the credit spreads by around 12 bps ($\beta_1 = 0.1343$ for yields and 0.1185 for credit spreads); the yields and credit spreads on non-English-law bonds issued by countries with some outstanding English-law bonds increase by 6 bps relative to the control ($\beta_2 = 0.0606$ for yields and 0.0611 for credit spreads) over the week following the ruling. The point estimates are smaller than those over the narrow window, indicating some reversal did occur, but their standard errors indicate similar statistical significance.

3.4.3 Event Study and Matching Estimation

Two robustness checks address any potential concerns that our results are driven by movements in the control group or selection between treatment and control: an event study and a matching estimation. For the event study, we abandon the control group and run specification (R1) for directly treated bonds and indirectly treated bonds separately. For matching, we run the specification on matched bond pairs, comparing English- to NY-law bonds for the direct effect and local-law bonds of English-law issuers to local-law bonds of non-English-law issuers for the indirect. The details of the matching procedure and a complete list of matched bond pairs are in Appendix B and, respectively, in the Supplementary Appendix (Table SA.2 and Table SA.3). Table 5 and Table 6 report results from each specification. They are in line with our baseline.

Some of the point estimates are about the same as in the baseline. Most are different, but for good reasons. In the event study specification, estimates are about the same using credit spreads but lower using yields, pointing to the value of differencing out risk-free rate movements (either by using credit spreads or by including a control group). In the matching specification, all estimates are lower, pointing to a selection inherent in restricting the sample to bonds with close matches (which turn out to be issued by either less indebted or low-credit-risk countries here; see Appendix B).

4   Model

Here we develop a model of sovereign default/restructuring (Section 4.1). We use it to investigate the mechanism behind the empirical findings above (Section 4.2), to interpret their magnitudes (Section 4.3), to calculate elasticities for a (back-of-the-envelope) policy counterfactual (Section 4.4), and to derive further predictions (Section 4.5).

4.1   Model Set-up

We consider a one-period model of a sovereign debtor. At the end of the period, it generates random output and its outstanding bonds come due. We assume that output is distributed uniformly on the unit interval under the risk-neutral measure, i.e. the $\mathbb{Q}$-distribution function is $F(z) = z$ with $z \in [0, 1]$.

The model has two key ingredients.

1. The sovereign has a willingness-to-pay problem.[18] It has the option to default strategically, but default destroys a fraction c of the output, i.e. the default cost is cz.

2. The sovereign has two different types of bonds with different haircuts in the event of default (viz. restructuring). It has "rigid bonds" D_r with haircut h_r in default and it has "flexible bonds" D_f with haircut h_f in default. Rigid bonds are harder to restructure, corresponding to a lower haircut: $h_r < h_f$.

In our empirical environment, English-law bonds correspond to rigid and local- to flexible. The High Court ruling, which allowed consent payments, corresponds to an increase in the haircut h_r on rigid bonds since, in the language of Section 2.1, it increased the bribe b payable to tendering bondholders and decreased the probability π with which hold-outs win in litigation. (The set-up in Section 2.1 also provides a micro-foundation for the heterogeneous haircuts ($h_f \neq h_r$) as b and π vary with governing law.)

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[18]On the importance of the problem, see Reinhart and Rogoff (2009), who say

If the reader has any doubt that willingness to pay rather than ability to pay is typically the main determinant of country default, he or she need only [observe] that more than half of defaults by middle-income countries occur at levels of external debt relative to GDP below 60 percent, when, under normal circumstances, real interest payments of only a few percent of income would be required to maintain a constant level of debt relative to GDP (p. 54).

The ingredients above generate the following trade-off between default and repayment. If it defaults, the sovereign suffers a deadweight loss in terms of destroyed output cz (per the first ingredient) but enjoys a lower repayment, repaying $(1 - h_i)D_i$ instead of D_i on each type of bond $i \in \{r, f\}$ (per the second ingredient). This can be seen from the following expression for the sovereign's payoff:

$$\text{payoff} = \begin{cases} z - D_r - D_f & \text{if repay} \\ \\ z - cz - (1 - h_r)D_r - (1 - h_f)D_f & \text{if default.} \end{cases} \quad (2)$$

Observe that we abstract from selective default for now—the sovereign defaults on either all debt or none.[19] However, we incorporate it explicitly in two extensions in Appendix C and show that our results are robust.

We normalize the face value of each of the sovereign's bonds to one and assume that they are priced competitively: The price p_i of a bond of type $i \in \{r, f\}$ is

$$p_i = \mathbb{Q}[\text{repay}] + \mathbb{Q}[\text{default}](1 - h_i), \quad (3)$$

where the net risk-free rate is set to zero for simplicity.

4.2 Model Results

The sovereign defaults if its payoff from defaulting exceeds its payoff from repayment, or, from equation (2), if

$$z - cz - (1 - h_r)D_r - (1 - h_f)D_f > z - D_r - D_f. \quad (4)$$

Re-writing, we see that it defaults whenever its assets z are below a threshold, which we denote by z^*:

$$z^* := \frac{h_r D_r + h_f D_f}{c}. \quad (5)$$

Thus, the $\mathbb{Q}$-probability of default is $F(z^*)$. We have the following immediate comparative statics:

Lemma 1. *The $\mathbb{Q}$-probability of default $F(z^*)$ is*

1. *decreasing in the default cost c,*

2. *increasing in the amount of outstanding debt of each type D_i, and*

3. *increasing in the haircut h_i on each type of debt.*

---

[19]Such selective default has become harder in recent decades due to the rise in bond terms, such as cross-default clauses, which say a default on one class constitutes a default on another, and pari passu clauses, which preclude payments to one class without payments to another (see Choi, Gulati, and Posner (2012)).

The first two comparative statics are typical of models with strategic default. The third points to something that is more specific to our environment: Increasing the haircut on either type of debt increases the probability of default on both types.

Our main results follow from comparative statics of the bond prices p_r and p_f with respect to h_r, capturing how the prices of each type of bond respond to the High Court ruling. To derive them, we can use the default threshold in equation (5) to re-write the bond price in equation (3) as

$$p_i = 1 - F(z^*)h_i. \tag{6}$$

We first compute the sensitivity of the price of rigid bonds to its own haircut:

$$\frac{\partial p_r}{\partial h_r} = -F'(z^*)\frac{\partial z^*}{\partial h_r}h_r - F(z^*) \tag{7}$$

$$= -\frac{D_r}{c}h_r - z^*, \tag{8}$$

having used that F is the uniform. This expression captures how an increase in the haircut h_r harms r-creditors in two ways. First, it decreases what they get in default (per the second term) and, second, it makes default more likely, because a higher haircut in default is attractive to the sovereign (per the first term).

We also compute the sensitivity of the price of flexible bonds to the haircut on rigid bonds:

$$\frac{\partial p_f}{\partial h_r} = -F'(z^*)\frac{\partial z^*}{\partial h_r}h_f \tag{9}$$

$$= -\frac{D_r}{c}h_f, \tag{10}$$

having again used that F is the uniform. This expression captures that an increase in the haircut h_r harms f-creditors. This is the spillover effect of the haircut on rigid bonds on the price of flexible: It increases the default probability, making f-bonds less likely to be repaid.

Equations (8) and (10) map to our first two main empirical findings (Section 3.3.1 and Section 3.3.2), which answer the questions we started with. The first describes how the price of rigid bonds responds to a change in its haircut:

Proposition 1. *The price of rigid bonds decreases if their haircut increases, i.e.*

$$\left.\frac{\partial p_r}{\partial h_r}\right|_{D_r>0} < 0. \tag{11}$$

The second main result describes how the price of flexible bonds responds to a change in the haircut on rigid:

Proposition 2. *The price of flexible bonds decreases if the haircut on rigid bonds increases, i.e.*

$$\left.\frac{\partial p_f}{\partial h_r}\right|_{D_r>0} < 0. \tag{12}$$

The results follow immediately from equations (8) and (10), respectively.

4.3 Implied Haircut Changes

The model can also help us to interpret our experiment, which does not immediately lend itself to quantification—it made restructuring easier, but by how much did it increase the haircut h_r? We use the model to back this number out from the yield changes we estimate empirically and a few other sufficient statistics. (We drop the assumption that the output distribution F is uniform, which, while convenient for illustration, is too restrictive for quantification.)

Using a first-order approximation, we derive a system of equations for the changes in yields on r- and f-debt, denoted by Δy_r and Δy_f, in terms of the change in haircut on rigid debt induced by the ruling, denoted by Δh_r:

Proposition 3. *A small change in the haircut on rigid bonds Δh_r induces changes in the yields of r- and f-bonds, Δy_r and Δy_f, approximately as follows:*

$$\Delta y_r \approx \frac{1}{p_r T_r}\Big(F_{h_r}h_r + F(z^*)\Big)\Delta h_r \tag{13}$$

$$\Delta y_f \approx \frac{1}{p_f T_f}F_{h_r}h_f\Delta h_r, \tag{14}$$

where T_i is the maturity of bonds of type $i \in \{h, f\}$ and F_{h_r} is short-hand for $\frac{\partial}{\partial h_r}F\big(z^*(h_r)\big)$.

We solve the system for countries with both English- and non-English-law bonds outstanding. As we detail in Appendix E, we use our estimates of Δy_r and Δy_f from our matched regression above (Section 3.4.3), the five-year CDS premiums from Markit to calculate $F(z^*)$, five-year bond maturities T_r and T_f to match the CDS, the model-implied haircuts from equation (6) for h_r and h_f, and yields adjusted for risk-free rates and currency risk from our data. These quantities are sufficient statistics to solve for Δh_r; we do not need to know the costs of default c or the sensitivity of the default probability to the r-haircut, F_{h_r} (in fact we back that out too; see the next section).

Solving (13) and (14), we find the ruling raised expected haircuts on English-law debt by about 35 bps on average.

4.4 The Default-to-haircut Elasticity and a Policy Counterfactual

We can also use the model to estimate by how much an increase in haircuts increases the default probability, a potentially relevant input for sovereign debt modeling and policy analysis. As many

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models, countries, and policies do not distinguish between r- and f-haircuts, we work with the average haircut here: $\bar{h} := \varphi h_r + (1 - \varphi) h_f$, where $\varphi := D_r/(D_r + D_f)$. The next result describes how to use observable quantities to translate F_{h_r}, which we can back out from the system in Proposition 3, into the desired elasticity:

Proposition 4. *The elasticity of the default probability $F(z^*)$ with respect to the average haircut $\bar{h}$ is*

$$\mathscr{E} \equiv \frac{\mathrm{d}F(z^*)/F(z^*)}{\mathrm{d}\bar{h}/\bar{h}} = \frac{\bar{h}}{\varphi F(z^*)} F_{h_r}, \tag{15}$$

where F_{h_r} solves the system in Proposition 3.

Applying formula (15) to averages from the raw data and the solution of the system (13) and (14), we find that $\mathscr{E} = 7.63$. I.e. a 1% increase in haircuts increases the risk-neutral default probability by 7.63%. (See Appendix E for details omitted from this section.)

To illustrate how $\mathscr{E}$ can be used for policy counterfactuals, we use it to approximate by how much a change in haircuts changes the expected deadweight costs of default for one country, Argentina, for which we have an estimate of these costs from Hébert and Schreger (2017). To do so, we use the Euler approximation to write

$$\Delta \ \mathbb{Q}\text{-expected cost} = \Delta \int_0^{z^*} cz \mathrm{d}F(z) \approx \frac{cz^* \mathscr{E}}{\bar{h}/F(z^*)} \Delta \bar{h}, \tag{16}$$

having used the fundamental theorem of calculus and substituted for $\mathscr{E}$ from above. Replacing cz^* with Hébert and Schreger's estimate, $\mathscr{E}$ with ours, and the other parameters with data on Argentina, we find that a one percentage point increase in haircuts saves about 1.3 billion USD in deadweight costs of default (see Appendix E for details).

4.5 Cross-sectional Predictions

The model generates cross-sectional predictions on the debt level and the proportion of rigid debt, which we establish in this section.

4.5.1 Law Spreads

We find that flexible bonds can be more or less sensitive to changes in h_r than rigid bonds, depending on whether the difference in the prices $p_r - p_f$, which we term the "law spread," is large to begin with:

Prediction 1. *Increasing h_r widens law spreads, i.e.*

$$\frac{\partial}{\partial h_r}\left(p_r - p_f\right) > 0 \tag{17}$$

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if and only if the law spread is sufficiently wide, i.e.

$$p_r - p_f > \frac{c(z^*)^2}{D_r}. \tag{18}$$

The result follows from subtracting equation (10) from equation (8) and substituting prices for haircuts using equation (6). It says that if a sovereign has wide law spreads to begin with ($p_r - p_f$ is large), an increase in h_r widens them further or, equivalently, p_f is more sensitive to h_r than p_r is: The haircut on rigid debt can matter more for the price of flexible debt than that of rigid debt, even though it has two effects on the latter—both via recovery values (the direct effect) and via the default probability (the indirect effect)—and only one on the former—via the default probability alone.

4.5.2 Total Debt

We find that both directly and indirectly treated bonds respond more to a change in rigid haircuts h_r when the total debt level, denoted by $D := D_r + D_f$, is higher (keeping the fraction of rigid bonds, denoted by D_r/D, constant):

Prediction 2(i). *Increasing h_r decreases the price of rigid bonds by more when D is higher:*

$$\left. \frac{\partial^2 p_r}{\partial D \partial h_r} \right|_{D_r > 0} < 0. \tag{19}$$

Prediction 2(ii). *Increasing h_r decreases the price of flexible bonds by more when D is higher:*

$$\left. \frac{\partial^2 p_f}{\partial D \partial h_r} \right|_{D_r > 0} < 0. \tag{20}$$

The intuition is that an increase in a percentage haircut matters more when there is more debt taking the haircut.

4.5.3 Rigid Bonds

We find that both directly and indirectly treated bonds respond more to a change in rigid haircuts h_r when the amount of rigid bonds D_r is higher:

Prediction 3(i). *Increasing h_r decreases the price of rigid bonds by more when D_r is higher:*

$$\left. \frac{\partial^2 p_r}{\partial D_r \partial h_r} \right|_{D_r > 0} < 0. \tag{21}$$

Prediction 3(ii). *Increasing h_r decreases the price of flexible bonds by more when D_r is higher:*

$$\left. \frac{\partial^2 p_f}{\partial D_r \partial h_r} \right|_{D_r > 0} < 0. \tag{22}$$

The intuition is that an increase in a percentage haircut on rigid bonds matters more when there are more rigid bonds taking the haircut.

5 Cross-sectional Heterogeneity

To test the additional predictions developed in Section 4.5, we interact the DiD regressions from specification (R1) with country-level variables, notably the "law spread" and several measures of debt and rigid bonds.

5.1 Law Spreads

To test Prediction 1—i.e. indirectly treated (local-law) bond yields increase more than directly treated (English-law) ones whenever the "law spread" is high to begin with—we turn to a within-country regression, with a "triple difference" specification. The first difference, as in the baseline, is between inside and outside the event window. The second difference is between the yields on a country's English-law bonds and on that country's other bonds. The third "difference" is an interaction with a continuous variable, comparing countries with higher versus lower "law spreads":

$$
\begin{aligned}
x_{i,t} = {}& \gamma_1 \mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}} \times S_{c,t}^{\text{E}} + \gamma_2 \mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}} \\
& + \gamma_3 \mathbb{1}_i^{\text{Direct}} \times S_{c,t}^{\text{E}} + \gamma_4 \mathbb{1}_i^{\text{Direct}} + \delta^\top \text{FE}_{i,t} + \psi^\top \text{NY}_{i,t} + \eta_{i,t} \,.
\end{aligned}
\tag{R2}
$$

The indicators are as in the baseline DiD regression (R1).

$S_{c,t}^{\text{E}}$ denotes the law spread, i.e. the difference in yields between a country's English- and local-law bonds, all else equal. All else is never equal, however; bonds with different governing laws tend to have systematically different currency and/or maturity. So we use three different measures, each adjusting the yield to address the concern in different ways. (1) We de-mean bond yields by country-time, currency, and maturity bin, (2) we subtract currency- and maturity-matched risk-free rates (i.e. we use credit spreads), and (3) we de-mean these credit spreads by country-time, currency, and maturity bin.

The term $\psi^\top \text{NY}_{i,t}$ focuses the comparison on English- versus local-law bonds by removing the second notable group of foreign-law bonds (under New York law) from the estimation of the γ-coefficients. Specifically, $\psi^\top \text{NY}_{i,t} = \psi_1 \mathbb{1}_t^{\text{Event}} \mathbb{1}_i^{\text{NY}} S_{c,t}^{\text{E}} + \psi_2 \mathbb{1}_t^{\text{Event}} \mathbb{1}_i^{\text{NY}} + \psi_3 \mathbb{1}_i^{\text{NY}} S_{c,t}^{\text{E}} + \psi_4 \mathbb{1}_i^{\text{NY}}$.

FE_{it} denotes a country-time fixed effect, making the regression, unlike the baseline, a within-country comparison, comparing the direct and indirect effects of the ruling.

The key coefficient of interest is γ_1 on the triple-interaction term, which captures the relative changes in x_{it} around the event for directly and indirectly treated bonds in high vs. low law spread countries.

Table 7 reports the results. We find that the indirect (spillover) effect is stronger relative to the direct treatment effect in countries whose English-law bonds trade at low yields relative to their

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local-law bonds, i.e. for countries with relatively high law spreads. Our results suggest that for a 1 percentage point increase in a country's law spread there is approximately a 4–13 bps increase in local-law yields/credit spreads relative to English-law yields around the ruling, depending on the specification.

While the indirect/spillover effect is about the same size as the direct effect *on average* ($\beta_2 \approx \beta_1$ in (R1)), it is smaller for some bonds and larger for others: The spillover is larger for bonds issued by countries with high law spreads and smaller for those issued by countries with low ones. This result is in line with the model mechanism, in which spillovers from harder-to-restructure English-law bonds on other bonds arise because the former deter default altogether. This effect is more valuable for easy-to-restructure local-law bonds with lower recoveries in the event of a restructuring, and these bonds should trade at higher yields ex ante.

5.2 Total Debt and Rigid Bonds

To test our remaining cross-sectional predictions, we interact the DiD estimators from the baseline with other bond characteristics, denoted by X. Specifically, we estimate models of the form:

$$
\begin{aligned}
x_{i,t} = {} & \kappa_1 \mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}} \times X_{c,t} + \kappa_2 \mathbb{1}_t^{\text{Event}} \times \mathbb{1}_{i,c}^{\text{Indirect}} \times X_{c,t} \\
& + \kappa_3 \mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}} + \kappa_4 \mathbb{1}_t^{\text{Event}} \times X_{c,t} + \kappa_5 \mathbb{1}_i^{\text{Direct}} \times X_{c,t} \\
& + \kappa_6 \mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Indirect}} + \kappa_7 \mathbb{1}_i^{\text{Indirect}} \times X_{c,t} \\
& + \kappa_8 \mathbb{1}_i^{\text{Direct}} + \kappa_9 \mathbb{1}_{i,c}^{\text{Indirect}} + \kappa_{10} X_{c,t} + \xi^\top \text{FE}_{i,t} + \zeta_{i,t},
\end{aligned}
\tag{R3}
$$

where the notation is analogous to that described in Section 3.1. The coefficients of interest are κ_1 and κ_2, which capture, respectively, how the direct and indirect effects above depend on (measures of) debt. We report all results in Table 8.

5.2.1 Total Debt

Letting X in equation (R3) be a measure of a country's *total* debt, we find that yields and credit spreads on English- and non-English-law bonds both rise by more on the event date when a country is more indebted (if X is the log of a country's total debt, $\kappa_1 = 0.0607$ (0.0600) and $\kappa_2 = 0.0790$ (0.0786); if it is its debt-to-GDP ratio, $\kappa_1 = 0.2483$ (0.1967) and $\kappa_2 = 0.1035$ (0.1239)).[20] These results confirm both Prediction 2(i) and Prediction 2(ii).

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[20]We do not use the level of total debt directly because it is highly skewed, but adjust for skewness in standard ways (taking logs and normalizing by GDP).

5.2.2 Rigid Bonds

Letting X be the ratio of a country's English-law debt to GDP (the level in logs is undefined for countries without English-law bonds), we find that on the event date, yields and credit spreads rise by more on English-law bonds ($\kappa_1 = 0.3766$ (0.2646), statistically significant), but not on non-English-law bonds ($\kappa_2 = -0.3048$ (-0.2576), statistically indistinguishable from zero) when a country has relatively more English-law debt. These results confirm Prediction 3(i). (They fail to confirm Prediction 3(ii), but do not provide significant evidence against it either.)

6 External Validity: Additional Events

We interpret our results broadly—bondholders' commitment not to restructure disciplines a sovereign debtor. But they are based on a comparatively narrow event—the English High Court ruling on consent payments. External validity remains a question. To address it, we consider three additional experiments that altered the ability to restructure sovereign debt, now making it harder, not easier, to restructure. The first two, like the baseline, applied to English-law bonds (Section 6.1) and the third to New York-law bonds (Section 6.2). Although each presents greater identification challenges than our baseline (see Section 6.1.2 and Section 6.2.3), together they affirm its message: The commitment not to restructure one class of bonds benefits not only that class but others as well.

6.1 Two More Experiments on Restructuring under English Law

Here we exploit a ruling and its (dropped) appeal pertaining to another controversial restructuring device used to combat the hold-out problem: exit consents, which are the stick to consent payments' carrot.

6.1.1 The Ruling and (Dropped) Appeal: Assenagon Asset Management v Irish Bank Resolution Corp

Just two months after its ruling on consent payments in Azevedo v Imcopa (Section 2.2), the High Court ruled on a related restructuring device, called exit consents, in Assenagon v Irish Bank Resolution Corp. Exit consents inflict a punishment on hold-out bondholders in exchange offers: When tendering creditors part with their bonds ("exit"), they agree ("consent") to changes in the terms of other bonds in the same class. Like consent payments, they counter the hold-out problem, but can create a hold-in problem, and therefore be deemed coercive.[21] That is how the Court ruled: It upheld a challenge to

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[21]They are nonetheless alive and well in the sovereign debt market broadly, as evinced by a current proposal to restructure Lebanese debt, namely Luo, Smith, and Xiao (2020).

their legality under English law.[22] The presiding judge, Michael Briggs, wrote that the case pertained to "a question of wide importance within the bond market."

The case was brought against Anglo Irish by a bondholder, Assenagon Asset Management, who held out from an exchange offer. After having received just €170 for Anglo-Irish bonds with face value of €17 million, Assenagon sued, submitting, inter alia, that the exit consents in the exchange offer were abusive. Briggs ruled in favor of the plaintiff on July 27, 2012. It was the Court's first decision on exit consents.

Anglo Irish appealed. The appeal was withdrawn on April 22, 2013.

6.1.2 Estimation and Identification Challenges

We run the same regression as in our baseline specifications (R1), setting the event dummy equal to one on the trading day after the respective event, that is, on July 30, 2012 (the Assenagon ruling) and April 23, 2013 (the withdrawal of the appeal).

As above, the identifying assumption in our DiD estimation is parallel trends in the absence of treatment (see Section 3.2). As above, we address it in two basic ways: (i) We include region-time fixed effects in our regressions and (ii) we consider narrow event windows of just two trading days. Thus, as above, to be a threat to our identification, an event must affect both treatment and control bonds differently within the same currency, maturity, and region/country and also happen at nearly the same time as the events.

Unlike above, such a threat is more than a remote possibility. Indeed, each experiment is at risk of being confounded by another event. The ruling could be confounded by a speech that ECB President Mario Draghi gave the previous day, in which he promised to do "whatever it takes to preserve the euro," a statement that could have affected treated and control bonds differently. The withdrawal of the appeal could be confounded by the Appellate Court upholding the ruling in Azevedo v Incompa, a decision that, by construction, affects treated and control bonds differently.

Although these threats make us more tentative in our interpretation, neither undermines our identification. The first experiment remains valid in an efficient market in which the information in Draghi's speech would be fully incorporated in prices before our event window. The second remains valid as long as the effect is interpreted as the net effect of the two events: the withdrawn appeal in the Assenagon case and the upheld ruling in Azevedo. There is little doubt that the net effect of the appeals is to make restructuring harder: Assenagon outweighs Azevedo. It overturned rather than upheld market practice, and it was surprising to market participants (e.g., Skadden (2012)).

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[22]See Assenagon Asset Management SA v Irish Bank Resolution Corp Ltd (formerly Anglo Irish Bank Corp Ltd) [2012] EWHC 2090 (Ch) (27 July 2012).

6.1.3 Do Sovereign Creditors Benefit from the Ability to Restructure?

Table 9 reports the estimated coefficients. The estimates on all eight coefficients of interest are in accordance with the analogous results for the baseline ruling (given that the direction of treatment has flipped): Both yields and credit spreads on both directly and indirectly treated bonds decrease for both the ruling and withdrawal of the appeal.

The standard errors are large for the ruling, perhaps due to increased volatility in the sovereign debt market following Draghi's speech, and render the estimates attenuated and statistically insignificant at conventional levels. They are smaller for the withdrawn appeal (relative to the baseline), perhaps due to the offsetting event. But the standard errors are also small in this case, with p-values from 4–7%.

6.2 An Experiment on Restructuring under New York Law

We exploit several court decisions throughout the Argentine restructuring saga, culminating in a 2014 Manhattan federal district court ruling that made it harder to punish hold-out bondholders.

6.2.1 The Argentine Restructuring Saga

In 2001 Argentina missed payments on NY-law bonds with face value of about \$82bn. Over the next decade, it restructured over 90% of the debt in exchange offers, with bondholders agreeing to a 70% haircut. It serviced the restructured debt and defaulted on the hold-outs.

Litigation ensued. Most notably, a hedge fund, NML Capital, sued Argentina in New York, demanding full repayment. It argued that defaulting on hold-outs while repaying other bonds violated a covenant in the bond indenture: the pari passu clause. The presiding judge, Thomas P. Griesa, ruled with the plaintiff. It prevented Argentina (via the trustee that distributed payments on its behalf) from servicing any debt until the hold-out bonds were paid in full.

To avoid another hard default, Argentina appealed, counter-sued, and even tried to service its debt outside of US jurisdiction. Ultimately, the US judiciary affirmed the decision and blocked the attempts to circumvent its implementation. Argentina defaulted on its bonds serviced via US-based payment systems.

6.2.2 Data Sources and Sample Construction

We supplement the Dealogic and Bloomberg data described above (Section 2.3) with data on changes in the estimated risk-neutral probability of Argentine default on fifteen event-days throughout the saga—including not only NML's filing and the court's ruling, but also Argentina's appeals and counter-suits—taken from Hébert and Schreger (2017).

6.2.3 Estimation and Identification Challenges

We run similar regressions to (R1) in the baseline. The key difference is the pre-/post-indicators: Given that we have multiple events, we multiply them by the intensity of treatment as captured by the change in the estimated risk-neutral probability of default, which we denote by ΔP_t^{AR}, following Hébert and Schreger (2017).

Specifically, to evaluate the association between the ability to restructure and bond prices, we estimate a model that we label (R1′), which nests two DiD specifications. In each, the first difference is the change in the risk neutral probability of default over a two-day window associated with a judiciary decision.[23] The second difference is between our control group of bonds issued by countries with *no* outstanding NY-law bonds and a treatment group. As above, the nested specifications differ in the treatment group, which is either (i) NY-law bonds ("directly treated" bonds) or (ii) non-NY-law bonds issued by countries with *some* outstanding NY-law bonds ("indirectly treated" bonds). Formally, the model is:

$$
\begin{aligned}
x_{i,t} = {}& \lambda_1 \Delta P_{t^*}^{\mathrm{AR}} \times \mathbb{1}_i^{\mathrm{Direct}} + \lambda_2 \Delta P_{t^*}^{\mathrm{AR}} \times \mathbb{1}_{i,c}^{\mathrm{Indirect}} \\
& + \lambda_3 \mathbb{1}_i^{\mathrm{Direct}} + \lambda_4 \mathbb{1}_{i,c}^{\mathrm{Indirect}} + \mu^\top \mathrm{FE}_{i,t} + \nu_{i,t}.
\end{aligned}
\tag{R1′}
$$

The notation is analogous to that described in Section 3.1 with the addition here that we denote the treatment intensity variable on event dates by $\Delta P_{t^*}^{\mathrm{AR}}$ to distinguish the change on event dates t^* from that on others. The key coefficients of interest are λ_1 and λ_2.

As above, the identifying assumption in our DiD estimation is parallel trends in the absence of treatment (see Section 3.2). As above, we address it in two basic ways: (i) We include region-time fixed effects in our regressions and (ii) we consider narrow event windows of two trading days. Thus, as above, to be a threat to our identification, an event must both affect treatment and control bonds differently within the same currency, maturity, and region and also happen at nearly the same time as the court decisions.

As above, such a threat seems hard to imagine: It is probably safe to interpret a change in yields as a causal effect of the events. Unlike above, however, our interpretation—that the effect is the result of bonds becoming harder to restructure—does not follow immediately. The reason is that the events did not only make NY-law bonds harder to restructure, but also made Argentina more likely to default. To preserve our interpretation, an additional assumption is necessary, albeit an apparently weak one: Countries' exposure to Argentine default is uncorrelated with their propensity to borrow under New York law *within region*.

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[23]As for the English High Court ruling, we do not use a single-day window because sovereign bonds are traded OTC in mainly local markets, which close at different times, and we do not always know the exact time that the relevant decision taken in New York was made public. Instead, we follow the two-day windows chosen by Hébert and Schreger (2017).

That said, while unlikely, such a violation of this assumption would probably not even alter our interpretation of the signs of our estimates, but only of their magnitudes. To see why, observe that should there be any correlation between issuing NY-law bonds and exposure to Argentina, it seems most likely to be positive: Argentina's small-open-economy neighbors Paraguay and Uruguay also borrow mainly via NY-law debt, in line with the idea that countries more exposed to Argentine default could be forced to borrow under foreign law themselves. This exposure effect would countervail against the disciplining effect of bonds becoming harder to restructure, biasing against our results.

6.2.4 Do Sovereign Creditors Benefit from the Ability to Restructure?

The two right-hand columns in Table 9 report the estimated coefficients. Again, the direction of treatment is reversed compared to our baseline results.

In accordance with the analogous result for the High Court ruling (Section 3.3.1), we find negative treatment effects for directly treated and indirectly treated bonds: For a 1% increase in the risk-neutral probability of Argentine default, they fall by about 0.2–0.3 bps. Three of the four estimates are statistically significant at the 5% level.

7   Conclusion

We bring evidence from natural experiments to bear on the value of commitment not to restructure sovereign bonds. Our research design allows us to distinguish between the value for bondholders individually and collectively, and thereby assess cross-bond spillovers, like those implied by the "burglar alarm" view that to commit not to restructure one bond is only to encourage the restructuring of others (Bolton and Jeanne (2007, 2009)).

Our empirical results suggest a perspective new to the literature. We find that committing not to restructure one bond is more like a successful neighborhood crime watch than a self-defeating burglar alarm, viz. it benefits both that bond and others as well. We develop a model of the "crime watch" perspective that not only formalizes the mechanism behind it but also proves useful to quantify natural experiments and analyze policy counterfactuals.

Figure 1: **Difference-in-differences plot.** We plot dynamic DiD coefficients for each 2-trading-day window two weeks before/after the English High Court ruling (Wednesday, May 30, 2012). To do so, we augment regression (R1) with additional interactions of the two treatment indicators with time dummies before and after our chosen event window. The plots show the coefficients and their 95% confidence intervals for two-day yield changes (with parametric adjustments for risk-free rates, top panel) and credit spread changes (non-parametric risk-free rate adjustment, bottom panel).
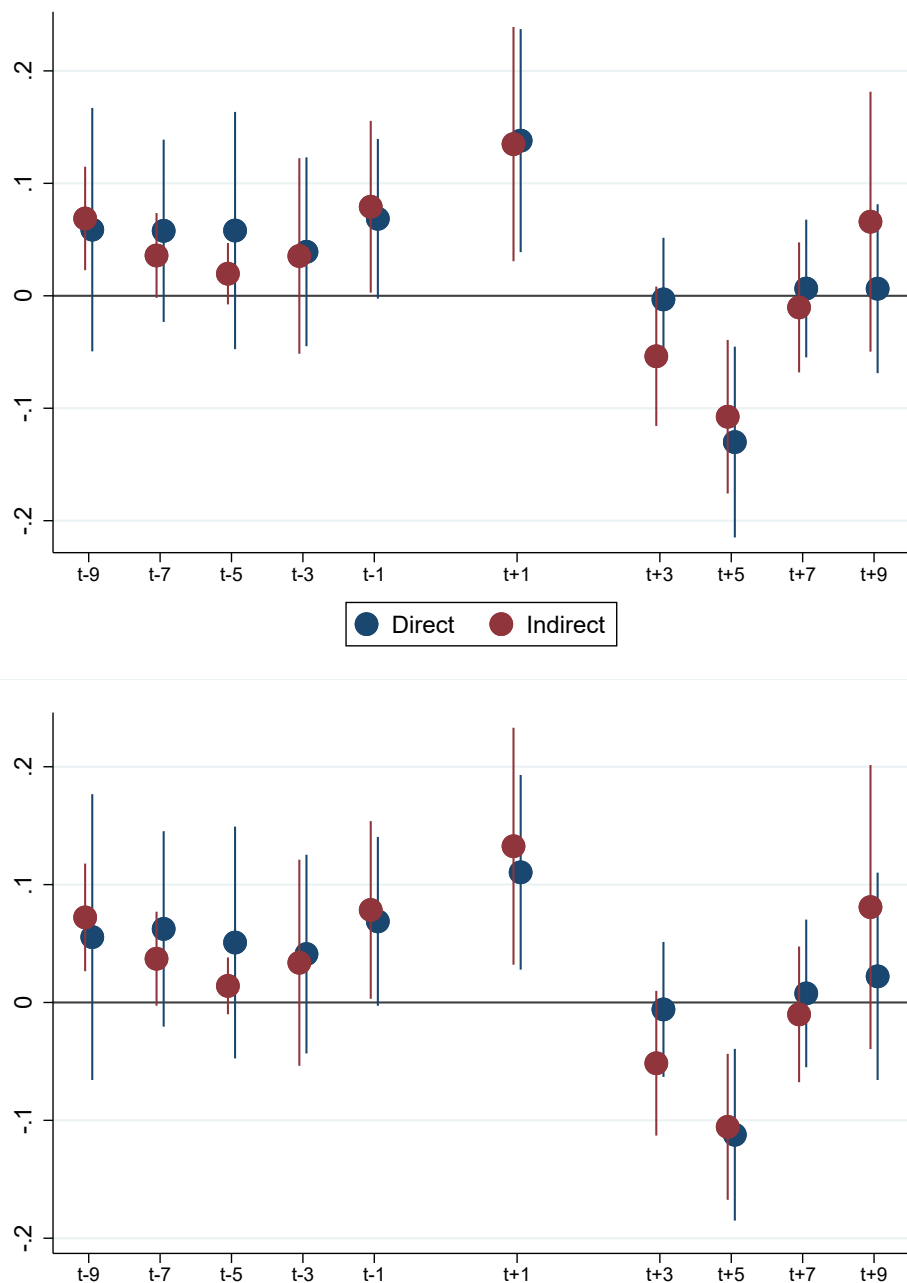
Figure 2: **Difference-in-differences levels.** To visualize the level change in yields (top panel) and credit spreads (bottom panel) around the English High Court ruling, we plot the cumulative "abnormal" changes for each trading day within a month of the ruling (Wednesday, May 30, 2012). The abnormal changes are composed of the estimated treatment effects and regression residuals from regression (R1) as $\hat{x}_{i,t} = \hat{\beta}_1 \mathbb{1}_t^{\text{Event}} \mathbb{1}_i^{\text{Direct}} + \hat{\beta}_2 \mathbb{1}_t^{\text{Event}} \mathbb{1}_{i,c}^{\text{Indirect}} + \hat{\varepsilon}_{i,t}$. Their cumulative value is on the vertical axis.
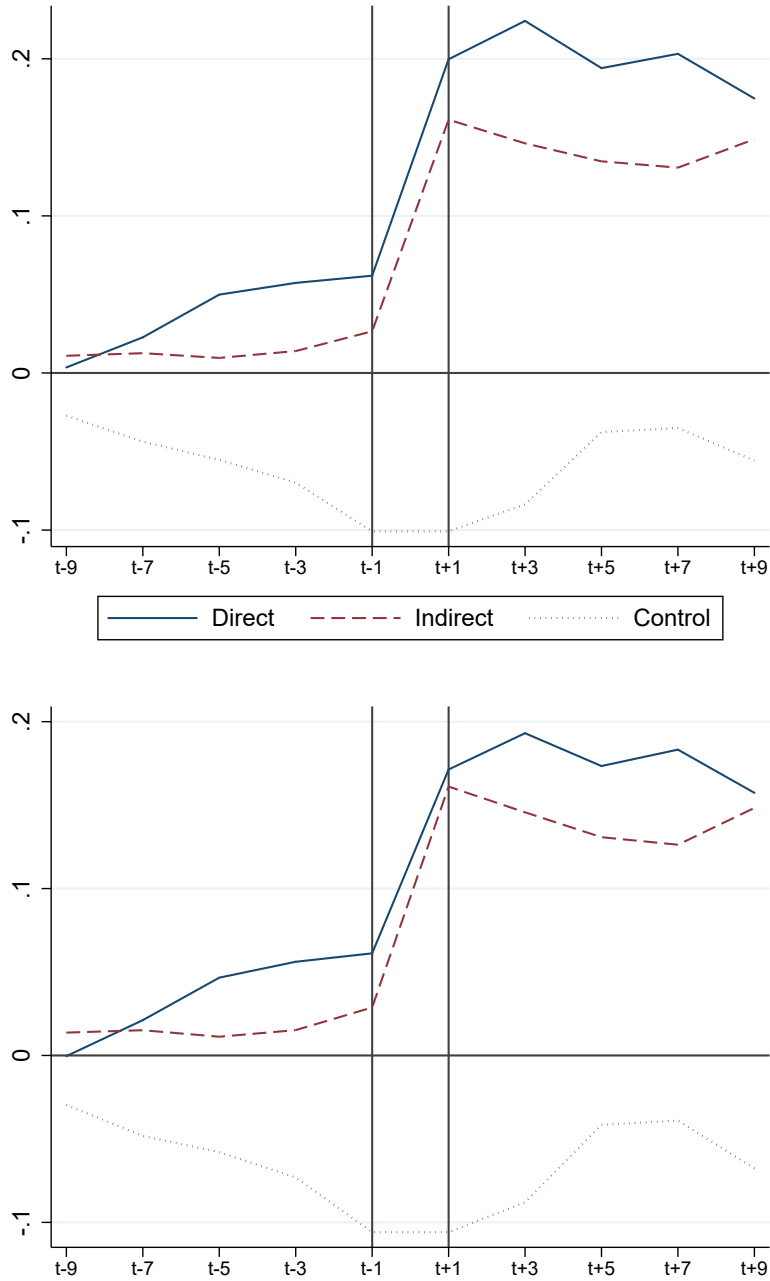
Table 1: **Summary statistics on the event date.** We report statistics for the 67 countries with bonds in our sample on the event date, Thursday, May 31, 2012, following the announcement of the ruling on May 30. In the first three columns, we report statistics by bond group (direct treatment, indirect treatment, and control), with the total number of bonds in the first row and group-means in the remaining rows. These columns provide bond-level comparisons between the two treatment groups and the control group. The other columns report the distributions across countries of the same variables averaged by country (e.g., the average number of bonds per country is 20.57). These columns describe the country-level heterogeneity in the respective variables.

Variable	Bond-level statistics			Distribution of country means				
	Direct	Indirect	Control	Mean	St. Dev.	1%	Median	99%
Number of bonds	237	786	355	20.57	22.22	1	14	124
Face value per bond ($bn)	1.44	2.66	3.45	1.75	2.16	0.17	0.96	14.65
Maturity	8.15	8.08	9.98	9.08	4.63	2.19	8.04	22.22
Yield (%)	5.22	4.55	3.13	5.39	3.24	0.51	4.75	13.85
Two-day Δ (yield)	0.05	0.04	−0.03	0.02	0.10	−0.20	0.00	0.40
Credit spread (%)	3.98	2.22	1.40	3.46	3.11	−1.43	2.93	11.51
Two-day Δ (credit sp.)	0.10	0.10	0.05	0.08	0.10	−0.09	0.06	0.41
Fraction English law	1.00	0.00	0.00	0.35	0.39	0.00	0.14	1.00
Fraction New York law	0.00	0.21	0.39	0.24	0.35	0.00	0.00	1.00
Fraction local law	0.00	0.75	0.60	0.39	0.37	0.00	0.29	1.00

Table 2: **Baseline estimates: The effect of the ruling on directly treated and indirectly treated bonds.** We estimate the DiD regression (R1) (Section 3.3). We report standard errors in parentheses, clustered at the country level.

	Δ yield		Δ credit spread	
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}}$	0.1027	0.1378	0.0921	0.1101
	(0.0409)	(0.0497)	(0.0363)	(0.0413)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Indirect}}$	0.1007	0.1347	0.0954	0.1324
	(0.0401)	(0.0522)	(0.0436)	(0.0503)
$\mathbb{1}_i^{\text{Direct}}$	-0.0041	-0.0066	-0.0053	-0.0065
	(0.0021)	(0.0022)	(0.0018)	(0.0020)
$\mathbb{1}_{i,c}^{\text{Indirect}}$	-0.0036	-0.0054	-0.0039	-0.0051
	(0.0015)	(0.0014)	(0.0012)	(0.0013)
Region-Time	Yes	Yes	Yes	Yes
Currency-Time	No	Yes	No	Yes
Maturity-Time	No	Yes	No	No
Observations	1013548	1011311	904945	904091
R^2	0.183	0.308	0.181	0.353

Table 3: **Placebo tests.** We run two placebo tests. *Time-series:* We re-estimate the DiD regression (R1) assigning the event indicator to 650 placebo dates. We then report in brackets the fraction of these dates for which the t-statistic of the estimated placebo-coefficients is larger than that obtained for the event window. The row labelled "Joint p-value" reports this statistic for the joint results (β_1, β_2). *Cross-sectional:* We estimate the placebo-treatment effect on NY-law bonds (direct placebo) and bonds of NY-law issuers (indirect placebo) on the event date against the remaining control group.

	Time-series placebo p-values (EN-law, placebo dates)		Cross-sectional placebo coefficients (NY-law, event date)	
	Δ yield	Δ credit spread	Δ yield	Δ credit spread
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}}$	[0.046]	[0.051]	0.0088	0.0101
			(0.0538)	(0.0523)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Indirect}}$	[0.063]	[0.067]	0.0607	0.0670
			(0.0716)	(0.0673)
Joint p-value	[0.026]	[0.023]		
Region-Time	Yes	Yes	Yes	Yes
Currency-Time	Yes	Yes	Yes	Yes
Maturity-Time	Yes	No	Yes	No

Table 4: **Baseline estimates over a one-week event window** We estimate the DiD regression (R1) for a longer event window of five trading days (yield changes from $t-1$ to $t+4$). We report standard errors (clustered at the country level) in parentheses.

	Δ yield	Δ credit spread
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}}$	0.1343	0.1185
	(0.0378)	(0.0362)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Indirect}}$	0.0606	0.0611
	(0.0278)	(0.0285)
Region-Time	Yes	Yes
Currency-Time	Yes	Yes
Maturity-Time	Yes	No
Observations	1003254	896743
R^2	0.354	0.348

Table 5: **Event study estimates.** We estimate the analog of regression (R1) absent the control group; i.e. we run separate regressions of (i) English-law bonds and (ii) non-English-law bonds from countries with English-law debt. We report clustered standard errors (at the country level) in parentheses.

	Δ yield		Δ credit spread	
	Direct	Indirect	Direct	Indirect
$\mathbb{1}_t^{\text{Event}}$	0.0588	0.0512	0.1109	0.1072
	(0.0380)	(0.0306)	(0.0341)	(0.0381)
Region-Time	Yes	Yes	Yes	Yes
Currency-Time	Yes	Yes	Yes	Yes
Maturity-Time	Yes	Yes	No	No
Observations	180821	573389	176507	494387
R^2	0.003	0.003	0.002	0.002

Table 6: **Matching estimates.** We estimate the analog of regression (R1) among matched bonds (see Section 3.4.3) separately for the (i) direct and (ii) indirect effects and their respective matches (Appendix B):

$$x_{i,t} = \theta_1 \mathbb{1}_t^{\text{Event}} \times \mathbb{1}_{i,c}^{\text{Treatment}} + \theta_2 \mathbb{1}_{i,c}^{\text{Treatment}} + \omega_{ij,t} + \sigma_{i,t}. \tag{23}$$

As in the baseline, the dependent variable $x_{i,t}$ is the two-day change in either the bond yield or the credit spread. The fixed effect $\omega_{ij,t}$ absorbs variation at the pair-time level. We report standard errors clustered by control bond in parentheses.

	Δ yield		Δ credit spread	
	Direct	Indirect	Direct	Indirect
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_{i,c}^{\text{Treatment}}$	0.0661	0.0433	0.0580	0.0425
	(0.0213)	(0.0083)	(0.0193)	(0.0085)
Pair-Time	Yes	Yes	Yes	Yes
Bond pairs	124	83	124	83
Country pairs	42	12	42	12
Observations	136586	90778	132234	87306
R^2	0.594	0.766	0.620	0.659

Table 7: **Baseline estimates: The effect of the ruling on "law spreads."** We estimate the DiD regression (R2) (Section 5.1). We report standard errors in parentheses, clustered at the country level. We construct three law spreads from three different yield estimates: (1) de-meaning yields by country-time and by currency-maturity bin, (2) yields net of maturity- and currency-matched swap rates (i.e. credit spreads), and (3) de-meaning credit spreads by country-time and currency-maturity bin. In each case, the law spread is the within-country difference between the average yield estimate for local-law bonds and that for English-law bonds.

Law spread	Δ yield			Δ credit spread		
	(1)	(2)	(3)	(1)	(2)	(3)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}} \times S_{c,t}^{\text{E}}$	−0.1319	−0.0414	−0.0835	−0.1226	−0.0364	−0.0764
	(0.0434)	(0.0145)	(0.0279)	(0.0385)	(0.0127)	(0.0241)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}}$	−0.0225	−0.0445	−0.0443	−0.0205	−0.0414	−0.0430
	(0.0187)	(0.0342)	(0.0301)	(0.0177)	(0.0323)	(0.0272)
$\mathbb{1}_i^{\text{Direct}} \times S_{c,t}^{\text{E}}$	−0.0045	−0.0007	−0.0031	−0.0052	−0.0011	−0.0045
	(0.0016)	(0.0010)	(0.0022)	(0.0012)	(0.0010)	(0.0022)
$\mathbb{1}_i^{\text{Direct}}$	−0.0014	−0.0008	−0.0007	−0.0011	−0.0009	−0.0007
	(0.0006)	(0.0006)	(0.0004)	(0.0005)	(0.0006)	(0.0004)
Region-Time	Yes	Yes	Yes	Yes	Yes	Yes
Currency-Time	Yes	Yes	Yes	Yes	Yes	Yes
Maturity-Time	Yes	Yes	Yes	No	No	No
Observations	606536	522602	522602	529061	488278	488278
R^2	0.510	0.534	0.534	0.591	0.594	0.594

Table 8: **Heterogeneity in treatment effects.** We estimate the triple-difference regression (R3) (Section 5). Column headers indicate the dependent variable and the triple-difference variable X in regression (R3): (i) the log of total outstanding debt, (ii) the debt-to-GDP ratio, or (iii) English-law debt as a fraction of GDP. We report standard errors (clustered at the country level) in parentheses.

$X_{c,t}$	log(Total Debt)		Debt/GDP		En.-law Debt/GDP	
Dependent variable	Δ yield	Δ credit sp.	Δ yield	Δ credit sp.	Δ yield	Δ credit sp.
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}} \times X_{c,t}$	0.0607	0.0600	0.2483	0.1967	0.3766	0.2646
	(0.0251)	(0.0205)	(0.0872)	(0.0825)	(0.1576)	(0.1388)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_{i,c}^{\text{Indirect}} \times X_{c,t}$	0.0790	0.0786	0.1035	0.1239	-0.3048	-0.2576
	(0.0170)	(0.0160)	(0.0819)	(0.0823)	(0.2935)	(0.2905)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}}$	-0.1547	-0.1807	0.0325	0.0296	0.0511	0.0496
	(0.0858)	(0.0777)	(0.0441)	(0.0428)	(0.0394)	(0.0377)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_{i,c}^{\text{Indirect}}$	-0.2921	-0.2922	0.0862	0.0759	0.1462	0.1423
	(0.0908)	(0.0857)	(0.0525)	(0.0506)	(0.0596)	(0.0584)
Region-Time	Yes	Yes	Yes	Yes	Yes	Yes
Currency-Time	Yes	Yes	Yes	Yes	Yes	Yes
Maturity-Time	Yes	No	Yes	No	Yes	No
Observations	1011311	904091	995799	888882	995799	888882
R^2	0.309	0.354	0.310	0.354	0.309	0.354

Table 9: **Estimates from additional events.** We estimate the DiD regressions (R1), using trading days following the event dates July 27, 2012 and April 22, 2013, and (R1′). We report clustered standard errors (at the country level) in parentheses.

Regression	(R1)		(R1)		(R1′)	
Event	July 31, 2012		April 23, 2013		$\Delta P_{t^*}^{\text{AR}}$	
Dependent variable	Δyield	Δcredit sp.	Δyield	Δcredit sp.	Δyield	Δcredit sp.
Event $\times \mathbb{1}_i^{\text{Direct}}$	-0.0648	-0.0582	-0.0384	-0.0406	-0.0022	-0.0034
	(0.0454)	(0.0476)	(0.0211)	(0.0213)	(0.0016)	(0.0017)
Event $\times \mathbb{1}_{i,c}^{\text{Indirect}}$	-0.0726	-0.0710	-0.0585	-0.0612	-0.0015	-0.0015
	(0.0368)	(0.0386)	(0.0296)	(0.0292)	(0.0007)	(0.0007)
$\mathbb{1}_i^{\text{Direct}}$	-0.0063	-0.0063	-0.0064	-0.0063	-0.0016	-0.0007
	(0.0021)	(0.0019)	(0.0022)	(0.0020)	(0.0012)	(0.0013)
$\mathbb{1}_{i,c}^{\text{Indirect}}$	-0.0051	-0.0048	-0.0051	-0.0049	-0.0014	-0.0012
	(0.0013)	(0.0013)	(0.0013)	(0.0013)	(0.0015)	(0.0015)
Region-Time	Yes	Yes	Yes	Yes	Yes	Yes
Currency-Time	Yes	Yes	Yes	Yes	Yes	Yes
Maturity-Time	Yes	No	Yes	No	Yes	No
Observations	1011311	904091	1011311	904091	1011311	904091
R^2	0.308	0.353	0.308	0.353	0.308	0.353

Table 10: **Estimates in levels.** We estimate the analogs of regressions (R1) and (R2) in levels, using yields and credit spreads immediately before and after the court ruling. We report standard errors (clustered at the country level) in parentheses.

	yield (R1)	credit spread (R1)	yield (R2)	credit spread (R2)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}} \times S_{c,t}^E$			−0.1450	−0.1444
			(0.0589)	(0.0533)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}}$	0.0886	0.0567	−0.0191	−0.0188
	(0.0293)	(0.0303)	(0.0204)	(0.0172)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_{i,c}^{\text{Indirect}}$	0.1258	0.1243		
	(0.0537)	(0.0521)		
$\mathbb{1}_i^{\text{Direct}} \times S_{c,t}^E$			−0.1323	−0.0713
			(0.3096)	(0.3007)
Region-Time	Yes	Yes	Yes	Yes
Currency-Time	Yes	Yes	Yes	Yes
Maturity-Time	Yes	No	Yes	No
Observations	2722	2548	1352	1246
R^2	0.0916	0.0865	0.0515	0.0654

Table 11: **Baseline estimates: The relative effect of the ruling on directly treated versus indirectly treated bonds within country.** We estimate the DiD regression (R1) (Section 3.3). We report standard errors in parentheses, clustered at the country level.

	Δ yield		Δ credit spread	
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}}$	−0.0131	−0.0012	0.0198	−0.0062
	(0.0165)	(0.0211)	(0.0214)	(0.0204)
$\mathbb{1}_i^{\text{Direct}}$	−0.0005	−0.0005	−0.0015	−0.0008
	(0.0007)	(0.0005)	(0.0010)	(0.0004)
Region-Time	Yes	Yes	Yes	Yes
Currency-Time	No	Yes	No	Yes
Maturity-Time	No	Yes	No	No
Observations	1009631	1007242	900660	899654
R^2	0.506	0.575	0.550	0.634

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Table 12: **Summary statistics on the event date, by country.** We report statistics for the 67 countries with bonds in our final sample on the event date, Thursday, May 31, 2012, following the announcement of the ruling on May 30.

Country	Bonds	Yield	Credit spread	Maturity	Total ($bn)	English law (%)	NY law (%)	Local law (%)
AE	13	3.73	2.62	9.84	15.56	1.00	0.00	0.00
AT	33	1.69	0.81	10.02	91.41	0.58	0.00	0.37
BB	4	6.92	4.30	14.42	1.20	0.90	0.00	0.10
BE	51	1.90	1.15	10.88	193.89	0.00	0.00	1.00
BG	23	3.28	2.85	4.97	6.84	0.17	0.56	0.24
BH	6	4.53	3.38	7.06	11.24	0.99	0.00	0.00
BR	35	5.32	1.86	8.66	1284.43	0.00	0.03	0.97
BS	6	6.03	4.06	18.99	1.80	0.00	1.00	0.00
BY	2	10.99	10.57	4.42	1.80	1.00	0.00	0.00
CI	2	10.37	8.38	20.60	5.22	0.00	1.00	0.00
CL	12	2.86	−1.43	14.68	13.15	0.00	0.38	0.62
CO	23	4.25	1.04	11.25	115.29	0.00	0.14	0.84
CR	4	4.58	3.32	12.67	46.68	0.00	0.16	0.68
CY	3	13.85	11.51	4.47	5.11	0.76	0.00	0.00
CZ	27	2.25	1.02	9.14	20.50	0.71	0.00	0.29
DE	66	0.51	−0.22	7.49	509.41	0.00	0.00	0.99
DK	17	0.61	−0.41	6.50	40.09	0.55	0.00	0.45
DO	7	6.31	4.05	8.40	13.38	0.00	0.80	0.20
EC	2	13.85	11.51	23.20	10.02	0.20	0.80	0.00
EG	3	7.33	4.72	12.94	26.55	0.14	0.16	0.70
ES	53	4.75	4.11	6.79	459.65	0.03	0.00	0.97
FI	23	1.12	0.20	7.04	80.02	0.26	0.00	0.74
FR	64	1.22	0.42	9.45	387.58	0.00	0.00	1.00
GE	2	5.89	3.05	8.87	1.00	1.00	0.00	0.00
GH	2	5.95	5.37	7.00	2.96	0.93	0.00	0.00
GR	20	13.85	11.51	17.61	360.71	0.46	0.00	0.53
GT	4	5.16	3.41	12.32	4.06	0.00	1.00	0.00
HR	22	6.11	5.13	7.05	26.76	0.77	0.00	0.23
HU	34	8.01	4.55	6.39	32.93	0.39	0.48	0.08
ID	56	4.77	2.64	8.16	213.17	0.04	0.28	0.67
IE	10	6.92	6.15	17.03	71.34	0.00	0.00	1.00
IL	7	2.44	1.63	8.88	13.95	0.43	0.57	0.00
IM	1	3.37	0.69	20.03	0.40	1.00	0.00	0.00
IS	2	5.44	3.73	12.77	4.90	0.24	0.20	0.55
IT	124	4.48	3.73	8.04	968.05	0.00	0.02	0.98
JM	8	7.53	6.23	10.85	4.18	0.00	0.87	0.08
JO	1	4.36	4.11	3.45	0.75	1.00	0.00	0.00
LB	21	4.92	4.03	6.64	21.75	0.06	0.87	0.08
LK	5	6.18	3.86	7.72	11.90	0.00	0.80	0.20
LT	15	4.35	2.93	5.55	20.09	0.98	0.00	0.02
LU	3	1.17	0.27	12.91	9.95	0.00	0.00	1.00
LV	5	4.38	3.41	6.06	9.94	0.98	0.00	0.02
ME	2	9.05	8.60	5.71	1.26	1.00	0.00	0.00
MK	2	5.50	5.22	2.19	0.50	0.81	0.00	0.19
MX	39	3.64	1.53	12.29	282.26	0.00	0.16	0.81
MY	45	3.26	0.23	8.20	208.83	0.04	0.00	0.96
NG	9	12.94	2.85	8.10	4.95	0.61	0.00	0.39
NL	28	0.88	0.18	7.28	306.46	0.00	0.00	1.00
PA	12	3.65	2.00	13.46	10.86	0.00	0.48	0.47
PE	12	3.89	1.99	14.33	21.03	0.00	0.35	0.65
PH	65	4.96	1.05	12.62	88.05	0.01	0.26	0.71
PL	62	3.70	1.22	8.71	87.85	0.50	0.14	0.29
PT	21	8.30	7.40	4.89	83.04	0.06	0.00	0.94
QA	18	3.43	1.93	11.43	38.05	0.14	0.86	0.00
RO	21	5.71	2.04	7.67	26.71	0.68	0.06	0.26
RS	2	7.28	4.44	8.83	12.65	1.00	0.00	0.00
RU	34	6.29	0.72	9.13	82.00	0.83	0.00	0.17
SE	22	0.78	−0.86	7.03	204.61	0.17	0.00	0.82
SI	10	4.58	3.71	8.04	37.04	0.57	0.00	0.43
SK	25	2.44	1.64	7.79	19.35	0.50	0.00	0.42
SV	7	6.82	4.57	17.19	9.69	0.00	1.00	0.00
TR	50	5.93	1.43	7.71	129.75	0.09	0.29	0.58
TT	3	4.44	2.29	12.26	2.04	0.07	0.93	0.00
UA	15	10.00	9.08	5.74	40.68	0.81	0.00	0.18
UY	15	3.63	2.15	13.64	15.92	0.00	0.83	0.12
VE	14	12.07	10.46	9.93	48.45	0.00	0.71	0.29
ZA	24	4.42	0.00	13.54	203.47	0.00	0.06	0.94

Table 13: **Estimates of nested and separate DiDs.** For illustrative purposes, we estimate the two nested DiDs in the yield-specification of regression (R1) along with separate DiD specifications.

$$
\begin{aligned}
x_{i,t} =\ & \beta_1 \mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}} + \beta_2 \mathbb{1}_t^{\text{Event}} \times \mathbb{1}_{i,c}^{\text{Indirect}} \\
& + \beta_3 \mathbb{1}_i^{\text{Direct}} + \beta_4 \mathbb{1}_{i,c}^{\text{Indirect}} + \alpha + \varepsilon_{i,t} && \text{(Nested)}
\end{aligned}
$$

$$
x_{i,t} = \beta_1 \mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}} + \beta_3 \mathbb{1}_i^{\text{Direct}} + \alpha + \varepsilon_{i,t} \qquad\qquad \text{(Direct)}
$$

$$
x_{i,t} = \beta_2 \mathbb{1}_t^{\text{Event}} \times \mathbb{1}_{i,c}^{\text{Indirect}} + \beta_4 \mathbb{1}_{i,c}^{\text{Indirect}} + \alpha + \varepsilon_{i,t} \qquad\qquad \text{(Indirect)}
$$

The nested specification is estimated on all bonds. The Direct (Indirect) specifications use only the direct (indirect) treatment group and the control group. The table shows that the nested and separate specifications produce the coefficient estimates absent controls. (One reason we nest the specifications in the baseline is precisely to include such controls and estimate their coefficients on the whole sample of bonds, not just subsamples. The point here is just to illustrate that the nested specification is otherwise equivalent to the two DiDs. We therefore omit standard errors and R^2.)

	Δ yield (Nested)	Δ yield (Direct)	Δ yield (Indirect)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}}$	0.0581	0.0581	
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_{i,c}^{\text{Indirect}}$	0.0511		0.0511
$\mathbb{1}_i^{\text{Direct}}$	-0.0045	-0.0045	
$\mathbb{1}_{i,c}^{\text{Indirect}}$	-0.0035		-0.0035
Time trend	-0.0027	-0.0027	-0.0027
Observations	1,013,640	440,163	832,811

A   Data

A.1   Geography

Our sample includes the following issuers, assigned to regions as listed below:

- **Europe**: Austria, Belgium, Bosnia and Herzegovina, Bulgaria, Croatia, Cyprus, Czech Republic, Denmark, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Isle of Man, Italy, Latvia, Lithuania, Luxembourg, Montenegro, the Netherlands, North Macedonia, Norway, Poland, Portugal, Romania, Serbia, Slovakia, Slovenia, Spain, Sweden.

- **South America**: Bolivia, Brazil, Chile, Colombia, Ecuador, Paraguay, Peru, Uruguay, Venezuela.

- **Central America**: Barbados, Bahamas, Belize, Costa Rica, Dominican Republic, El Salvador, Grenada, Guatemala, Honduras, Jamaica, Mexico, Panama, Trinidad and Tobago.

- **Middle East**: Bahrain, Egypt, Israel, Jordan, Lebanon, Qatar, UAE.

- **Africa**: Côte d'Ivoire, Ghana, Nigeria, South Africa.

- **Central Asia**: Azerbaijan, Belarus, Georgia, Kazakhstan, Russia, Turkey, Ukraine.

- **East Asia**: Indonesia, Malaysia, Philippines, Sri Lanka.

A.2   Risk-Free Interest Rate Proxies

The credit spread at time t of bond i with yield $y_{i,t}$ maturity T, currency denomination f is defined as $cs_{i,t} := y_{i,t} - r_{t,T,f}$, where the yield and $r_{t,T,f}$ is the currency-specific risk-free rate from t to T. As $r_{t,T,f}$ is unobservable, we proxy for it with the rate on the appropriate (i.e. currency and maturity matched) swap rate.[24]

We match bonds to swap rate maturities of 1, 2, 3, 4, 5, 10, 15, 20, and 30 years. Where swap rates for the longer maturities are not available, we use the longest available maturity. Where no swap rate is available, we drop all bonds denominated in that currency from the sample. In Table SA.1 in the Supplementary Appendix, we list the different currencies in our sample along with the floating rate underlying the swap used to construct credit spreads.

B   Matching procedure

The matching procedure is as follows. For each (directly or indirectly) treated bond, we consider all untreated bonds that have (i) the same currency denomination as, (ii) maturity within one year of,

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[24]Both rates are (basically) risk-neutral expectations of the average spot rate over the maturity. The difference is the time weighting: The swap rate discounts longer maturities. If the discount rate is about constant (e.g., because maturities are short or spot rates low) then the two rates are about equal. Even if it is not, swap rate is still an effective proxy for the risk-free rate for our purposes, as it captures variation in currency-specific risk-free term-structures.

and (iii) a credit rating within two notations of the treated bond's. If the set is empty, we discard the treated bond. If it is not, we choose the bond with the closest pre-event credit spread to the treated bond as its control bond. (Note that we do not exclude matching multiple treated bond with the same control bonds, i.e. we "match with replacement.") In Table SA.2 and Table SA.3 of the Supplementary Appendix, we list the matched pairs for directly and indirectly treated bonds.

C   Selective Default

In our baseline model, we capture the "crime watch" effect of the ruling by assuming that making it unattractive to restructure one bond makes it unattractive to start a restructuring altogether. That abstracts from the "burglar alarm" effect stressed in the literature, which focuses on selective default.

Although that abstraction explains our empirical results, it is not a perfect description of reality: Selective defaults do happen in practice (Erce and Mallucci (2018)). To show that our results do not turn on an unrealistic assumption, we show that our mechanism does not turn on there being no selective default whatsoever. We do that in two ways. First, we show that if the haircuts in Section 4.2 are interpreted as expected, not realized, haircuts that come out of a risky version of the restructuring game in Section 2.1, then sometimes ostensible selective defaults do occur. The rest of our analysis, however, does not change. Second, we show that our main mechanism is robust to including strategic selective default with bond-specific default costs: Decreasing the haircut on one class makes default on the other more attractive/likely. The extension also provides a microfoundation for our baseline assumption of all-or-nothing default (albeit under a parametric assumption).

C.1   Expected Haircuts in Risky Restructurings

So far, we have assumed that restructuring is riskless. If the sovereign chooses to default it restructures its debt for sure. Combined with the assumption that it chooses to default on either all debt or none, this precludes selective default. In reality, however, (i) restructuring is risky and (ii) selective defaults do occur. Here we show how including uncertainty in the restructuring game in Section 2.1 captures both of these things but does not affect the main analysis. The only difference is one of interpretation. The haircuts in the baseline model should be interpreted as expected, not realized, haircuts.

In equation (1), the sovereign offers each class of bondholders the maximum haircut that would make them break even for sure. In practice, however, the sovereign would not know bond holders' belief π that litigation would be successful. To accommodate such uncertainty, we assume the sovereign treats π as a random variable and maximizes the expected haircut (for a fixed b): Using h to denote the

$\mathbb{Q}$-expected haircut[25] and $\hat{h}$ the offered haircut, we can write:

$$h := \mathbb{Q}[\text{accept}]\hat{h} = \mathbb{Q}[\hat{h} \leq b/\pi]\hat{h} = G(b/\hat{h})\hat{h}, \tag{24}$$

where G is the distribution function of π.

Supposing the first-order approach is valid here, the maximizer solves

$$G(b/\hat{h}) = g(b/\hat{h})b/\hat{h}, \tag{25}$$

where $g := G'$ is the density of π. In this case, the restructuring fails, so there is no observed haircut, with probability $1 - G(b/\hat{h})$, i.e. if $\pi > b/\hat{h}$ at the optimum. A "selective default" is observed if the condition holds for one class of debt and not the other.

We conclude with an example, which illustrates a case in which the first-order approach assumed above is indeed valid: Suppose $G(\pi) = \frac{(A+B\pi)^{1+\alpha}}{A+B}$ for $\alpha > 0$ and $0 \leq \pi \leq 1$. Thus equation (25) for the offered haircut reads

$$(A + Bb/\hat{h})^{1+\alpha} = (1 + \alpha)B(A + Bb/\hat{h})^{\alpha}b/\hat{h}. \tag{26}$$

In this case, $\hat{h} = \alpha bB/A$, the probability of acceptance $G(b/\hat{h}) = A^{1+\alpha}(1 + 1/\alpha)^{1+\alpha}/(A + B)$, and the expected haircut $h = G(b/\hat{h})\hat{h} = \alpha bA^{\alpha}(1 + 1/\alpha)^{1+\alpha}/(A + B)$. So increasing the bribe b increases the expected haircut, keeping the probability of acceptance constant.

C.2 Bond-specific Default Costs

So far, we have assumed that any choice to default/restructure results in a loss of the fraction of output c. But one reason selective default could arise in practice is that defaulting on one class of bonds brings separate costs from defaulting on another.

Here we suppose that defaulting on one class i causes a loss of a fraction c_i of output z and defaulting on the other i' causes a loss of $c_{i'}$ of the remaining output. That means that defaulting one class is relatively less costly to the sovereign if it defaults on the other as well, because it destroys the same fraction of a smaller amount: It destroys $c_{i'}(1 - c_i)z$ instead of $c_{i'}z$. The sovereign's payoff in this case

---

[25]With this interpretation, the assumption that $h_f > h_r$ says that f- are more likely than r-bonds to be restructured selectively. That lines up with Erce, Mallucci, and Picarelli's (2021) finding that sovereigns are more likely to default selectively on local- than foreign-law bonds.

is:

$$
\text{payoff} = \begin{cases} z - D_r - D_f & \text{if repay,} \\[2ex] (1 - c_r)z - (1 - h_r)D_r - D_f & \text{if default on } r\text{-bonds,} \\[2ex] (1 - c_f)z - D_r - (1 - h_f)D_f & \text{if default on } f\text{-bonds,} \\[2ex] (1 - c_r)(1 - c_f)z - (1 - h_r)D_r - (1 - h_f)D_f & \text{if default on both.} \end{cases} \tag{27}
$$

Observe that even if defaulting on class i' bonds selectively is unattractive, defaulting on them in concert with i bonds can be attractive, i.e. it can be that $c_{i'}z > h_{i'}D_{i'} > c_{i'}(1 - c_i)z$.

Now we define the threshold z^{**} to make a parametric assumption. Define z^{**} as the output at which the sovereign is indifferent between defaulting on all debt or none, so

$$
z^{**} - D_r - D_f = (1 - c_r)(1 - c_f)z^{**} - (1 - h_r)D_r - (1 - h_f)D_f \tag{28}
$$

or

$$
z^{**} = \frac{h_r D_r + h_f D_f}{1 - (1 - c_r)(1 - c_f)}. \tag{29}
$$

The parametric assumption is

$$
z^{**} - D_r - D_f \geq (1 - c_i)z^{**} - (1 - h_i)D_i - D_{i'} \tag{30}
$$

for $i \in \{r, f\}$. I.e., near z^{**}, the sovereign would prefer not to default selectively. The assumption above implies that the sovereign defaults on all debt or none, and z^{**} is the default threshold.

From equation (29) we see that default on *each* type of bond—indeed on all bonds—becomes more attractive as the haircut on *either* increases.

Thus the "crime watch" effect by which low haircuts on one bond benefit others arises even when selective default is allowed, whenever inequality (30) is satisfied. (As default costs and haircuts are likely to be random, depending on market and political conditions, it is enough for it to be satisfied with positive probability.)

The parametric assumption in equation (30) above says that it is attractive to default on both classes together, but neither individually. That might seem unintuitive or, even, pathological. But a little algebra suggests it is not. Substituting for z^{**} and re-writing, it reads

$$
\frac{c_i}{c_{i'}} > (1 - c_i)\frac{h_i D_i}{h_{i'} D_{i'}} \tag{31}
$$

for $i \in \{r, f\}$. That is satisfied, e.g., whenever the bonds are symmetric, $c_r = c_f, h_r = h_f$, and $D_r = D_r$. That provides some support for our assumption of all-or-nothing default in the baseline model.

40

D   Proofs

D.1   Proof of Proposition 1

The result is immediate from equation (8). □

D.2   Proof of Proposition 2

The result is immediate from equation (10). □

D.3   Proof of Proposition 3

To derive the result, we use the definition of yield, a linear approximation, and the chain rule:

1. By definition, the yield (on a zero-coupon bond) of type $i \in \{r, f\}$ is $y_i = -\frac{1}{T_i} \log p_i$.

2. Differentiating the price, we get

$$\mathrm{d}p_i = \mathrm{d}e^{-y_i T_i} = -e^{-y_i T_i} T_i \mathrm{d}y_i = -p_i T_i \mathrm{d}y_i. \tag{32}$$

3. Rearranging, approximating, and applying the chain rule, we get

$$\Delta y_i \approx -\frac{\Delta p_i}{p_i T_i} \approx -\frac{1}{p_i T_i} \frac{\partial p_i}{\partial h_r} \Delta h_r. \tag{33}$$

Using equations (7) and (9) for $\frac{\partial p_i}{\partial h_r}$, we have the system in the statement of the proposition. □

D.4   Proof of Proposition 4

We write $F_{h_r} \equiv \frac{\partial}{\partial h_r} F(z^*)$ and $F_{\bar{h}} \equiv \frac{\partial}{\partial \bar{h}} F(z^*)$ using $cz^* \equiv h_r D_r + h_f D_f \equiv \bar{h} D$, for $D := D_r + D_f$:

$$F_{h_r} = F'(z^*)\frac{D_r}{c}, \tag{34}$$

$$F_{\bar{h}} = F'(z^*)\frac{D}{c}. \tag{35}$$

Dividing one by the other gives the result, recalling that $\varphi = D_r/D$.

D.5   Proof of Prediction 1

To see the intuition behind the result, consider the two effects of the haircut h_i on the price p_i, viewing p_i as a function of both $F(z^*)$ and h_i and then applying the chain rule:[26]

---

[26]Note that, unlike above, the second partial derivative is taken holding $F(z^*)$ constant given here we are treating the default probability $F(z^*)$ as an argument of the price p_i.

$$\frac{dp_i}{dh_r} = \frac{\partial p_i}{\partial F(z^*)}\frac{dF(z^*)}{dh_r} + \frac{\partial p_i}{\partial h_r} \tag{36}$$

$$= -\underbrace{h_i\frac{dF(z^*)}{dh_r}}_{\text{indirect effect}} + \underbrace{\frac{\partial p_i}{\partial h_r}}_{\text{direct effect}} . \tag{37}$$

The direct effect matters for rigid debt but not for flexible debt ($\partial p_f/\partial h_r = 0$). But, as h_i enters the indirect effect multiplicatively and $h_f > h_r$, the indirect effect matters more for flexible debt than rigid debt. Indeed, if h_f is sufficiently large relative to h_r—or law spreads are sufficiently high per equation (18)—f-debt is more sensitive to h_r than r-debt is. This captures our empirical finding that a sovereign's law spreads widen after the ruling when its spreads are high to begin with (Table 7). $\square$

D.6 Proofs of Prediction 2(i) and Prediction 2(ii)

The results follow immediately from differentiating equations (8) and (10) for a fixed fraction of rigid debt $\varphi \equiv D_r/D$:

1. Part (i):
$$\left.\frac{\partial^2 p_r}{\partial D \partial h_r}\right|_{D_r>0} = -\frac{\varphi h_r}{c} - \frac{z^*}{D} < 0. \tag{38}$$

2. Part (ii):
$$\left.\frac{\partial^2 p_f}{\partial D \partial h_r}\right|_{D_r>0} = -\frac{\varphi h_f}{c} < 0. \tag{39}$$

$\square$

D.7 Proofs of Prediction 3(i) and Prediction 3(ii)

The results follow immediately from differentiating equations (8) and (10):

1. Part (i):
$$\left.\frac{\partial^2 p_r}{\partial D_r \partial h_r}\right|_{D_r>0} = -\frac{2h_r}{c} < 0. \tag{40}$$

2. Part (ii):
$$\left.\frac{\partial^2 p_f}{\partial D_r \partial h_r}\right|_{D_r>0} = -\frac{h_f}{c} < 0. \tag{41}$$

$\square$

E Details of Quantitative Analysis in Section 4.3 and Section 4.4

Here we describe omitted details from our quantitative analysis.

E.1 Data Selection

We begin with the full cross section of our sample of bonds the day before our baseline ruling (May 29, 2012) and we retain only bonds that are (i) issued under English or local law, (ii) not missing yield or credit spread data, and (iii) issued in US dollars, euros, or local currency.[27] We are left with sixteen countries for which we observe both local- and English-law bonds that satisfy all criteria.[28]

E.2 Yield Adjustments

We need to adjust the raw yields to account for our modeling assumptions, namely that risk-free rates are normalized to zero and that all bonds are denominated in the same currency:

- *Risk-free rate.* To undo the rate normalization, we can just replace yields in the model with credit spreads in the data (this follows immediately from multiplying both sides of equation (6) by e^{-r_f}).

- *Currency risk.* The model is written from the point of view of a US investor, that is, under the dollar risk-neutral measure.[29] We therefore need to adjust observed credit spreads on non-dollar bonds (see the "quanto adjustment" in Du and Schreger (2016)). The intuition is as follows: if the dollar is expected to appreciate against the currency of denomination (say, the euro) in the event of default, the dollar-denominated credit spread will exceed the observed euro-denominated credit spread even if the default probability and percentage haircut are equal for dollar- and euro-bonds. This is because, expressed in a common numéraire, the dollar haircut exceeds the euro haircut. As a result, dollar-denominated CDS on eurozone sovereigns tend to have higher premiums than euro-denominated ones, even though they share a common trigger event and recovery. Since euro-denominated CDS quotes tend to be reliable for most sovereigns, we adjust the credit spread on euro-denominated bonds by adding the difference between US dollar and euro denominated CDS premiums. For emerging markets sovereigns, however, local-currency CDS quotes are often unreliable (Du and Schreger, 2016). We therefore use the dollar-denominated five-year CDS premium as a proxy for the currency-adjusted credit spread in local-currency, local-law bonds.[30]

For each country, we define the yield variable y_i as the average currency-adjusted credit spread on its English-/local-law bonds for $i = r/i = f$.

---

[27]We discard the few bonds issued in Japanese yen, Swiss francs, or British pounds to simplify the credit spread currency adjustment below.

[28]Those countries are AT, BE, CZ, DK, ES, FI, HR, HU, MY, PH, PL, RO, RU, SE, SK, and TR.

[29]I.e. the $\mathbb{Q}$ measure in the model should be interpreted as the one that makes asset prices discounted by the US dollar-denominated money market account martingales.

[30]While sovereign CDS in emerging markets are not triggered by default on local-currency, local-law bonds, using the CDS premium as a proxy for currency adjusted credit spreads is consistent with our model, in which the default event coincides between local- and foreign-law bonds.

E.3 Variables Definitions and Values

We use the following variables to solve the system in Proposition 3 and to compute the elasticity $\mathscr{E}$ in Proposition 4.

- Yields: $y_r = 2.55\%$ and $y_f = 2.47\%$ from equal-weighted average of (currency-adjusted) English- and local-law spreads across countries.

- Maturity: $T_r = T_f = 5$ from assuming a flat term structure to match the maturity of the most reliable CDS quotes. (For most countries, bonds in this sample have a similar average maturity of about six years anyway).

- $\mathbb{Q}$-default probability: $F(z^*) = 1 - e^{-\frac{\xi T}{1-R}} = 18.7\%$, per the "triangle method" (Hébert and Schreger (2017, Appendix A.1). Here ξ is T-year CDS premium and R the recovery value from the Markit CDS data. We use $T = 5$ years because the five-year premiums are deemed most reliable (Hébert and Schreger (2017)). We compute the five-year default probability for each of the countries involved in this exercise and take an equal-weighted average.

- Prices p_i: $p_i = e^{-y_i T_i}$ by definition.

- Haircuts: $h_i = (1 - p_i)/F(z^*)$ from the model (equation (6)).

- Fraction of English-law bonds: $\varphi = 38.1\%$, the equal-weighted cross-country average.

- Average haircut: $\bar{h} = \varphi h_r + (1 - \varphi)h_f$ by definition.

- Yield changes: $\Delta y_r = 5.8$ bps and $\Delta y_f = 4.25$ bps from Table 6 (these are the estimates from the matched regression which is more representative of the countries involved in this restricted sample).

- The sensitivity of default to h_r: $F_{h_r} = 0.87$, from solving the system in Proposition 3.

E.4 Variable Values for Argentina Policy Counterfactual

For the policy counterfactual in Section 4.4, we use data from Argentina the day before our ruling to line up be consistent with the other variables. The default costs, taken from Hébert and Schreger (2017), are estimated over a longer period that includes that date.

- $\mathbb{Q}$-default probability: $F(z^*) = 57\%$ applying the triangle method described above for five-year dollar-denominated CDS on Argentina.

- Haircut: $\bar{h} = 91\%$ (based on $F(z^*)$ and Argentine credit spreads on dollar-denominated five-year bonds under local and New York law. For this exercise, we are careful to derive credit spreads from bonds that are not in default at the time of our baseline ruling).

- Total default cost: $cz^* = 27.8$ billion USD from Hébert and Schreger (2017, Table 3). They describe this number as the cost of the $\mathbb{Q}$-default probability going from zero to one, which arguably corresponds to the cost at our threshold z^*.

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SOVEREIGN BOND RESTRUCTURING: SUPPLEMENTARY APPENDIX (FOR ONLINE PUBLICATION ONLY)

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August 25, 2022

Table SA.1: **Swap rates used in credit spread adjustment.** We list the different currencies in our sample along with the floating rate underlying the swap used to construct credit spreads (and the Bloomberg ticker for the swap rate).

Currency	Swap rate
AUD Australian dollar	Swap vs 6-month Bank Bill rate (ADSW)
BGN Bulgarian lev	Swap rate unavailable
BRL Brazilian real	Swap vs one-day Brazil Cetip Interbank Deposit rate (BCSW)
CAD Canadian dollar	Swap vs 3-month CDOR (CDSW)
CHF Swiss franc	Swap vs SARON (SFSNT)
CLP Chilean peso	Swap vs interbank overnight rate CAMARA (CHSWP)
COP Colombian peso	Swap rate unavailable
CNY Chinese renminbi	Swap vs CNY 7-day repo rate (CCSWO)
CZK Czech koruna	Swap vs 6-month Prague IBOR (CKSW)
DKK Danish krone	Swap vs 6-month Copenhagen IBOR (DKSW)
DOP Dominican peso	Swap rate unavailable

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Swap rates (continued)

Currency	Swap rate
EGP Egyptian pound	Swap rate unavailable
EUR euro	Swap vs EONIA (EUSWE)
GBP British pound	Swap vs SONIA (BPSW)
GHS Ghana cedi	Swap rate unavailable
HRK Croatian kuna	Swap rate unavailable
HUF Hungarian forint	Swap vs 6-month Budapest IBOR (HFSW)
IDR Indonesian rupiah	Swap rate unavailable
ISK Icelandic krone	Swap rate unavailable
JPY Japanese yen	Swap vs 6-month ICE JPY LIBOR (JYSW)
MXN Mexican peso	Swap vs 28-day Interbank TIIE (MPSW)
MYR Malaysian ringgit	Swap vs 3-month Kuala Lumpur IBOR (MRSWQO)
NGN Nigerian naira	Swap rate unavailable
NOK Norwegian krone	Swap vs 6-month Norway IBOR (NKSW)
PEN Peruvian sol	Swap rate unavailable
PHP Philippines peso	Swap vs 3-month Philippine Interbank Reference Rate (PPSWO)
PLN Polish zloty	Swap vs 6-month Warsaw IBOR (PZSW)
RON Romanian leu	Swap vs 3-month Bucharest Interbank Offer/Bid Rates (RNSW)
RUB Russian ruble	Swap vs 3-month Moscow Prime Offered Rate (RRSWM)
SEK Swedish krona	Swap vs 3-month Stockholm IBOR (SKSW)
SGD Singapore dollar	Swap vs 6-month Association of Banks in Singapore Swap Offer Rate (SDSW)
TRY Turkish lira	Swap vs TRY Overnight Reference rate (TYSO)
UAH Ukranian Hryvnia	Swap rate unavailable
USD US dollar	Swap vs US Federal Funds Effective Rate (USSO)
UYU Uruguayan peso	Swap rate unavailable
ZAR South African rand	Swap vs 3-month Johannesburg Interbank Agreed Rate (SASW)

Table SA.2: **Matched pairs: Directly treated bonds.** We list 124 directly treated bond pairs below with bond characteristics for treated (English law, left) and control (New York law, right) bonds in each column. Yields and credit spreads are as of the day before the baseline ruling (May 29, 2012).

Country		Currency	Maturity		Yield		Credit spread		Rating	
AE	CO	USD	10/5/2015	2/15/2016	4.53	4.24	4.23	3.76	A	BBB+
AE	MX	USD	11/3/2014	3/3/2015	3.97	1.52	3.76	1.22	A	BBB+
AE	MX	USD	4/8/2019	3/19/2019	2.74	2.82	2.07	2.14	A	BBB+
AE	MX	USD	4/8/2014	1/15/2014	1.04	1.11	0.83	0.90	A	BBB+
AE	MX	USD	4/8/2014	1/15/2014	1.05	1.11	0.84	0.90	A	BBB+
AE	MX	USD	7/22/2014	3/3/2015	2.60	1.52	2.40	1.22	A	BBB+
AE	MX	USD	4/8/2019	3/19/2019	2.74	2.82	2.06	2.14	A	BBB+
AE	ZA	USD	10/5/2020	3/9/2020	6.27	3.77	3.43	0.93	A	BBB+
AE	ZA	USD	6/22/2021	5/30/2022	5.16	4.05	2.32	1.21	A	BBB+
AE	ZA	USD	5/2/2022	5/30/2022	6.13	4.05	3.29	1.21	A	BBB+
AE	ZA	USD	5/2/2017	6/23/2017	4.46	3.55	3.78	2.87	A	BBB+
BB	PA	USD	8/4/2022	1/16/2023	6.74	5.03	3.90	2.18	BBB	BBB
BB	UY	USD	12/5/2035	3/21/2036	7.53	4.94	5.36	2.78	BBB	BB+
BB	ZA	USD	12/15/2021	5/30/2022	6.81	4.05	3.97	1.21	BBB	BBB+
BH	CO	USD	6/30/2015	2/15/2016	4.79	4.24	4.49	3.76	BBB	BBB+
BH	MX	USD	3/18/2013	2/17/2014	2.98	1.18	2.81	0.98	BBB	BBB+
BH	PA	USD	6/17/2014	3/15/2015	2.80	1.68	2.59	1.38	BBB	BBB
BH	ZA	USD	11/22/2018	5/27/2019	4.97	3.57	4.29	2.89	BBB	BBB+
BH	ZA	USD	3/31/2020	5/27/2019	5.78	3.57	2.94	2.89	BBB	BBB+
BH	ZA	USD	3/31/2020	5/27/2019	5.79	3.57	2.94	2.89	BBB	BBB+
BR	MX	EUR	6/26/2017	7/14/2017	2.50	2.55	1.81	1.86	BBB	BBB+
BY	DO	USD	1/26/2018	1/23/2018	11.11	5.39	10.43	4.71	B	B
BY	LK	USD	8/3/2015	1/22/2015	10.80	5.29	10.50	4.99	B	B+
ES	MX	USD	5/9/2013	2/17/2014	3.01	1.18	2.83	0.98	BBB+	BBB+
ES	MX	USD	5/9/2013	2/17/2014	2.46	1.18	2.29	0.98	BBB+	BBB+
ES	PA	USD	5/5/2014	3/15/2015	4.44	1.68	4.23	1.38	BBB+	BBB

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Matched pairs: Directly treated bonds (continued)

Country		Currency	Maturity		Yield		Credit spread		Rating	
GE	LK	USD	4/12/2021	10/4/2020	5.82	6.40	2.97	3.55	B+	B+
GE	LK	USD	4/12/2021	10/4/2020	5.85	6.40	3.01	3.55	B+	B+
HR	PA	USD	12/3/2022	1/16/2023	7.53	5.03	4.69	2.18	BBB-	BBB
HR	PA	USD	12/3/2022	1/16/2023	7.52	5.03	4.68	2.18	BBB-	BBB
HR	PE	EUR	4/15/2014	10/14/2014	2.88	1.82	2.59	1.53	BBB-	BBB-
HR	UY	EUR	9/7/2018	6/28/2019	6.32	4.30	5.64	3.61	BBB-	BB+
HR	UY	EUR	5/6/2015	1/19/2016	4.68	2.38	4.30	1.86	BBB-	BB+
HR	ZA	USD	5/11/2019	5/27/2019	7.49	3.57	6.81	2.89	BBB-	BBB+
HR	ZA	USD	2/7/2021	3/9/2020	7.50	3.77	4.66	0.93	BBB-	BBB+
HR	ZA	USD	2/7/2021	3/9/2020	7.57	3.77	4.72	0.93	BBB-	BBB+
HR	ZA	USD	3/4/2019	5/27/2019	7.04	3.57	6.36	2.89	BBB-	BBB+
HR	ZA	USD	5/11/2019	5/27/2019	7.50	3.57	6.82	2.89	BBB-	BBB+
HR	ZA	USD	3/4/2019	5/27/2019	7.01	3.57	6.33	2.89	BBB-	BBB+
HU	MX	EUR	6/1/2017	5/8/2017	7.75	2.63	7.06	1.94	BBB-	BBB+
HU	MX	EUR	4/2/2017	5/8/2017	8.00	2.63	7.31	1.94	BBB-	BBB+
HU	MX	EUR	2/6/2013	4/8/2013	5.59	1.28	5.34	1.03	BBB-	BBB+
HU	PE	EUR	1/29/2014	10/14/2014	7.05	1.82	6.76	1.53	BBB-	BBB-
HU	UY	EUR	4/7/2016	1/19/2016	7.09	2.38	6.57	1.86	BBB-	BB+
HU	UY	EUR	11/6/2018	6/28/2019	8.27	4.30	7.58	3.61	BBB-	BB+
HU	UY	EUR	11/5/2019	6/28/2019	8.33	4.30	7.64	3.61	BBB-	BB+
ID	PA	USD	4/23/2014	3/15/2015	2.87	1.68	2.66	1.38	BB+	BBB
ID	PA	USD	4/23/2014	3/15/2015	2.73	1.68	2.53	1.38	BB+	BBB
ID	PE	USD	11/21/2018	3/30/2019	4.08	2.74	3.40	2.06	BB+	BBB-
ID	PE	USD	11/21/2018	3/30/2019	4.09	2.74	3.41	2.06	BB+	BBB-
IS	ZA	USD	4/6/2017	6/23/2017	4.82	3.55	4.14	2.87	BBB-	BBB+
JO	LK	USD	11/12/2015	1/22/2015	4.38	5.29	4.08	4.99	BB	B+
LB	SV	USD	3/9/2020	12/1/2019	5.74	5.79	2.89	2.95	B	BB-
LT	CO	USD	3/10/2016	2/15/2016	3.75	4.24	3.28	3.76	BBB	BBB+
LT	CO	USD	3/10/2016	2/15/2016	3.80	4.24	3.32	3.76	BBB	BBB+

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Matched pairs: Directly treated bonds (continued)

Country		Currency	Maturity		Yield		Credit spread		Rating	
LT	MX	EUR	3/5/2013	4/8/2013	1.49	1.28	1.24	1.03	BBB	BBB+
LT	UY	EUR	7/10/2018	6/28/2019	4.63	4.30	3.94	3.61	BBB	BB+
LT	UY	EUR	10/6/2015	1/19/2016	2.44	2.38	2.07	1.86	BBB	BB+
LT	UY	EUR	7/4/2018	6/28/2019	4.61	4.30	3.92	3.61	BBB	BB+
LT	UY	EUR	2/10/2016	1/19/2016	3.41	2.38	2.89	1.86	BBB	BB+
LT	ZA	USD	2/9/2018	6/23/2017	4.44	3.55	3.76	2.87	BBB	BBB+
LT	ZA	USD	1/2/2022	5/30/2022	5.50	4.05	2.65	1.21	BBB	BBB+
LT	ZA	USD	1/2/2022	5/30/2022	5.45	4.05	2.60	1.21	BBB	BBB+
LT	ZA	USD	9/3/2021	5/30/2022	5.30	4.05	2.45	1.21	BBB	BBB+
LT	ZA	USD	11/2/2020	3/9/2020	5.15	3.77	2.31	0.93	BBB	BBB+
LT	ZA	USD	9/3/2021	5/30/2022	5.33	4.05	2.49	1.21	BBB	BBB+
LT	ZA	USD	11/2/2020	3/9/2020	5.14	3.77	2.30	0.93	BBB	BBB+
LT	ZA	USD	2/9/2018	6/23/2017	4.44	3.55	3.76	2.87	BBB	BBB+
LV	MX	EUR	5/3/2018	5/8/2017	3.78	2.63	3.10	1.94	BBB	BBB+
LV	PA	USD	4/6/2022	1/16/2023	5.72	5.03	2.88	2.18	BBB	BBB
LV	PE	EUR	4/2/2014	10/14/2014	2.06	1.82	1.78	1.53	BBB	BBB-
LV	ZA	USD	10/2/2018	5/27/2019	5.12	3.57	4.44	2.89	BBB	BBB+
LV	ZA	USD	10/2/2018	5/27/2019	5.14	3.57	4.46	2.89	BBB	BBB+
ME	UY	EUR	8/4/2016	1/19/2016	9.09	2.38	8.57	1.86	BB	BB+
ME	UY	EUR	2/9/2016	1/19/2016	9.19	2.38	8.67	1.86	BB	BB+
MK	UY	EUR	8/12/2015	1/19/2016	6.25	2.38	5.88	1.86	BB	BB+
MY	MX	USD	7/6/2016	1/15/2017	2.17	2.34	1.70	1.66	A-	BBB+
MY	MX	USD	7/6/2016	1/15/2017	2.17	2.34	1.69	1.66	A-	BBB+
MY	MX	USD	7/6/2021	3/15/2022	3.28	3.31	0.44	0.46	A-	BBB+
MY	PA	USD	6/4/2015	3/15/2015	1.90	1.68	1.61	1.38	A-	BBB
MY	PA	USD	6/4/2015	3/15/2015	1.79	1.68	1.51	1.38	A-	BBB
NG	LK	USD	1/28/2021	10/4/2020	5.55	6.40	2.71	3.55	B+	B+
PH	UY	USD	10/7/2016	5/17/2017	2.47	2.28	2.00	1.60	BB	BB+
PL	MX	EUR	3/29/2017	5/8/2017	2.82	2.63	2.13	1.94	A-	BBB+

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Matched pairs: Directly treated bonds (continued)

Country		Currency	Maturity		Yield		Credit spread		Rating	
PL	MX	EUR	8/6/2019	2/17/2020	3.30	3.33	2.61	1.96	A-	BBB+
PL	MX	EUR	3/10/2020	2/17/2020	3.52	3.33	2.14	1.96	A-	BBB+
PL	MX	EUR	2/5/2013	6/10/2013	0.97	1.17	0.72	0.92	A-	BBB+
PL	MX	EUR	3/2/2014	4/8/2013	1.65	1.28	1.36	1.03	A-	BBB+
PL	ZA	EUR	1/2/2016	4/5/2016	2.44	2.37	1.92	1.85	A-	BBB+
RO	SV	USD	7/2/2022	1/24/2023	6.64	6.43	3.80	3.59	BB+	BB-
RO	UY	EUR	6/3/2016	1/19/2016	4.42	2.38	3.90	1.86	BB+	BB+
RS	SV	USD	4/9/2023	1/24/2023	7.00	6.43	4.16	3.59	BB	BB-
RS	SV	USD	1/4/2024	1/24/2023	7.28	6.43	4.44	3.59	BB	BB-
RU	CO	USD	4/29/2015	11/16/2015	2.27	2.16	1.97	1.86	BBB	BBB+
RU	CO	USD	4/29/2015	11/16/2015	2.27	2.16	1.97	1.86	BBB	BBB+
RU	PA	USD	3/31/2030	4/1/2029	4.15	4.27	1.99	2.29	BBB	BBB
RU	PA	USD	3/31/2030	4/1/2029	4.17	4.27	2.01	2.29	BBB	BBB
RU	UY	USD	6/26/2028	7/15/2027	5.45	4.41	3.47	2.44	BBB	BB+
RU	ZA	USD	4/29/2020	3/9/2020	3.98	3.77	1.14	0.93	BBB	BBB+
RU	ZA	USD	4/4/2017	6/23/2017	3.15	3.55	2.47	2.87	BBB	BBB+
RU	ZA	USD	4/29/2020	3/9/2020	3.94	3.77	1.10	0.93	BBB	BBB+
RU	ZA	USD	4/4/2022	5/30/2022	4.23	4.05	1.39	1.21	BBB	BBB+
RU	ZA	USD	4/4/2017	6/23/2017	3.16	3.55	2.48	2.87	BBB	BBB+
RU	ZA	USD	4/4/2022	5/30/2022	4.23	4.05	1.39	1.21	BBB	BBB+
SK	MX	EUR	5/20/2014	6/10/2013	1.51	1.17	1.22	0.92	A	BBB+
SK	MX	EUR	3/5/2018	5/8/2017	2.78	2.63	2.09	1.94	A	BBB+
SK	ZA	USD	9/5/2023	1/17/2024	4.89	4.38	2.04	1.54	A	BBB+
SK	ZA	USD	9/5/2023	1/17/2024	4.91	4.38	2.06	1.54	A	BBB+
SK	ZA	EUR	9/5/2016	4/5/2016	1.74	2.37	1.22	1.85	A	BBB+
TR	UY	EUR	5/18/2020	6/28/2019	4.81	4.30	3.43	3.61	BB	BB+
TR	UY	EUR	3/1/2016	1/19/2016	3.66	2.38	3.14	1.86	BB	BB+
TR	UY	EUR	4/2/2019	6/28/2019	4.63	4.30	3.94	3.61	BB	BB+
UA	DO	USD	6/11/2013	1/23/2013	9.63	1.28	9.46	1.10	B+	B

Matched pairs: Directly treated bonds (continued)

Country		Currency	Maturity		Yield		Credit spread		Rating	
UA	DO	USD	5/6/2017	1/23/2018	9.78	5.39	9.20	4.71	B+	B
UA	DO	USD	5/6/2017	1/23/2018	9.81	5.39	9.23	4.71	B+	B
UA	DO	USD	6/11/2013	1/23/2013	9.90	1.28	9.73	1.10	B+	B
UA	DO	USD	9/11/2017	1/23/2018	9.84	5.39	9.25	4.71	B+	B
UA	DO	USD	9/11/2017	1/23/2018	9.83	5.39	9.24	4.71	B+	B
UA	DO	USD	2/11/2018	1/23/2018	9.58	5.39	8.91	4.71	B+	B
UA	DO	USD	11/9/2021	5/6/2021	10.09	6.61	7.24	3.77	B+	B
UA	DO	USD	2/11/2018	1/23/2018	9.61	5.39	8.99	4.71	B+	B
UA	DO	USD	11/9/2021	5/6/2021	9.82	6.61	6.98	3.77	B+	B
UA	LK	USD	2/6/2014	1/22/2015	6.36	5.29	6.17	4.99	B+	B+
UA	SV	USD	11/2/2022	1/24/2023	10.28	6.43	7.44	3.59	B+	BB-

Table SA.3: **Matched pairs: Indirectly treated bonds.** We list 83 indirectly treated bond pairs below with bond characteristics for treated (local-law/English-law issuer, left) and control (local-law/English-law non-issuer, right) bonds in each column. Yields and credit spreads are as of the day before the baseline ruling (May 29, 2012).

Country		Currency	Maturity		Yield		Credit spread		Rating	
AT	FR	EUR	1/15/2018	10/25/2017	1.55	1.48	0.87	0.80	AA+	AAA
AT	FR	EUR	1/26/2062	1/3/2062	2.93	3.25	1.21	1.53	AA+	AAA
AT	FR	EUR	9/15/2021	1/2/2022	2.16	2.16	0.78	0.78	AA+	AAA
AT	FR	EUR	4/20/2022	4/25/2022	2.26	2.34	0.88	0.97	AA+	AAA
AT	FR	EUR	10/20/2013	7/12/2013	0.21	0.21	-0.04	-0.04	AA+	AAA
AT	FR	EUR	9/15/2017	10/25/2017	1.43	1.48	0.75	0.80	AA+	AAA
AT	FR	EUR	3/15/2026	10/25/2025	2.60	2.88	0.91	1.19	AA+	AAA
AT	FR	EUR	12/1/2014	7/12/2015	0.81	0.72	0.44	0.35	AA+	AAA
AT	FR	EUR	2/20/2017	7/25/2016	1.25	1.10	0.56	0.58	AA+	AAA
AT	FR	EUR	11/22/2022	4/25/2022	2.35	2.34	0.97	0.97	AA+	AAA
AT	FR	EUR	9/15/2016	7/25/2016	1.09	1.10	0.57	0.58	AA+	AAA
AT	FR	EUR	1/15/2014	4/25/2014	0.36	0.33	0.08	0.04	AA+	AAA

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Matched pairs: Indirectly treated bonds (continued)

Country		Currency	Maturity		Yield		Credit spread		Rating	
AT	LU	EUR	10/20/2014	12/4/2013	0.42	0.42	0.14	0.14	AA+	AAA
AT	LU	EUR	3/1/2020	5/18/2020	1.83	1.65	0.45	0.28	AA+	AAA
AT	NL	EUR	3/15/2037	3/4/2038	2.85	2.17	1.11	0.45	AA+	AAA
AT	NL	EUR	7/15/2014	7/15/2015	0.38	0.50	0.09	0.13	AA+	AAA
AT	NL	EUR	7/15/2027	1/15/2028	2.67	2.12	0.98	0.42	AA+	AAA
BE	DE	EUR	10/11/2031	1/4/2031	3.32	1.99	1.58	0.25	AA+	AAA
BE	FR	EUR	4/5/2014	9/25/2014	0.43	0.44	0.15	0.16	AA+	AAA
BE	FR	EUR	4/12/2019	10/25/2019	3.04	2.04	2.35	1.35	AA+	AAA
BE	FR	EUR	6/28/2017	4/25/2018	2.14	1.63	1.45	0.94	AA+	AAA
BE	FR	EUR	1/12/2013	7/12/2013	0.22	0.21	-0.04	-0.04	AA+	AAA
BE	FR	EUR	5/28/2021	4/25/2022	3.25	2.51	1.87	1.13	AA+	AAA
BE	FR	EUR	3/28/2019	10/25/2019	2.55	2.04	1.86	1.35	AA+	AAA
BE	FR	EUR	3/28/2015	2/25/2016	1.18	0.94	0.81	0.43	AA+	AAA
BE	FR	EUR	7/7/2013	4/25/2013	0.15	0.14	-0.10	-0.12	AA+	AAA
BE	FR	EUR	8/9/2013	7/12/2013	0.20	0.21	-0.05	-0.04	AA+	AAA
BE	FR	EUR	2/2/2014	4/25/2014	0.32	0.33	0.04	0.04	AA+	AAA
BE	FR	EUR	3/28/2017	10/25/2017	2.01	1.48	1.32	0.80	AA+	AAA
BE	FR	EUR	3/28/2018	10/25/2018	2.32	1.77	1.64	1.08	AA+	AAA
BE	FR	EUR	3/28/2015	2/25/2016	1.21	0.94	0.83	0.43	AA+	AAA
BE	FR	EUR	4/9/2032	10/25/2032	2.48	3.16	0.74	1.42	AA+	AAA
BE	FR	EUR	4/1/2022	4/25/2022	2.89	2.51	1.51	1.13	AA+	AAA
BE	FR	EUR	4/8/2013	4/25/2013	0.14	0.14	-0.11	-0.12	AA+	AAA
BE	FR	EUR	9/28/2014	7/12/2015	0.92	0.72	0.64	0.35	AA+	AAA
BE	FR	EUR	9/12/2041	4/25/2041	3.50	3.41	1.78	1.69	AA+	AAA
BE	FR	EUR	3/28/2014	10/25/2014	0.72	0.46	0.43	0.17	AA+	AAA
BE	FR	EUR	9/28/2017	4/25/2018	2.21	1.63	1.52	0.94	AA+	AAA
BE	FR	EUR	3/28/2035	4/25/2035	3.60	3.18	1.86	1.44	AA+	AAA
BE	FR	EUR	3/28/2041	4/25/2041	3.62	3.41	1.90	1.69	AA+	AAA
BE	FR	EUR	9/28/2022	4/25/2023	3.20	2.56	1.82	1.18	AA+	AAA

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Matched pairs: Indirectly treated bonds (continued)

Country		Currency	Maturity		Yield		Credit spread		Rating	
BE	FR	EUR	2/15/2016	10/25/2016	1.39	1.16	0.87	0.64	AA+	AAA
BE	FR	EUR	4/3/2020	10/25/2019	3.23	2.04	1.86	1.35	AA+	AAA
BE	FR	EUR	12/24/2012	10/25/2013	0.25	0.25	0.00	0.00	AA+	AAA
BE	FR	EUR	9/28/2021	4/25/2022	3.04	2.51	1.66	1.13	AA+	AAA
BE	FR	EUR	3/11/2013	7/12/2013	0.21	0.21	-0.04	-0.04	AA+	AAA
BE	FR	EUR	3/28/2016	10/25/2016	1.57	1.16	1.05	0.64	AA+	AAA
BE	FR	EUR	3/28/2032	10/25/2032	3.62	3.16	1.88	1.42	AA+	AAA
BE	FR	EUR	5/23/2022	4/25/2023	3.40	2.56	2.03	1.18	AA+	AAA
BE	FR	EUR	9/28/2019	10/25/2019	2.73	2.04	2.05	1.35	AA+	AAA
BE	FR	EUR	3/28/2022	4/25/2022	3.07	2.51	1.69	1.13	AA+	AAA
BE	FR	EUR	9/28/2016	4/25/2017	1.80	1.34	1.28	0.65	AA+	AAA
BE	FR	EUR	9/28/2013	9/25/2014	0.58	0.44	0.32	0.16	AA+	AAA
BE	FR	EUR	11/10/2031	10/25/2032	3.30	3.16	1.56	1.42	AA+	AAA
BE	FR	EUR	9/28/2015	7/25/2016	1.36	1.10	0.99	0.58	AA+	AAA
BE	LU	EUR	3/28/2013	12/4/2013	0.41	0.42	0.16	0.14	AA+	AAA
BE	LU	EUR	6/4/2014	12/4/2013	0.41	0.42	0.12	0.14	AA+	AAA
BE	LU	EUR	2/3/2014	12/4/2013	0.38	0.42	0.09	0.14	AA+	AAA
BE	NL	EUR	4/6/2028	1/15/2028	3.40	2.12	1.71	0.42	AA+	AAA
BE	NL	EUR	5/1/2014	7/15/2014	0.28	0.28	-0.01	-0.00	AA+	AAA
BE	NL	EUR	3/28/2028	1/15/2028	3.48	2.12	1.78	0.42	AA+	AAA
FI	DE	EUR	3/3/2015	10/9/2015	0.16	0.14	-0.21	-0.23	AAA	AAA
FI	FR	EUR	4/15/2017	1/7/2017	0.79	0.81	0.10	0.13	AAA	AAA
FI	FR	EUR	4/10/2025	10/25/2025	2.02	2.88	0.33	1.19	AAA	AAA
FI	FR	EUR	4/15/2016	1/2/2017	0.47	0.63	-0.05	-0.06	AAA	AAA
FI	FR	EUR	4/4/2019	1/2/2019	1.33	1.32	0.65	0.63	AAA	AAA
FI	FR	EUR	7/4/2013	4/5/2013	0.08	0.07	-0.17	-0.18	AAA	AAA
FI	NL	EUR	7/4/2028	1/15/2028	2.18	2.12	0.48	0.42	AAA	AAA
FI	NL	EUR	4/15/2020	7/15/2020	1.49	1.51	0.11	0.14	AAA	AAA
FI	NL	EUR	7/4/2015	7/15/2014	0.35	0.28	-0.02	-0.00	AAA	AAA

Matched pairs: Indirectly treated bonds (continued)

Country		Currency	Maturity		Yield		Credit spread		Rating	
FI	NL	EUR	3/5/2018	3/7/2018	0.92	0.98	0.23	0.29	AAA	AAA
FI	NL	EUR	4/15/2021	7/15/2021	1.64	1.64	0.26	0.26	AAA	AAA
IT	PA	USD	8/9/2017	6/15/2018	5.09	3.42	4.41	2.74	BBB+	BBB
SI	FR	EUR	6/3/2028	4/25/2029	5.85	3.12	4.15	1.42	AA	AAA
SI	FR	EUR	10/3/2019	10/25/2019	4.24	2.04	3.55	1.35	AA	AAA
SI	FR	EUR	9/9/2024	10/25/2023	5.53	2.65	4.15	1.27	AA	AAA
SI	FR	EUR	6/2/2019	10/25/2019	4.84	2.04	4.15	1.35	AA	AAA
SI	FR	EUR	1/10/2022	4/25/2022	5.49	2.51	4.11	1.13	AA	AAA
SI	FR	EUR	2/4/2014	10/25/2014	2.36	0.46	2.07	0.17	AA	AAA
SI	FR	EUR	2/1/2022	4/25/2022	5.19	2.51	3.81	1.13	AA	AAA
SI	FR	EUR	2/17/2016	10/25/2016	3.57	1.16	3.05	0.64	AA	AAA
SI	FR	EUR	6/1/2022	4/25/2023	5.28	2.56	3.90	1.18	AA	AAA
SI	FR	EUR	5/3/2016	4/25/2017	2.64	1.34	2.12	0.65	AA	AAA