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 investigate whether sovereign bondholders benefit from committing not to restructure their debt. To do so, we use a court ruling that made one class of bonds easier to restructure. We find that, relative to a control group, not only did that class depreciate, so did other classes. We rationalize these findings with a model in which bondholders benefit from disciplining a sovereign with a willingness-to-pay problem.

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1 Introduction

Sovereign default is on the rise. Argentina, Ecuador, Lebanon, and Zambia have defaulted. Other emerging-markets countries are pleading for debt relief.[1] Some defaults are hard defaults in which creditors get nothing. But most defaults are soft/partial defaults in which creditors take a haircut but still get something. For bonds, such defaults typically constitute debt restructuring via a distressed exchange offer.

There is a longstanding debate about whether the ability to perform such a restructuring is good or bad for a sovereign's creditors. If restructuring is the sovereign's last-ditch alternative to hard default, creditors could get more in a restructuring than they would have otherwise (e.g., Krueger (2002) and White (2002)). If, in contrast, restructuring is a sovereign's opportunistic alternative to full repayment, creditors would get less in a restructuring than they would have otherwise (e.g., Shleifer (2003)).

A more recent literature adds another dimension to the debate. It points out that not all of a sovereign's creditors are created equal (Bolton and Jeanne (2007, 2009)). Different classes of bonds convey different rights via different non-price terms such as governing law and covenants. The ability to restructure one class of debt could be good or bad for other classes. If it reduces its total debt burden, the sovereign could be less likely to default on them. If it reduces discipline, the sovereign could be more likely to default on them.

Given these countervailing arguments, the questions remain empirical: (i) Does the ability to restructure one class of bonds benefit *that* class? (ii) Does it benefit *other* classes too? (iii) Does it benefit one class more than others?

Addressing these questions empirically is challenging. A few papers make progress on the first question by comparing different bonds with different governing law or covenants under the assumption that they are otherwise identical after matching on observables.[2] But no other paper, to our knowledge, addresses our other questions, which rely on comparing not only the terms of an individual bond, but also those of other bonds issued by the same country.

To confront these challenges, we exploit a landmark ruling by the English High Court (see Section 2.2). It made it easier to restructure all English-law bonds, allowing us to compare the same bonds on the trading days before and after it, thus obviating

[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, 2010), and "Emerging economies plead for more ambitious debt relief programmes" (*Financial Times*, October 11, 2020).

[2] 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.

the need for matching. The ruling pertained to bond restructuring specifically, but was not directly associated with the sovereign-debt market (the defendant was a firm, not a country). Still, it mattered for hundreds of outstanding sovereign bonds as many countries issue bonds under English law. Other countries do not. Their bonds provide us with a control group for a difference-in-differences (DiD) analysis.

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 bonds in our control group described above to those in a treatment group of bonds issued under English law. We find that treated bond yields increase by 14 bps relative to control bonds, suggesting that making it easier to restructure one class of debt 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 bonds in our control group to those in 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 increase by 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.

A naive answer to our third question, on whether some bonds benefit—or suffer—more than others, comes from comparing the results so far: On average across countries, yields on English-law bonds increase by slightly more than those on non-English-law bonds issued by English-law issuers. To test whether some countries' bonds respond more than others', we turn to a triple-difference regression: We compare within-country differences in yields around the ruling for directly and indirectly treated bonds between countries with higher versus lower "law spreads," defined as the country-level difference between the yields on English- and local-law bonds. We find that the spillover effect is stronger in countries with larger law spreads: A 1% increase in a country's law spread indicates a 2 bps increase in local-law yields relative to English-law ones.

Overall, our results are in line with Shleifer's (2003) view that a sovereign's ability to restructure is bad for creditors: The commitment *not* to restructure one class of bonds is valuable for both (i) that class and (ii) other classes especially (iii) those of countries where they trade at relatively high yields.

The results stand up to additional tests. Notably, we omit the comparison with the control group from our DiD regressions, running event studies instead. The signs of estimates stay the same, suggesting they are not driven by changes in control bonds, but are true "treatment effects." Several other robustness checks support this interpretation (Section 6). However, they necessarily leave the question of external validity open. To check that our results are not context specific, we repeat our baseline analysis using a similar (though arguably more imperfect) experiment: the Argentine restructuring saga (Section 7). We find analogous results.

We develop a simple model of a sovereign restructuring to rationalize our findings, in which restructuring, being a form of partial default, is captured by the haircuts it imposes on creditors. The model is based on two key ingredients. The first is the willingness-to-pay problem, which is common in the literature: Costs of default (e.g., trade sanctions) are the only thing that deters default (Eaton and Gersovitz (1981)). The second is heterogeneity in the ease of restructuring, something that appears in only a few papers (Bolton and Jeanne (2007, 2009) and Carletti et al. (2020)): Creditors differ in the haircut suffered in default, with higher haircuts, i.e. lower repayments, to holders of easier-to-restructure bonds. Specifically, there is (i) hard-to-restructure "rigid debt" associated with a low haircut h_r and (ii) easier-to-restructure "flexible debt" associated with a higher haircut $h_f > h_r$. For example, flexible debt could correspond to domestic law debt, which a sovereign can restructure unilaterally by changing the law (as Greece did in 2012; see Zettelmeyer, Trebesch, and Gulati (2014)).

The model has close counterparts in our empirical environment: Rigid debt corresponds to English-law bonds, flexible to domestic, and the High Court ruling to an increase in the haircut on rigid debt h_r, as it made English-law 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 debt in default, increasing its yield. (ii) There is an effect that works via default probabilities: It encourages strategic default, increasing the yield on *both* types of debt. The model captures all three of 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)). Per our third, "law spreads" widen if and only if they are wide to begin with, or, equivalently, if h_f is large relative to h_r. Intuitively, for $h_f > h_r$ flexible debt is more sensitive to the probability of default—its low payoff in default makes avoiding default that much more valuable—so (ii) matters more for f- than r-debt.

The model abstracts from the negative roles of rigid debt, which could, in theory, be a straightjacket that stifles growth and ultimately reduces expected repayments or be a device for opportunistic dilution that induces selective defaults and simply expropriates value from other bondholders. It focuses solely on the disciplining role of rigid debt: Low haircuts decrease the sovereign's payoff in default and hence mitigate the willingness-to-pay problem. But it captures our empirical findings when other theories are likely to imply the opposite: Given the High Court ruling made bonds easier to restructure, a straightjacket would imply a decrease in yields and a dilution device a positive spillover on other classes.

The model also abstracts from other bond terms, such as collective action clauses (CACs), a term that helps dispersed creditors coordinate and can thereby both (i) facilitate efficient restructuring and (ii) impede inefficient restructuring (see Section 2.1). Whereas the literature tends to ascribe the value of CACs to (i), our results, which underscore value of the commitment not to restructure, suggest that (ii) could be even more important.[3]

Such abstractions notwithstanding, the model is rich enough to generate a number of additional cross-sectional predictions to test: Both the direct and indirect effects should be stronger in countries with higher levels of debt and of rigid debt. Empirical tests support three of the four predictions and do not reject the other (the indirect effect is not significantly stronger or weaker in countries with more rigid debt), providing additional support for our model overall.

Finally, a proviso: Although our results suggest the commitment *not* to restructure debt is good for sovereign creditors, they say little about whether it is good for sovereign borrowers. It could be good, e.g., facilitating access to credit (Shleifer (2003)) or bad, e.g., leading to prolonged defaults.

2 Institutional Background and Data

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

2.1 Bond Restructuring: Exchange Offers and Consent Solicitations

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. For bonds, such partial defaults constitute debt restructuring via a

[3] See also Bond and Eraslan (2010) for an information-based model in which CACs can be good or bad for creditors.

distressed exchange offer: The sovereign offers bondholders new securities in exchange for their bonds.

Given bondholders are dispersed, distressed exchange offers 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 makes it 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 it worse off.[4] (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 increase the likelihood it can repay in full. In equations: If the sovereign repays its debt D with probability q_D and nothing otherwise, bondholders benefit collectively from taking a haircut H, which increases the repayment probability to $q_{D-H} > q_D$, as long as

$$q_D D < q_{D-H}(D - H). \tag{1}$$

But an individual bondholder still might not accept the haircut. This is because by holding out, it can free ride on others accepting haircuts. As a small bondholder with debt d has a negligible effect on the repayment probability, it never accepts a haircut h; indeed,

$$q_{D-H} d > q_{D-H}(d - h) \tag{2}$$

for all h. It prefers to benefit from the overall debt reduction without taking a haircut itself. As all creditors have an incentive to act this way, the whole restructuring can fall apart even though it would have made everyone better off.

To counter the hold-out problem, debtors frequently include so-called "consent solicitations" in exchange offers. When tendering creditors accept the offer, they agree ("consent") to changes in the terms of *all* bonds in the same class in exchange for an effective bribe ("solicitation"), which we denote by b. These terms can facilitate restructuring: By agreeing to haircuts on other bonds, tendering creditors effectively punish hold-outs. However, as the legality of such solicitations is uncertain (see below), there is some probability, which we denote by π, that hold-outs could get paid in full (possibly following litigation). Thus, as a small bondholder has a negligible effect on the repayment probability, it accepts a haircut h whenever

$$q_{D-H}(d - h) + b \geq \pi q_{D-H} d + (1 - \pi) q_{D-H}(d - h), \tag{3}$$

[4]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.

or $b/\pi \geq q_{D-H}h$. In words, haircuts h are larger when consent solicitations are more generous (b is larger) or holdouts are less likely to win in court (π smaller), two relationships we use off-the-shelf in the model below (see Section 4).

What kinds of consent solicitations (and associated haircuts) are feasible depends on the law. We focus on a High Court case in which creditors sued a debtor for using them to reduce interest payments year after year. Even more extreme cases occur. The Court saw a related case only months later in which hold-outs found themselves with each 1000€ in principal reduced to a cent.[5] Such restructurings are dubbed "coercive."

This is where the hold-in problem comes in: Consent solicitations can force bondholders into a prisoner's dilemma in which they tender even if restructuring is against their collective interest (cf. equation (3)). The sovereign is thus tempted to use consent solicitations simply to expropriate value from creditors.

Overall, consent solicitations are a double-edged sword. By making it easier to restructure, they mitigate the hold-out problem but can exacerbate the hold-in problem.

2.2 The Ruling: Azevedo v. Imcopa Importação, Exportação e Indústria de Óleos Ltda

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 solicitations and, thus, made it easier to restructure bonds in exchange offers.[6] 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*.[7] The newspaper also stresses that consent solicitations are ubiquitous but 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.[8] Imcopa embarked on a plan to reduce

[5] See Assenagon Asset Management SA v Irish Bank Resolution Corp Ltd (formerly Anglo Irish Bank Corp Ltd) [2012] EWHC 2090 (Ch) (27 July 2012)). Although this case resembles our baseline experiment, the Court surprised legal commentators and ruled the other way, a fact that provides some assurance that the ruling we study was not a foregone conclusion (see, e.g., Jones Day (2012)). (The case does not, however, provide an ideal natural experiment in itself as Mario Draghi's "Whatever it takes" speech caused substantial movements in sovereign bond markets just before the ruling.)

[6] See Azevedo v. Imcopa Importacao, Exportacao e Industria de Oleos Ltda [2012] EWHC 1849 (Comm) (30 May 2012)).

[7] See "The consent of the (bondholder) governed" (April 22, 2013).

[8] See, e.g., macrotrends.net.

its interest payments via a restructuring of its bonds, which were issued under English law. Over the next two years, it restructured its bonds four times. Each time, rather than making its semi-annual interest payment in full, it offered bondholders consent solicitations, inducing them to put off the interest they were owed for a small upfront payment. As predicted by the prisoner's dilemma described in Section 2.1, nearly all bondholders accepted.

But the fourth time Azevedo and Alvarez held out. Equation (3) would suggest 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 solicitations constituted illegal bribery.

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

2.3 Data Sources and Sample Construction

Bond data come from Dealogic DCM and Bloomberg's records of sovereign bond issues from 1980 to 2020. We collect data on bonds outstanding between May 2012 and October 2014, a sample period that includes both our baseline event (the High Court ruling described in Section 2.2) and our test of external validity (the Argentine restructuring saga described in Section 7.1.1), while avoiding the Greek restructuring in early 2012. We focus on bonds issued by countries with significant English- and NY-law borrowing and relevant control countries, e.g., in Europe and Latin America. We include only bonds with available data on governing law, our key variable of interest. Dealogic DCM 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 for one of the following two reasons. (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. (ii) The laws listed by Bloomberg and Dealogic are different. 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)).

We exclude bonds issued by publicly-owned companies or state/local governments. We also exclude Argentina since, as of 2012, it was still in litigation following its 2001 default and subsequent restructuring. We exclude bonds with missing ISIN, and collect daily yield data from Bloomberg. 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.

As our event window, we will look at the change in yields between May 29, and May 31. Since bonds are traded OTC in different time zones, this ensures that bonds traded in European, Middle Eastern, and Asian markets reflect the new information. On our event date, May 31, 2012, the sample contains 1,375 bonds, issued by 67 countries in 31 currencies. To curb the impact of outliers, we winsorize yield changes at 1% and 99%.

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 \$0.90bn; 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 2 shows statistics by country.

Figure 1 visualizes the countries in our sample and their English-law bonds outstanding.

GDP data come from the World Bank.

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), and we report our main empirical results (Section 3.3 and Section 3.4).

3.1 Estimation Strategy

To evaluate the association between the ability to restructure and bond prices, we estimate two models. The first, which we label (R1), nests two DiD specifications, each addressing one of our first two motivating questions. The second, which we label (R2), is a triple-difference specification, addressing our third question.

In each of the two DiD specifications nested in regression (R1), the first difference is before and after the ruling 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 Appendix D, we show empirically that absent controls the nested specification is equivalent to two separate DiD specifications.)

Model (R2) is a within-country triple-difference specification. The first difference is likewise before and after the ruling. The second difference is between a country's English-law bonds and the same country's non-English-law bonds. The third difference is between countries with higher versus lower "law spreads," i.e. the difference in yields between a country's English- and local-law bonds.

Formally, the two models are:

$$\begin{aligned}\Delta y_{i,t} = {} & \beta_1 \mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}} + \beta_2 \mathbb{1}_t^{\text{Event}} \times \mathbb{1}_{i,c}^{\text{Indirect}} \\ & + \beta_3 \mathbb{1}_i^{\text{Direct}} + \beta_4 \mathbb{1}_{i,c}^{\text{Indirect}} + \alpha_{f,t} + \alpha_{m,t} + \alpha_{r,t} + \varepsilon_{i,t}\end{aligned} \tag{R1}$$

and

$$\begin{aligned}\Delta y_{i,t} = {} & \gamma_1 \mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}} \times S_{c,t}^{\text{E}} + \gamma_2 \mathbb{1}_i^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}} \\ & + \gamma_3 \mathbb{1}_i^{\text{Direct}} \times S_{c,t}^{\text{E}} + \gamma_4 \mathbb{1}_i^{\text{Direct}} + \delta_{f,t} + \delta_{m,t} + \delta_{c,t} + \eta_{i,t}\,.\end{aligned} \tag{R2}$$

Above, the dependent variable is the change in the yield on bond i from date $t-2$ to date t. The subscripts c, r, m, and f denote, respectively, the issuing country, region, maturity (in bins of five years), and currency denomination. 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. $S_{c,t}^{\text{E}}$ denotes the average "law spread" between local-law bonds and English-law bonds for country c on date t. We compute this spread as the average difference in date-t yields between a country's English- and local-law bonds after controlling for currency and maturity.[9] The α's and δ's are fixed effects at the maturity- and currency-time levels in both regressions as well as at the region-time level in regression (R1) and at the country-time level in regression (R2).[10] The ε and η are error terms.

In regression (R1), the key coefficients of interest are β_1 and β_2. They measure the yield changes around the event date for, respectively, directly treated and indirectly treated bonds (viz. English-law bonds and non-English law bonds issued by countries with some outstanding English law bonds). In each case, the yield changes are measured relative to our control group (viz. bonds issued by countries with no English-law debt outstanding on the event date). We use fixed effects, α, at the maturity- and currency-time level and the region-time level to account for, respectively, shifts in risk-free yield curves across different currencies and differential shocks on the event date between, say, South American and European issuers. We cluster standard errors at the country-

[9] Specifically, we regress the panel of yields on time-currency and time-maturity fixed effects, with bond maturities assigned to two-year bins. We then average the residuals from this regression by date, country, and governing law.

[10] 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 C.

maturity level, with bond maturities assigned to five-year-bins.

In regression (R2), the key coefficient of interest is γ_1 on the triple-interaction term. It measures the relative yield changes around the event for directly and indirectly treated bonds in high vs. low law spread countries. In addition to the remaining interactions and fixed effects to control for daily maturity- and currency-specific variation, the specification includes country fixed effects, which constitute a key distinction from regression (R1), as it ensures that directly and indirectly treated bonds are compared within-country.[11]

3.2 Identifying Assumptions

The identifying assumption in our DiD estimation is parallel trends. That is, to interpret the estimates as being a causal result of the ruling, the expected yield change 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 fundamentally 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)). And 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 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 currency-, maturity-, and region-/country-time fixed effects in our regressions. These absorb variation on the event date common to a given maturity bin, currency denomination, or region/country. Second, we choose

[11] To focus on law spreads, we can include only countries with both English- and non-English-law bonds outstanding. Thus, unlike regression (R1), the regression does not have a control group; it compares two treatment effects.

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 currency, maturity, and region/country 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.

We provide some visual reassurance that the parallel trends assumption is likely to hold. Figure 3 plots the coefficient estimates for regression R1 with placebo event dates for the two weeks around the ruling. There is a marked jump up in the event window (labeled t and $t+1$). Arguably, the figure could be consistent with some anticipation effects (as if some news of the High Court decision leaked at $t-1$) and post-announcement drift (as if the news was still being impounded into prices at $t+2$). Both of these effects would bias against our finding a result in the event window. Besides, they are substantially smaller than the movements during the event window.

3.3 Do Sovereign Creditors Benefit from the Ability to Restructure?

Table 3 reports the results from estimating equation (R1), our DiD regression of English- and non-English-law-bond yield reactions of countries with outstanding English-law debt, each relative to non-English-law bonds of countries without English-law debt.

3.3.1 The Effect on English-law Bonds

After the ruling, the yields on English-law bonds increase by close to 14 bps relative to the control group of bonds issued by countries with no English-law debt outstanding ($\beta_1 = 0.1377$).

The result suggests that the ruling, which made bonds easier to restructure, harmed bondholders. Within the set-up in Section 2.1, the result suggests that bondholders care more about protection against hold-in problems than the risk of running into hold-out problems.

3.3.2 The Effect on the Other Bonds of English-law Issuing Countries

After the ruling, the yields 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 ($\beta_2 = 0.1343$).

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.

3.4 Which Bondholders Benefit More?

Table 4 reports the results from estimating equation (R2), our triple-difference regression estimating the relative treatment effects between directly and indirectly treated bonds as a function of the country's law spread.

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 local-law bonds: A 1% increase in a country's law spread indicates a 2 bps increase in local-law yields relative to English-law yields around the ruling ($\gamma_1 = -0.0237$).

This result suggests that spillovers from harder-to-restructure English-law bonds on other bonds are more pronounced in countries with larger law spreads. Whereas the indirect/spillover effect is about the same size as the direct effect on average ($\beta_2 \approx \beta_1$), 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, a fact we rationalize in the next section.[12]

4 Model

Here we develop a model of sovereign default/restructuring (Section 4.1), we use it to explain the empirical findings above (Section 4.2), and we derive further predictions (Section 4.3).

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 debt comes 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 $F(z) = z$.

The model has two key ingredients.

1. The sovereign has a willingness-to-pay problem.[13] It has the option to default strategically, but default destroys a fraction c of the output, i.e. the default cost is cz.

[12] Our findings here reify Berg, Reisinger, and Streitz's (2020) point that neglected spillovers can even flip the interpretation of DiD estimates.

[13] 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, an ability that is usually viewed as an important indicator of sustainability (p. 54).

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

In our empirical environment, English-law bonds correspond to rigid debt, non-English-law to flexible, and the High Court ruling, which allowed consent solicitations, to an increase in the haircut h_r on rigid debt since, in the language of Section 2.1, it increased the bribe b tendering creditors receive and decreased the probability π with which hold-out creditors receive full repayment.[14]

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 debt $i \in \{r, f\}$ (per the second ingredient). This can be seen from the following expression for the sovereign's payoff:

$$\text{payoff} = \begin{cases} z - D_r - D_f & \text{if repay} \\ z - cz - (1 - h_r)D_r - (1 - h_f)D_f & \text{if default.} \end{cases} \tag{4}$$

Observe that the sovereign either defaults on all debt or none—there is no selective/partial default.[15]

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

$$p_i = \mathbb{Q}[\text{repay}] + \mathbb{Q}[\text{default}](1 - h_i), \tag{5}$$

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

4.2 Model Results

The sovereign defaults if its payoff from defaulting exceeds its payoff from repayment, or, from equation (4), if

$$z - cz - (1 - h_r)D_r - (1 - h_f)D_f > z - D_r - D_f. \tag{6}$$

[14]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 can depend on the bonds' governing law.

[15]See Bolton and Jeanne (2007, 2009) and Carletti et al. (2020) for models of selective and partial default with heterogeneous debt.

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

$$z^* := \frac{h_r D_r + h_f D_f}{c}. \tag{7}$$

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.*

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. This holds almost by assumption, namely by our assumption that default is all or nothing. But it turns out that this seems to be a good assumption to explain our empirical results, notably those on spillovers. To demonstrate how, we turn to prices.

We can use the default threshold in equation (7) to re-write the bond price in equation (5) as

$$p_i = 1 - F(z^*) + F(z^*)(1 - h_i) \tag{8}$$

$$= 1 - F(z^*)h_i. \tag{9}$$

From here, our main results follow from comparative statics of p_r and p_f with respect to h_r, capturing how the prices of each type of debt respond to the High Court ruling.

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

$$\frac{\partial p_r}{\partial h_r} = -F'(z^*)\frac{\partial z^*}{\partial h_r}h_r - F(z^*) \tag{10}$$

$$= -\frac{D_r}{c}h_r - z^*, \tag{11}$$

having used that F is the uniform. This expression captures how an increase in the haircut h_r harms r-creditors in two ways. It has the direct effect (the second term) of decreasing what they get in the event of default. But it also has an indirect effect (the first term) of making default more likely because a higher haircut in default is attractive to the sovereign.

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

$$\frac{\partial p_f}{\partial h_r} = -F'(z^*)\frac{\partial z^*}{\partial h_r}h_f \tag{12}$$

$$= -\frac{D_r}{c}h_f, \tag{13}$$

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 debt on the price of flexible debt. It corresponds to the indirect effect above: It increases the default probability, making f-debt less likely to be repaid.

Equations (11) and (13) map to our first two main empirical findings (Section 3.3.1 and Section 3.3.2), which answer the first two of the three questions we started with. The first describes how the price of rigid debt responds to a change in its haircut:

Proposition 1. *The price of rigid debt decreases if its haircut increases, i.e.*

$$\left.\frac{\partial p_r}{\partial h_r}\right|_{D_r>0} < 0. \tag{14}$$

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

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

$$\left.\frac{\partial p_f}{\partial h_r}\right|_{D_r>0} < 0. \tag{15}$$

The results follow immediately from equations (11) and (13), respectively.

Our third and final main result is a characterization of when the sovereign's rigid debt responds more to changes in h_r and when flexible does, which corresponds to our third empirical result on the relative magnitudes of the two treatment effects (Section 3.4) and answers the third question we started with:

Proposition 3. *Increasing h_r widens law spreads, i.e.*

$$\frac{\partial}{\partial h_r}(p_r - p_f) > 0 \tag{16}$$

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

$$p_r - p_f > \frac{c(z^*)^2}{D_r}. \tag{17}$$

The result follows from subtracting equation (13) from equation (11) and substituting prices for haircuts using equation (9). 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—via both recovery values (the direct effect) and via the default probability (the indirect effect)—and only one on the former—via the default probability alone.

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:[16]

$$\frac{dp_i}{dh_r} = \frac{\partial p_i}{\partial F(z^*)}\frac{dF(z^*)}{dh_r} + \frac{\partial p_i}{\partial h_r} \tag{18}$$

$$= -\underbrace{h_i \frac{dF(z^*)}{dh_r}}_{\text{indirect effect}} + \underbrace{\frac{\partial p_i}{\partial h_r}}_{\text{direct effect}}. \tag{19}$$

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 as per equation (17)—f-debt is more sensitive to h_r than r-debt. This captures our empirical finding that a sovereign's law spreads widen after the ruling when its spreads are high to begin with (Table 4).

4.3 Additional Cross-sectional Predictions

The model generates a number of additional cross-sectional predictions to test, on the debt level, the proportion of rigid debt, and the default cost/riskiness, which we establish in this section.

4.3.1 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 debt, denoted by $\varphi := D_r/D$, constant):

Prediction 1. *(i) Increasing h_r decreases the price of rigid debt by more when D is higher:*

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

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

Likewise, (ii) increasing h_r decreases the price of flexible debt by more when D is higher:

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

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

4.3.2 Rigid Debt

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

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

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

Likewise, (ii) increasing h_r decreases the price of flexible debt by more when D_r is higher:

$$\left.\frac{\partial^2 p_f}{\partial D_r \partial h_r}\right|_{D_r>0} < 0. \tag{23}$$

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

5 Cross-sectional Heterogeneity

To test the additional predictions developed in Section 4.3, we run triple-difference regressions building on the DiD in the specification (R1) by interacting the "diff-in-diff" regressor with the appropriate additional variable X. Specifically, we estimate models of the form:

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

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 X. Below we let X be (measures of) total debt and rigid debt. We report all results in Table 5.

5.1 Total Debt

Letting X in equation (R3) be a measure of a country's total debt, we find that yields 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, $\kappa_1 = 0.0611$ and $\kappa_2 = 0.0781$; if it is its debt to GDP ratio, $\kappa_1 = 0.2430$ and $\kappa_2 = 0.1010$[17]).

These results confirm both statements (i) and (ii) of Prediction 1.

5.2 Rigid Debt

Letting X in equation (R3) be a measure a country's English-law debt, we find that yields rise by more on English-law bonds, but not on non-English-law bonds on the event date when a country has relatively more English-law debt (if X is English-law debt relative to GDP,[18] we find $\kappa_1 = 0.3795$; our estimate of the indirect effect is $\kappa_2 = -0.2945$, but it is is not statistically different from zero at conventional significance levels).

These results confirm statement (i) of Prediction 2. (They fail to confirm statement (ii), but do not provide significant evidence against it either.)

6 Robustness

We have found three main empirical results: In response to a court ruling weakening the rights of hold-out creditors in English-law bonds (i) English-law bonds depreciate relative to bonds in countries with no English-law debt, (ii) non-English-law bonds in countries with English-law debt depreciate relative to the same control group as in (i), and (iii) within-country, the indirect effect (ii) is larger than the direct effect (i) for countries where local-law bonds trade at high yields relative to English-law bonds (see Section 3). In this section, we address concerns about the robustness of these results.

6.1 Placebo Tests

One concern is that, statistical significance notwithstanding, our result are spurious. To address it, we conduct a data-driven test based on placebo events. Specifically, we assign the event dummy to each of the 650 days in our sample period, other than the day of the High Court ruling and the subsequent day. We then re-estimate regressions (R1) and (R2). We compare the signs and magnitudes of the re-estimated t-statistics

[17] 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).

[18] Like for total debt, we do not use the level of English-law debt directly because it is highly skewed. Unlike for total debt, we do not take its log, because it is often zero (making the log undefined), but just normalize by GDP.

of interest. We find that (i) holds on 2.31% of days with a t-statistic as large as that on the true event date, (ii) on 7.69% of them, and (iii) on 5.54% of them. Although these estimates suggest that the theoretical p-values we report could overstate the statistical significance, they imply that it is unlikely that any individual result would arise by chance. But our results do not matter only individually; our theory predicts that all three should hold jointly. There are *zero* placebo dates on which the results (i)–(iii) hold jointly with t-statistics as large as those on the true event date. We conclude our results are unlikely to be spurious.

6.2 Event Studies

One concern is that the control group is affected on the event date, leading to a mistaken interpretation of our DiD coefficients in regression (R1).[19] To address it, we abandon the control group and do event studies on the treatment group alone. Specifically, we run regression (R1) separately among only our directly treated (English-law) bonds and among our indirectly treated ones (non-English-law bonds from countries with outstanding English-law debt).[20] As reported in Table 6, we find point estimates on the relevant coefficients with the same sign as, if smaller in magnitude than, those we found in our baseline DiD ($\beta_1 = 0.0586$ and $\beta_2 = 0.0513$); cf. Table 3. We conclude that our results are unlikely to be driven by the control group.

6.3 Collapsing the Data

One concern—echoed by our placebo exercise—is that high correlation between similar bonds generates downward-biased standard errors in bond-level regressions, a problem that plagues DiD settings (Bertrand, Duflo, and Mullainathan (2004)). Our baseline analysis addresses this concern by clustering standard errors at the country-maturity level. To address it further, we take a brute-force approach and "collapse" the data. Specifically, we divide bonds into buckets and assign a single yield to each, equal to the simple average of the yields in the bucket. We then repeat regression (R1) on several collapsed samples, using both WLS, weighted by the number of bonds in each bucket, which avoids over-weighting countries with few bonds in the sample, and OLS, which weights buckets equally.

Overall, the analysis, reported in Table 7, supports our baseline findings. When we collapse the data into country-currency-maturity buckets, we find WLS coefficient estimates identical to the baseline model. Notably, they retain statistical significance in both specifications, despite the sample reduction to 569 observations on the event

[19]There is no analogous concern for regression R2, which does not have a control group.

[20]Naturally, we drop the time fixed effects from the baseline specifications, as these sub-samples contain no cross-sectional variation in the English-law dummy.

date (of which 148 are treated directly and 255 indirectly). They lose significance in regression (R2) in the OLS, something consistent with the "fringes" of the bond market being less sensitive to news about the ruling. When we collapse more aggressively to the country level, the estimates all retain their signs and significance in a sample reduced to 108 observations on the event date (of which 45 are treated directly and 36 indirectly). Together, the results trace out the strength of our findings.

7 External Validity: Rigidity under New York Law

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. External validity remains a question. To address it, we consider another event that altered the ability to restructure sovereign debt, this time making it harder: a 2014 Manhattan federal district court ruling that made it harder to punish hold-out bondholders, analogously to the High Court ruling making it easier. A couple of things could make the New York ruling less ideal for our purposes than the English one. (i) It was a blow to Argentina, causing a significant decline in its stock market (Hébert and Schreger (2017)) with potential spillovers throughout Latin America. (ii) It was specific to the wording of a covenant, the pari passu clause, which is not in all bond indentures and not in the same words if it is.

These shortcomings notwithstanding, our results confirm those above: The NY ruling benefited both directly treated bonds and indirectly treated ones and it benefited indirectly treated ones issued by high-law-spread countries more (the last result misses statistical significance at conventional levels, albeit narrowly).

7.1 Institutional Background and Data

7.1.1 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.

7.1.2 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).

7.2 Empirical Framework and Results

7.2.1 Estimation Strategy

We run similar regressions to (R1) and (R2) 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 two models. The first, which we label (R1$'$), 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.[21] 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).

The second model, which we label (R2$'$), is a within-country triple-difference specification. The first difference is the change in the risk neutral probability of default. The second difference is between a country's NY-law bonds and the same country's non-NY-law bonds. The third difference is between countries with higher versus lower "law spreads," i.e. the difference in yields between a country's NY- and local-law bonds.

[21] 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).

Formally, the two models are:

$$\begin{aligned}\Delta y_{i,t} = {} & \lambda_1 \Delta P^{\mathrm{AR}}_{t^*} \times \mathbb{1}^{\mathrm{Direct}}_{i} + \lambda_2 \Delta P^{\mathrm{AR}}_{t^*} \times \mathbb{1}^{\mathrm{Indirect}}_{i,c} \\ & + \lambda_3 \mathbb{1}^{\mathrm{Direct}}_{i} + \lambda_4 \mathbb{1}^{\mathrm{Indirect}}_{i,c} + \mu_{f,t} + \mu_{m,t} + \mu_{r,t} + \nu_{i,t}\end{aligned} \tag{R1$'$}$$

and

$$\begin{aligned}\Delta y_{i,t} = {} & \tau_1 \Delta P^{\mathrm{AR}}_{t^*} \times \mathbb{1}^{\mathrm{Direct}}_{i} \times S^{\mathrm{NY}}_{c,t} + \tau_2 \Delta P^{\mathrm{AR}}_{t^*} \times \mathbb{1}^{\mathrm{Direct}}_{i} \\ & + \tau_3 \mathbb{1}^{\mathrm{Direct}}_{i} \times S^{\mathrm{NY}}_{c,t} + \tau_4 \mathbb{1}^{\mathrm{Direct}}_{i} + \phi_{f,t} + \phi_{m,t} + \phi_{c,t} + \omega_{i,t}\,.\end{aligned} \tag{R2$'$}$$

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 $\Delta P^{\mathrm{AR}}_{t^*}$ to distinguish the change on event dates t^* from that on others.

The key coefficients of interest are λ_1 and λ_2 in regression (R1$'$) and τ_1 in regression (R2$'$).

7.2.2 Identifying Assumptions

As above, the identifying assumption in our DiD estimation is parallel trends (see Section 3.2). As above, we address it in two basic ways: (i) We include currency-, maturity-, and region-/country-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/country 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*.

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 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.

7.2.3 *Do Sovereign Creditors Benefit from the Ability to Restructure?*

Table 8 reports the estimated coefficients. (When comparing them with the results from the High Court ruling, keep in mind that the direction of treatment is reversed.)

The Effect on NY-law Bonds. In accordance with the analogous result for the High Court ruling (Section 3.3.1), we find a negative effect on directly treated yields for countries with outstanding NY-law debt: For a 1% increase in the risk-neutral probability of Argentine default, they fall by about 0.2 bps ($\lambda_1 = -0.0022$).

The Effect on the Other Bonds of NY-law Issuing Countries. In accordance with the analogous result for the High Court ruling (Section 3.3.2), we find a negative effect on the indirectly treated yields for countries with outstanding NY-law debt: For a 1% increase in the risk-neutral probability of default, they fall by about 0.2 bps ($\lambda_2 = -0.0017$).

7.2.4 *Which Bondholders Benefit More?*

In accordance with the analogous result for the High Court ruling (Section 3.3.2), we find that the indirect (spillover) effect is weaker relative to the direct treatment effect in countries where NY-law bonds trade at low yields relative to local-law bonds: For a 1% increase in Argentina's risk-neutral probability of default, a 1% increase in the issuing country's law spread indicates that its non-NY-law yields fall by an additional 0.1 bps relative to its NY-law yields ($\tau_1 = 0.001$); however, the estimate is not statistically significant at conventional levels.

8 Conclusion

Restructuring sovereign bonds is riddled with problems. It can be too hard: Due to the hold-out problem, creditors could reject a restructuring offer that would have made them all better off. But it can also be too easy: Due to the hold-in problem, they could accept an offer that makes them all worse off. Not to mention that sovereigns typically need to restructure multiple classes of debt at once, and what is good for one class might be bad for another.

We investigate whether creditors perceive it as too hard or too easy in equilibrium. We find that creditors value increased hurdles to restructuring not only for their own class of debt, but for other classes as well. We present a model to argue that these results suggest that the commitment not to restructure debt is a valuable disciplining device, which deters strategic default.

A Figures

Figure 1: **Countries in the sample and treatment intensity (English law).** Countries in the treatment group for the shock to English law are in color, with a darker color indicating a larger fraction of English-law bonds outstanding (according to the legend on the right). Countries in white are in the control group. We plots English-law Fractions on the event date, May 31, 2012.

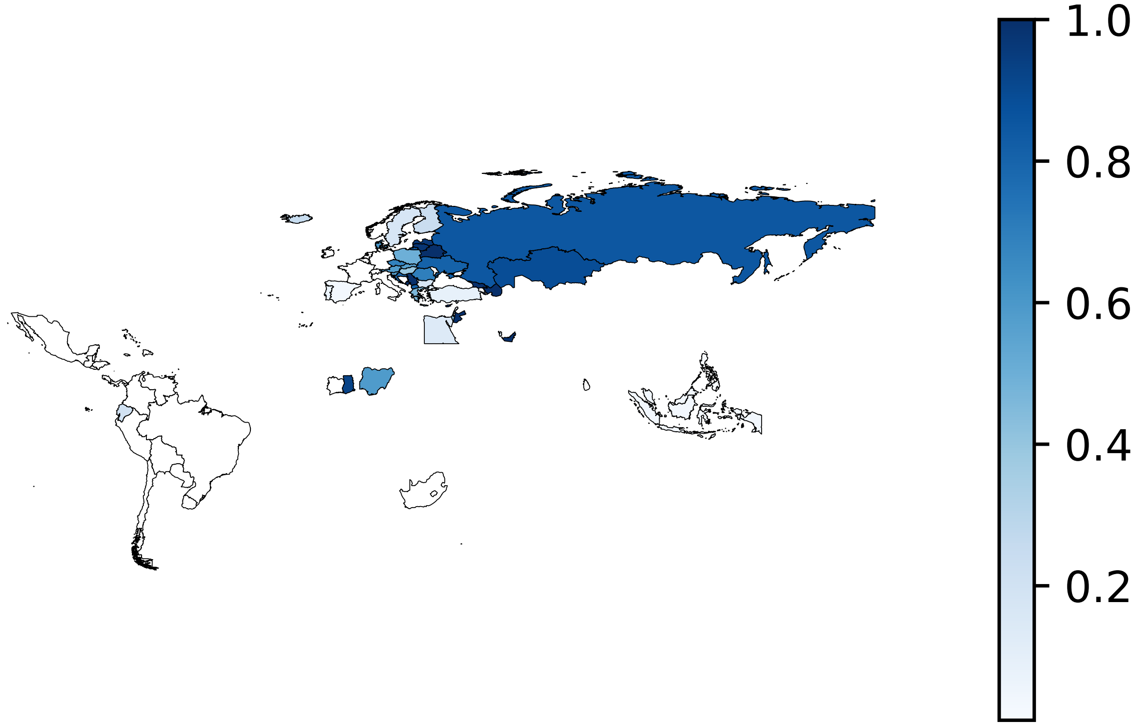


Figure 2: **Countries in the sample and treatment intensity (NY law).** Countries in the treatment group for each shock are in color, with a darker color indicating a larger fraction of NY-la bonds outstanding (according to the legend on the right). Countries in white are in the control group. Due to the staggered nature of the experiment, we plot sample averages.

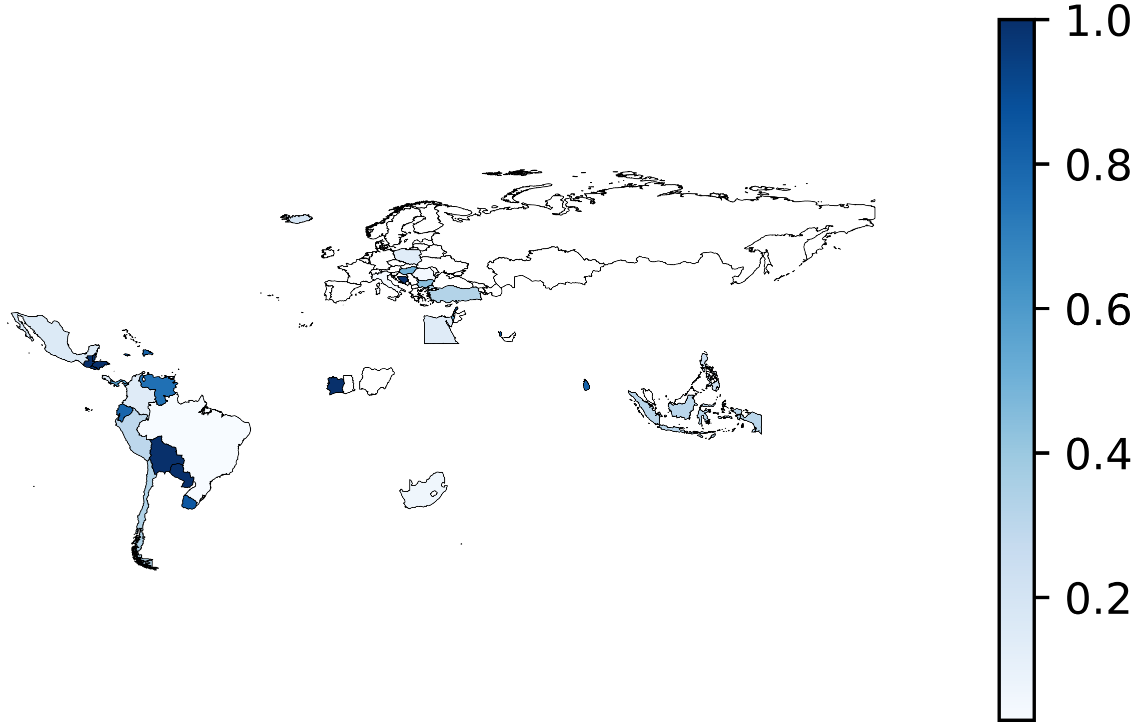
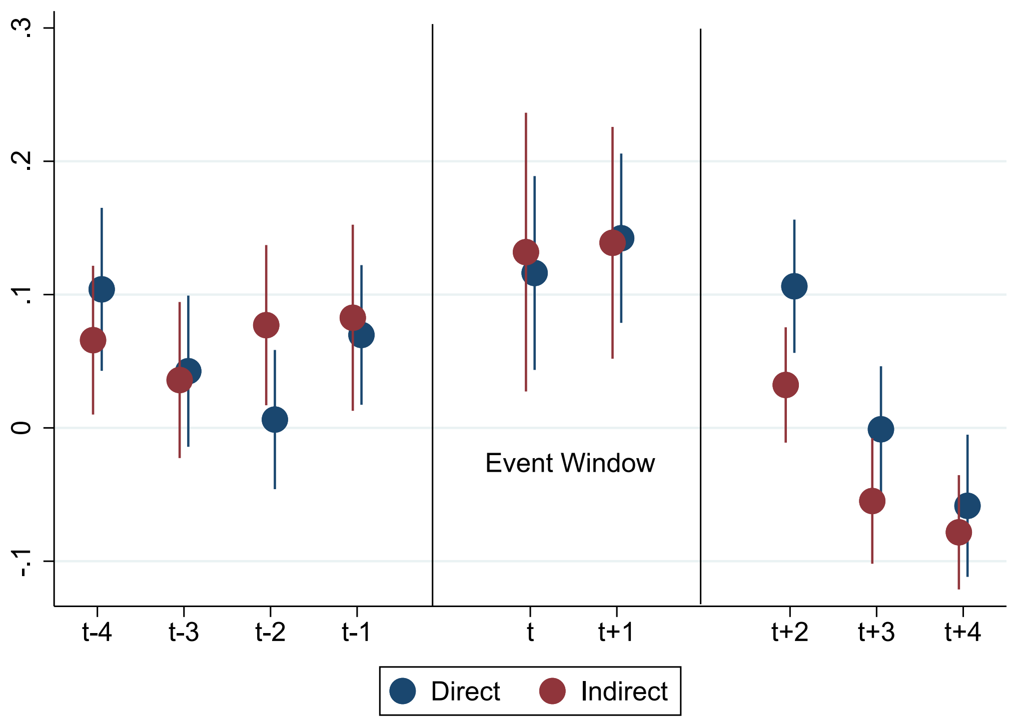


Figure 3: **Difference-in-differences plot.** We plot dynamic DiD coefficients for each trading day within a week of the English High Court ruling (Wednesday, May 30, 2012). To do so, we augment the specification (R1) with additional interactions of the two treatment indicators with time dummies before and after our chosen event window. The plot shows the coefficients and their 98% confidence intervals for two-day yield around our event window (May 30 and 31 (t and $t+1$)).



B Tables

Table 1: **Summary statistics on the event date.** 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.

Variable	Mean	St. Dev.	1%	25%	Median	75%	99%
Bonds	20.49	22.19	1	4	14	27	124
Face value per bond ($bn)	1.75	2.15	0.17	0.57	0.90	2.21	14.67
Maturity	9.10	4.64	2.20	6.03	8.04	11.39	22.22
Yield (%)	5.39	3.21	0.53	3.46	4.74	6.81	13.85
Two-day change in yield (%)	0.02	0.11	−0.23	−0.04	−0.00	0.02	0.43
Face value total ($bn)	105.79	216.87	0.40	6.85	21.25	87.69	1287.71
English-law face value ($bn)	10.31	23.38	0.00	0.00	1.20	12.65	166.51
Fraction English law	0.36	0.40	0.00	0.00	0.14	0.79	1.00
Fraction local law	0.39	0.37	0.00	0.00	0.29	0.73	1.00

Table 2: **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	Maturity	Face value ($bn)	English ($bn)	NY ($bn)	English (%)	NY (%)	Local (%)
AE	13	3.74	9.85	15.56	15.56	0.00	1.00	0.00	0.00
AT	33	1.75	10.07	82.22	47.79	0.00	0.58	0.00	0.37
BB	4	6.91	14.42	1.20	1.08	0.00	0.90	0.00	0.10
BG	23	3.31	4.97	6.85	1.17	3.85	0.17	0.56	0.24
BH	6	4.53	7.07	11.24	11.10	0.00	0.99	0.00	0.00
BR	35	5.30	8.67	1287.71	0.16	32.94	0.00	0.03	0.97
BS	6	6.04	18.99	1.80	0.00	1.80	0.00	1.00	0.00
BY	2	10.91	4.42	1.80	1.80	0.00	1.00	0.00	0.00
CI	2	10.30	20.60	5.22	0.00	5.22	0.00	1.00	0.00
CL	12	2.88	14.69	13.01	0.00	4.82	0.00	0.37	0.63
CO	23	4.28	11.26	116.25	0.00	17.46	0.00	0.15	0.83
CR	4	4.64	12.67	46.88	0.00	7.50	0.00	0.16	0.68
CY	3	13.85	4.47	5.11	5.11	0.00	1.00	0.00	0.00
CZ	27	2.28	9.19	21.25	14.56	0.00	0.69	0.00	0.31
DE	66	0.53	7.44	498.59	0.00	0.00	0.00	0.00	0.99
DK	17	0.65	6.57	38.18	22.74	0.00	0.60	0.00	0.40
DO	7	6.28	8.40	13.39	0.00	10.66	0.00	0.80	0.20
EC	2	13.85	23.21	10.02	2.00	8.02	0.20	0.80	0.00
EG	3	7.28	12.94	26.53	3.70	4.25	0.14	0.16	0.70
ES	53	4.79	6.79	448.84	13.10	0.00	0.03	0.00	0.97
FI	23	1.16	7.16	87.69	21.37	0.30	0.24	0.00	0.75
FR	64	1.30	9.47	410.47	0.00	0.00	0.00	0.00	1.00
GE	2	5.85	8.87	1.00	1.00	0.00	1.00	0.00	0.00
GH	2	5.92	7.00	2.96	2.75	0.00	0.93	0.00	0.00
GR	20	13.85	17.62	359.11	166.51	0.00	0.46	0.00	0.53
GT	4	5.19	12.32	4.06	0.00	4.06	0.00	1.00	0.00
HR	22	6.12	7.05	26.06	20.52	0.00	0.79	0.00	0.21
HU	34	7.94	6.34	32.69	12.83	15.45	0.39	0.47	0.08
ID	56	4.74	8.16	212.62	8.30	59.50	0.04	0.28	0.67
IE	10	6.86	17.09	79.76	0.13	0.00	0.00	0.00	1.00
IL	7	2.45	8.88	13.95	6.00	7.95	0.43	0.57	0.00
IM	1	3.46	20.03	0.40	0.40	0.00	1.00	0.00	0.00
IS	2	5.46	12.78	4.89	1.20	1.00	0.25	0.20	0.55
IT	124	4.44	8.05	986.74	0.37	20.40	0.00	0.02	0.98
JM	8	7.56	10.86	4.18	0.00	3.63	0.00	0.87	0.08
JO	1	4.36	3.45	0.75	0.75	0.00	1.00	0.00	0.00
LB	21	4.91	6.65	21.39	1.20	18.49	0.06	0.86	0.08
LK	5	6.17	7.73	11.90	0.00	9.50	0.00	0.80	0.20
LT	15	4.34	5.55	20.10	19.64	0.00	0.98	0.00	0.02
LU	3	1.20	12.91	9.96	0.00	0.00	0.00	0.00	1.00
LV	5	4.36	6.07	9.96	9.70	0.00	0.97	0.00	0.03
ME	2	9.04	5.71	1.26	1.26	0.00	1.00	0.00	0.00
MK	2	5.57	2.20	0.50	0.40	0.00	0.81	0.00	0.19
MX	39	3.63	12.29	285.99	0.00	46.33	0.00	0.16	0.82
MY	43	3.25	8.19	207.48	7.74	0.00	0.04	0.00	0.96
NG	9	12.93	8.10	5.05	3.00	0.00	0.59	0.00	0.41
NL	29	0.88	7.10	298.21	0.00	0.00	0.00	0.00	1.00
PA	11	3.83	14.29	11.49	0.00	6.32	0.00	0.55	0.40
PE	12	3.89	14.34	20.45	0.00	6.79	0.00	0.33	0.67
PH	65	4.94	12.62	86.60	0.69	21.63	0.01	0.25	0.73
PL	62	3.72	8.73	89.10	44.43	12.20	0.50	0.14	0.30
PT	21	8.26	4.84	73.34	4.80	0.00	0.07	0.00	0.93
QA	18	3.43	11.44	38.05	5.25	32.80	0.14	0.86	0.00
RO	21	5.68	7.62	26.03	18.22	1.50	0.70	0.06	0.24
RS	2	7.22	8.83	12.65	12.65	0.00	1.00	0.00	0.00
RU	33	6.24	9.09	80.53	68.38	0.00	0.85	0.00	0.15
SE	22	0.81	7.03	204.67	34.88	0.90	0.17	0.00	0.82
SI	10	4.51	8.05	37.10	20.99	0.00	0.57	0.00	0.43
SK	25	2.45	7.78	17.02	9.74	0.00	0.57	0.00	0.34
SV	7	6.81	17.20	9.69	0.00	9.68	0.00	1.00	0.00
TR	51	5.87	7.74	131.46	12.30	37.57	0.09	0.29	0.58
TT	3	4.44	12.26	2.04	0.14	1.90	0.07	0.93	0.00
UA	15	9.78	5.74	40.55	32.79	0.00	0.81	0.00	0.18
UY	15	3.54	13.64	15.62	0.00	12.89	0.00	0.83	0.12
VE	14	12.05	9.93	48.45	0.00	34.54	0.00	0.71	0.29
ZA	22	4.70	13.97	197.35	0.00	12.72	0.00	0.06	0.94

Table 3: **Baseline estimates: the effect of the ruling on directly treated and indirectly treated bonds.** We estimate the DiD regression (R1) (Section 3.3). We cluster standard errors at the country and maturity level and report p-values in parentheses.

	Δ yield
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}}$	0.1377
	(0.000)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Indirect}}$	0.1343
	(0.000)
$\mathbb{1}_i^{\text{Direct}}$	-0.0068
	(0.000)
$\mathbb{1}_{i,c}^{\text{Indirect}}$	-0.0053
	(0.000)
Fixed Effects	Region-Time
	Maturity-Time
	Currency-Time
N	1,011,753
R^2	0.309

Table 4: **Baseline estimates: the effect of the ruling on "law spreads."** We estimate the DiD regression R2 (Section 3.4). We cluster standard errors for the within-country regression at the region and maturity level and report p-values in parentheses.

	Δ yield
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}} \times S_{c,t}^{\text{E}}$	-0.0237
	(0.000)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}}$	-0.0111
	(0.240)
$\mathbb{1}_i^{\text{Direct}} \times S_{c,t}^{\text{E}}$	-0.0005
	(0.443)
$\mathbb{1}_i^{\text{Direct}}$	-0.0007
	(0.262)
Fixed Effects	Country-Time
	Maturity-Time
	Currency-Time
N	606,002
R^2	0.510

Table 5: **Heterogeneity in treatment effects.** We estimate the triple-difference regression (R3) (Section 5). Column headers indicate 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 cluster standard errors at the country and maturity level and report p-values in parentheses.

	log(Total Debt)	Debt/GDP	En.-debt/GDP
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}} \times X_{c,t}$	0.0611	0.2430	0.3795
	(0.000)	(0.000)	(0.000)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_{i,c}^{\text{Indirect}} \times X_{c,t}$	0.0781	0.1010	−0.2945
	(0.000)	(0.112)	(0.125)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}}$	−0.1547	0.0345	0.0498
	(0.003)	(0.232)	(0.045)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_{i,c}^{\text{Indirect}}$	−0.2872	0.0862	0.1447
	(0.000)	(0.026)	(0.001)
$\mathbb{1}_t^{\text{Event}} \times X_{c,t}$	−0.0265	−0.0286	
	(0.001)	(0.503)	
$\mathbb{1}_i^{\text{Direct}} \times X_{c,t}$	−0.0020	−0.0142	
	(0.015)	(0.001)	
$\mathbb{1}_{i,c}^{\text{Indirect}} \times X_{c,t}$	−0.0013	−0.0013	
	(0.011)	(0.730)	
$\mathbb{1}_i^{\text{Direct}}$	0.0023	−0.0014	−0.0041
	(0.445)	(0.406)	(0.001)
$\mathbb{1}_{i,c}^{\text{Indirect}}$	0.0015	−0.0052	−0.0044
	(0.596)	(0.002)	(0.000)
$X_{c,t}$	0.0012	0.0034	−0.0127
	(0.008)	(0.332)	(0.000)
Fixed Effects	Region-Time Maturity-Time Currency-Time	Region-Time Maturity-Time Currency-Time	Region-Time Maturity-Time Currency-Time
N	1,011,753	996,241	996,241
R^2	0.309	0.310	0.310

Table 6: **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 cluster standard errors at the country and maturity level and report p-values in parentheses.

	Δ yield Treated, Direct	Δ yield Treated, Indirect
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}}$	0.0586 (0.009)	
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_{i,c}^{\text{Indirect}}$		0.0513 (0.028)
$\mathbb{1}_i^{\text{Direct}}$	-0.0072 (0.000)	
$\mathbb{1}_{i,c}^{\text{Indirect}}$		-0.0062 (0.000)
Fixed Effects	Region Maturity Currency	Region Maturity Currency
N	180,829	573,477
R^2	0.003	0.003

Table 7: **Collapsed estimates.** We estimate regressions (R1) and (R2) after collapsing bonds by country, currency, and five-year maturity bin (Section 6.3). The fixed effects are omitted from the table for readability, but described in specifications (R1) and (R2), and Table 3 and Table 4. The two right-hand columns report the same exercise for data collapsed at the country level. In this case we first regress the panel of yields on currency-time and maturity-time fixed effects. The residuals from this regression are net of changes in currency and maturity-specific yield components, leaving only country- and bond-specific components. We therefore interpret changes in these residual yields as changes in credit spreads. We difference these credit spreads over two trading days, average by country and treatment (direct, indirect, or control), and use them as the dependent variable to run variations of (R1) and (R2). We report p-values in parentheses, based on heteroskedasticity-robust (White) standard errors.

	Δ yield (R1)	Δ yield (R2)	Δyield (R1)	Δyield (R2)	Δcredit spr. (R1)	Δcredit spr. (R2)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}} \times S_{c,t}^{\text{E}}$		−0.0237		−0.0149		−0.0237
		(0.024)		(0.190)		(0.000)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}}$	0.1377	−0.0111	0.0908	0.0027	0.0751	−0.0297
	(0.000)	(0.501)	(0.000)	(0.839)	(0.008)	(0.071)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Indirect}}$	0.1343		0.0703		0.1047	
	(0.000)		(0.000)		(0.010)	
$\mathbb{1}_i^{\text{Direct}} \times S_{c,t}^{\text{E}}$		−0.0005		−0.0005		−0.0004
		(0.506)		(0.519)		(0.696)
$\mathbb{1}_i^{\text{Direct}}$	−0.0068	−0.0007	−0.0036	−0.0004	−0.0046	−0.0010
	(0.000)	(0.159)	(0.000)	(0.428)	(0.001)	(0.317)
$\mathbb{1}_{i,c}^{\text{Indirect}}$	−0.0053		−0.0026		−0.0045	
	(0.000)		(0.000)		(0.000)	
Weights	WLS	WLS	OLS	OLS	WLS	WLS
N	294,503	158,543	294,503	158,543	64,829	32,216
R^2	0.428	0.772	0.352	0.659	0.143	0.794

Table 8: **NY-law estimates.** Estimates of DiD in regressions (R1′) and (R2′). We estimate the DiD regressions (R1′) and (R2′). We cluster standard errors at the country and maturity level for regression (R1′) and at the region and maturity level for the within-country regression (R2′). We report p-values in parentheses.

	Δ yield Regression (R1′)	Δ yield Regression (R2′)
$\Delta P^{\text{AR}}_{t^*} \times \mathbb{1}^{\text{Direct}}_i \times S^{\text{NY}}_{c,t}$		0.0008 (0.192)
$\Delta P^{\text{AR}}_{t^*} \times \mathbb{1}^{\text{Direct}}_i$	-0.0021 (0.070)	0.0008 (0.304)
$\Delta P^{\text{AR}}_{t^*} \times \mathbb{1}^{\text{Indirect}}_{i,c}$	-0.0015 (0.007)	
$\mathbb{1}^{\text{Direct}}_i \times S^{\text{NY}}_{c,t}$		-0.0009 (0.164)
$\mathbb{1}^{\text{Direct}}_i$	-0.0016 (0.045)	0.0010 (0.095)
$\mathbb{1}^{\text{Indirect}}_{i,c}$	-0.0014 (0.199)	
Fixed Effects	Region-Time Maturity-Time Currency-Time	Country-Time Maturity-Time Currency-Time
N	1,011,753	479,122
R^2	0.308	0.489

C Data

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.

D Nested DiD Specification

We estimate the two nested DiDs in (R1) along with separate DiD specifications.

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

The nested specification is estimated on all bonds. The Direct (Indirect) specifications use only the direct (indirect) treatment group and the control group. Since the time trend, α, is estimated purely based on the control group, and all other coefficients in the Direct (Indirect) specification are purely estimated using observations absent from the Indirect (Direct) specification, the estimates for the β coefficients are identical in the nested and the separate specifications. We report the results in Table 9 below.

Table 9: **Estimates of nested and separate DiDs.** As in the baseline, we cluster standard errors at the country and maturity level and report p-values in parentheses.

	Δ yield (Nested)	Δ yield (Direct)	Δ yield (Indirect)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}}$	0.0581	0.0581	
	(0.009)	(0.009)	
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_{i,c}^{\text{Indirect}}$	0.0511		0.0511
	(0.027)		(0.027)
$\mathbb{1}_i^{\text{Direct}}$	−0.0045	−0.0045	
	(0.000)	(0.000)	
$\mathbb{1}_{i,c}^{\text{Indirect}}$	−0.0035		−0.0035
	(0.000)		(0.000)
Time trend	−0.0027	−0.0027	−0.0027
	(0.000)	(0.000)	(0.000)
N	1,013,640	440,163	832,811
R^2	0.001	0.001	0.001

E Proofs

E.1 Proof of Proposition 1

The result is immediate from equation (11). □

E.2 Proof of Proposition 2

The result is immediate from equation (13). □

E.3 Proof of Prediction 1

The results follow immediately from differentiating equations (11) and (13):

1. Part (i):
$$\left.\frac{\partial^2 p_r}{\partial D \partial h_r}\right|_{D_r>0} = -\frac{\varphi h_r}{c} - \frac{z^*}{D} < 0. \tag{24}$$

2. Part (ii):
$$\left.\frac{\partial^2 p_f}{\partial D \partial h_r}\right|_{D_r>0} = -\frac{\varphi h_f}{c} < 0. \tag{25}$$

□

E.4 Proof of Prediction 2

The results follow immediately from differentiating equations (11) and (13):

1. Part (i):
$$\left.\frac{\partial^2 p_r}{\partial D_r \partial h_r}\right|_{D_r>0} = -\frac{2h_r}{c} < 0 \tag{26}$$

2. Part (ii):
$$\left.\frac{\partial^2 p_f}{\partial D_r \partial h_r}\right|_{D_r>0} = -\frac{h_f}{c} < 0. \tag{27}$$

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