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 the yields of that class increase, so did those of other classes, suggesting forgoing commitment has negative cross-bond spillovers. We develop a model to rationalize and help quantify these findings. Three more experiments provide further support for the mechanism in the model.

1 Introduction

Sovereign default is on the rise. Argentina, Ecuador, Lebanon, and Zambia have defaulted recently. Other emerging-markets countries are pleading for debt relief.[1] Some defaults are hard defaults in which creditors get little or nothing. But most defaults are soft/partial defaults in which creditors take a haircut but still get something. For bonds, such defaults typically constitute a 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 restructuring debt could help some creditors at the expense of others, as all classes of a sovereign's bonds are not created equal, but convey different rights via different non-price terms such as governing law and covenants (Bolton and Jeanne (2007, 2009)). The ability to restructure one class of debt could thus 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 bonds with different governing laws or covenants.[2] But no other paper, to our knowledge, addresses our other questions, which rely on comparing multiple bonds issued by the same country.

To confront these challenges, we exploit a landmark ruling by the English High Court that had "worldwide ramifications" (White & Case (2013); see Section 2.2). The ruling made it easier to restructure all English-law bonds by affirming the legality of a controversial restructuring device known as "consent solicitations" (see Section 2.1). We thus can compare the same bonds on the trading days before and after this shock, and thereby avoid relying entirely on matching. The ruling pertained to bond restructuring specifically, but was not directly associated with the sovereign bond 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 to decrease their borrowing costs.[3] Other countries do not. Their bonds provide us with a control group for a difference-in-differences (DiD) analysis over a narrow event window of two days.

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 (described above) 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 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

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[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.

[3]See, e.g., Chamon, Schumacher, and Trebesch (2018), who show that high-credit-risk countries' foreign-law bonds have lower yields than their domestic ones. One reason is that foreign-law debt has higher recovery rates, as evidenced by, e.g., the Greek restructuring, in which many English-law bonds were repaid in full, whereas no domestic-law bonds were (see also Section 4.3).

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 increase by around 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.

An initial 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, the direct effect on English-law bonds is around the same size as the spillover effect on non-English-law bonds issued by English-law issuers.

A fuller answer should acknowledge differences among countries, as governing law could matter more in some countries than others, as suggested by differences in countries' "law spreads"—i.e. yield spreads between English- and local-law bonds. Thus 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. 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 2–4 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 benefits that class directly. It also benefits other classes indirectly, and these spillovers can be even larger than the direct effects. Thus ignoring spillovers—e.g., using one class of the same issuer's bonds as a control for another class's treatment group—could lead to estimates with not only the wrong size, but potentially even the wrong sign, a finding that reifies something Berg, Reisinger, and Streitz (2020) show can happen theoretically.

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

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that yields on treatment and control bonds would have moved identically absent the ruling (parallel trends). To address concerns about SUTVA, we omit the control group entirely, running our regressions as plain event studies. We find analogous results, suggesting our findings are indeed "treatment effects" (see Section 3.3.3). 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 could affect the groups differently. But a data-driven placebo-type test suggests such an event is unlikely to generate our results spuriously (Section 3.3.3).

Our baseline results are based on a single ruling applying to a single restructuring device (consent solicitations). 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 broadly analogous results.

Next, we develop a simple model of a sovereign restructuring, which serves both to investigate the mechanism behind our results and to interpret their magnitudes. Since restructuring is a form of partial default, we model it 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)). 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, corresponding to 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

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debt, which a sovereign can restructure unilaterally by changing the law (as Greece did in 2012; see Zettelmeyer, Trebesch, and Gulati (2014)). (Such law-dependent haircuts arise from including litigation risk in a model of exchange offers à la Gertner and Scharfstein (1991) (see Section 2.1).)

The model has close counterparts in our empirical environment: Rigid debt corresponds to English-law bonds, flexible to domestic, and, as shown in Section 2.1, the High Court ruling to an increase in the haircut on rigid debt h_r (consent solicitations 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 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 straitjacket 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 straitjacket would imply a decrease in yields and a dilution

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device a positive spillover on other classes.

The model also abstracts from other bond terms, such as collective action clauses (CACs), which help dispersed bondholders to act in unison. CACs provide a remedy for some of the collective action problems that plague exchange offers. They can solve the so-called "hold-out problem" whereby bondholders reject an offer that would make them better off. But they are not a panacea, in part because the sovereign need not always invoke them. They do not solve the so-called "hold-in problem" whereby a sovereign traps bondholders in a prisoner's dilemma, inducing them to accept an offer that makes them worse off (see Section 2.1).[4]

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 do not reject any of them and provide significant support for all but one of them (the indirect effect is not significantly stronger or weaker in countries with more rigid debt), providing additional support for our model overall.

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 out this number. We derive a formula for the implied h_r in terms of our estimated yield changes and several other sufficient statistics, notably estimates of risk-neutral default probabilities (from CDS) and expected haircuts (from the Greek restructuring). We estimate that the ruling increased expected haircuts on English-law debt by 3–5 percentage points, a meaningful effect to be sure, but not an implausible one, as sovereign debt haircuts often exceed 50% (Cruces and Trebesch (2013)).

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

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

(2003)) or bad, e.g., leading to prolonged defaults. 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 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: the first ever English High Court ruling on consent solicitations, which 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 distressed exchange offer: The sovereign offers bondholders new securities in exchange for their bonds.

Given bondholders are typically 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 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

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collectively worse off.[5] (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 can repay in full. In equations: If the sovereign would repay its debt D with probability q_D and nothing otherwise, bondholders benefit collectively from taking a haircut H, which would increase 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

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[5]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.

(possibly following litigation).[6] Thus, as a small bondholder has a negligible effect on the repayment probability, it accepts a haircut h whenever

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

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 take off the shelf in the model below (see Section 4).

What kinds of consent solicitations (and associated haircuts) are feasible depends on the law. In our baseline analysis 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 (see Section 6). Such restructurings are dubbed "coercive."

This is where the hold-in problem comes in: Consent solicitations can force bond-holders into a prisoner's dilemma in which they tender even if restructuring is against their collective interest (i.e. inequality (3) is satisfied but (1) violated). 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

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

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[6]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, providing support for our π. See Schumacher, Trebesch, and Enderlein (2021) for an overview of sovereign debt litigation in recent decades.

restructure bonds in exchange offers.[7] 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*.[8] The newspaper also stresses that consent solicitations, 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.[9] 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 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 the consent solicitation was offered to all bondholders, and therefore was not an illegal bribe.

The decision was not a foregone conclusion (see, e.g., Jones Day (2012)). Just two months later, the High Court ruled the other way in an analogous case (see Section

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

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

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

6.1.1). 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 to 2020. 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), while avoiding the Greek restructuring in early 2012. We focus on bonds issued by countries with significant English- and NY-law borrowing and by relevant control countries, e.g., 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

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credit spreads and thereby difference out risk-free rate changes.

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. 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%.

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 2 shows statistics by country.

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

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 bond market response to a change in the ease of bond restructuring, we estimate two models using a high-frequency difference-in-differences (DiD) approach. 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, 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 D.3, we show 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 between inside and outside the event window. 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:

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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}
$$

and

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

In our baseline specifications, the dependent variable x_{it} is the change in one of two high-frequency ($t-2$ to t) 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-specific risk-free rates. Hence we include currency- and maturity-time fixed effects in our yield regressions. 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 (2020), or Binsbergen, Diamond, and Grotteria (2021)). Hence we construct the credit spread by subtracting the maturity- and currency-matched swap rate from the yield. (These baseline specifications compare changes ("abnormal returns") in and out of the event window, as is standard in event studies; however, as a robustness check, we also compare levels immediately before and after the event (see Table D.2), 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. $S_{c,t}^{\text{E}}$ 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 credit spreads between a country's English- and local-law bonds.

The terms $\text{FE}_{i,t}$ denote vectors of fixed effects. In all (and only) the yield regressions, they include the aforementioned risk-free rate controls; in regression (R1), they also include region-time fixed effects; in regression (R2), they include country-time fixed effects instead.[10]

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, $\psi^{\top}\text{NY}_{i,t} = \psi_1 \mathbb{1}_t^{\text{Event}} \mathbb{1}_i^{\text{NY}} S_{c,t}^{\text{E}} + \psi_2 \mathbb{1}_t^{\text{Event}} \mathbb{1}_i^{\text{NY}} + \psi_3 \mathbb{1}_i^{\text{NY}} S_{c,t}^{\text{E}} + \psi_4 \mathbb{1}_i^{\text{NY}}$.

The ε and η are error terms.

In regression (R1), 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

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[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.

treated bonds. In each case, the changes are measured relative to our control group. We cluster standard errors at the country level.

In regression (R2), the key coefficient of interest is γ_1 on the triple-interaction term. It measures the relative changes in x_{it} around the event for directly and indirectly treated bonds in high vs. low law spread countries.[11]

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 control group (which contains more Latin American ones).

We cannot completely rule out concerns (i) or (ii), as the parallel trends assumption

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[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.

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-/country-time fixed effects in our regressions. These absorb variation on any given date common to a given region/country. 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/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. 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 2 and Figure 3. Figure 2 plots the coefficient estimates for regression (R1) with placebo event dates (and associated partially overlapping two-day event windows). Only in the true event window are both the direct and indirect effects significantly greater than zero at the 5% level. Figure 3 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. The figures could arguably be consistent with some post-announcement drift (as if the news was still being impounded into prices at $t+2$) in English-law bonds; however, any underreaction in bond markets would bias against our finding a result in the event window. They could also 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 3.3.3).

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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 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 9 bps relative to the control ($\beta_1 = 0.1377$ for yields and 0.0921 for credit spreads).

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

3.3.2 The Spillover Effect on the Indirectly Treated Bonds

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; the credit spreads increase by around 9 bps relative to the control ($\beta_2 = 0.1347$ for yields and 0.0954 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.

3.3.3 Standard Errors and Robustness

Standard errors (placebo p-values). One concern is that the statistical significance we report could be exaggerated because the distributional assumptions behind our standard errors could be violated. To address this concern, 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 that fall outside our event window. We then re-estimate regressions (R1) and (R2) and construct an empirical distribution of the estimated t-statistics of interest in order to calculate "placebo p-values" for the results.

We find that the result for the direct treatment effect (β_1) holds for yields (credit spreads) on 4.6% (8.0%) of days with a t-statistic as large as that on the true event date, the result for indirect treatment (β_2) holds on 6.3% (7.8%) of them, and the two hold jointly on 2.6% (1.7%) of sample days. Although these estimates suggest that the theoretical p-values we report could overstate the statistical significance, they imply that it is unlikely that these results would arise by chance.

Robustness (event study estimation). As a robustness check, we abandon the control group and run regression (R1) as plain event studies for directly treated bonds and indirectly treated bonds separately. This addresses any potential concern that our results are driven by movements in the control group or selection between treatment and control.[12] Table D.1 in the Appendix reports results in line with our baseline specifications.

3.4 Which Bondholders Benefit More?

Table 4 reports the results from estimating equation (R2), our triple-difference regression capturing 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 treat-

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[12] We note that a shock to the so-called GIIPS countries most affected by the European sovereign debt crisis would be unlikely to create bias, as they are split roughly evenly between treatment (Greece, Portugal, Spain) and control (Italy, Ireland).

ment effect in countries whose English-law bonds trade at low yields relative to their local-law bonds, i.e. for countries with relatively high law spreads: For a 1 percentage point increase in a country's law spread, there is approximately a 4 bps increase in local-law yields relative to English-law yields around the ruling; the analogous number for credit spreads is approximately 2 bps ($\gamma_1 = -0.0414$ for yields and -0.0183 for credit spreads).

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.[13]

Placebo p-values. We find that the result holds for yields (credit spreads) on 0.9% (17.1%) of days with t-statistics as large as that on the true event date, supporting the significance of the result in the yield specification, but suggesting our p-value could be exaggerated in the credit spread specification.

But we find the set of the three results from regressions (R1) and (R2) above hold jointly in the yield (credit spread) specification on zero (0.5% of) sample days with t-statistics as large as those on the true event date. We conclude that the set of results is unlikely to be spurious, especially since the model developed in the next section suggests that the High Court ruling should lead them to arise simultaneously.

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), and to derive further predictions (Section

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[13]Note that there is no "bad control" concern like that discussed in Section 3.3.3, as regression (R2) does not have a control group.

4.4).

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 $F(z) = z$ with $z \in [0, 1]$.

The model has two key ingredients.

1. The sovereign has a willingness-to-pay problem.[14] 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 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 and local-law to flexible. The High Court ruling, which allowed consent solicitations, corresponds 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. (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.)

---

[14]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).

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

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

---

[15]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)).

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.

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 debt respond to the High Court ruling. To derive them, we can use the default threshold in equation (7) to re-write the bond price in equation (5) as

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

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

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 responds 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

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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—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.

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

---

[16]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.

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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 (17)—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 4).

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 reduce the haircut h_r? We use the model to back this number out from the yield changes we estimate empirically and several 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 4. *A small change in the haircut on rigid debt Δh_r induces changes in the yields of r- and f-debt Δy_r and Δy_f approximately as follows:*

$$\Delta y_r \approx \frac{1}{p_r T_r}\Big(F_{h_r} h_r + F(z^*)\Big)\Delta h_r \tag{20}$$

$$\Delta y_f \approx \frac{1}{p_f T_f}F_{h_r} h_f \Delta h_r, \tag{21}$$

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

In addition to the yield changes Δy_i from our baseline regressions, we require a number of other parameters to solve the system:

- We estimate the average maturities $T_r = 8.15$ and $T_f = 8.08$ and yields $y_r = 5.22\%$ and $y_f = 4.55\%$ for directly and indirectly treated bonds from our sample (implying $p_r = e^{-5.22\%\times 8.15} \approx 0.65$ and $p_f = e^{-4.55\%\times 8.08} \approx 0.69$).

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- We estimate the risk-neutral default probability from 1-year CDS spreads and recovery values reported by Markit for the treated countries in our sample. We obtain $F(z^*) = 3.7\%$ on the day before the ruling.

- We estimate the haircuts based on the Greek restructuring, for which we observe recent haircuts for both rigid, English-law bonds and flexible, local-law bonds simultaneously (Zettelmeyer, Trebesch, and Gulati (2014)): A haircut of 76.9% applied to all local-law bonds and three quarters of English-law bonds (the remainder of which were repaid in full), making $h_f = 76.9\%$ and $h_r = 57.3\%$. (Cruces and Trebesch's (2013) data set delivers a similar number (around 75%) for restructurings post-2000. So do current CDS-implied haircuts. Neither of these sources, however, permits us to estimate the haircuts on r- and f-debt separately.)

- We do not need to estimate the sensitivity of the default probability to the haircut F_{h_r}. The parameters above are sufficient statistics for Δh_r given the system above (equations (20) and (21)).

Solving (20) and (21) given these parameter estimates, we find $\Delta h_r = 4.7\%$ from our baseline yield specification ($\Delta y_r \approx \Delta y_f \approx 14$ bps per Section 3.3) and $\Delta h_r = 2.5\%$ from our credit spread specification ($\Delta y_r \approx \Delta y_f \approx 9$ bps per Section 3.3): The ruling raised expected haircuts on English-law debt by 3–5 percentage points on average, or about 4–8% of the average haircut in sovereign restructurings since 1990 (see Cruces and Trebesch (2013)).

4.4 Additional 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.

4.4.1 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:*

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

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

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

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

4.4.2 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:*

$$\left.\frac{\partial^2 p_r}{\partial D_r \partial h_r}\right|_{D_r > 0} < 0. \tag{24}$$

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

$$\left.\frac{\partial^2 p_f}{\partial D_r \partial h_r}\right|_{D_r > 0} < 0. \tag{25}$$

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 4.4, 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:

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

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 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 (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, $\kappa_1 = 0.0607$ (0.0533) and $\kappa_2 = 0.0790$ (0.0785); if it is its debt-to-GDP ratio, $\kappa_1 = 0.2483$ (0.1856) and $\kappa_2 = 0.1035$ (0.1669)).[17]

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

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[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).

5.2 Rigid Debt

Letting X in equation (R3) be a measure of a country's English-law debt, we find that yields (credit spreads) 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 $\kappa_1 = 0.3766$ (0.2372); our estimate of the indirect effect is $\kappa_2 = -0.3048$ (−0.3482), but it 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 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. 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.

(We do not, however, find clear results on which bondholders benefit more (Section 3.4) and therefore omit a discussion of it.)

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[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.

6.1 Two More Experiments on Restructuring under English Law

Here we exploit a ruling and its (dropped) appeal pertaining to consent solicitations' evil stepbrother, a restructuring device called exit consents.

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

Just two months after its ruling on consent solicitations 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 bond holders 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 solicitations, they counter the hold-out problem, but can create a hold-in problem, and therefore be deemed coercive.[19] That is how the Court ruled: It upheld a challenge to their legality under English law.[20] 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.

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[19]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).

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

6.1.2   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 31, 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 (see, however, footnote 12). 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)).

6.1.3 Do Sovereign Creditors Benefit from the Ability to Restructure?

Table 6 reports the estimated coefficients. The estimates on all eight coefficients of interest are in accordance with the analogous results for the High Court 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 (although not all estimates are statistically significant at conventional levels).

The standard errors are large for the ruling, perhaps due to increased volatility in the sovereign debt market following Draghi's speech. The coefficient estimates are relatively small for the withdrawn appeal, perhaps due to offsetting events.

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.[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).

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[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 model is:

$$\Delta x_{i,t} = \lambda_1 \Delta P_{t^*}^{\mathrm{AR}} \times \mathbb{1}_i^{\mathrm{Direct}} + \lambda_2 \Delta P_{t^*}^{\mathrm{AR}} \times \mathbb{1}_{i,c}^{\mathrm{Indirect}}$$
$$+ \lambda_3 \mathbb{1}_i^{\mathrm{Direct}} + \lambda_4 \mathbb{1}_{i,c}^{\mathrm{Indirect}} + \mu^\top \mathrm{FE}_{i,t} \nu_{i,t}. \tag{R1$'$}$$

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/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 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 6 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 in our yield specification and for directly treated bonds in our credit spread specification: For a 1% increase in the risk-neutral probability of Argentine default, they fall by about 0.1–0.2 bps, although only one estimate is statistically significant at the conventional levels. (We find no effect for indirectly treated bonds in our credit spread specification.)

One reason to expect larger standard errors for this experiment is that it is 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. So there is noise in the treatment.

7   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. Evidence from four natural experiments suggests that making it harder to restructure one class of debt benefits not only that class, but other classes as well. We develop a model to argue that these results suggest that the commitment not to restructure debt is a valuable disciplining device that 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 on the event date, May 31, 2012 (according to the legend on the right). Countries in white are in the control group.
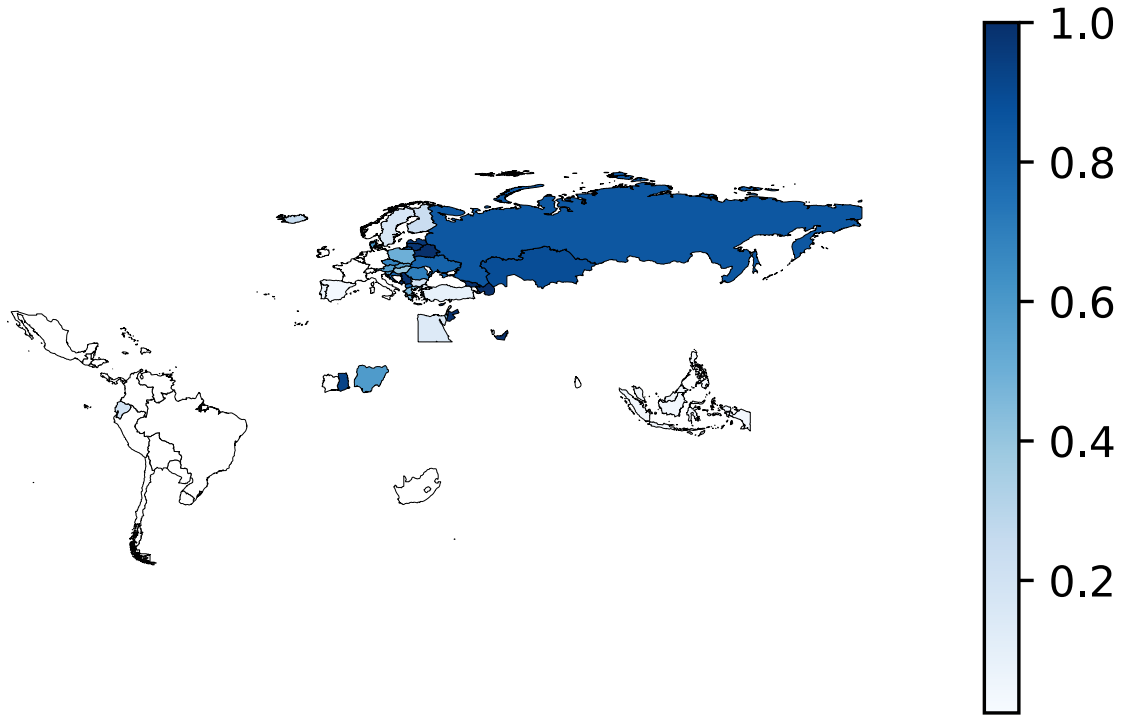
Figure 2: **Difference-in-differences plot.** We plot dynamic DiD coefficients for each trading day within a week of the English High Court ruling (Wednesday, May 30, 2012). To do so, we augment regression (R1) with additional interactions of the two treatment indicators with time dummies before and 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).
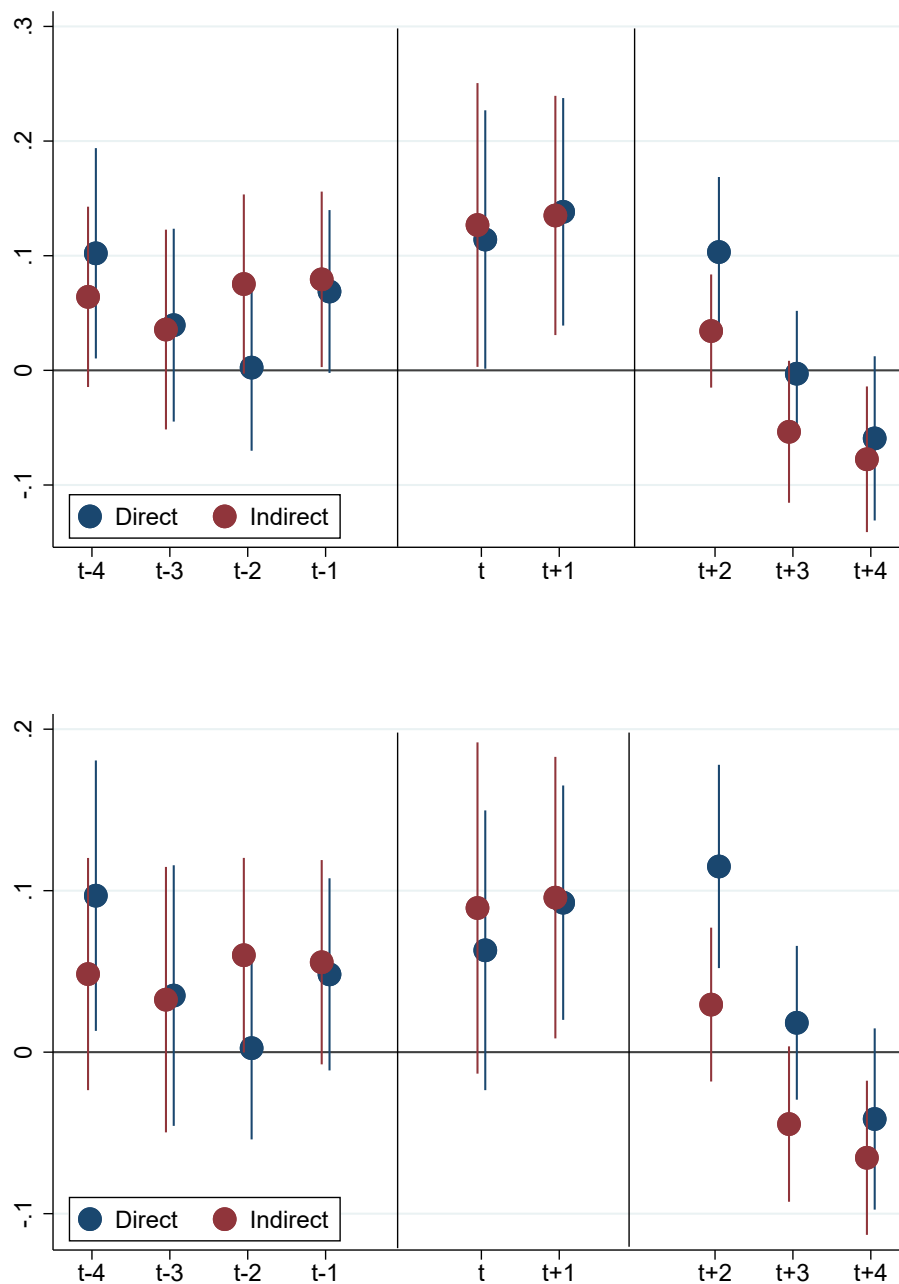
Figure 3: **Difference-in-differences levels.** To visualize the level change in yields (top panel) and credit spreads (bottom panel) around the English High Court ruling, we plot the cumulative "abnormal" changes for each trading day within a week of the ruling (Wednesday, May 30, 2012. The abnormal changes are composed of the estimated treatment effects and regression residuals from regression (R1) as by $\Delta \hat{x}_{i,t} = \hat{\beta}_1 \mathbb{1}_t^{\text{Event}} \mathbb{1}_i^{\text{Direct}} + \hat{\beta}_2 \mathbb{1}_t^{\text{Event}} \mathbb{1}_{i,c}^{\text{Indirect}} + \hat{\varepsilon}_{i,t}$. Their cumulative value is on the vertical axis.
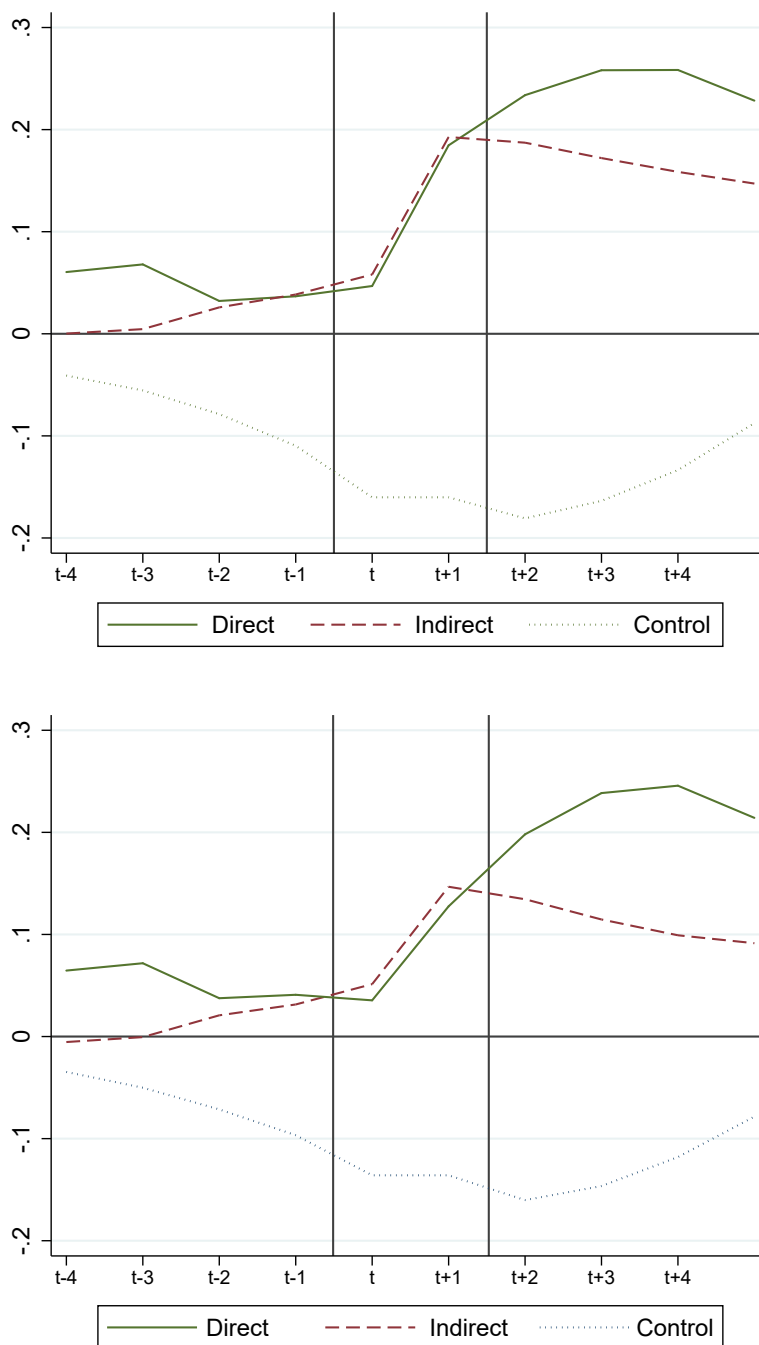
Figure 4: **Countries in the sample and treatment intensity (NY law).** Countries in the treatment group for each shock are in color, with a darker color indicating a larger fraction of NY-law bonds outstanding on average over the sample (according to the legend on the right).
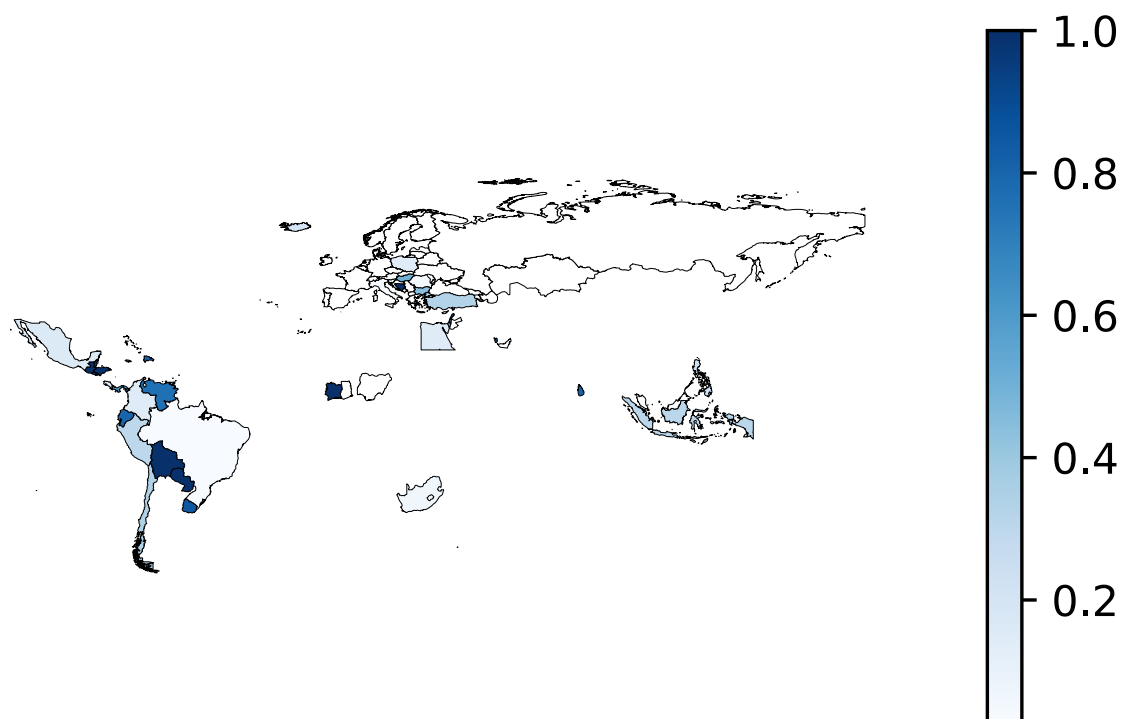
B   Tables

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: **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 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 level and report p-values in parentheses and "placebo p-values" in brackets, where the placebo p-value is the fraction of sample days (650 in total), on which the event dummy produces t-statistics for the coefficient of interest exceeding those for the actual event date. The row labelled "joint placebo p-value" reports this statistic for the joint results β_1 and β_2.

	Δ yield	Δ credit spread
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}}$	0.1378	0.0921
	(0.007)	(0.013)
	[0.046]	[0.080]
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Indirect}}$	0.1347	0.0954
	(0.012)	(0.032)
	[0.063]	[0.078]
$\mathbb{1}_i^{\text{Direct}}$	-0.0066	-0.0053
	(0.004)	(0.004)
$\mathbb{1}_{i,c}^{\text{Indirect}}$	-0.0054	-0.0039
	(0.000)	(0.003)
Joint placebo p-value	[0.026]	[0.017]
Fixed Effects	Region-Time	Region-Time
	Maturity-Time	
	Currency-Time	
N	1,011,311	904,945
R^2	0.308	0.181

Table 4: **Baseline estimates: the effect of the ruling on "law spreads."** We estimate the DiD regression R2 (Section 3.4). Due to the reduced number of countries in the within-country regression, we cluster standard errors at the country and maturity level. We report p-values in parentheses and "placebo p-values" in brackets, where the placebo p-value is the fraction of sample days (650 in total), on which the event dummy produces t-statistics for the coefficient of interest exceeding those for the actual event date. The row labelled "joint placebo p-value" reports this statistic for the joint results from regression (R1) and (R2), that is, β_1, β_2, and γ_1.

	Δ yield	Δ credit spread
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}} \times S_{c,t}^{\text{E}}$	-0.0414	-0.0183
	(0.000)	(0.063)
	[0.009]	[0.171]
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}}$	-0.0445	-0.0024
	(0.115)	(0.926)
$\mathbb{1}_i^{\text{Direct}} \times S_{c,t}^{\text{E}}$	-0.0007	-0.0020
	(0.513)	(0.206)
$\mathbb{1}_i^{\text{Direct}}$	-0.0008	-0.0036
	(0.158)	(0.001)
Joint placebo p-value	[0.000]	[0.005]
Fixed Effects	Region-Time	Region-Time
	Maturity-Time	
	Currency-Time	
N	522,602	489,284
R^2	0.534	0.435

Table 5: **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 cluster standard errors at the country level and report p-values 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	0.0533	0.2483	0.1856	0.3766	0.2372
	(0.000)	(0.018)	(0.000)	(0.070)	(0.000)	(0.174)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_{i,c}^{\text{Indirect}} \times X_{c,t}$	0.0790	0.0785	0.1035	0.1669	-0.3048	-0.3482
	(0.000)	(0.000)	(0.081)	(0.098)	(0.108)	(0.356)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}}$	-0.1547	-0.1603	0.0325	0.0199	0.0511	0.0423
	(0.003)	(0.060)	(0.254)	(0.660)	(0.039)	(0.300)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_{i,c}^{\text{Indirect}}$	-0.2921	-0.3123	0.0862	0.0233	0.1462	0.1117
	(0.000)	(0.002)	(0.023)	(0.569)	(0.001)	(0.047)
$\mathbb{1}_t^{\text{Event}} \times X_{c,t}$	-0.0270	-0.0227	-0.0342	-0.0356		
	(0.000)	(0.033)	(0.392)	(0.587)		
$\mathbb{1}_i^{\text{Direct}} \times X_{c,t}$	-0.0021	-0.0018	-0.0145	-0.0139		
	(0.011)	(0.174)	(0.000)	(0.004)		
$\mathbb{1}_{i,c}^{\text{Indirect}} \times X_{c,t}$	-0.0013	-0.0015	-0.0017	-0.0028		
	(0.017)	(0.010)	(0.643)	(0.484)		
$\mathbb{1}_i^{\text{Direct}}$	0.0028	0.0029	-0.0011	-0.0005	-0.0039	-0.0030
	(0.347)	(0.491)	(0.520)	(0.801)	(0.001)	(0.047)
$\mathbb{1}_{i,c}^{\text{Indirect}}$	0.0011	0.0038	-0.0050	-0.0032	-0.0044	-0.0030
	(0.689)	(0.214)	(0.002)	(0.069)	(0.000)	(0.039)
$X_{c,t}$	0.0012	0.0010	0.0034	0.0038	-0.0126	-0.0116
	(0.008)	(0.022)	(0.314)	(0.253)	(0.001)	(0.062)
Region-Time	Yes	Yes	Yes	Yes	Yes	Yes
Maturity-Time	Yes	No	Yes	No	Yes	No
Currency-Time	Yes	No	Yes	No	Yes	No
N	1,011,311	904,945	995,799	889,736	995,799	889,736
R^2	0.309	0.182	0.310	0.181	0.309	0.180

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

Regression	(R1)		(R1)		(R1′)	
Event	July 31, 2012		April 23, 2013		$\Delta P_{t^*}^{\text{AR}}$	
Dependent var.	Δyield	Δcredit sp.	Δyield	Δcredit sp.	Δyield	Δcredit sp.
Event $\times \mathbb{1}_i^{\text{Direct}}$	-0.0648	-0.0509	-0.0371	-0.0515	-0.0022	-0.0012
	(0.158)	(0.208)	(0.005)	(0.001)	(0.175)	(0.416)
Event $\times \mathbb{1}_{i,c}^{\text{Indirect}}$	-0.0726	-0.0484	-0.0236	-0.0272	-0.0015	0.0000
	(0.052)	(0.162)	(0.061)	(0.046)	(0.028)	(0.966)
$\mathbb{1}_i^{\text{Direct}}$	-0.0063	-0.0051	-0.0064	-0.0051	-0.0016	-0.0011
	(0.004)	(0.004)	(0.004)	(0.005)	(0.196)	(0.378)
$\mathbb{1}_{i,c}^{\text{Indirect}}$	-0.0051	-0.0037	-0.0051	-0.0037	-0.0014	0.0000
	(0.000)	(0.003)	(0.000)	(0.003)	(0.368)	(0.988)
Region-Time	Yes	Yes	Yes	Yes	Yes	Yes
Maturity-Time	Yes	No	Yes	No	Yes	No
Currency-Time	Yes	No	Yes	No	Yes	No
N	1,011,311	904,945	1,011,311	904,945	1,011,311	904,945
R^2	0.308	0.181	0.308	0.181	0.308	0.181

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 Supplemental Tables

Table D.1: **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 level and report p-values in parentheses.

	Δ yield Direct	Δ yield Indirect	Δ credit spread Direct	Δ credit spread Indirect
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}}$	0.0588 (0.129)		0.1106 (0.002)	
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_{i,c}^{\text{Indirect}}$		0.0512 (0.103)		0.1072 (0.008)
$\mathbb{1}_i^{\text{Direct}}$	-0.0071 (0.000)		-0.0059 (0.000)	
$\mathbb{1}_{i,c}^{\text{Indirect}}$		-0.0062 (0.000)		0.0045 (0.000)
Fixed Effects	Region Maturity Currency	Region Maturity Currency	Region	Region
N	180,821	573,389	176,507	494,387
R^2	0.003	0.003	0.002	0.002

Table D.2: **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 cluster standard errors at the country level and report p-values in parentheses.

	yield (R1)	credit spread (R1)	yield (R2)	credit spread (R2)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}} \times S_{c,t}^E$			-0.0472	-0.0150
			(0.000)	(0.007)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_i^{\text{Direct}}$	0.0886	0.0524	-0.0472	-0.0149
	(0.004)	(0.067)	(0.036)	(0.225)
$\mathbb{1}_t^{\text{Event}} \times \mathbb{1}_{i,c}^{\text{Indirect}}$	0.1258	0.0888		
	(0.022)	(0.054)		
$\mathbb{1}_i^{\text{Direct}} \times S_{c,t}^E$			-0.4717	-0.8450
			(0.078)	(0.000)
Fixed Effects	Region	Region	Region	Region
	Bond	Bond	Bond	Bond
	Maturity		Maturity	
	Currency		Currency	
N	2,722	2,552	1,152	1,146

Table D.3: **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.

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

The nested specification is estimated on all bonds. The Direct (Indirect) specifications use only the direct (indirect) treatment group and the control group. 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
N	1,013,640	440,163	832,811

E Proofs

E.1 Proof of Proposition 1

The result is immediate from equation (11). $\qquad\square$

E.2 Proof of Proposition 2

The result is immediate from equation (13). □

E.3 Proof of Proposition 3

The argument is in the text following the proposition. □

E.4 Proof of Proposition 4

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

$$\mathrm{d}p_i = \mathrm{d}e^{-y_i T_i} = -e^{-y_i T_i} T_i \mathrm{d}y_i = -p_i T_i \mathrm{d}y_i. \tag{26}$$

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

$$\Delta y_i \approx -\frac{\Delta p_i}{p_i T_i} \approx -\frac{1}{p_i T_i} \frac{\partial p_i}{\partial h_r} \Delta h_r. \tag{27}$$

Using equations (10) and (12) for $\frac{\partial p_i}{\partial h_r}$, we have the system in the statement of the proposition. □

E.5 Proof of Prediction 1

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

1. Part (i):

$$\left. \frac{\partial^2 p_r}{\partial D \partial h_r} \right|_{D_r > 0} = -\frac{\varphi h_r}{c} - \frac{z^*}{D} < 0. \tag{28}$$

50

2. Part (ii):

$$\left.\frac{\partial^2 p_f}{\partial D \partial h_r}\right|_{D_r>0} = -\frac{\varphi h_f}{c} < 0. \tag{29}$$

$\square$

E.6 Proof of Prediction 2

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

1. Part (i):

$$\left.\frac{\partial^2 p_r}{\partial D_r \partial h_r}\right|_{D_r>0} = -\frac{2h_r}{c} < 0 \tag{30}$$

2. Part (ii):

$$\left.\frac{\partial^2 p_f}{\partial D_r \partial h_r}\right|_{D_r>0} = -\frac{h_f}{c} < 0. \tag{31}$$

$\square$

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